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West Berlin Reactor.

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

HEARINGS

BEFORE THE

SPECIAL SUBCOMMITTEE ON RADIATION

OF THE

JOINT COMMITTEE ON ATOMIC ENERGY
CONGRESS OF THE UNITED STATES

EIGHTY-FIFTH CONGRESS

FIRST SESSION

ON

THE NATURE OF RADIOACTIVE FALLOUT AND
ITS EFFECTS ON MAN

MAY 27, 28, 29, AND JUNE 3, 1957

PART 1

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WASHINGTON : 1957

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FOREWORD

The public hearings on The Nature of Radioactive Fallout and Its Effects on Man had their origin in studies initiated over a year ago—in July 1956—by the staff of the Joint Committee on the general subject of long term radiation hazards, both from the military and peacetime atomic energy program.

During the summer recess, following the conclusion of the 84th Congress, the staff assembled background materials on fallout, with primary emphasis on the research aspects. Following official announcement of the hearings in March of this year a detailed technical outline describing the proposed scope and subject matter of the hearings was prepared by the staff. This outline and related problems were discussed informally with scientific experts in the fallout field. The outline and comments of the expert group were most useful and helpful in providing the necessary ground work for the hearings.

On April 18, 1957, a Special Subcommittee on Radiation under the chairmanship of Representative Chet Holifield of California was established to conduct the hearings and to look into radiation problems in general. The subcommittee, with the assistance of the committee staff, then set about the task of selecting a representative group of expert witnesses from the major scientific areas involved, and extending invitations to them to testify. An effort was made, in this regard, to achieve a balanced presentation and to provide an opportunity for the expression of varied points of view.

The hearings, which were all open to the public, were held on May 27–29 and June 3–7, and covered the major aspects of the fallout problem from its inception in nuclear weapons explosions to its effects on man. In all, some 50 witnesses either appeared personally before the committee or submitted statements for the record.

The staff has prepared a summary analysis of the hearings which is aimed at pointing up the more significant information which emerged from the hearings. This analysis does not cover all points that were discussed in the hearings. An effort was made to describe the general areas of agreement which developed and to delineate those areas in which unresolved questions still exist.

On behalf of the Joint Committee we would like to extend our sincere thanks to all the expert witnesses, who gave generously of their time and effort to make the hearings a success. We are also appreciative of the excellent support we received from the staff and from the committee's consultant, Dr. Paul Tompkins, technical director of the Naval Radiological Laboratory at the University of California, whose advice was most helpful in connection with technical questions which arose during the course of the hearings.

CARL T. DURHAM,
Chairman, Joint Committee on Atomic Energy.
CHET HOLIFIELD,
Chairman, Special Subcommittee on Radiation.

CONTENTS

(Pt. 2 contains further statements by leading scientists, together with statements and papers submitted for the record.)
(A detailed subject index is now being prepared and will be published at a later date.)

[1] Each witness was asked to submit a short professional biography. Most, but not all, of the witnesses did, or, as in the case of some Government witnesses, their departments did. In a few cases the biographies came from American Men of Science.
In some cases the JCAE staff has abbreviated or deleted from the biography, for brevity, no matter what the original source. These biographies are for information only and should not be regarded as a formal statement.

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

MONDAY, MAY 27, 1957

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION OF THE
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The special subcommittee met, pursuant to call, at 10:05 a. m., in the caucus room, Senate Office Building, Hon. Chet Holifield, chairman of the subcommittee, presiding.

Present: Representatives Holifield, Durham (chairman of the Joint Committee), Price, Cole, Van Zandt; Senators Anderson, Jackson, Hickenlooper, and Bricker.

Present also: Professional staff members, James T. Ramey, executive director, George E. Brown Jr., Paul C. Tompkins, consultant, and Hal Hollister, staff technical adviser.

Representative HOLIFIELD. The committee will be in order.

This is the opening day of public hearings by the Special Subcommittee on Radiation of the Joint Committee on Atomic Energy on the nature of radioactive fallout and its effect on man. The primary purpose of the bearing is to bring together in one forum competent scientific opinion on the various major aspects of the fallout problem. An effort has been made to have a well-balanced presentation, with witnesses representing varied points of view within the scientific community.

It is the committee's intention through the presentation of expert scientific testimony, to trace the fallout cycle from the moment of the nuclear explosion, through the scattering of radioactive debris in the atmosphere, its descent to the ground, and finally its effect on human beings, livestock, and agriculture. Each of the various scientific areas and disciplines involved will be considered in sequence and an attempt will be made at the conclusion of the hearings to bring together, through general discussion, some of the major points developed at the hearings. In particular, the committee hopes to be able to delineate those areas where we have knowledge from those where we have little or no knowledge, with a view to determining the areas of research which need more intensive effort.

It is not the purpose of the committee, in this set of hearings, to draw any moral, political, or philosophical conclusions; nor to get into other associated fields, such as disarmament. Nor is it our purpose at this time to cover in detail the question of hazards in connection with nuclear powerplants, or the matter of workmen's compensation for employee radiation hazards. These subjects might more appropriately be taken up in a subsequent series of hearings.

For the purposes of these hearings we will be devoting our attention mainly to the radioactive fallout problem from nuclear explosions and to a possible projection of what the hazards are. We will have before us in the next 2 weeks a representative group of top ranking scientists in their chosen fields who have generously consented to appear before the committee to give us their expert opinion on a problem which has caused untold concern and confusion among the people of this country and among our friends in foreign lands.

The suggestion has been made by the chairman to our scientific witnesses that their presentation be made in laymen's language as much as possible rather than in complicated technical terms. The committee hopes this will be possible so that the printed record of the hearings may be understood by persons from all walks of life. This request should not be interpreted as precluding the submission of statements for the record which by their nature must include technical terminology and formulas. We also recognize and hope that the hearings will provide valuable source material for students and professional scientists throughout the world.

The committee and staff will avail themselves of the opportunity to question witnesses during their presentation or at the conclusion of their testimony, in order to clarify points at issue or to expand the record on a particular subject. At certain times later on in the hearings we may wish to try the technique of having certain expert witnesses comment on the presentation of a principal scientific witness, with opportunity, of course, for rebuttal by the latter.

It is my sincere hope, and I am sure the hope of all the members of the committee, that these hearings will result in a better understanding of this difficult and complex problem. Such understanding is essential, it seems to me, if we are to develop sound national policies.

At this point, I would like to place in the record, without objection, the outline of the subject matter for the hearings which was prepared by Mr. Hal Hollister who is our technical adviser on the staff of the Joint Committee. I might say that the list of witnesses is not closed; it is subject to addition later on if the committee deems it necessary. We were also advised in the development of the outline and in the selection of witnesses by a representative group of scientists familiar with this field.

(The subject matter referred to follows:)

CONGRESS OF THE UNITED STATES

JOINT COMMITTEE ON ATOMIC ENERGY

April 29, 1957

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN—OPEN HEARINGS MAY 27–JUNE 7, 1957

GUIDANCE TO THOSE PRESENTING TESTIMONY

Scope of hearings

The hearings will deal primarily, but not exclusively, with the scientific (physical, geological, biological, and medical) subject matter associated with radioactive fallout. Matters of program, money and policy as they relate to the scientific research aspect will be dealt with. The hearings will also deal for background purposes with scientific subject matter in topics related to weapon-caused fallout such as fallout from reactor accidents, and the biological effects of radioactivity caused by events other than fallout.

Approach of hearings

The hearings are to educate the committee and the public about fallout, how it originates, what happens to it, why it constitutes a hazard, what our sources and methods of getting information are, how adequate our knowledge is, and what the program in research for the near future should be. The hearings thus amount to a report on the progress of research. But because fallout is of intense concern to the committee and the public from a hazards point of view, and because of differences of opinion as to the facts and conclusions as to fallout hazards and their control, a special effort will be made, by means of the hearings, to assemble and disseminate information that is understandable and useful.

The record of the hearings should help competent persons to make the following sorts of judgments:

(1) What actual experimental, clinical, or operational data is a given result based on;

(2) How good is existing data on a given subject;

(3) How good is our understanding of the phenomena for which data is being collected; how good are hypotheses used to relate data to arrive at results;

(4) What are the results, as opposed to the conclusions, of work to date;

(5) What data should be collected urgently, because such data might never again be available assuming a continuation of tests;

(6) What further should be done by way of standardizing definitions, assumptions, etc.;

(7) What information can specifically be earmarked for use by civil defense and other agencies as working information?

Plan of presentation

The following order of presentation has been worked out:

I. An organized presentation by expert witnesses chosen from qualified persons in particular scientific fields:

A. Presentation of "background statements" by experts in the field of physics, meteorology, geology, biology, and medicine, to be followed by questioning confined to and appropriate for the statement but not broadening the subject matter.

B. Presentation of detailed statements on specific topics that should be examined carefully because they are particularly relevant, important or controversial.

C. Presentation of testimony designed to pull things together and point out the impact of the situation as it stands:

(1) What we know, what we can predict and how surely we can predict it; what we don't know.

(2) What action might serve to change the situation.

NOTE.—Oral presentation by expert witnesses should attempt to present scientific data in a form understandable to informed laymen. More detailed and technical data may be submitted for the record to supplement oral presentation.

II. An open presentation by those working in the field, or interested members of the public who requested an opportunity to testify before the committee.

MAIN TOPICS OF THE ORGANIZED PRESENTATION

The organized presentation will be broken down into the following main topics:

I. Introduction.

II. Background information—Radioactivity and Radiation.

III. Background Information—Controlled Fission and Fusion Reactions and Their Potential as a Source of Hazard.

IV. The Natural Occurrence of Radioactivity and Radiation.

V. The Production of Radiation and Radioactivity by Detonating Nuclear Weapons.

VI. Atmospheric Transport, Storage, and Removal of Particulate Radioactivity.

VII. Local Fallout: The Mechanisms by Which It Can Affect Man and the Measures He Can Take To Minimize Exposure.

VIII. Delayed Fallout: The Behavior in Geological and Physical Processes and the Mechanism by Which Delayed Fallout Enters into the Biological Processes and Reaches Man.

IX. A Detailed Discussion of the Occurrence of Strontium 90 and Cesium 137 in the Atmosphere, Biosphere, and Its Uptake and Behavior in Man.

X. The Effects of Radiation on Man; Somatic Effects—Pathology; Genetic Effects; Methods and Standard of Radiation Protection as Applied to Fallout Problems.

XI. The Impact of the Present State of Affairs: Summary, Interrelationships, and Implications on Policy.

XII. The Impact of the Present State of Affairs: What should the Research Program in the Physical, Geological, Biological, and Medical Sciences Be?

CONGRESS OF THE UNITED STATES

JOINT COMMITTEE ON ATOMIC ENERGY

April 27, 1957

OUTLINE FOR OPEN HEARINGS: THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

May 27–June 7, 1957

("Oral" denotes topic to be presented before JCAE orally, but may be read from prepared text.)

("Insert" denotes topic to be presented by a prepared statement be inserted in the record but not presented orally.)

("Bibliography" denotes topic that should be covered, in addition to the above, by some references to the literature.)

Any one or all of these methods of presentation may be indicated for the topics below.

I. Introduction (oral).[1]

A brief discussion for JCAE orientation, along the following lines:

A. The general nature of radioactivity and radiation.

B. Why radioactivity, radiation, and nuclear energy are closely associated.

C. Man's relationship to radioactivity and radiation.

D. The general nature of the biological effects of radioactivity and radiation.

E. The nature of the impact of applying nuclear energy for man's benefit on the health of individuals and on the health and welfare of the population as a whole.

F. Some factors that might be considered in deciding whether or not the hazards associated with radiation and radioactivity are worth risking to try to get the benefits expected from applying nuclear energy.

II. Background information—Radioactivity and radiation (oral, insert if desired, bibliography if desired).

A. The nature of radioactivity and radiation.

B. Mass, energy, and radiation.

C. Quantum and corpuscular radiation:

1. Energy relationships, the radiation field, definition of roentgen.
2. Fundamental particles.

D. Reactions of radiation with matter:

1. Ionization and energy transfer:
   a. Definitions, quantitative relationships.
   b. Specific ionization.
   c. Chemical and physical changes.
2. Penetration, absorption, attenuation, etc. of radiation:
   a. Definitions, quantitative relationships.
3. Induced radioactivity.
4. Secondary radiations.

E. The phenomenon of radioactive decay:

1. Modes of decay, decay chains, etc.
2. Definitions: half-life, average life, decay constant, curie, relation between mass, half-life, and curie for different isotopes.

F. Neutron radiation:

1. Special characteristics.

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[1] To be preceded by opening statement by the hearing committee chairman to orient for the record the purpose, scope, and approach of the hearings.

G. Neutron fission and fission chain reactions:

1. Fission, number of neutrons released, energy spectrum, prompt and delayed neutrons, fraction of total.

2. Fission energy release, its nature and distribution by type (radiant, kinetic, potential, etc.), primary and secondary fission energy.

3. Fission products: Yield versus mass number, physical and chemical properties (particle size, vapor condensability, water solubility, etc.).

a. Radioactive decay of fission product mixtures, the simple models.

b. The limits of validity of the $t^{-1.2}$ law.

H. Nuclear fusion and thermonuclear processes:

1. Contrasts between fusion and fission, relationship to binding energy, energy and neutron release per unit weight of material consumed, etc.

2. Products and energy produced, including neutrons, gamma rays, and alpha particles.

3. Radioactivity of fusion products.

J. Particle accelerators and X-ray machines as sources of radiation and radioactivity:

1. The potential radiation hazards associated with accelerators.

III. Background information—Controlled fission and fusion reactions and their potential as a source of hazard (oral).

A fairly brief discussion for orientation, along the following lines:

A. Controlled fission reactions and nuclear reactors:

1. Types and characteristics of neutron chain reactions as employed in reactors.

2. Prompt and delayed neutrons and their role in control.

3. Other reactor characteristics that lend themselves to application for control.

4. Types and characteristics of reactors (from hazards standpoint):

a. Research, test, power, production.

b. Liquid fuel, solid fuel, homogeneous.

5. Sources of radiation and radioactivity hazards from reactor operation, associated chemical processing, and waste disposal, including liquid and gaseous effluents (bibliography, including recent AEC report).

B. Controlled thermonuclear reactions:

1. Contrast control problems with fission reactor situation.

2. Compare as potential source of radiation and radioactivity hazard.

a. Time schedule for controlled TN development.

b. Possible place in meeting power demand of future.

IV. The natural occurrence of radioactivity and radiation (oral, inserts, and bibliography).

A review and discussion of this topic, citing as appropriate such treatments as appear in the National Academy reports, the United Kingdom Medical Council report, the World Health Organization report (Sievert), the Government of India study, the statement by Dr. Warren Weaver (hearings, Foreign Relations Subcommittee, January 16, 1957), the British Journal of Radiology (29, pp. 409–417, 1956), and Dr. Libby's article in Science:

A. Naturally occurring radioactive materials and decay products and radiations.

B. Cosmic radiations, composition, characteristics, effect of altitude.

C. Spontaneous fission and induced radioactivity.

D. Measurement methods and limitations of the data.

V. The production of radiation and radioactivity by detonating nuclear weapons (oral, inserts, bibliography).

A. Description of nuclear weapon explosion:

1. Heat, blast, radiation, and neutron production in a bomb—rough models for scaling and attenuation.

2. Division of radiant energy into:

a. Prompt gamma and X-rays

b. Kinetic energy of neutrons—induce activity and cause direct damage

c. Potential energy which will manifest itself as—

(1) Direct fission product activity.

(2) Induced radioactivity (e. g. C^{14}).

B. The effect of weapon type and size from the point of view of fallout produced, both local and delayed.

C. The effect of the type of burst on the radiation and radioactivity resulting and on the fallout produced:

1. Air bursts.
2. Ground or surface bursts.
3. Underground bursts.
4. Underwater bursts.

VI. Atmospheric transport, storage, and removal of particulate radioactivity (oral, inserts, bibliography).

A. The types of fallout defined and described, and the conditions under which each type is produced—physical characteristics of particles, cloud formation, condensation, etc.

B. Local fallout:

1. The predictability of local fallout:
   a. Theory of predicting fallout.
   b. Models of radioactivity within the cloud (both Nevada and PPG), dependence on type and yield of weapon and type of scavenging material.
   c. Meteorological transport, examples of fallout under different winds, and in massive attacks.
   d. Uncertainties in model and meteorology.
   e. Weathering and redeposition of particles.
   f. Decay.
2. Observed patterns of local fallout:
   a. Patterns of external radiation in Nevada and the PPG.
   b. Radiation levels as a function of time. Radiation dose to unprotected persons as a function of time of fallout from fallout.
   c. Fractions of fallout observed locally.
   d. Factors affecting patterns of fallout.
   e. Physical, chemical, and radiochemical properties.
   f. Deposition of toxic materials.

C. Intermediate and delayed fallout:

1. The production and distribution of fallout in the atmosphere:
   a. Dependence on height of burst, yield, type of explosion, and scavenging material.
   b. Observed or inferred physical, chemical and radiochemical properties, with special reference to fractionation.
   c. Division of material: (local), tropospheric, and stratospheric; determining conditions.
2. Transport through and removal from the atmosphere:
   a. The stratosphere:
      (1) Transport, mixing, possible methods of removal.
      (2) Storage time; cumulative worldwide fallout as a function of time; predictions of future fallout from single event; from past weapons tests.
      (3) Sources of information; measurements and estimates of radioactivity in stratosphere.
   b. The troposphere:
      (1) Tropospheric removal processes; storage time:
         (*a*) Precipitation, interception, dry deposition.
      (2) Possible regions of unusual removal (fallout) because of meteorology or topography:
         (*a*) Exposure to prevailing winds and the sometimes accompanying effect, orographic rainfall.
         (*b*) Effect of large bodies of water on the distribution of fallout on adjoining land areas.
      (3) Meteorological tracking and other discussion of transport.
3. Observed deposition on the ground:
   a. Geographical distribution; dependence on physical factors
   b. Physical, chemical, and radiochemical properties, with special reference to fractionation.
   c. Measurement techniques and limitations of the data:
      (1) gummed paper, (2) gauze, (3) pots, collection of rainfall, and (4) soil samples.
4. Quantitative predictions of future fallout:
   a. From past tests.
   b. From future tests.

D. Interrelationships between study of fallout and study of meteorology; contributions of fallout studies to meteorology.

VII. Local fallout: The mechanisms by which it can affect man and the measures he can take to minimize exposure (oral, inserts, bibliography).

A. The relative importance of external radiation compared with internal radioactive emitters for the local fallout situation:

1. The source of the external radiation:
   a. Properties.
   b. Decay.
2. Internal emitters:
   a. Radioiodine, inhalation and ingestion.
   b. Other emitters.

B. Shelter and shielding and their effects.

C. Other immediate emergency measures that can reduce hazard.

D. Dose and dose-rate versus time.

VIII. Delayed fallout: The behavior in geological and physical processes and the mechanisms by which delayed fallout enters into the biological processes and reaches man (oral, inserts, bibliography).

A. The relative importance of internal emitters compared with external radiation in general for the long-run fallout situation (other than local fallout).

1. Factors of interest; criteria for deciding what radiation and which emitters should be worried about:
   a. Yield.
   b. Half-life.
   c. Physical distribution.
   d. Physical, chemical, and radiochemical properties.
   e. Uptake by plants from soil.
   f. Uptake by animals and man from diet, water, and air.
   g. Retention and distribution in man.

NOTE.—This topic up to this point is primarily an orientation for the detailed treatment to follow in the remainder of this and the following topics.

B. Deposition on and migration in soil and transport by surface waters.

1. Dependence on chemical properties:
   a. Strontium.
   b. Cesium.
   c. Plutonium.
   d. Rare earths.
2. The effect of river basin and ground water flow patterns; the effect of porous substructure such as Idaho lavas; the effect of inland sinks such as Salton Sea.
3. Effect of soil type and rock structure.
4. Mechanisms for fallout penetration into soil.
5. Decay.

C. The effect of agricultural practices on the distribution of fallout.

1. Possibilities for modifying agricultural practices.
2. Possibilities for liming soils.

D. The effect of fallout on water supplies for human, agricultural, and industrial use.

1. Possibilities for water treatment.

E. Possibilities for modifying present food collection and distribution and handling systems to guard against hazard.

F. Behavior in oceans mixing above thermocline, waste-disposal techniques, and ultimate effects.

G. Entry into biological processes, including man's food chain.

1. Deposition and retention on surfaces of vegetation.
2. Uptake by vegetation from soil.
   a. Characteristics for various plants and various radioactive materials:
      (1) Strontium.
      (2) Cesium.
      (3) Rare earths.
      (4) Plutonium.
   b. Dependence on soil characteristics.
   c. Other factors: decay, biological half-life, effective half-life.
3. Soil-plant discrimination factors.
4. Uptake by marine life and algae.
5. Uptake by animals, animal products, and man.

H. Retention and decay in animals and man.
1. Distribution of fallout in body tissues, fluids, milk:
a. Tendencies for localization:
(1) Radioiodine.
b. Dependence of equilibrium values on effective half-life.
2. Discrimination factors (preferential uptake of particular fallout products by particular species of plants, animals, and man); types and how measured or inferred:
a. Experiments for determining discrimination factors.
b. Numerical values for the various factors.
c. Combining values for the individual factors.

IX. A detailed discussion of the occurrence of strontium-90 and cesium-137 in the atmosphere, biosphere, and its uptake and behavior in man (oral, inserts, and bibliography). NOTE.—Sr-90 will be outlined in detail below.
A. Distribution, storage time, and fallout rate from atmosphere:
1. Combination of local and worldwide fallout of Sr-90 resulting from fractionation; long half-life; stratospheric holdup and mixing; decay.
B. Deposition on soil and plants—variations of Sr-90 level in environment as a result of weapons detonated in a relatively short period of time—from a few moments to 2 or 3 years.
1. Predicted fallout as a function of mixing and time.
2. Effects of retention of fallout on surfaces of vegetation.
C. The calcium model as a basis for predicting Sr-90 behavior.
1. Similarities and differences in behavior in the biosphere and in man.
a. How much do we know about calcium?
b. How much do we know about strontium?
2. Influence of amount of calcium in soil, diet; dilution and discrimination:
a. Dependence of occurrence of Sr-90 in animal and plant life on calcium in soil and diet.
b. Practicability of controlling occurrence of Sr-90 by adding calcium to soil and diet:
(1) calcium additives to milk.
c. Removal of Sr-90 from foods:
(1) calcium considerations.
D. Deposition in man—variations of Sr-90 level.
1. Function of age and time and location.
2. Observed occurrence in man and corresponding observed occurrences in soil and food.
E. Observed occurrence of Sr-90 in soil, food, and man (brief summary with detailed supplementary insert).
F. Predicted occurrence from weapons tests held prior to 1957:
1. Relation to accepted concentration standards (the basis of which is to be discussed later).

NOTE.—Cs-137 will be outlined below.
G. Distribution in the physical environment:
1. Half-life, stratospheric storage, chemical properties, similarities to potassium.
2. Deposition.
H. Occurrence in food supplies; probable sources:
1. Potassium model for discussing behavior of Cs-137 in biosphere.
J. Observed occurrence in humans—relationship to acceptable concentrations, on basis to be discussed later.
K. Predicted occurrence in humans as a result of weapons tests to 1957.

X. The effects of radiation on man (oral, inserts, bibliography).

SOMATIC EFFECTS—PATHOLOGY

A. Introduction and orientation: distinction between somatic and genetic effects, between acute effects of high-level radiation and long-term effects of low-level radiation and radioactivity, between damage per se and the standards developed to protect against damage.
B. Early effects of exposure of animals and man to external radiation:
1. Gamma and X-radiation: syndrome of radiation sickness:
a. Fallout on Marshallese: Rongelap, Uterik:
(1) Children recently returned to Marshall Islands.
b. Los Alamos incidents.
c. Other examples—radium.

2. Beta radiation—beta burns:
   a. Marshallese.
   b. Other examples—radium.

C. Early effects of exposure to internal radiation.

D. What are the criteria for picking out the harmful radioelements included in fallout?
1. How sure are we that all the harmful ones have been picked out:
   a. Strontium.
   b. Cesium.
   c. Rare earths.
   c. Plutonium.
   e. Iodine.

E. Delayed effects due either to single massive doses or to protracted chronic exposure; enumeration of effects of interest; dose dependence:
1. Examples.
2. Relationships between the two types of dosings.

F. Mechanisms and responses of man to radiation and radioactivity.
1. Briefly review chain of events:
   a. Physical effects.
   b. Biochemical and chemical effects.
   c. Cellular effects.
   d. Effects on whole organism.
2. Processes of physical interaction—physical effects:
   a. Significance of alpha, beta, gamma rays, and neutrons in the process.
   b. Significance of these rays with regard to penetration and whether introduced within the organism or arising from outside.
3. Chemical and biochemical changes:
   a. Direct effect of ionization on vital cell molecules.
   b. Indirect effects as a result of ionization of water in the presence of oxygen.
   c. Relationship and importance.
4. Cellular changes:
   a. Range of sensitivity of cells (list most sensitive—gonads—to least sensitive—nerve, muscle, bone).
   b. Relate sensitivity of nucleus to cytoplasm.
5. Effects on the whole organism:
   a. Range of survival dose on mammals (guinea pigs 200 r., rabbits 800 r.).
   b. Compare with nonmammalian radiation (virus, for instance, 1,000,000 r.).
      (1) Point out species variation and position of man.
6. Clinical syndrome in man (nausea and vomiting, hematopoietic depression, epilation, bleeding, etc.):
   a. Special place of hematopoietic response to radiation.
   b. Delayed effects.
      (1) Reduced longevity.
         (*a*) reduction in life expectancy—validity of concept at low levels of radition.
      (2) Production of leukemia and neoplasms (tumors).

G. Relationships of damage mechanisms to dosages.
1. Aplastic anemia, leukemia, and cancer as a result of exposure to radiation:
   a. Doses at which observable damage occurs; relationship of probability of damage to dose and dose rate; latent periods; doubling doses; relationship to tissue irradiated.
   b. Relative importance of cancer and leukemia under various conditions: external source; exposure of various critical organs to radiation from external and internal sources: lungs, gut, thyroid, skeleton, etc.

GENETIC EFFECTS

H. The nature of genetic effects: evidence, experience, data.

J. Relationship between radiation and change in mutation rate:
1. Natural mutation rate (2 percent).
2. Dose necessary to double mutation rate (50 r.).
3. Apparent linear nonthreshold relationship between dose and effects.
4. Cumulative character of genetic effects.
5. Mechanics of introducing and eliminating mutants in genetic pool

K. Predicted increase in mutation rate as a result of postulated increase in radiation levels from fallout:

1. Effects on population, as individuals and as a whole.

METHODS AND STANDARDS OF RADIATION PROTECTION AS APPLIED TO FALLOUT PROBLEMS

L. Standards for external-radiation effects: the concepts and definitions relating the amount of damage to the amount and kind of radiation causing the damage:

1. Definitions and concepts behind the units used for dose rate, cumulative dosage, biological effectiveness, etc.: the r, rad, rem, RBE; ionizing density; linear energy transfer.
2. Kinds of radiation and varying conditions of exposure.
3. Simplifying assumptions to get practical standards.
4. Calculation of dose and dose rate resulting from several kinds of radiation acting together.
5. Calculation of dose and dose rate resulting from one or several kinds of radiation acting on different parts of the body.

M. Standards for internal-radiation effects:

1. Definitions and concepts behind the measurement of internal body-burdens and dosages; the models used:
   a. Maximum permissible concentration.
   b. Safety factors:
      (1) Population versus occupational dose.
      (2) Young versus adult.
      (3) Whole body versus localized dose.
   c. Relationships between internal and external radiation dose.
2. Calculation of cumulative dose rates and dosages from external radiation and internal radiation of various kinds and under various conditions of exposure.

N. Philosophy: the assumptions and models behind the establishment of the standards:

1. Historical trends to the present and trends for the future.
2. The validity of the assumptions now used in the light of up-to-date knowledge.
3. Possibility of hazards resulting from low-level exposure: threshold considerations:
   a. Why do we not know whether or not there is a threshold for each of the various radiation effects of interest? How and when can we improve knowledge on this point?
   b. A radiologist may believe that the existence of a threshold is probable or he may not—what are the pros and cons?
   c. What about the acceptability of the currently recommended standards under either belief of (b)?
4. The probable trend of the standards for the future:
   a. Will new standards have to be developed to cover certain hazards not now adequately protected against?
   b. Are the standards defined in such a way that they can be ranked for any given situation so that the proper standard among several can be chosen to give the least likelihood of hazardous exposure? Is there any ambiguity if several standards apply?
   c. Do there now exist, or are there likely to be, different standards in use by the United States and other governments, or by the United States and States and municipalities?

SPECIFIC QUESTIONS FOR DISCUSSION

A. All low level effects are extrapolations from high level effects. How secure is this extrapolation? Discuss its relationship to the nonthreshold character of genetic effects, and to the question of threshold of bone cancer.

B. Are there any distinctions between temporary and permanent (long term) damages, between repairable and irreparable damage?

C. Are there special criteria for small groups of persons as compared with large populations with regard to radiation? Is there a difference between small and large populations? Between large populations and the whole population? Does the distinction apply only to genetic effects?

D. Discuss the known effects of radiation on such aspects of the human being as mental posture, personality, intelligence, other, etc.

E. Are there any chemical reinforcements of body defenses against radiation? What about drugs recently announced as being of possible use for X-ray dosages?

F. What is to be gained or lost by the recordkeeping recently proposed for each person covering his lifetime history of radiation dosage?

G. How much radiation and radioactivity was man naturally exposed to and medically exposed to before weapon firing began?

H. Are the dosage standards for individuals and populations adequate for Sr-90? What are the factors for genetics, skeleton age, age of individual, health of person, etc.?

J. How adequate is the radium model as a basis for predicting Sr-90 damage in man?

K. What is the behavior of radioiodine in man from a damage and dosage point of view? What about Cs-137, C-14, etc.?

L. Is the biological effect of Sr-90 and its daughter Y-90 similar or the same as that of external radiation of any sort?

XI. The Impact of the Present State of Affairs: Summary, Interrelationships, and Implications on Policy (oral, inserts, bibliography).

A. In essence, what is the state of our knowledge in the areas discussed as relevant to the "fallout problem"; what do we know about:

1. The amount of radiation and radioactivity released by weapons fired to date.
   (a) by the United States.
   (b) by others.
2. The amount of local and delayed fallout created by these weapons.
3. Where this fallout is:
   (a) How much has decayed.
   (b) How much has fallen out and where.
   (c) How much is still up there and where.
4. What has happened to the ground fallout that has fallen out:
   (a) How much got on or in soil and where.
   (b) How much got on plants.
   (c) How much got in the ocean.
   (d) How much got elsewhere.
   (e) How much of all this has decayed after it fell out.
   (f) How much has directly affected man as external radiation.
   (g) How much as internal radiation.
5. The mechanisms by which fallout gets distributed in the atmosphere and on the earth.
6. The mechanisms by which fallout gets into the biosphere and to man—or gets to man directly.
7. The mechanisms by which exposure to fallout leads to damage.
8. The amount of damage, if any, that man has so far sustained from fallout.
9. The mechanisms and measurement of biological damage from radiation.
10. The relationships between damage and dose.

B. Using the knowledge now available, how well can one predict—and how would one predict—the following:

1. The amount of fallout still to fall out from weapons already fired.
2. Where this fallout will fall out.
3. What will happen to it:
   (a) How much will decay or otherwise be harmless.
   (b) How much will directly affect man as internal or external radiation.
   (c) How much damage, if any, man will suffer from it.

C. Using the knowledge now available, how much information does one need to postulate concerning the characteristics of future weapon firings (test or war) so that one could predict with a certainty appropriate for policymaking purposes the same sort of information as discussed above for future firings?

1. Is such a prediction possible even assuming unlimited information concerning the firing characteristics? How would it be made?
2. Is a postulated rate of firing (yield per unit time) meaningful? What in principle does "present rate of firing" mean? Is a postulated rate of firing sufficient information by itself for making the sort of prediction named here?

3. How does one take into account such problems as divers sites of firing, firing of weapons whose characteristics are not known, differences in weapon type and burst?

4. Are the present criteria for biological damage adequate and are the related measurements adequate so that one could predict with a certainty appropriate to policymaking the future hazard, if any, owing to future weapon firings—even if he could forecast how much fallout there would be and what would happen to it? If the criteria are adequate, how are they put together?

5. If one had before himself a working definition of hazard that was satisfactory from a moral or ethical, social, political, and economic point of view, and if this definition was stated in terms of measurable or observable phenomena in nature (including man), does sufficient information exist so that he could determine, again with a degree of certainty appropriate to policymaking, whether or not a hazardous situation exists now or will exist in the future for various possible circumstances of weapon firings and radioactive fallout? Could he determine the degree of hazard? If their answers are "No," is it possible to state what information is lacking and how it might be obtained?

XII. The impact of the present state of affairs: What should the research program in the physical, geological, biological, and medical sciences be? (oral, inserts, bibliography).

A. Information sources and distribution.

1. Do and must private research groups depend on the Government, particularly the AEC, for most of their data? To what extent does the depth and breadth of the research program rest on what the Government is doing and on what the Government is willing to turn over to private research institutions?

2. Is scientific information adequately and promptly distributed and available?

3. To what extent are Government classification and other information-withholding mechanisms interfering with the distribution of information to the public and to scientific groups?

4. How much and what kind of data on radioactive fallout remain classified? What justification is given by AEC and other Government agencies for continued classification of such information? How much effort does the Government make to let it be known that material has been declassified after that action has actually occurred?

5. Is information exchange occurring properly between the United States and foreign countries and the U. N.? Is the United States adequately represented on international scientific and policymaking groups related to this problem?

B. The research program: What is the extent of research on radioactive fallout?

1. Is the AEC presented with a conflict of interest when it is required to act on the one hand as an agent in developing nuclear weapons, and on the other hand as an agent in providing safeguards against weapon hazards? If a conflict does exist, what would be effective ways of removing or at least minimizing it?

2. How much of the research is being done by the Government and how much by private research groups under Government sponsorships and with Government funds?

3. Are there serious soft spots in either the experimental or theoretical aspects of the sciences related to fallout; in particular are there any that limit a thorough understanding of the civilian and military implications of fallout?

4. How well is the research program in balance?

5. Is the general level of the research program adequate in view of the obvious policy implications of fallout in such areas as weapons testing, nuclear weapons bans, civil defense, the military posture?

6. Is the scope of inquiry on fallout problems broad enough so that it is not likely that the United States could be surprised by an enemy using the properties of fallout in a manner that we have no notion of how to cope with?

7. Is the atmospheric, biospheric, and medical sampling program adequate? Should more work be done, for example, on determining the normal incidence of bone cancer in areas of various background levels?

8. What, if any, data should be sought after urgently on grounds that it may never again become available assuming tests continue; that is, what virgin data and what check points should be found?

9. Should the United States prepare, through cooperative programs, to process fallout samples from all parts of the world?

10. Are Federal funds made available for fallout research adequately protected?

11. Is cooperation between Government and non-Government research adequate?

12. If the program is inadequate, should Congress increase appropriations for fallout research?

C. JCAE information.

1. Should the results of fallout research be made available to and reviewed by the JCAE as well as the AEC?

2. Would the creation of a special group of scientists be an effective way of reviewing information and resolving differences of opinion?

Before announcing our leadoff witness, I would like to announce that this afternoon we will have the subjects of The Production of Radiation and Radioactivity by Detonating Nuclear Weapons, and Atmospheric Transport, Storage and Removal of Particulate Activity, and as witnesses we will have Dr. Alvin C. Graves, Los Alamos Scientific Laboratories; Dr. Frank Shelton, Armed Forces Special Weapons Project; General Alfred B. Starbird, Division of Military Application of the Atomic Energy Commission; and Dr. W. W. Kellogg of the RAND Corp.

Leading off today will be Dr. Dunham, Director of the Division of Biology and Medicine of the Atomic Energy Commission. First, I would like to qualify Dr. Dunham for the record.

I see you received your M. D. at the Rush Medical College of the University of Chicago, that you taught there for many years, and that you have served in various capacities with the Atomic Energy Commission since its establishment in 1946. Most recently you have been Chief of the Medical Branch and then Deputy Director, of the Biology and Medical Division of the AEC.

We are glad to have you with us today, Dr. Dunham. As I understand it, you are going to give a brief general background of the problem. Would you please proceed.

STATEMENT OF DR. CHARLES L. DUNHAM, DIRECTOR, DIVISION OF BIOLOGY AND MEDICINE, ATOMIC ENERGY COMMISSION [1]

Dr. Dunham. Mr. Chairman, I appreciate very much the privilege of being called to testify before your subcommittee on the very important subject of the nature of radioactive fallout and its effect on man. In my own capacity as Director of the Division of Biology and Medicine of the Atomic Energy Commission, I feel that I can properly claim a certain perspective on this subject which may be helpful to you as you prepare to hear testimony from the outstanding technical experts invited to detail for you from their own researches and experience the many facets of this problem.

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[1] Date and place of birth: December 28, 1906; Evanston, Ill. Education: Bachelor of arts, Yale University, 1929; doctor of medicine, Rush Medical College, University of Chicago, 1933. Work history: University of Chicago, assistant and instructor, 1936–43; U. S. Army Medical Corps, medical officer; chief, Preventive Medicine Section, 1943–46; University of Chicago, assistant professor of medicine; medical consultant, 1946–49; Atomic Energy Commission, Assistant Chief, Medical Branch, Division of Biology and Medicine, 1949–50; AEC, Chief, Medical Branch, Division of Biology and Medicine, 1950–54; AEC, Deputy Director, Division of Biology and Medicine, 1954–55; AEC, Director, Division of Biology and Medicine, 1955—. (Submitted by Atomic Energy Commission.)

The one property of fallout from nuclear weapons which is of interest in connection with these hearings is its radioactivity. Radioactivity, of course, is not new. While it has been recognized by man for only the last 60 years, it has always existed in some degree throughout the earth—in rocks and soils, in the water we drink and in the food we eat, and of course, in our own bodies.

Perhaps the radioactive material best known to the public before the advent of strontium 90 was radium, of which the most commonly used radioisotope is radium 226. Radium, concentrated from certain ores, has long been an article of commerce. Its interest to man has been almost entirely due to its radioactivity; that is, to the fact that its atoms spontaneously change into atoms of another chemical element and, in so doing, emit radiation from their nuclei. Radiation from radium and from its radioactive decay products is used to activate the luminous dials of our alarm clocks and our wristwatches; in sufficient quantity it may be used to treat cancer or to test the integrity of a welded steel joint.

We recall also that, although small quantities of radium have always been one of the constituents of the human skeleton, a few years after the First World War the public was shocked that many painters of luminous dials and other radium workers were dying as a result of radiation from radium in their skeletons—from skeletal contents of radium thousands of times as large as those which occur naturally. In the ensuing 30 years, the radium industry has achieved high standards of safety in the handling of radium, while radiobiologists have developed an imposing amount of information concerning the effects of radiation on man and on other forms of life. Yet the phenomenal increase in the importance to man of radiations from radioactive materials from other sources during the past 15 years have made drastic demands upon this knowledge. The result is that, while the best informed radiobiologists are keenly aware of the limits of our knowledge, many persons with various degrees of understanding of the relationships involved are apprehensive, and many others are confused.

I believe it is the purpose of this hearing to examine as critically as possible the biological implications of possible increases in the exposure of man to radiations from radioactive materials and from other sources. The most immediate interest, since for various reasons this has been given the greater publicity, is exposure to radiations from those particular radioactive materials which have resulted, or may be expected to result, from the detonation of nuclear weapons.

Before the hearings go into detailed technical discussions, I would like to spend a few minutes trying to give some feeling for the nature of radioactivity, of radiation, and of the biological effects of radiation. While there are several hundred different radioactive isotopes, or species of atoms, they account for only 3 or 4 kinds of radiation of interest in these discussions. These radiations are commonly called ionizing radiations, because the amount of energy contained in a single unit of the radiation as emitted by an individual atom, is sufficient to disrupt or ionize other atoms by separating electrons from them.

One type of radiation which will be of frequent interest in these discussions has many of the characteristics of visible light. It is commonly called X-radiation or gamma radiation, depending upon its origin. X-rays are well known for their ability to penetrate relatively

large thicknesses of materials. Variations in penetration depending upon the density of materials are the basis for the diagnostic use of X-rays in medical practice and for their use in radiograph. Two other types are more like particles, much smaller than atoms but moving with speeds measured in thousands of miles per second.

Most of us have difficulty in comprehending the significance of the very large and very small numbers required to describe atomic processes. One's reaction to the statement that the radioactive materials normally in the body cause millions of such missiles to plow through the body each day may be terrifying, or dry, matter-of-fact, depending upon his knowledge of chemical and physical processes. It is much less impressive to say that as a result of this number of missiles moving through the cells of the body an average of 1 out of every 100 billion molecules is affected per year. In general, the effects on living cells of ionizing radiations are chemical in nature. Most of the chemical reactions which are induced differ in no way from the normal chemical reactions which occur in day-to-day body processes. Those induced by natural radioactivity are very few in number when compared with what is going on all the time. On the other hand, they are completely random in nature and consequently do not serve a physiological or useful function. Consequently, a basic question to be considered in these hearings is how much increase in the frequency of these random occurrences is required to represent a significant hazard to human welfare?

This question quite properly raises another. Why should we have any increase in our exposure to radiation? Let us stop a moment and consider one of the basic facts of life, namely, to attain any specific objective almost invariably involves the surrender of something else. Reluctance to face this issue is commonly described as wanting to eat one's cake and have it too.

The development of nuclear energy and the production of radioactive materials are insepaable. With moderate effort, we can prevent release of a large fraction of these materials. With greater effort we can further reduce the release. However, if we continue to reduce the fraction we are willing to release, we eventually reach a cost of control which makes the operation prohibitive. At some point we must balance the undesirability of further increase in the quantity of radiation to which we are subject against the benefits to be anticipated from whatever application of nuclear energy we are considering. To say that we will tolerate no increase in exposure to radiation is equivalent to saying that we will make no use of nuclear energy.

This dilemma applies equally well to the use of radiation in medical practice. A physician uses X-rays to diagnose an injury or a disease because he believes that the benefits to the patient outweigh any detrimental effect of the X-rays. Although the X-ray doses required for diagnosis are generally small, a physician could not justify exposure of the patient without some reason. A physician may use much larger doses on a patient suffering from cancer—in fact he may use doses which in themselves may induce cancer. The treatment is obviously justified when one weighs the anticipated benefits against the possible undesirable effects. In this case, the question is quite complex because, while some of the undesirable effects may be certain, the cure of the disease may itself be in doubt.

The production of greater or lesser amounts of radioactive materials is an inevitable result of nuclear explosions. In succeeding testimony, witnesses will describe the processes by which such materials are disseminated in the atmosphere and are eventually deposited on the surface of the earth where they contribute to the exposure of man to radiation, both from locations outside the body and as a result of ingestion of radioactive materials which have gained access to the food supply. You will be given factual data on present levels of fallout radioactivity on the ground and in our bodies. Other witnesses will discuss in detail the present status of our knowledge of the biographical effects of large and small doses of radiation, however incurred, and the bases for some of the differing views of the effects of very small doses of radiation.

I believe that one of the objectives of your committee in holding these hearings is to develop some basis for judging how undesirable any particular quantity of fallout might be. There is no question, of course, that any quantity of radioactive fallout is undesirable, just as any quantity of exhaust gases is undesirable. Likewise, there is no question that as we proceed downward from lethal amounts of fallout, such as might be widely experienced in a nuclear war, to no fallout at all, we pass through degrees of undesirability which decrease progressively to zero. In these discussions, we are seeking standards of evaluation which can aid us in weighing the degree of undesirability of any given level of fallout against the advantages which may be anticipated from activities which are inevitably accomplished by fallout.

Although the testimony which you have scheduled for these hearings does not include the major considerations which must be weighed against the undesirable nature of fallout in order to justify any particular amount, I am sure that neither the members of your committee nor individual witnesses can avoid thinking of fallout in such terms. It is my hope, and I am sure it is yours, that during the technical presentations of the biological effects of radiation, the discussions of opposing theories concerning these effects will not become confused by the controversial aspects of other policy considerations.

Thank you.

Representative HOLIFIELD. Thank you, Dr. Dunham, for your statement.

Dr. DUNHAM. Mr. Chairman, may I introduce into the record certain documents and papers which Dr. Libby has asked me to bring to the committee?

Senator ANDERSON. Can he indicate what they are, Mr. Chairman?

(The list of material referred to follows:)

1. Strontium 90 concentration data for biological materials, soils, waters, and air filters. Project Sunshine bulletin No. 12. By E. A. Martell. August 1, 1956. [AECU-3297 (Rev.).] (See p. 617.)
2. The Chicago sunshine method. Absolute assay of strontium 90 in biological materials, soils, water, and air filters. By E. A. Martell. May 1956. (AECU-3262.)
3. Radioactive strontium fallout. By W. F. Libby. Proceedings of the National Academy of Sciences, vol. 42, June 1956: 365–390. (See p. 1468.)
4. Current research findings on radioactive fallout. By W. F. Libby. Proceedings of the National Academy of Sciences, vol. 42, December 1956: 945–962. (See p. 1494.)
5. Radioactive fallout. By W. F Libby. Remarks before the spring meeting of the American Physical Society, Washington, D. C. April 26, 1957. (Proceedings of the National Academy of Sciences, in press.) (See p. 1519.)

6. Dosages from natural radioactivity and cosmic rays. By W. F. Libby. Science, vol. 122, July 8, 1955: 57–58. (See p. 1459.)
7. Radioactive fallout in the United States. By Merrill Eisenbud and J. H. Harley. Science, vol. 121, May 13, 1955: 677–680.
8. Radioactive fallout through September 1955. By Merrill Eisenbud and J. H. Harley. Science, vol. 124, August 10, 1956: 251–255.
9. Strontium 90 in man. By J. L. Kulp, W. R. Eckelmann, and A. R. Schulert. Science, vol. 125, February 8, 1957: 219–225. (See p. 694.)
10. Worldwide travel of atomic debris. By L. Machta, R. J. List, and L. F. Hubert. Science, vol. 124, September 14, 1956: 474–477. (See p. 162.)
11. Project Sunshine. Worldwide effects of atomic weapons. The RAND Corp. August 6, 1953. [R–251–AEC (Amended).]

Representative Holifield. This material will be received for consideration by the committee, and its staff, with the disposition to be determined by the committee.

Dr. Dunham. Thank you very much.

Representative Holifield. Are there any questions of Dr. Dunham at this time from members of the committee?

Representative Cole. Yes, Mr. Chairman.

Representative Holifield. Mr. Cole.

Representative Cole. Dr. Dunham, you indicated that man first discovered the existence of radioactivity 60 years ago. Would you mind indicating very briefly what the circumstances were in connection with that discovery?

Dr. Dunham. There were two circumstances. One, of course, was Bequerel's discovery of radioactivity in uranium ores, in which he was studying fluorescence and laid a piece of uranium ore against a photographic film. He left it in his drawer for several years, and found the film was faulty.

The other was the discovery of roentgen rays by Roentgen, who was at that time doing research with cathode rays.

Representative Cole. When was radium first discovered, or realized?

Dr. Dunham. About 1896. I do not recall the exact date. But the radioactivity of uranium was discovered about that time, and within a year or two Madame Curie was working industriously on separating out radium as the active principal.

Representative Cole. Radium was discovered along about the same time as the fact of radioactivity?

Dr. Dunham. That is right.

Representative Cole. In different elements was discovered?

Dr. Dunham. That is right.

Representative Holifield. Are there any further questions?

Representative Price. I would like to ask one question.

Representative Holifield. Mr. Price.

Representative Price. Dr. Dunham, in your statement you referred to the use of radium in painting luminous dials of watches. You made a general statement that there was found to be a number of the painters of these luminous dials dying from their work. Do you have any record of the number of such casualties?

Dr. Dunham. It was up toward a hundred. It occurred in the early twenties.

Representative Price. For what period of years were they engaged in the work before these casualties began to occur?

Dr. DUNHAM. Most of this work was done during World War I, painting dials not so much for watches, but for other military equipment. It was along about 1923 or 1924, I believe, that the first cases came to the attention of Dr. Martland.

Representative PRICE. Is there any way to give any appraisal of the amount of radiation which they were dealing with in that type of work, compared to the amounts of radiation which people in the atomic energy program are dealing with every day?

Dr. DUNHAM. Well, the quantities in total of radium were infinitesimally small compared to the amounts of number of curies we deal with today. On the other hand, the amounts they ingested were hundreds, and probably thousands, of times what are considered a maximum permissible body burden today.

Representative PRICE. Can you give any estimate on the relative comparison of the record of casualties as between the two different programs?

Dr. DUNHAM. We do not believe that there is anybody associated with the Atomic Energy Commission program, or the Manhattan District program who will have any such outcome as a result of ingestion of radioactive material. There are a few persons in the early days who got a little more plutonium, who were just above the permissible body burden.

Representative PRICE. What is the reason for the greater evidence of safety in the handling of greater quantities of radiation now than in the early days in the painting of luminous dials, used in very small quantities?

Dr. DUNHAM. I think it was the experience in the luminous dial industry that made everybody particularly careful.

As some of you will recall, the problem of internal emitters, specifically with reference to plutonium, was the most troublesome one, and the one which more effort was put into to control by very elaborate ventilating procedures, use of dry boxes, and the like.

Representative PRICE. That is all I have, Mr. Chairman.

Representative HOLIFIELD. Senator Anderson?

Senator ANDERSON. Dr. Dunham, I have seen an advance copy of a magazine article that is soon to appear in a very reputable magazine, and I do not want to spoil it for the magazine by going into details. But, among other things, it suggests that when these rounds of reactors are finished, that they will regularly discharge in the atmosphere considerably more radioactive fission products than many of the hydrogen-type bombs. Will there be a place in this hearing where that can be considered? Is it something that should be evaluated and explored?

Dr. DUNHAM. I do not know what the wishes of the committee are on that. It was our understanding that the hazards from reactors would be touched upon somewhat by Dr. Mills, who, I believe, follows me. But I do not think there is any place, as far as I know. Mr. Hollister would know the answer there.

Representative HOLIFIELD. The Chair might say that we do not intend to explore the hazards from nuclear reactors in detail in this particular set of hearings. However, we are certainly not precluded from going into it at a later time.

Dr. DUNHAM. Yes.

Representative HOLIFIELD. But we will, of necessity, have to refer to this type of radiation from time to time.

Dr. DUNHAM. Yes.

Senator ANDERSON. Do you think it would be possible to get a scientific appraisal of the total amount of radioactive fission products to be put into the atmosphere when Commonwealth Edison gets its reactor running, and when Consolidated Edison gets its reactor running, and when Detroit Edison gets its reactor running, and when we have a dozen other operations of this 100,000-kilowatt size?

Dr. DUNHAM. I think once the actual final design has been settled on, the characteristics known, it will be relatively simple to calculate that.

Senator ANDERSON. The design for Detroit-Edison must be pretty well decided, since they have broken ground for it. They are going to break ground for the Commonwealth Edison plant about June 14 or June 19. And Consolidated Edison is going to build. Could not somebody evaluate that?

Dr. DUNHAM. I think it could be done.

Senator ANDERSON. Has your office made any evaluation?

Dr. DUNHAM. We would work with the reactor radiation hazards group on it. We would be very happy to evaluate some information here. You are talking about the normal operation of reactors?

Senator ANDERSON. Yes. There has been a good deal said about the testing of atomic weapons, and I have some worries, along with others, about that. If we can ignore testing because the daily run-of-the-mill operations of these reactors is going to place more radioactivity in the atmosphere than testing, he should pay some attention to that, too, should we not?

Dr. DUNHAM. I think one should. I would be very surprising if this were the case, however.

Senator ANDERSON. How would we go about getting a jury that would give some sort of answer that the common people can trust? You can get one group of scientists together, and they say one thing, and you get another group together, and they say another thing. What does a man who is not a scientist have that he can tie to?

Dr. DUNHAM. I think it is always important to ask your questions of the scientist who has worked diligently on the particular subject at hand.

Senator ANDERSON. I do not know who Dr. Pauling is out in California, but he has some ideas, and Mr. Muller has some ideas, and various people have ideas. Are they to be excluded just because they do not work for the Atomic Energy Commission, or have any contract from them?

Dr. DUNHAM. No; this was not my intention. I was merely suggesting, in the particular reference to the reactor problem that you ask the question of reactor engineers who can define the amounts of releases that will normally take place.

Senator ANDERSON. They do not have too much experience themselves in the operation of a reactor to show what might take place.

Dr DUNHAM. I think the Commission has had a considerable experience with a variety of experimental reactors—small, but still they have the same inherent characteristics of the larger ones.

Senator ANDERSON. Yes; they had experience with the prototype that went into the *Seawolf*, but when it got there it performed differently than all the prototypes had performed. Therefore, should we only trust this theoretical appraisal?

Dr. DUNHAM. The only other answer is that this will have to be proven as these reactors are started up. But I think some very useful information could be developed prior to that.

Senator ANDERSON. We had a meltdown of the EBR, and we have had some other things going on. I realize it is nice to say you must get your answer from the person who is in the field, but probably along the line somewhere they had to take some new ideas back as far as Einstein's whole theory.

Dr. DUNHAM. Surely.

Senator ANDERSON. There are a great many scientists that are worried, are there not, Doctor?

Dr. DUNHAM. I think there is no question about it.

Senator ANDERSON. Did you read the news story, I believe, this morning, that somebody was circulating a roundrobin among scientists who might express their views?

Dr. DUNHAM. I am aware of that.

Senator ANDERSON. How do you account for the fact that these people can read all this nice literature that the AEC has put out, giving them absolute assurance, and still be scientists, and they are still worried?

Dr. DUNHAM. I wonder if they are so worried about the fallout as they are about the spread of nuclear war?

Senator ANDERSON. You mean they are not sincere?

Dr. DUNHAM. No.

Senator ANDERSON. What do you mean?

Dr. DUNHAM. I would like to call your attention to what Dr. Arthur Holly Compton said the other day.

Senator ANDERSON. I saw that. Everytime one man comes out on one side, the AEC produces one on the other side, and we who stand in between are preplexed.

Representative COLE. The record will not show what Dr. Compton said, and I suggest you allow him to indicate that.

Senator ANDERSON. Go right ahead, Doctor. What did Dr. Compton say?

Dr. DUNHAM. I am afraid I no longer have it with me. I brought it over this morning.

Senator ANDERSON. I am sorry. It will probably be better, if Mr. Cole is willing, to have it come at a later time.

Representative COLE. I was going to ask, could not Dr. Dunham give his understanding of Dr. Compton's statement, not quoting him verbatim, but the substance of his statement. I do not know what it was, and I seek to know what it was.

Dr. DUNHAM. His statement was generally to the effect that he was as sincerely interested in peace as Dr. Schweitzer was; that is why he was in Stockholm with the World Federalist Movement, which is a peace movement. But he felt that Dr. Schweitzer has grossly exaggerated the hazards of fallout; and further, this was not the way to approach a permanent peace in the world, by simply exaggerating the hazards of fallout from what was tested.

Senator ANDERSON. We have many witnesses, and I do not wish to prolong this. I would just hope, Dr. Dunham, because we all have a great deal of confidence in you—and justifiably so—that you might

indicate how we might go about reaching a determination as to how the average man can have his fears allayed. He looks to two conflicting groups of scientists, and they tell diametrically opposite stories. It is pretty hard for one in between to decide which one is right, and which one is wrong.

That is all.

Representative HOLIFIELD. Are there further questions?

Senator Bricker.

Senator BRICKER. Just one question. You said, I believe, a moment ago, in your colloquy with Senator Anderson, that in your judgment there is no anticipated danger of fallout from the reactor program?

Dr. DUNHAM. That is right.

Senator BRICKER. How far could it go before there becomes a danger of fallout?

Dr. DUNHAM. I think one can have a great many reactors operating normally without any serious pollution of the world.

Senator BRICKER. There is a degree of pollution that come from normal operation, is there not?

Dr. DUNHAM. Yes, but relatively small.

Senator BRICKER. Relatively small. Excessive pollution would only come from a breakdown?

Dr. DUNHAM. From a specific accident in a reactor.

Representative HOLIFIELD. Mr. Durham.

Chairman DURHAM. Since the Government Agency of AEC under your direction is, of course, one of the prime agencies from which we can secure information that is worthwhile, would you give to the committee just how many you have in your dpeartment, the number, and how you evaluate the problem, and give the committee a little background?

Dr. DUNHAM. Yes.

In the Division of Biology and Medicine, we currently have 67 people, including secretaries, mail clerks, and the like.

As you know the Commission's operations are generally by contract. We also have related to us in the New York office, the Health and Safety Laboratory, which has about 70 or 80 people who actually do technical scientific work on the analyses of fallout material and the like.

This relatively small staff means that we have to work very closely with our prime contractors, with people in other agencies, and at our university projects in leaning very heavily on them for guidance in the actual development of information.

Chairman DURHAM. Can you give the committee the number of contracts you have with medical institutions throughout the country, actively working on this subject?

Dr. DUNHAM. I do not know that I can limit it to medical institutions.

Chairman DURHAM. Not necessarily that, but I mean people that are in the field of biology and medicine.

Dr. DUNHAM. The whole field?

Chairman DURHAM. The whole field.

Dr. DUNHAM. This would be a matter somewhere between 500 and 550 at the present time, from little contracts of a few thousand dollars a year, to very large ones.

Chairman DURHAM. Then you have under your direction over 500 scientists, we would call them, in the field of biology, as to the radiation hazards on man?

Dr. DUNHAM. No. In the first place, they are not under our direction in that sense. In the second, many of these people are working in the peaceful uses of atomic energy in the field of agriculture and the like. On the other hand, there is a great deal of work that goes on, say, in the treatment of cancer which is terribly important to our understanding of the effects of radiation.

Chairman DURHAM. But, as I recall, in the Commonwealth Edison matter, in regard to the reactor safety, I believe you introduced in the hearings—I forget how many—almost a hundred pages, or something like that, of testimony as to the safety of that under your direction. Is that correct?

Dr. DUNHAM. That was not under my direction, but it was a Commission activity.

Chairman DURHAM. The work was primarily done by you, was it not, and your staff?

Dr. DUNHAM. I am not familiar with the document you refer to. We have about a hundred projects strictly in this field, related to fallout, if that is what you are speaking of. But I am not familiar with just what document was introduced.

Chairman DURHAM. When you people down at the AEC laboratories receive a complaint, or make a study, say, for instance, as you did on the radiation dial case, how do you proceed? Do you assign a group of biologists to that particular complaint?

Dr. DUNHAM. We find people who are interested in the problem, such as Dr. Evans of Massachusetts Institute of Technology, and give him all the funds he needs to go ahead and get ahold of these people, study them, and come up with answers.

Chairman DURHAM. There is no delay when you do have a complaint as to a hazard in entering into an investigation as far as your department is concerned?

Dr. DUNHAM. As far as a specific hazard goes, that is a little different. What I have been talking about is this long-term approach to the thing.

If we are aware of a problem somewhere, we usually advise the Health and Safety Laboratory, or an operations office that a hazard is supposed to exist, and send somebody out to look into it right away.

Chairman DURHAM. That is all, Mr. Chairman.

Representative COLE. Mr. Chairman, following up Mr. Durham's inquiry, which I think is very, very apt, could you indicate the number of scientists who are engaged in connection with these hundred contracts which you say the Commission has, with varied persons, from one person, I judge, up to an institution that may have several persons?

Dr. DUNHAM. That is right.

Representative COLE. But you say there are a hundred contracts the Commission has for study in connection with the field of radioactive hazards?

Dr. DUNHAM. Yes.

Representative COLE. How many scientists do you estimate would be involved in that overall study?

Representative HOLIFIELD. The Chair might state that the staff has asked for a complete breakdown of the contracts and the men assigned to these different contracts, and that will be furnished to the committee.

Dr. DUNHAM. I will be glad to submit it for the record (see p. 1393).

Representative HOLIFIELD. If you wish to respond in general, go ahead.

Dr. DUNHAM. It would be a number of times the number of contracts.

Representative COLE. As many as 500?

Dr. DUNHAM. Somewhere between 300 and 500, I would assume.

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. Dr. Dunham, in addition to the efforts that the AEC is making in this field, do you know of any other effort in the United States?

Dr. DUNHAM. Yes. The Public Health Service, through its Environmental Health Laboratory at Cincinnati, and its engineering laboratory, is making very important contributions. The Bureau of Standards is making very important contributions. The various branches of the Department of Defense are making important contributions, and the Department of Agriculture.

Representative VAN ZANDT. Do you coordinate your efforts with these various agencies that you named?

Dr. DUNHAM. Very definitely.

Representative VAN ZANDT. With respect to private industry—do you know of any corporation or business firm sponsoring a research program in this field?

Dr. DUNHAM. How broad an organization do you have in mind? You mean to look into the whole problem of radiation safety?

Representative VAN ZANDT. Yes.

Dr. DUNHAM. I do not know of any which has a whole division directed to that effort, other than in our prime contracts with large organizations. Some of those companies have a real effort in their home plants too, but probably not comparable in size or scope to, say, what we have at Hanford in radiological safety.

Representative VAN ZANDT. Is it proper for me to assume that the information at your disposal reveals the best cross-section of effort is being made in this country at this time?

Dr. DUNHAM. I believe that is right, because we have access to all the work being done in other Government agencies, in close liaison.

Representative VAN ZANDT. That is all, Mr. Chairman.

Representative HOLIFIELD. Senator Bricker.

Senator BRICKER. There are many university research programs being conducted at the present time in this field of radiation hazards; are there not?

Dr. DUNHAM. Very definitely, some with AEC funds, some with Health Service funds, some with university funds, their own endowment.

Representative HOLIFIELD. Thank you very much, Dr. Dunham, for your statement.

I might say the next witness, Dr. Mark Mills, of the University of California radiation laboratory, is a man of distinguished scientific background. He comes from one of the university projects, I understand, which is contracted for by the AEC.

Dr. Mark Mills, will you please come forward.

Representative COLE. Could I ask that you give us a brief synopsis of Dr. Mills' background?

Dr. MILLS. Mr. Holifield, I wonder if I could use the blackboard. I think it will be a little more understandable.

Representative HOLIFIELD. For your presentation?

Dr. MILLS. Yes, sir.

Representative HOLIFIELD. All right, Doctor, go ahead.

STATEMENT OF DR. MARK MILLS, ASSOCIATE DIRECTOR, UNIVERSITY OF CALIFORNIA RADIATION LABORATORY [2]

Dr. MILLS. First of all I would like to thank you for the opportunity to speak before you. I believe this is a very welcome review of a problem that is troubling people.

I would also like to say two things about my testimony:

First of all, I will mention something about biological effects in order to tie the physical quantities to biological quantities. It was my understanding this part of the testimony was to be background testimony, and it is very helpful in fixing ideas if one ties these things together.

On the other hand, I want to be quite explicit and disqualify myself as a biological expert. So this is just to help to jell ideas and not to represent any expert understanding of the biological effects of radiation.

Representative HOLIFIELD. Doctor, we had asked you to give us background information on radioactivity and radiation. We understand that.

Dr. MILLS. One of the things in the outline has to do with the roentgen, which is close to biological effects.

Finally, I wanted to say I am going to give a descriptive picture of matter as current physics thinks of it, without trying to explain where all of the notions come from.

Representative HOLIFIELD. The Chair will again request, as he will with many of the witnesses, probably, that you make your presentation in lay terms as much as possible, so that the members of the committee can understand it.

RADIATION AND RADIOACTIVITY

Dr. MILLS. Yes, sir. I hope very much it will turn out this way. I have been trying to do it in that way.

1. The nature of matter

In order to understand radiation and radioactivity and its effect, it is helpful to understand how physics people think of ordinary matter, or the matter we are used to, which comes in many different chemical kinds.

Ordinary matter is made up really of five ingredients.

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[2] Date and place of birth: August 8, 1917; Estes Park, Colo. Education: Bachelor of science, California Institute of Technology, 1940; doctor of philosophy, 1948. Work history: Instructor, physics, California Institute of Technology, 1940–41; section chief, Jet Propulsion Laboratory, 1941–45; lecturer, jet propulsion, 1943–48; group leader atomic energy research department, North American Aviation, Inc., 1948–51; Director, Project SQUID, Princeton University, 1951–52; staff specialist, North American Aviation, Inc., 1952–53; group leader, theoretical physics, University of California Radiation Laboratory, 1953—. (Submitted by Witness.)

There are the three kinds of particles that you have probably heard of. One is the proton, which is the nucleus of the hydrogen atom. One is the electron, and one is the neutron.

These three kinds of particles are then joined together by forces to make up matter in various forms.

The forces that we know in nature are gravity, which is believed not to play any important role in ordinary matter—that is, it holds the stars together, but it does not hold the atoms together.

Electric forces, which we are used to. Originally there was lightning, I suppose, and later one has gotten a great deal of experience with electromagnetic forces in our large electrical industry.

These forces, then, are things man is used to, and are forces which man can see.

Finally, there are things called nuclear forces, of which we have only indirect knowledge, but we do know they surely exist.

Given then these three particles—the neutron, protron, and electron—and given the forces, nuclear and electric, we can then construct all the matter we know about.

I would like to sketch very briefly, then, some piece of matter.

First of all, there is the atomic nucleus, which contains neutrons and protons. It is customary in describing the atomic nucleus to give usually the number of protons, and the total number of particles, both neutrons and protons. The number of protons is given the symbol Z.

Representative HOLIFIELD. I suggest that you take the microphone in your hand, so that it will be easier for you to make your presentation.

Dr. MILLS. Yes, thank you.

The number of protons represented by the plus lines here, is called Z. Just count them up.

The number of neutrons represented by the little open circles is called N and the total number of particles in the nucleus is usually called mass number A. This is a sharp number. Its numerical value is very closely equal to the mass of the nucleus in the usual unit, but the actual mass and mass number are different things.

If you then take a nucleus and add to it electrons, as you are all used to, the symbol [indicating] with the electrons swimming around. This whole thing altogether is an atom.

In a neutral atom the number of electrons is equal to the number of protons, and the electrical charge, then, of the whole atom—since the protons are positively charged and the electrons are negatively charged, the total charge is zero, and the atom is neutral.

Now, I might mention a little bit how all these things look together. The neutrons have no electrical force associated with them, but they have a nuclear force. The protons have both a nuclear force and electrical force. The charge on them is what makes the electric force. So the neutrons and protons stick together by their nuclear force in this thing we call the nucleus, which is very small, 10 to minus 12 centimeters in size.

Then the electric force which the electrons and protons have for each other—the electrons feel no nuclear force—allows the electrons to be held attracted by the nucleus—the negative electrons are attracted by the positive charge of the nucleus and they hover around, making up the complete atom.

This, then, is a thing about 10 to minus 8 centimeters in size.

Now, if you stick several atoms together—here the little circles are supposed to be the electron shells, and the tiny circle in the middle is the nucleus. If I stick several atoms together, then I form a chemical substance or a molecule. Most of the matter we are used to is made up of molecules, groups of atoms. Essentially all of the chemical forces which hold molecules together, for example molecules of water, H_2O, and salt, NaCl, sodium chloride, have to do with juggling electrons. That is, the electrons tend to repel each other, but it turns out they can sort of see the two positively charged nuclei if brought near each other, and the net effect is that the atoms hold hands with electric force. So actually all of chemistry really has only to do with juggling electrons and the electrical forces.

2. The effects of radiation

Now I wanted to switch almost immediately to the problem of radiation and its effect.

One of the things one might do is describe one other situation, and that is the thing called an ion, and what is meant by ionization.

The simplest one, I think, is probably to look at a neutral atom. It, of course, happens with molecules, too.

Here is my nucleus. Here are a number of electrons all hovering around it. If I can disturb this some way—and I will return in a second to how—I can break off one of the electrons and essentially fracture the atom, but mainly in its electrical part, and have left then a fragment with a net positive charge. The remaining electrons will join up with other atoms.

This one [indicating] is transformed into these, and a number, 1 or 2 or 3 electrons. Each of these things that have a net charge is called an ion, and the process of making them is called ionization.

Now, radiation essentially can come from two places. First, of all, one can take any of these particles, the proton or the electron, and by various indirect ways, the neutron, and make them move very fast, and when they are moving fast, they are called radiation.

There is another kind of radiation which is called the gamma ray, and that is essentially a particle of light that Dr. Dunham spoke of, or radio wave. If it comes from an X-ray machine, that radiation is called X-rays. When a quantum of electromagnetic radiation comes from a nucleus, it is called a gamma ray. Instead of making radiation by machinery, it turns out that one can get radiation of this kind from the nuclei of unstable atoms.

Some of the atoms that are found on the earth naturally are already radioactive. Uranium, thorium, and radium are naturally radioactive atoms.

Essentially, somehow the atom is not happy the way it is, and ejects some of its nuclear particles, or possibly it makes and ejects an electron.

One can also by means of various machines, including things like a nuclear reactor or accelerator, make normal matter radioactive. This is called induced radioactivity.

I would just like to list the different kinds of radiation which I believe are pertinent for your review. There are some other kinds that are not very well understood, but I do not believe they are of practical concern here.

The normal radiations are alpha particles, beta particles, gamma radiation. I would like to add to this list, fission fragments, which I designated by "FF," and then the only other particle we really need, I believe, is the neutron.

Now, the alpha particle is essentially the nucleus of a helium atom. It contains 2 neutrons, 2 protons. Alpha particles emitted from actual radioactive materials like radium move at about one-twentieth the speed of light. Because of their strong electric field, when they go by, say, an atom or a molecule, this electric field can take hold of the electron on the atom, break it off, and now you have ionized this atom.

Representative HOLIFIELD. Dr. Mills, as you explain each one of these components, will you also give the half life, and explain what the term "half life" is, and also the power of penetration and the durability?

Dr. MILLS. Yes, sir. I beg your pardon. The half life I will come back to that—the half life is the property of the atom that is emitting the particle, rather than the particle itself.

I want to very quickly, if I could, list the particles and say something about them.

The alpha particles that occur naturally, and also occur mainly at the heavy elements of the atomic table—uranium, thorium, radium, protactinium, and so forth—generally have energies of 1 to 5 Mev. They have speeds of about one-twentieth of the speed of light. They go in air distances like 2 to 5 centimeters.

In tissue, which is of some interest for our current thinking, which I will say is approximately like water, these particles go essentially one one-thousandth of that distance, or .003 centimeter, say, or three-hundredths of a millimeter, and a very tiny distance.

Beta particles are also emitted from nuclei. They are very light. They are, of course, electrons of sometimes positrons. Beta particles are usually considered negative, just like this [indicating] that circles around the outer shell of the atom. Sometimes the nucleus can emit a positive beta particle, which is a special unstable particle which does not live very long in ordinary matter, and atoms are not made of positrons.

Essentially, these beta particles move with the speed of light—not quite, but very close to the speed of light when they are emitted.

For example, for 1 Mev. beta particle, can go distances like a few meters, say 3 meters, that is, 300 centimeters, about 10 feet, in the air, and in tissue they go a correspondingly shorter distance, about half a centimeter, say, in tissue.

Now it is convenient, in thinking about these, if you will forgive me, to bring in the fission fragment up here [indicating] and the gamma rays down there [indicating].

The fission fragments, which we will return to still again, are the fragments produced when a fissionable nucleus, such as uranium or plutonium, is fissioned by neutrons. The neutrons attack the nucleus, and the nucleus breaks in two.

The fission fragments weigh about a hundred mass units, and they carry energies like a hundred million electron volts, and, therefore, they move at about the speed an alpha particle moves, because their

mass and energy are up proportionately, and, therefore, their speed is about the same.

They will go about 2 centimeters in air, and, therefore, distances again like alpha particles, distances like three one-thousandths of a centimeter in tissue or water.

Now, all of these particles here—the alpha, beta, and fission fragments—have definite ranges. They are essentially a little projectile that goes plowing through matter until it runs out of gas, and then it stops and is not doing anything more.

The gamma ray, which is this little particle of light, does not have a definite range, and it interacts with matter in a hit-or-miss fashion (like a man running through a forest blindfolded—he will miss the trees for a while, and finally he smacks into one). Well, the gamma rays and neutrons both behave in this way in matter. They do not do much of anything until they more or less have a catastrophic collision.

Generally, the gamma ray interacts with electrons and turns its energy into fast beta rays. Fundamentally, this is what it does. So the effect of the gamma ray on matter is really the effect of beta rays on matter because it changes its energy into stirring up the electrons in matter until they make beta rays.

Chairman DURHAM. What causes that, Doctor?

Dr. MILLS. I think it is actually sort of visualizable this way, sir. I know these drawings are not very elegant, but you have this nucleus here [indicating], and here comes a gamma ray, and the standard picture one draws is a sort of a wiggly line. This gamma ray runs into the electron. Now, the reason the gamma ray and the electron interact is the electric charge on the electron. That is how the electron makes gamma rays itself, and how the gamma rays get hold of electrons.

The electric charge has to do—you see, the gamma ray is a piece of electromagnetic field, and when you move an electric charge around violently, you generate electromagnetic waves. It is sort of like paddling on the ocean and sending waves out. These are electromagnetic waves rather than ocean waves.

I beg your pardon. Was this helpful?

Chairman DURHAM. Yes, as far as I know.

Senator ANDERSON. We are certainly glad to have it in simple language.

Representative HOLIFIELD. Doctor, again I ask you, would you also give us the persistence of gamma rays as compared to beta rays?

Dr. MILLS. Yes, sir.

Representative HOLIFIELD. And their penetrability, their ability to penetrate matter, the ability to penetrate shielding?

Dr. MILLS. Yes, sir. I have not quite gotten it filled in.

Representative HOLIFIELD. All right.

Dr. MILLS. Gamma rays are able to go—let us talk about a 1-million-electron-volt gamma ray. In the air they go in the order of 10 to 100 meters, depending on their energy. In water they will go a distance of the order of 10 centimeters, and in lead they go distances of the order of 1 centimeter.

By that I mean this: They do not go a certain distance and then stop. They will go a certain distance and then there is a pretty good chance they will do something, and be stopped, or collected, or ab-

sorbed. But some of the special ones, with the poorer and poorer probability, can go greater distances than these characteristic distances I am talking about, tens of meters in the air, tens of centimeters in the water, and 1 centimeter in lead represents how far a strong beam of gamma ray goes before it is reduced to about half intensity.

Representative VAN ZANDT. What about concrete, Doctor?

Dr. MILLS. In concrete, depending on the gamma ray and kind of concrete, about 3 or 4 inches are required to reduce the gamma rays by half intensity.

Some of the gamma rays and neutrons are very clever, you see, and can dodge all of these things trying to trip them up and get clear through. On the other hand, alpha and beta particles really stop. If you get to the end of their range, they are completely stopped.

Representative HOLIFIELD. What is their effect if they hit human tissue?

Dr. MILLS. The gamma ray essentially converts itself into fast-moving electrons, and the fast-moving electrons then serve to ionize the atom in the tissue, or the molecule. I am coming back to that in just a second.

The neutron is a neutral particle with no electric charge, and on its own hook, then, has no electrical interaction; so it cannot ionize matter directly. It has a nuclear force associated with it, so it can run into the nuclei of matter.

What a neutron does is, it sails along through matter, dense matter—I am sorry, not air—distances like 3, 4, or 5 centimeters, and bumps into a nucleus with its nuclear force. It just does not see electrons, does not touch them. That makes this nucleus take off and go along at high speed, and it is now a charged particle. It has gotten a severe bump, and it is moving fast, so it is ionizing just like this alpha particle ionizes.

So, the neutrons in water, say move distances like 1 to 3 centimeters. Finally, after many collisions, the neutron stops, and nearly always it will be absorbed by nuclear forces, sucked in and made part of the group inside a nucleus. Usually, this nucleus, after it has eaten this neutron, feels unhappy and wants to do something about it—it feels that perhaps it made a mistake after all—and it will try to remedy the situation usually by emitting a beta particle and changing this neutron, say, into a proton. In doing this, the whole nucleus is disturbed, and the general effect is not only to eventually emit a beta particle, but also to emit gamma rays. So, when a neutron is captured, the induced activity, or secondary radiation created by this step, is to produce gamma rays and beta rays.

I wanted now to come back to what all of these things do.

What I have tried to show is that all of these particles eventually produced ionization, which means they break up the electrical structure of matter. They have knocked the electron out of the atom, out of the molecule, and the molecule may break up into fragments. It is believed, in general terms, that biological damage results when some of these fractured molecules go back together the wrong way.

Chairman DURHAM. If a hospital or a doctor would reecive a patient, and he had an overdose of radiation, how quickly could they determine whether it was alpha, beta, or gamma rays that poisoned the man? Or could it be done?

Dr. MILLS. I do not know whether or not it could be done, and although I am not a medical doctor, I believe that it makes very little difference insofar as treatment is concerned. One has to be a little careful here. There is external radiation and internal. Alpha rays do not get through the skin, because they do not get through the layer of epidermis that is no longer living, and, therefore, it does not matter if it gets cooked a little by the alpha particle. They do not get through that into the living skin, and, therefore, you can stick your hands in alpha rays and nothing will happen.

Chairman DURHAM. Is it important to identify the type of radiation in regard to the treatment?

Dr. MILLS. I think if I could just go along, maybe it will all come out in the wash here.

The way these things then affect matter, they ionize material, they break it up, and then the material will recombine in the wrong way, and you then have a foreign chemical substance somewhere in the cell, something bad for the cell. I do not know really enough about this part, except to give the rough notion; the point being that alpha particles in the air, for example, produce about a million ion pairs per centimeter of air. That is, along the path of alpha particle goes in air there are all these fractured atoms, and if you count them, there will be a million ion pairs, or 2 million total fragments left behind. A beta particle makes only about 50 ion pairs per centimeter, although it may produce the same total number of fragments.

The qualitative understanding of the reasons that alpha particles are more damaging than beta particles, is directly related to the greater density of fragments in the case of the alpha particle.

I have probably said more than I really know as to how these effects occur.

3. Radioactive decay

Now, I wanted to talk about radioactive decay, and then talk about the roentgen and the curie.

First of all, there are radioactive atoms. There are some occurring naturally in nature. There are others that are manmade, either with electrical machines, or with nuclear reactors, or with nuclear weapons.

Generally speaking, these radioactive materials decay, and the first thing I shall say is this: That the danger they produce has to do with the ultimate production of ionizing radiation, usually directly.

Perhaps one of the simplest cases—I wanted to put down the strontium chain. Consider the nucleus, krypton 90, which is one of the fission fragments. It can undergo radioactive decay, emit an ordinary beta particle, electron; and it does this on the average within about 33 seconds. As it does this it changes over into rubidium 90.

Let's talk briefly about what is meant by radioactive decay. I think an analogy that may be helpful is this one:

We have this krypton nucleus here, and it is thinking about ejecting this electron. It has not done it yet. It has a certain chance—in fact a 50–50 chance to do this within the next 33 seconds. It is trying to do it, but it has not done it yet. If it does not do it, then it is just as good as if nothing had ever happened. It is not penalized at the end of the 33 seconds for not having emitted the beta particle before.

Again, it is very much like a person running blindfolded through a woods. His chances of running into a tree next is not affected by

the fact he missed all the trees in back of him when he was running along.

This is the essential reason, then, that radioactive decay is exponential in character. The fact that the remaining nuclei have lived, all of this time up to now does not either increase nor reduce the probability that in the next period of time they will decay. One that has already decayed, of course, is now a different substance, and cannot decay again in the same way. But if you went back to the period just before it decayed, its chances of decaying, say, in the next 33 seconds, in this case, was just the same a year ago as the chance of a not-yet-decayed fragment now decaying in the next 33 seconds. It is not prejudiced by the past. The chance of its doing something is always the same until it does it and then it is not the same thing any more. It is something new.

Senator BRICKER. When krypton decays and gives off the beta rays, what is the remaining substance?

Dr. MILLS. Rubidium 90.

I was not going to write this whole chain down, because I was trying to avoid too many symbols.

Senator BRICKER. There is no other isotope of crypton left?

Dr. MILLS. No, sir; crypton has gone away entirely, and there is a new chemical element. The new chemical element has the same number of neutrons and protons total, but essentially one of these neutrons is changed into a proton, and you see that determines how it juggles the electrons. The whole juggling of electrons is what we know as chemistry.

Senator BRICKER. And the remaining rubidium remains stable?

Dr. MILLS. No, the rubidium does not. I will write down the whole chain for you, if you give me a minute.

Representative VAN ZANDT. Is this krypton related to the krypton taken from the atmosphere?

Dr. MILLS. No, this is not the naturally occurring krypton in the atmosphere. I must say I do not know, but it is a different isotope and has a different number up here [indicating]. It has the same number here [indicating]. I do not need to write krypton at all. I could just say Z is 36, and give the meaning. No one does that because we are not used to thinking that way. I would just like to write down this chain.

We have krypton 90, atomic No. 36. That goes to rubidium 90, and its atomic number is one greater, 37, and this takes about 33 seconds.

Rubidium 90 goes to the famous strontium 90, and that has atomic No. 38. There is another beta particle emitted, and this takes 2.7 minutes on the average to go from rubidium to strontium.

Representative HOLIFIELD. Will you please use the microphone, Dr. Mills?

Dr. MILLS. Yes, sir; I beg your pardon.

This goes to yttrium 90, atom No. 39, which is also nonstable. It decays emitting beta particle, and in about 65 hours—these are all half lives I am writing underneath—which goes finally to zirconium 90, atomic No. 40, and this one is stable.

Representative HOLIFIELD. Now is a good time to describe what half life is, and give the persistence of strontium 90.

Dr. MILLS. Yes; I left that one out. Thank you.

Representative HOLIFIELD. That is the one I do not want you to leave out.

Dr. MILLS. Right you are; yes.

Representative HOLIFIELD. You wrote it. Will you say it?

Dr. MILLS. Yes, sir. The half life of strontium 90 is 28 years.

Now, this particular chain is of special interest in the hearing, so I thought it would be helpful to put it down.

The meaning of half life is this: if I have a substantial quantity of material, that at the end of a time equal to a half life, half of it has decayed, and I have only half of it left.

There is also a quantity called the mean life, which says instead of reducing the amount by one-half, it is reduced to 1 over *e* of its value (e is 2.718). The mean life just makes calculation easier, although the half life is easier to think about.

Chairman DURHAM. What is left has the same half-life as before?

Dr. MILLS. Yes, sir.

Chairman DURHAM. Has the same 28 years.

Dr. MILLS. If I imagine I have an amount here, and wait a half-life, this is transformed. Of course, it is all mixed up. You do not know which one is really going to do it. But this stuff is now rubidium, and this is krypton. If I wait one more half-life this will transform. So I now, at the end of 66 seconds have a quarter as much as I did at the start. If I want another half-life, this much will transform, and at the end of 99 seconds I now have an eighth as much as I did at the start, and so on. It just keeps right on doing this. So once it starts to go, it goes quickly.

What is implied here—if I suddenly make some krypton, then in a relatively short time I have made strontium. That is, this (indicating) has gone to here (indicating), and practically all of it has, you see, at the end of a couple of minutes—nearly all of it is over here in the form of rubidium. I wait, say 10 to 20 minutes, and practically everything is strontium. But now the strontium persists for a long time. I have to wait 28 years to get rid of strontium.

Representative HOLIFIELD. Twenty-eight years to get rid of half?

Dr. MILLS. Yes, sir.

Representative HOLIFIELD. And 28 years to get rid of half of what is left?

Dr. MILLS. Of half of what is left. So at the end of 56 years, I have a quarter as much strontium as I have at the beginning.

Representative HOLIFIELD. How long does it take for it to completely decay?

Dr. MILLS. Forever.

Representative HOLIFIELD. Forever?

Dr. MILLS. But there is not much left after a while.

Representative HOLIFIELD. Krypton 90 is in form of a gas, it is not?

Dr. MILLS. Yes, sir.

Representative HOLIFIELD. How about rubidium-90?

Dr. MILLS. Could I describe what occurs—if this fission fragment were produced in air—let's imagine that—then this is not only a gas, but it is a noble gas, it does not react with things chemically.

So when it finally decays there has been some time—I have 33 seconds for my krypton to run around before it changes into rubidium. The rubidium is now an isolated rubidium atom, just sitting out in the

air without any friends around to speak of. It will probably make friends with an oxygen atom in the air and make a compound, but it is a single isolated molecule.

Most of the chemical substance we think of, we think of good bits of them, large amounts. This fragment produced in air we believe to produce quite small particle size material.

Representative VAN ZANDT. What is the size of the particle?

Dr. MILLS. First of all, I believe this will be mentioned later, but sizes of the order of 10 to the minus 6 centimeters, like a hundred of these things will be joined up and flitting loosely in the air.

Representative HOLIFIELD. Doctor, in the explosion of a large nuclear weapon, while the individual particle is small, there is a tremendous number of those individual particles?

Dr. MILLS. Yes.

Representative HOLIFIELD. That are transferred?

Dr. MILLS. Yes, sir.

Representative HOLIFIELD. Is it not true that this same krypton 90 can be emitted in a gaseous form from a reactor?

Dr. MILLS. Yes. Well, it is harder to do it in a reactor. At least the reactors we want do not immediately breed, you see, they keep everything locked up. That is the big problem.

Representative HOLIFIELD. That is true, but we do have to take preventive measures to keep that from escaping over the atmosphere?

Dr. MILLS. Yes. I was going to mention that. You hold it usually for times like several months. This is a time between changing fuel elements. The old fuel element will have the fissionable material you put into it, and also the fission products in running the reactor, or you have taken time in the reactor for this decay change, in most cases to occur, and essentially you have strontium left, rather than krypton.

Representative HOLIFIELD. What form is the strontium when you get to this point? Is it in the form of a chemical or gas?

Dr. MILLS. The strontium is not a gas. It is probably combined—well, it depends what is in the fuel element. It may just be sitting there as a single isolated strontium atom, say, surrounded by uranium atoms. It might react with other fission products. Or if, for some reason, it is water, if you had a homogeneous reactor with uranium in it, the strontium might form strontium oxide by stealing some of the oxygen from the water molecules.

Representative HOLIFIELD. Any strontium emitted from a weapon is in such a form that can be absorbed by vegetation and ingested into animals, transferred to milk or to eggs, and then deposited in the human body by eating these substances, or drinking the milk, and its half-life persists regardless of the form it is in?

Dr. MILLS. Yes, sir.

Representative HOLIFIELD. When it goes into the human body, it goes into the bone, ordinarily, does it not?

Dr. MILLS. I would rather not——

Representative HOLIFIELD. And it continues the emission of particles at that point?

Dr. MILLS. That is correct. You see the nucleus just lives undisturbed essentially by what the electrons are doing. So all of these chemical changes do not affect nuclear half-life, the decaying, or the emitting of beta particles.

The sequence of events you mentioned I have not really brushed up on, and I believe that testimony comes later. It certainly sounds correct to me, but I would not like to prejudice a later speaker by commenting on that.

Senator ANDERSON. You did say there was a possibility that krypton could come from reactors?

Dr. MILLS. Yes, sir. I was about to get to the fission process. There is a different krypton which is made. There is more than one krypton isotope made, and some of them are longer-lifed, although more radioactive than krypton 90.

Senator ANDERSON. Is there any way we can find out, for example, what type of krypton a reactor would develop?

Dr. MILLS. Yes, sir. We can not only find out, I believe we know.

Senator ANDERSON. Do you know whether a pressurized water reactor puts out the type of krypton you have there in that diagram?

Dr. MILLS. Yes, sir, it puts out this kind. I beg you pardon. By "put out," I mean it produces it. Whether the krypton gets out of the reactor or not is a second problem.

Senator ANDERSON. I understand that.

If it does get into the atmosphere, that kind of krypton leads to strontium 90?

Dr. MILLS. Not in every case.

Senator ANDERSON. You see the difficulty people have, when we ask, "Does this reactor do it?" "Not in every case."

How do we get to know if it ever does it?

Dr. MILLS. Let me go just a little longer. I am going to get to that next in the fission process, and fission fragments.

Senator ANDERSON. All right.

Representative VAN ZANDT. I would like to ask the doctor about strontium 90.

Dr. MILLS. Yes, sir.

Representative VAN ZANDT. Is it not possible that strontium 90 when in the atmosphere can be, in addition to gas or water, a metal?

Dr. MILLS. Well, it is nearly always chemically joined up with something in the atmosphere. It is certainly possible, yes, sir, but very unlikely.

I took the time to write something down. So far, I had mentioned how matter looked, and how these nuclear particles charging around through it can break up atoms and molecules, and then they may rejoin into a new kind of atom, or molecule that causes trouble if it is in a human body. You helped me point out that the radioactive property of the material is not affected by the state of chemical combination; that it can go through very many steps and continue to be radioactive until it actually decays.

One other thing I meant to say about this chain here is that the decay of the yttrium takes place very soon after the decay of the strontium. Therefore, if the strontium, say, is ingested, there is very little chance that the yttrium will be ejected in this short time. So that the proper way to view the total amount of energy deposited in the body is by adding together the energy of the strontium beta ray and the yttrium beta ray, and this is what is usually done.

Representative HOLIFIELD. Do they both have the same property of damage to human tissue?

Dr. MILLS. Both beta rays have the same property of damage to human tissue. The importance of strontium is that it does tend to go to the bone. It is like calcium in its chemistry. The importance of yttrium is that it just cannot get out of the bone in time. So it is almost certain to deposit its energy there along with the strontium.

Representative VAN ZANDT. Doctor, what part of the human body attracts strontium 90?

Dr. MILLS. We all—first of all, this is a little out of my field—but we drink milk, and so on, to get calcium because this is what our bones require. There is also certain turnover of calcium in our bones. The strontium is chemically not identical to, but very similar to calcium, and the body does not always decide it is not calcium, so it slips up and lets some get in. This is a very complicated medical problem which I do not know much more than that about.

Representative HOLIFIELD. We are going to go into this field in detail later on in the hearings, as you know.

Dr. MILLS. Yes, sir.

4. Radiation dosage

I want to say briefly what a roentgen and curie are.

The roentgen was originally defined in a rather complicated way, but it has now been defined in a simple way. At least a physicist can always calculate a roentgen equivalent physical, called a rep. That really means nowadays 93 ergs of energy—which is an energy unit like a kilowatt-hour, only a different one——

Representative VAN ZANDT. A measure of energy.

Dr. MILLS. A measure of energy, 93 ergs of energy deposited per gram of tissue, and that is a simple thing. That is energy out of a radiation field deposited in a gram of tissue.

There is another thing called roentgen equivalent man, "rem," for which beta rays and gamma rays is essentially the same as rep, but the rem is supposed to measure the biological damage rather than just the energy. It turns out, as I explained in the case of comparing alpha particles, and beta rays, that alpha particles are more damaging than the beta rays.

There is, finally, a thing called a relative biological effectiveness (RBE), which the biological people have been working to measure and identify, and essentially the amount of biological damage is given by the product of rep, that is, the amount of energy you put there, and then by the RBE, relative biological effectiveness, which says how damaging that energy is per unit of energy.

For beta and gamma rays, the RBE is 1; for alpha particles it is taken to be 20; and for protons, it is taken to be 10.

There is a list of relative biological effectivenesses for different kinds of radiation, which is used by people in physics laboratories to estimate dosages, and to see they are carrying on safe operations.

These things are revised from time to time, and I am not enough of a biologist to be able to say more than these qualitative indications of how it works.

The other unit that you wished me to define is the curie, and the curie is 37 billion disintegration per second. This is the definition of a curie. It used to be the number of disintegrations of a gram of radium. The number sticks but the definition is used more widely.

To give one some feeling about this: If one has a source producing 1 curie of 1 million volt gamma rays per second, and stands 1 meter away from this source, you will accumulate a biological dose of approximately 1 roentgen of radiation each hour. One thing about the dose, it is an energy per unit mass; so that you could, for example, give the tip of your finger an enormous dose, and it would hurt, all right, but it would not be really a fatal thing. It would be like hitting it with a hammer or getting it burned.

If your whole body receives radiation, then a much smaller dose is dangerous, and roughly, a dose of 400 to a thousand roentgens is likely to be fatal if given to the whole body.

Representative HOLIFIELD. Is it not true that that amount is not applicable to every human being?

Dr. MILLS. Your statement is correct. This is a statistical thing, and the proper way to say it can be quite complex. Also, it is impossible to get volunteers to get good statistics and indirect inference from animal experiments to human effects is necessary.

Representative HOLIFIELD. Is it not true also that the amount of lethal dose for children would be much less than for an adult?

Dr. MILLS. I do not know the proper answer to that question, sir.

Representative COLE. Mr. Chairman, I do not understand the witness' response. You asked him if it were not true, a certain hypothesis, and he said no. Now, with respect to whether the same dosage would affect all people the same——

Dr. MILLS. I beg your pardon. Different people quite clearly have different tolerances for different dosages, but the more complete experiments are usually carried on with laboratory animals, so you can get statistics. And a feeling how wide the swing is of the different tolerances of different individuals to a fixed amount of radiation is usually done with laboratory animals, not people. But it does fluctuate quite a bit.

For example, some people will get quite ill, it is believed. They have not done this with people, but it is believed to be so at 200-roentgen whole-body radiation, and other people would perhaps survive 400-roentgen whole-body radiation.

Representative HOLIFIELD. But, in any event, whether they survived or not, the radiation would be harmful to tissue and would result in a possible destruction of the cells of the body, and the ability to recreate those cells, and, therefore, it would be considered to be harmful regardless of the amount. Is that true?

Dr. MILLS. You will have to ask a biologist. I believe, though, that Dr. Durham said that any quantity of radiation is considered to be harmful. I am not expert on these things.

The other way, of course, one can have trouble with radiation we have already mentioned. You can ingest a substance, take it into your body, that is radioactive, and then it can go to a more sensitive place, or to a less sensitive place, and decay, and in decaying produce the radiation damage.

Alpha particle emitters are a particular case in point. The alpha particles are the most heavily damaging, but ouside the body they do not get through the skin. Inside of the body many of the alpha emitters seem to go to the bone, and then they sit and bombard the bone marrow.

Representative HOLIFIELD. The alpha particles do not have penetrating quality, and it results in skin lesions and burns?

Dr. MILLS. No, they do not.

Representative HOLIFIELD. What I said is true?

Dr. MILLS. Yes.

Representative HOLIFIELD. And your gamma rays can penetrate the body?

Dr. MILLS. Clear through.

Representative HOLIFIELD. Or penetrate a cement wall?

Dr. MILLS. Yes.

Representative HOLIFIELD. What is the difference between the neutron effect and the gamma ray effect?

Dr. MILLS. Well, the alphas—let me go quickly in sequence—the alphas do not get in, really, to the skin. The beta rays get onto the living skin and can cause trouble, but not deep into the tissue at all. The gamma rays of appreciable energy can essentially go anywhere in the body, and if a beam of gamma ray comes in here [indicating], there is a certain number of them that come clear out the other side, and some of them run into something inside. And a similar sort of thing can be said for neutrons, except a little bit depending on their energy.

One would think, for example, if you were irradiated from outside with neutrons, only a layer of an inch or so would get the brunt of the radiation, and maybe things deeper inside would have a much weaker neutron band. The neutron does most of its trouble by creating secondary radiation, usually gamma rays. Although the gamma rays they create might be produced here, those gamma rays, in turn, can be diffused through the whole body. So even though the neutrons tend to be stopped near the surface, it does irradiate a good deal of the body, not localized near the surface.

I wanted to say one other thing. Radioactivity is usually spoken of in quantity according to the word "curie." A given weight of matter will have a certain number of atoms in it. Since each of those atoms will decay with certain half-life, you can connect-up the half-life and the quantity of material and the kind of material. The kind has to do with how many atoms per given weight, and, therefore, so many curies of any given substance also corresponds to a certain given weight of that substance.

In the particular case of strontium 90, a microcurie, one-millionth of a curie, corresponds to one-billionth (one over a thousand million) one-billionth of a gram of strontium 90. Just to give one piece of orientation.

III. FISSION AND FUSION

I wanted to quickly mention the fission process, and the fusion process. This has to do with part III, really, of your outline.

1. *Fission*

The fission process is spoken of this way: U-235 plus a neutron makes fission fragments, usually 2 in number, plus 2½ neutrons.

Now the clue that something is quite complicated, is the 2½ neutrons. There is either a whole neutron, or no neutron. There is not even a half neutron. This means some of the time you make 3 neutrons, and some of the time you make 2. And indeed the thing is quite complicated.

There are about 170 different nuclear species produced if you fission quite a bit of material and then count things up.

Representative HOLIFIELD. And all of which vary in half-life?

Dr. MILLS. And all of which vary in half-life.

Furthermore, they have chains, even as if I made this [indicating], this, and this, each separately, and each decayed its own way. But it is much worse than that.

Not only do I make a number of different chain leaders—say this is No. 1 man—I make a variety of these. There are about 70 of these chains. But then each of these chains run this way [indicating].

Furthermore, in the fission process—I do not know if this is a good one, but I can assume so—sometimes the fission will make the second member instead of the first member of the chain, or sometimes even a third member. So I have all these chains, and I have things starting at the front which are the mainline, but then I have now and then a fission that will cheat and skip that [indicating], and just make this one [indicating].

The products then are quite varied in number and complexity. There are 35 different chemical substances, 95 different masses, and 170 different isotopes.

2. Fusion

One can contrast that with fusion. There are some more fusion reactions one can write down, but these are usually considered the most interesting.

This is heavy hydrogen, D for deuterium, is the conventional symbol. The deuterium nucleus has a neutron and proton in it. And one electron outside makes the complete deuterium atom.

The deuterium and deuterium reaction makes helium 3 which is the neucleus of the helium atom, but shy one neutron; and also an energetic neutron.

Sometimes deuterium plus deuterium reaction goes to make tritium, which instead of having 2 protons and 1 neutron like helium, has 1 proton and 2 neutrons. It is extra heavy hydrogen, and weighs three times instead of twice as much as regular hydrogen plus an ordinary proton, the ordinary nucleus of the hydrogen atom. Then I can also react deuterium with tritium to make normal helium plus a 14 Mev. neutron. The 14 means the energy at which the neutron comes scooting out of here [indicating]. This neutron from the DD reaction is about 2 Mev. in energy.

There is one other interesting thing, deuterium in water.

It occurs in nature to a small extent. Most of the hydrogen in nature is ordinary hydrogen, having just a proton and electron. But heavy hydrogen occurs with the other hydrogens to the extent of about a little more than a hundredth of a percent. If one calculates the amount of energy you can get from a gram of water if you react or fusion the deuterium in that gram of water, it turns out to be about 100 times the amount of energy you can get from a gram of fuel oil by burning the fuel.

Since there is a tremendous amount of water in the ocean, all of which has this trace of deuterium in it, it means the oceans are sort of like oceans of fuel oil, only about 100 times as good in terms of total energy reserve. I understand there have been a few problems in getting at this energy.

The other comment you wanted me to make—am I skipping along too fast?

Representative HOLIFIELD. No.

Senator ANDERSON. Did you say if the ocean were filled with deuterium, as a fuel it would be 100 times more valuable than if it were filled with oil?

Dr. MILLS. It has potentially 100 times as much energy. I just say it that way because apparently there is quite a bit of work in research and development to be able to burn the ocean.

Senator ANDERSON. I did not say we had it solved. But if you did have it solved, that is the fact?

Dr. MILLS. Yes.

Senator ANDERSON. One hundred time as much energy in the ocean, if filled with deuterium as it would be if the present water could be scooped out and the areas filled with oil?

Dr. MILLS. That is correct, yes, to use it all.

Representative VAN ZANDT. It is contingent upon a hundred million degrees of Fahrenheit.

Dr. MILLS. Yes. I was going to cycle a little bit.

To make these reactions go in a useful way people feel they have to be thermonuclear reactions. That is, you get the materials so hot that these particles will run into each other and fuse and make the products and the energy you are after. The temperatures one talks about are 100 million to 1,000 million degrees to do this. So people who are practitioners of this art are likely to get their fingers burnt if they are not careful.

This is a difficult but very interesting development, and I am sure a very important one, because it offers to humanity the possibility of an unlimited energy reserve.

Representative VAN ZANDT. How close close are we to fusion?

Dr. MILLS. I was going to answer that in a little complicated way. Maybe I should now.

The current situation, in rough terms, is that the fusion process is not yet at the state of physical practicability. A physical demonstration of the controlled fusion process is not yet to the state of the 1942 Chicago fission reactor.

Representative HOLIFIELD. You mean fission.

Dr. MILLS. Thank you. Fission reactor, first of the chain reactors.

Representative VAN ZANDT. I imagine you have reference to Dr. Fermi's first atomic pile.

Dr. MILLS. Dr. Fermi's CP-1, the first Chicago pile. Of course, that one is rather modest compared to fission reactors people are thinking of now.

So there is, first of all, about a 14-year delay at least, if you say the developments are of comparable difficulty.

My own feeling is the fusion development is somewhat more difficult, and I tend to add another 5 years to that. So that would put fusion about 20 years behind fission in terms of potential use and economic use in particular.

Now, Mr. Davis, of the Division of Reactor Development of the Atomic Energy Commission has been studying the economic feasibility of the fission reactor, and he has estimated that as of about 1970 the fission reactors will begin to compete effectively with chemical plants, coal or oil-burning plants, for economic electric power production in

stationary plants. So if you say that one of the things you would like to get from the fusion process is economically competitive, stationary electric power, this would infer that with this about 20-year lag I have put in, it would be in the 1990's before you might expect to worry about the economic impact of the fusion process.

Probably it would be a little longer, because we all believe, I think, that the fission process will take hold sooner than that, and it will ease the economic pressure, and conserve conventional fuels. The fission process itself then may add some additional delay by taking some of the economic urgency away from the fusion development.

Representative HOLIFIELD. Will you go to the radiation comparison between fission and fusion?

3. Fission and fusion radiation

Dr. MILLS. Yes. I did a rough-and-tumble estimate, and I would like to reiterate this is crystal gazing.

There are some things you can say on the basis of the fundamental reaction, some things you can guess at, but it is awfully hard to compare this reaction (fission) with this one (fusion) in practical plants, because there is not any practical plant like this (fusion). The possible radioactive dangers, therefore, first of all, may be very different than you think of. They may be nonexistent. There might be a danger of an entirely different kind that no one has thought of at all, but would have the main importance.

Representative HOLIFIELD. You are referring to plants to produce energy, are you not?

Dr. MILLS. Yes.

Representative HOLIFIELD. I am not referring to that.

Dr. MILLS. Beg your pardon.

Representative HOLIFIELD. I am referring to the present use of fission and fusion in weapons.

Dr. MILLS. I see.

Representative HOLIFIELD. And the comparison between the radioactivity of the different fusion products and the fission products. I realize that is not in your field, but as long as your talk is on the background of radiation and radioactivity, I thought you might make a general comment on that.

Dr. MILLS. I would like to make a very general comment of two kinds.

First of all, the fission process itself produces directly radioactive material, namely, these fission fragments, a large variety of them, a very complex set of them.

These fusion reactions I have listed here—of all the things they produce only tritium is itself radioactive. It decays slowly to helium 3, which is stable.

Chairman DURHAM. Is there any difference as to the stability of it, whether it is a gas or whether it is a solid?

Dr. MILLS. No; its radioactive decay continues no matter what form it is in, gas or solid. You can make tritium water, for example. It does not matter.

Representative HOLIFIELD. At the present time fusion has to occur after fission occurs; it is dependent upon the heat developed by fission, is it not, to bring about fusion, Doctor?

Dr. MILLS. If you will forgive me, sir, I would rather not comment on that question.

Representative HOLIFIELD. All right.

Representative VAN ZANDT. Dr. Mills, what about the penetrating strength of the radiation produced by fusion?

Dr. MILLS. The only direct production of radiation by fusion is tritium, which produces about a 10-kilovolt, very weak beta particle, and that beta particle cannot penetrate the skin.

Senator BRICKER. What is the half life of tritium?

Dr. MILLS. About 12 years. However, fusion does produce neutrons, as also does fission, and these neutrons can be captured in the surroundings one way and another, and activate things.

For example, neutrons captured in sodium will produce sodium 24, which has a half life of about 15 hours, and produces beta and gamma rays, and the gamma rays are rather penetrating.

So if there were some reason—for example, if both of these things occurred with sodium, these neutrons here from fission would make a certain amount of radioactivity, and so would these from fusion. So that the induced activity might possibly be comparable.

You see, I have 2 neutrons here, and 2 there.

There is finally one other thing you can say. Generally there does not have to be things that these neutrons will react to, and they can be captured more or less harmlessly in the air. So that for many practical purposes, there is essentially no radioactivity associated with the fusion process.

Finally, the fission fragments are of such a great variety that if you were now to switch from the general problem to a machine, there are very many different chemical things you have to keep track of and prevent leakage—for example, if you spill them there are many varieties of things you would have to pick up. It is hard to get them all. With fusion, by making the machine out of the right thing, you may make it so these neutrons essentially cause no trouble, or you can select a material that produces one kind of neutron-activated product that is simple to clean up and pick up.

Representative VAN ZANDT. Do I understand you now that fusion will produce the alpha, the beta——

Dr. MILLS. No, sir; fusion will not. Fusion just makes these things here [indicating] of which only tritium is radioactive. Fission produces an enormous variety of products, and both gamma and beta rays come pouring out of the fission fragments.

Chairman DURHAM. From a distribution standpoint, Doctor, fission, and also the thermonuclear, would you hazard an opinion as to which one is more easily distributed in the air and on the ground? Would there be any difference?

Dr. MILLS. The fission products, there are some volatile ones that are carried around easily in the air. Some will dissolve in water. There is such a great variety.

The tritium produced in fusion would normally be a gas unless it combines with oxygen to make water. So it can blow around in the air as a gas or it can combine with oxygen and flow in water. Tritium is a radiological hazard, but generally speaking, nothing like fission products. The rest of these things are not radioactive. They are gases. Helium is a gas not chemically reactive, not harmful chemically, and, of course, not radioactively harmful.

Representative HOLIFIELD. Are there any further questions?

Had you finished your general statement, Doctor?

Dr. MILLS. Yes, sir; I had.

Representative HOLIFIELD. Dr. Mills, the committee appreciates you coming here from California to make this statement. We know how complicated it is, and we do feel you have given us in layman's language an understanding of this in about as simple a form as it can be made.

You will have access to the transcript so that you can be sure the reporter has caught properly your symbols and other references, and for the purposes of correction of anything which may not be clear on the record.

Dr. MILLS. Thank you, sir.

Representative HOLIFIELD. Thank you very much, Dr. Mills, your submitted material will be inserted in the record at this point.

(The matter referred to is as follows:)

STATEMENTS ON TOPIC II RADIATION AND RADIOACTIVITY, AND TOPIC III FISSION AND FUSION REACTORS, FOR THE JOINT COMMITTEE ON ATOMIC ENERGY

Prepared by Mark Mills, University of California Radiation Laboratory, June 1957

Mr. Chairman and gentlemen, I welcome the review of the Nature of Radiation and Its Effects on Man, which you are conducting in these hearings during May 27 through June 7. I want to thank you for the honor of inviting me to address you. I will be glad to answer questions to the best of my ability.

II. RADIATION AND RADIOACTIVITY

In order to tie together the results of my discussion of radioactivity, I will include some remarks about the biological effects of radiation. This is only to give perspective, and I wish to emphasize that I am not a radiologist nor a medical doctor. In making these statements about the biological effects of radiation, I am merely repeating some of the things medical experts tell physics people so that, in turn, the physicist can design adequate shielding and take suitable precautions when working with radiation and radioactivity. It is my understanding that a careful and expert review of the biological effects will be a portion of the subsequent hearings.

1. Radiation and radioactivity

Radiation consists of very energetic fast-moving particles. These particles are usually given the name of alpha rays, beta rays, and gamma rays, and sometimes one has to be concerned about radiation consisting of neutrons.

There are a number of different sources of radiation. The special manmade electrical machines can accelerate a particle of ordinary matter to such high speeds that it deserves the name of, and in fact is, radiation. A neutron chain reaction can be sustained in a nuclear reactor and produce fission products which, in turn, emit beta and gamma rays. Nuclear weapons can produce radioactive materials, which in turn produce beta and gamma rays.

Radioactivity is the name given to the process whereby an atom produces radiation. A material which emits radiations is called radioactive. Radium is probably the most famous example of radioactive material.

In undergoing radioactive decay, the radiation is actually emitted from the nucleus of the radioactive atom. Furthermore, there is a mother-daughter sequence. The mother atom emits the radiation and in so doing is transformed automatically to the daughter atom. Usually the mother and daughter atoms are different chemical substances, although in those cases where the gamma rays are emitted, the mother and daughter are the same chemical substance.

It turns out that the radioactive decay of a mother nucleus to a daughter nucleus can be described by the term "half life." At the end of a given period of time, for example in the case of radium, at the end of 1,600 years, half of the mother substance has transformed into the daughter substance. Since the emission of radiation is the step that makes this transformation possible,

a given quantity of material can produce very intense radiation provided it has a short half life, or very weak radiation provided it has a long half life. The half lives of radioactive transitions have been measured and cover an enormous range. For example, the half life of uranium 238 is approximately 4 billion years. Whereas the half life of krypton 90 is about 33 seconds.

To visualize the meaning of half life a little better, it may be helpful to say the following. At the end of a half life, there is half as much material present as there was in the beginning of the half life. If we start with 1 pound of a given radioactive substance, at the end of the first half life there would be one-half pound, at the end of another half life there would be half this amount, or one-quarter pound, at the end of another half life there would be one-eighth pound, etc. A very long time would be required to get rid of all of the original material in this way, but at the end of only 10 half lives there is only one one-thousandth of the original material left, so that for most practical purposes practically all of the material has disappeared at the end of a few half lives.

2. Radioactive decay cannot be disturbed

Nothing disturbs in any way the radioactive decay of a radioactive substance. For example, if one had fuel oil containing radiocarbon and radiohydrogen, and burned this fuel oil, the smoke would carry the same radioactivity and to the same extent as was previously present in the fuel oil. The radioactive process cannot be speeded up, nor slowed down, but depends only on the nature of the radioactive substance involved. Fundamentally, this is due to the fact that the radiations are emitted from the nucleus of the atom, and all of the chemical processes like burning, melting, boiling, etc. disturb only the outermost electrons of an atom and affect the nucleus hardly at all.

As another example, the radioactive isotope strontium 90 is formed very quickly by the successive beta radioactive decay of the fission fragments krypton 90 (half life 33 seconds) which turns into rubidium 90 (half life 2.7 minutes) which decays into strontium 90 (half life 28 years). If one imagines that the krypton 90 is produced in an atomic explosion, the strontium 90 will probably be produced in the air. It will then probably form strontium oxide with the oxygen in the air, fall slowly to the ground (fallout), and may then possibly settle into the soil, be absorbed by plants, the plants may be eaten by a cow and some small fraction of the strontium going into the milk, finally, if a person drinks the milk, some small fraction of the strontium will go to his bones (since strontium is similar to calcium in its chemical properties). Despite this long sequence of events, the radioactive behavior of the strontium continues at its regular pace.

3. Penetrating power of radiation

Different kinds of radiations can penetrate different distances through different materials. However, for radiation consisting of a given kind of particle, the depth of penetration in a given material depends only on the energy of that particle, being greater for more energetic particles. For alpha, beta, and gamma rays, the depth of penetration is less in more dense substances than in light materials.

The following is a list of the names and penetrating powers of the different radiations:

The alpha particle is a fast-moving helium nucleus consisting of 2 neutrons and 2 protons tightly bound together. Alpha particles emitted by naturally occurring radioactive substances move at about one-twentieth the speed of light and will penetrate distances of 2 to 5 cm. of air before being brought to rest. They will penetrate about 0.002 to 0.005 cm, in tissue, and since the epidermal protective layer of dead skin on the body has a minimum thickness of 0.007 cm, alpha particles are harmless when the source emitting them is outside the body.

The beta particles consist of a fast-moving electron essentially moving with the speed of light, although just a little less than the speed of light. They can penetrate distances of a few meters in air, or ½ cm. into water or tissue before being brought to rest.

The gamma particles or gamma ray, are really the same kind of thing as light or radio waves, but the energy of a single gamma ray is very much greater, amounting to something like 1 million electron volts. A gamma ray does not penetrate a definite distance into matter, but has a certain prob-

ability of being absorbed. If a large number of gamma rays are fired into matter, after some distance only half of them will remain, and at double the distance only one-fourth of them remain, and so forth. Gamma rays of 1 million electron volts energy are reduced to half intensity by 1 cm. of lead, or by 10 to 20 cm. of water, or 100 to 200 meters of air.

Any of these radiations may have any given amount of energy, but in most cases energies of a few million electron volts correspond to "typical" radiation.

The characteristics of neutrons, deutrons, protons, and other radiations have not been mentioned because they will be of little concern for present considerations.

A signlficent feature of the properties of these various radiations is in their relative danger inside and outside the body. For example, a substance which emits alpha particles does not constitute a hazard outside the body, since alpha particles are not energetic enough to penetrate the epithelial layer of dead skin and reach the living skin. A substance which emits beta rays can cause skin burns and irradiate the tissue to a depth of a few tenths of an inch. This is not likely to cause damage to internal organs. Radioactive materials which emit gamma rays, however, can pass this radiation clear through the body and therefore can irradiate the entire body even though such gamma ray emitting substances are kept outside the body.

Consequently, it is convenient to think of the danger of radiation as dependent upon whether the radioactive material is outside of the body or inside the body. In general, the danger is greater when the material gets inside the body. A dense cloud of a mixture of radioactive materials in the air, such as the complex set of fission products, could pass by an individual and give him a radiation dose throughout his body by means of gamma rays. The cloud might later settle into a water supply and a different individual could drink the water containing the radioactive material and receive an appreciable radiation dose to his internal organs from all three types of alpha, beta, and gamma rays emitted by material absorbed in his body.

4. The effect of radiation on matter

Almost all of the material that we use in our everyday lives: glass, steel, grass, wood, air, human tissue, water, and food are made from groups of atoms called molecules. When a molecule is bombarded by radiation, it may be broken up into fragments consisting of one atom or smaller groups of atoms. These atoms may then remain permanently broken up (for example, by irradiating water one can change some of it into hydrogen and oxygen gas), or the fragments of the original molecule may recombine to form a new molecule. Most of the bad effects of radiation are believed to be due to the formation of new, unsuitable, molecules within the irradiated material.

Radiations are much more effective in producing transformations of this kind than energy in other forms. For example, it has been estimated that a dose of whole body radiation sufficiently large to be fatal to a human being, if applied in the form of ordinary heat would only increase his body temperature by about 1/1000° F. Merely by running upstairs the body temperature is increased more than this amount. Therefore, although radiation does consist of ordinary particles moving with a great deal of energy, it does have special qualitative features:

5. The same kind of radiation always has the same effect

We believe that the same kind of radiation always has the same effect. For example, if tissue is bombarded by a beta ray from strontium or a beta ray from Yttrium, we believe the effect on the tissue is the same. We also believe that the effect of gamma rays and beta rays are the same. It has been observed that alpha rays are more damaging than beta rays. For example, a 5-Mev. alpha particle appears to be 20 times as damaging as a 5-Mev. beta particle when both are absorbed entirely within the same kind of tissue. In general terms, this is believed to be due to the much greater intensity of disturbance along the path of the alpha particle. Since the alpha ray stops in such a short distance (0.003 cm.) as compared to the beta ray (0.5 cm.), it produces a much greater density of disturbed matter along its path.

6. The roentgen and the curie

The roentgen is used as a measure of radiation dose to a physical system. There are essentially two kinds of roentgens of practical importance. One is called the rep, which is roentgen equivalent physical, and is defined to be the

absorption of 93 ergs of energy per gram of tissue. The other is the rem, the roentgen equivalent man, and is intended to measure how damaging the radiation energy is to a biological system. We have already pointed out that for the same amount of energy, alpha particles are 20 times as damaging as beta particles. Gamma rays are considered the same as beta particles in their damaging effects.

For people working around radioactive substances, tolerances have been established. The basic tolerance requires that a worker should receive no more than 0.3 rem per week. Similarly, workers with such substances may inadvertently inhale or swallow some of them, and there are limits set on the quantities of various radioactive materials that may be allowed to enter the body and corresponding limits on the concentration of radioactive substances in the air or in water.

For example, in the case of strontium 90, the allowable amount in the body has been set at 1 microcurie, which corresponds to six one-billionths of a gram. Strontium 90 tends to go to the bone and then irradiates the bone with beta particles. The daughter of strontium 90 is yttrium 90, which is also a beta particle emitter with a lifetime of 65 hours. Sixty-five hours is such a short time that it is generally assumed that the yttrium 90 stays in the bone in the same place in which its parent, strontium 90, was located. Consequently, the dose received by the bone is the accumulated dose of both strontium 90 and yttrium 90.

Since the dose is defined per gram of tissue, it is conceivable that an intense dose to a small region of the body, such as the tip of the finger, of thousands of rem would be analogous to a severe burn, or hitting the end of the finger with a hammer. It would hurt, one might lose the end of his finger, but it would not be fatal or even very serious. On the other hand, a whole body dose of 500 rem can be fatal. The estimates of lethal levels of radiation must be inferred from animal experiments and it is observed that different individuals have different tolerances to a given dose of radiation—therefore, the 500-rem estimate given above for a lethal dose would not apply to everyone. It is believed to apply to about half of the individuals.

The conventional measure of radioactivity is the curie. This is now defined to be 37 billion disintegrations per second. In the case of long-lived radioactive materials, a large amount of material may be required to produce an activity of one curie. Short-lived materials, since each bit of material transforms so much more quickly, can produce a curie of activity with much less of material present. Furthermore, if the product of the radioactive decay is a stable atom, eventually the total curies of activity will decrease just according to the half life involved.

For orientation, a person 1 meter from a 1 curie source of gamma rays, each gamma ray of 1 million electron volts energy, will receive about 1 roentgen of radiation dose for each hour of exposure.

III. CONTROLLED FISSION AND FUSION REACTORS

1. The fission reactor

The fission reactor consists of an assembly of fissionable material arranged so that a neutron chain reaction can maintain a steady rate of fissioning in the machine. The reactor may be used to produce heat, which in turn might be used to produce electricity; there are also research reactors which can be used to produce radioactive isotopes for chemical, metallurgical, or biological research, or used in certain kinds of physical experiments.

The fission reaction itself is a very remarkable one. If a neutron enters the nucleus of U-235, the nucleus will split roughly in 2, producing 2 fission fragments of great energy and also 2.5 new neutrons which can serve to carry the chain reaction further. The same fission fragments are not produced in each instance, and with greater or lesser abundance some 170 fission fragments with 95 different masses and representing 35 different chemical substances are made. These fission fragments are generally radioactive chains, or materials decaying one into another. Of the total energy produced in the fission reaction, about 200 million electron volts per fission, some 13 million electron volts or about 6 percent of the total energy is produced by the radioactive decay of the fission fragments. With the great variety of fission products, a large numebr of different radioactive lifetimes are involved. Consequently, fission products produce sustained radioactivity as the short-lived products decay away, some of the longer-lived products remain. This complex set of products, instead of decaying to one-half intensity every half life does not appear to have a very definite half life, but gradually loses in intensity over long periods of time.

2. The fusion reaction (or controlled thermonuclear reaction)

Since there are at this time no practical machines which produce energy from the fusion reaction, all one can do is write down a number of reactions which have been suggested of interest for the fusion process. Some of these reactions are:

$D+D \rightarrow He^3+n$ (2 Mev.) ---------------------- (over all, 3.3 Mev.)
$D+D \rightarrow T+H$ ---------------------------------- (over all, 4 Mev.)
$D+T \rightarrow He^4+n$ (14 Mev.) --------------------- (over all, 18 Mev.)

The symbols have the following meanings: D is deuterium, the nucleus of heavy hydrogen, and weighs twice as much as regular hydrogen. H stands for ordinary hydrogen nucleus. T is tritium, the nucleus of extra heavy hydrogen, weighing three times as much as ordinary hydrogen. Tritium decays to He–3 with a half life of 12 years, emitting a weak beta particle. He^4 is the ordinary helium nucleus or alpha particle; He–3 is a helium nucleus lacking one neutron and only three-fourths as heavy as He–4. The symbol n stands for the neutron. The energies given right after the neutrons indicate how energetic (energy of motion) the neutrons are when produced. The overall reaction energies are also listed. Clearly, the energies of the neutrons should be usefully captured since they carry a large part of the overall energy produced.

H, D, He–3, He–4 are all stable, T is the only radioactive material (except for neutrons).

In order to achieve these reactions in a useful way (they have been achieved in the laboratory with accelerators but not with high temperatures), temperatures ranging between one-hundred million and one-billion degrees centigrade are required. Nuclear reactions driven by high temperatures are achieved in the stars, and are often called thermonuclear reactions. The same name will be suitable for the fusion reactions if they are achieved in a useful way. Since no material can stand such temperatures, a magnetic field is usually considered as the means to hold thermonuclear fusion materials together. The goal of this area of research is the controlled release of thermonuclear energy.

You will notice that the important source of material for these reactions is deuterium, which is heavy hydrogen, which is the hydrogen in heavy water. Deuterium occurs to the extent of 0.014 percent in natural (ordinary) water, and a little calculation shows that on the basis of deuterium content, the potential energy available from a gram of water is about 100 times as great as the energy available from a gram of fuel oil. Consequently, the world's oceans represent an enormous potential energy supply. If a practical fusion reactor can be found, to make use of the deuterium from the world's oceans, then the energy supply would be the same as if the oceans were filled a hundred times over with fuel oil.

3. The comparative hazards of fission and fusion reactors

Since there is no practical fusion reactor machine, it is very, very difficult to compare the hazards of fission reactors, whose characteristics are at least generally known, with the so-far undeveloped fusion reactor. Anything said here must be considered as crystal gazing. Nevertheless, one can make the following very general statements: The fission reactor produces radioactive fission products and neutrons; the fusion reactor produces only radioactive tritium, and the remaining products are not radioactive. The fusion reaction also produces neutrons. In both the case of the fission and fusion reactor, the neutrons may serve to activate materials in the reactor or in the surrounding area, leading to an accumulation of radioactivity.

The real hazard from power-producing machines is due to the possible escape of radioactivity from the machine leading to contamination of the surrounding area. If all the radioactivity produced is extremely short lived, then it would decay away before constituting a hazard even if some of it were to be released. It is the delayed, or long-lived radiation, or radioactivity, that is the hazard.

Finally, I would like to point out that the fission products are a very complex mixture, that there are nearly always some of them that have a disagreeable half life, that they represent very many chemical species so some of them are bound to be especially troublesome, and finally, they will be rather hard to clean up because of their very great variety. In addition, the fission products are a direct product of the fission reaction and must be produced if this reaction is used.

In the case of the fusion reaction, the only direct radioactive product is tritium, which is a mild material. Activation of the surrounding structure can be con-

trolled if it is constructed from suitable materials. For this reason, it appears that radioactivity danger from controlled fusion reactors, when they are invented, could very well be much less than danger from the fission reactors.

4. Economic power from fusion

This prediction of a time scale for controlled fusion (thermonuclear) development is difficult and uncertain, and I will assume that economic fusion power is the question of main interest. One may argue as follows:

(1) Currently, fusion has not reached the state of development of the late 1942 Chicago fission pile. This pile is sometimes called Fermi's pile. (Fourteen years or more behind fission.)

(2) Fission itself is not yet economic, but may be so by 1970. (W. K. Davis, private communication.) (Twelve years.)

(3) Fusion development seems to be more difficult than fission development. I do not know exactly how to weigh this, but a 5-year delay seems reasonable (5 years).

(4) Fossil fuel shortage will be eased by fission reactors by the time that fusion techniques are of practical interest. Therefore, the economic pressure for fusion development will be reduced (5 years).

If all these estimates are combined, then:

Practical economic fusion power production will probably not be achieved before the 1990's. It is possible, but unlikely, that this could move up to the 1980's with remarkably good luck.

SUPPLEMENTARY STATEMENT OF MARK M. MILLS, UNIVERITY OF CALIFORNIA RADIATION LABORATORY

PLEASE NOTE.—The written material in part II below is essentially just bibliography. By verbatim abstract of various texts and data tables something like a combined textbook-handbook may be compiled. This possibility is indicated by the notes:

Verbatim (for text).
Copy figure (for diagrams).
Copy table (for data).

General bibliography is indicated separately.

II. RADIOACTIVITY AND RADIATION

A. Atom, nucleus radioactivity

(Includes: Nature of radiation, mass and energy, fundamental particles, radioactivity, nuclear reactions.)

Verbatim (including figures): From: H. D. Smyth, Atomic Energy for Military Purposes, Princeton Press, 1945. Part of chapter I, pages 1–21.

Additional bibliography:

1. I. Kaplan, Nuclear Physics, Addison-Wesley, 1955. Especially chapters 1, 2, 3, 8, 9, 10, 11, 13, 14, 15, 16, 19.
2. E. Pollard and W. L. Davidson, Jr., Applied Nuclear Physics, Wiley, 1942. Especially chapters 1, 2, 5, 6, 10.
3. F. Rasetti, Elements of Nuclear Physics, Prentice-Hall, 1936. Epecially chapter II.

B. Interaction of radiation with matter

(Ionization, energy transfer, penetration, absorption, attenuation, induced radioactivity, secondary radiations, neutrons.)

Verbatum (including figures and tables): From: E. Pollard and W. L. Davidson, Jr., Applied Nuclear Physics, Wiley, 1942. All of chapter 2, pages 10–17; part of chapter 3, pages 18–23 (down to "Applications of").

Additional bibliography:

F. Rasetti, Elements of Nuclear Physics, Prentice-Hall, 1936. Especially chapter III.

I. Kaplan, Nuclear Physics, Addison-Wesley, 1955. Especially chapters 13, 14, 15, 18.

C. The roentgen and the curie

(Roentgen, curie, relative biological effectiveness (RBE)).

Verbatum: From: M. S. Fair, chapter 2.2, volume I, The Reactor Handbook (AECD–3645), United States Government Printing Office, 1955. All of chapter 2.2, pages 629–635.

Additional bibliography: United States Department of Commerce, National Bureau of Standards, Maximum Permissible Amounts of Radioisotopes in the Human Body and Maximum Permissible Concentrations in Air and Water. Superintendent of Documents, 1953. (Usually known as NBS Handbook 52.)

D. Neutron fission and chain reactions

(Includes: fission reaction, chain reaction).

Verbatum (including figures):

(1) From: H. D. Smyth, Atomic Energy for Military Purposes, Princeton University Press, 1945. Parts of chapters I and II, pages 22–39, ending on page 39 at "Availability of materials".

(2) From: The Reactor Handbook (AECD-3645) volume I, Physics. Tables 1.2.11, 1.2.8, 1.2.9, 1.2.10, figure 1.2.13.

Additional bibliography:

I. Kaplan, Nuclear Physics, Addison-Wesley, 1956. Especially chapters 18, 19, 20.

S. Glasstone and M. C. Edlund, The Elements of Nuclear Reactor Theory, Van Nostrand, 1952.

The Plutonium Project, Nuclei Formed in Fission, Decay Characteristics, Fission Yields, and Chain Relationships, Rev. Mod. Phys. *18*, 513 (1946).

E. Nuclear fusion and thermonuclear energy

Verbatum: (None).

Bibliography: R. F. Post, Controlled Fusion Research, Rev. Mod. Phys. *28*, 338 (1956).

Nucleonics, page 23, December 1955; page 42, February 1956.

F. Particle accelerators as source of radiation hazard

Particle accelerators produce infinitesimal amounts of radioactivity as compared to fission nuclear reactors. Safe management of radiation and radioactivity is, of course, required; but there is no hazard to the public.

Bibliography: I. Kaplan, Nuclear Physics, Addison-Wesley, 1956, chapter 21, pages 541–560.

Note.—The bibliography for part III below is given at the end. The material given below is itself an actual insert for the record.

III. CONTROLLED FISSION AND FUSION REACTORS AND THEIR POTENTIAL AS A SOURCE OF HAZARD

A. Fission reactor (nuclear reactors)

1. The nature of fission chain reactors.—(*a*) Description: Geometrically, a nuclear reactor installation is often arranged (starting from the center and working outward) as follows: core region, consisting of fuel, moderator, fertile material if any, cooling passages; reflector and/or blanket, consisting of neutron reflector and additional fertile material, if any; cooling if needed; reactor tank, a substantial mechanical structure containing the core and reflector regions; shielding to confine nuclear radiations inside the core-reflector system (for power reactors the shielding can be ten feet thick and weigh many hundred tons—it represents a substantial mechanical barrier as well as a radiation barrier); reactor room, containing instruments and controls; and containment shell outside the reactor room to prevent escape of radioactivity to the surrounding area if material should escape the shield. The massive shield can serve to protect the containment shell from any mechanical violence originating inside the reactor.

The components of a nuclear reactor installation may be classified into six main parts according to function: basic nuclear system, nuclear control system, heat removal (cooling) system, reactor structure, shielding, and external containment shell.

The basic nuclear system includes the nuclear fuel, fertile material if any, and neutron moderator inside the core and the neutron reflector outside the core. Coolant or structure may provide some of these functions. The basic nuclear system sets up the conditions enabling the system to go critical, and determines the neutron spectrum and neutron economy. A high ratio of fuel (U–235, Pu–239, U–233) to other materials can lead to a fast spectrum reactor, the neutrons are absorbed to cause a fission before slowing down.

The nuclear control system interacts with the basic nuclear system to provide control of the chain reaction and set the reactor power level. Important elements are: Instruments such as sensing elements (ion chamber, fission chamber) and circuitry to indicate the intensity of the chain reaction. An auxiliary neu-

tron source to be inserted in the reactor at low power levels so that the instruments may always have a detectible level of neutrons and monitor low-power operation. Neutron control elements (control rods) that are inserted in the core or reflector and actually control the neutron by absorption or multiplication. Mechanical linkages and servo systems that move the control elements on human or automatic command.

The heat removal system consists of coolant flow passages inside the core and appropriate external pumps and piping. An ultimate heat dump to the atmosphere or a river must be provided.

The reactor structure consists of the basic mechanical support to keep the various components in proper position. For example, a portion of a fuel element is usually present for structural reasons.

The shielding and containment components have been mentioned previously.

(*b*) Complexity: From the brief description above, it may not be obvious that nuclear reactors are complex. In general, except for very simple critical assemblies, nuclear reactor installations are large, complex facilities requiring considerable engineering management and coordination in their design and construction, and good administrative management in their operation.

Even if all the basic problems are understood (and there still remain some unknowns), this complexity makes it very difficult to predict the detailed behavior of such a machine from first principles. Seemingly unimportant features can be overlooked, and can be of practical significance.

Currently, with the rapid strides in reactor development which are underway in the United States, not only are individual reactors complex but also the whole picture of reactor technology is changing extremely rapidly as many novel types of machines are investigated and put into development (reference 1).

(*c*) Reactor control:

(1) Delayed neutrons: A brief description of the elements of a nuclear reactor control system has been given. The feasibility of practical reactor control lies in the fact that some of the neutrons emitted in the fission reaction are emitted after some delay from the decay chains of the fission fragments.

Within a nuclear reactor neutrons are being repeatedly produced and absorbed. The time between the birth and death of a neutron is called the neutron lifetime (or neutron generation time). This time is quite short, one millisecond for a large graphite moderated thermal reactor to as short as one tenth of one microsecond for a fast neutron spectrum reactor. The quantity, k, the multiplication of the reactor, is defined to be the ratio of the average number of neutrons in a given generation to the average number in the preceeding generation. The following definitions are usual:

If:

(1) $k>1$, the reactor is supercritical, the neutron level and power level are increasing.

(2) $k=1$, the reactor is critical, the neutron level and power level are constant.

(3) $k<1$, the reactor is subcritical, the neutron level and power level are decreasing.

Now k works on each new generation of neutrons, and acts like compounding interest. Consider a supercritical reactor assuming no delayed neutrons and with $k=1.02$. The neutron level (and power level) will double in 35 generations or (with one millisecond generation time) at the end of 0.035 second! With $k=1.005$, the time to double the power is 0.14 second.

We now consider the delayed neutrons. In the case of U–235 of all neutrons produced in fission, a fraction 0.0073 are produced with an average delay of 10 seconds. As long as (k—1) is less than the delayed fraction the reactor acts as if the neutron generation time were several seconds rather than the 0.001 second of the preceeding example. Detailed calculations give the table:

k	Time to double power [1]	
	No delayed neutrons	Fraction delayed neutrons is 0.0073
1.005	0.14 second	2.1 seconds.
1.020	0.035 second	0.054 second.

[1] Neutron generation time 0.001 second.

This is an example of a general result: as long as $k-1$ is less than the fraction of delayed neutrons, the reactor power level changes over periods of seconds to minutes and the reactor is easy to control. If $k-1$ appreciably exceeds the delay fraction (0.0073), the reactor changes power quite rapidly. The control of all reactors exploits this fact, and their planned operation keeps the excess reactivity, $k-1$, less than the delayed fraction of neutrons. It is entirely practical to design reactors and control systems to do this, although variations due to fuel burnup and reactor temperature changes may require as much as 0.30 excess reactivity in the basic nuclear system which is "held down" by control rods.

As long as the excess reactivity is well within the fraction of delayed neutrons (sometimes called a dollar and indicated by $), the time behavior of the reactor is determined by the delayed neutron periods (seconds to minutes) and a fast spectrum reactor is as readily controlled as a thermal spectrum reactor despite the much shorter fast spectrum nutron generation time. With a fast reactor care must be taken not to exceed the delayed fraction.

(ii) Reactivity effects: In many reactors it turns out that the reactivity, or value of $k-1$, is changed when the reactor temperature is changed. Since a reactor produces heat, it can change its own temperature.

If increasing temperature increases the reactivity, this can lead to higher power levels, more heating, and the reactor can boot-strap itself into a nuclear excursion. Such a reactor is said to have a positive reactivity coefficient, to be unstable, and is called autocatalytic. Such machines are dangerous and should be avoided.

If increasing temperature decreases the reactivity, it is said to have a negative reactivity coefficient and is usually called a stable reactor.

For either positive or negative reactivity cofficient there may be a time delay of several seconds or more between a change in power and an effect on k. (For example, the time to conduct heat from a fuel element into a moderator which changes the behavior of thermal neutrons). This delay may allow feedback and divergent reactor power level oscillation even when the cofficient is negative. The oscillations take some time to build up, and there is usually ample time to shut the reactor down if this occurs (reference 2). Nevertheless, even this should be avoided whenever possible.

(iii) Built-in control: At sometime, practically everyone makes mistakes. for this reason, it is usually considered very desirable to have negative, quick-acting, reactivity coefficients. Then if a mistake is made in manipulating the controls (as in wiring-up the control system if it is automatic), the reactor itself will control itself. This intrinsic, built-in, stability appears to be attainable for many reactors and is a continuing goal of reactor design.

2. The Nature of Nuclear Reactor Hazards.—The hazard of a nuclear reactor is due to the possibility of release of the large quantity of radioactive material contained within it.[1] The bulk of this radioactivity is due to fission products although some is due to neutron activation and the nuclear fuels.

If substantial quantities of radioactivity escape, we know from the Brookhaven report that this can be a very serious matter (reference 3). The essential point is to prevent an escape of radioactivity.

Nuclear reactors cannot explode like an atomic bomb, or produce a nuclear explosion as violent as chemical explosives.[2] This is true for even fast reactors (reference 4). There is no explosive danger to off-site people (the public).

If material does escape from the reactor, the major radioactivity and major hazard is due to fission products. They constitute many different chemical species, and this makes cleanup and decontamination difficult since no single procedure will get them all.

This hazard is real; in its worst form it can be serious indeed (reference 3), and it should not be ignored. However, I feel sure that if proper attention is given to the problem, particularly the technological aspects of safe reactors, that controlled fission reactors can very rapidly supply increasing material benefits to mankind. In this connection, the Division of Reactor Development of the Atomic Energy Commission has an effective and expanding program of research underway directed to the technological aspects of reactor hazards (reference 5).

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[1] By employing liquid fuel, and by continuous chemical processing during reactor operation, this large accumulation of radioactivity can be almost eliminated. This is an attractive safety feature of the liquid homogenous reactor.

[2] For certain kinds of reactors, if they should get out of control and melt, there is a possibility of chemical explosion from reacting core components. Aluminum fuel plates in water coolant-moderator are an example. If the plates were to melt (due to a nuclear excursion) an explosive aluminum water reactor could theoretically occur. Whether it will really occur, and occur rapidly, in a nuclear reactor is not clearly settled. More experimental work is needed to clarify this problem. Even so, there is no off-site explosive hazard.

The hazard of a nuclear reactor can be thought of as the product of two factors: the consequences of a radioactivity release, and the chance that such a release might occur. Both are discussed in the Brookhaven report (reference 3). The chance of a release is not known on the basis of experimental statistics (of the kind obtained by insurance companies on human life expectancy). Fortunately, no serious releases of radioactivity from a nuclear reactor have occurred so there are no statistics. I hope such statistics will not be forthcoming.

As pointed out in Section 1: The Nature of Fission Chain Reactors, reactor technology is undergoing rapid growth in the number of reactors, in the variety of reactor types, in the number of novel kinds being investigated, and in the number and variety of new organizations undertaking reactor activities. Furthermore, reactors are complex machines. Consequently, the ultimate responsibility for reactor safety will have to rest with the organizations that design, build, and operate nuclear reactors. However, systematic review by a skilled body of experts can help such organizations to achieve adequate coverage of hazard problems.

3. Technical aspects of hazards.—The following description is not exhaustive but illustrative. It is frequently helpful in organizing hazard considerations. Often some particular reactor will exhibit some specific characteristics which require careful attention, but which are not included in the following summary.

Nuclear reactors may break open and release fission products by undergoing (1) a runaway nuclear excursion (as runaway nuclear oscillation, (2) by meltdown due to radioactive decay of the accumulated fission products if adequate cooling of the reactor is not maintained (this can happen even if the neutron chain reaction and the associated fissions are stopped), and by (3) possible chemical reactions between the core components.

The possibility of runaway, and desirable reactor stability features were mentioned in the section 1 (*c*) Reactor control.

Cooling failure may occur due to failure in the external cooling system (pipe failure, loss of pumping power) as due to plugging of flow passages in the core (perhaps due to melting brought about by a nuclear runaway). The inability to completely "shut-off" heat generation poses a difficult problem. Continuous fission product removal requires liquid fuel, or emergency, standby, cooling systems are ways to mitigate this problem.

Chemical reaction, clearly not present under normal conditions, may take place if the core is melted either by cooling failure or by a nuclear runaway.

More conventional sources of disruptive energy, possibilities which also require care, are: Steam explosion in boiling water or pressurized water reactors, sodium fire with liquid metal cooled reactors, combustion of organic moderators, or burning of air-cooled graphite piles.

4. Reactor types.—The power level, and therefore the quantity of fission products, usually progresses with reactor types from extremely large for power and production reactors, to large for research and test reactors, to substantial for reactor experiments, to negligible for critical experiments. Fortunately, the chance of a radioactive release would appear to run the other way. The power and production reactors are run on steady routine schedules, the large research reactors are nearly the same in this regard, the "reactor experiment" reactors are less known and less routinely operated, and the critical experiments are flexible (variable in geometry and nuclear characteristics). The only nuclear runaway accidents of any moment have occurred with critical experiments or with reactor experiment reactors.

At startup, a brand new nuclear reactor has not accumulated radioactivity. Although the startup procedure is more risky than regular operation because the precise calibration of the reactor is not established (the startup procedure supplies this precise calibration, the absence of accumulated radioactivity minimizes the risk and makes public hazard almost negligible.

5. Radioactive waste.—Radioactive waste materials from reactor operation present a problem requiring care and good management. They do not represent an acute hazard problem like that of a malfunctioning reactor.

B. Controlled thermonuclear reactors.—The hazards that may eventually be encountered from controlled thermonuclear reactors are very difficult to anticipate, may turn up in a totally unexpected quarter, and may well turn out to be negligible. With the understanding that what follows is "crystal gazing," I will attempt to answer the questions suggested by the outline.

1. Radioactive hazard: The nuclear fuels for thermonuclear system are not radioactive, with the exception of tritium (Post, reference 6, suggests D, T, He-3,

Li-6, Li-7). Tritium is no hazard outside the body. The fission fuels are hazardous with Pu-239 and U-233 having much more stringent limits than tritium in the body.

Perhaps the best test is delayed, activated, and produced, activity associated with the reactions. Assume for example:

For the fission reaction

$n+U-235 \rightarrow f.f.+2.5\ n$ (2 Mev., each)

For the thermonuclear reaction we add up the first three given by Post (reference 6):

$D+D \rightarrow He^3+n$ (2 Mev.)
$D+D \rightarrow T+p$
$D+T \rightarrow He^4+n$ (14 Mev.)

$5D \rightarrow He^3+He^4+p+n$ (2 Mev.) $+n$ (14 Mev.)

Assuming arbitrarily that:

(1) 2 Mev. neutrons do some activation in fission and fusion.
(2) 14 Mev. neutrons to 3 times the activation as 2 Mev. neutrons.
(3) Half the activity is prompt and half is delayed.
(4) The 2.5 x 2 Mev. neutron activation is 6 Mev. prompt and 6 Mev. delayed.

For the fission system the assumptions (3) and (4) are reasonable but arbitrary. They may be meaningless for fusion. For example, fusion systems may be designed to eliminate activation effects.

With these assumptions one can make the table:

	Fission	Thermonuclear
Immediate energy:	*Mev*	*Mev*
Direct	178	25
Activation	6	9
Delayed energy:		
Direct	13	
Activation	6	9
Total energy	203	43
Ratio: Delayed/total	0.093	0.21

This implies that the delayed energy (accumulated radioactivity) could be about twice as bad for thermonuclear as for fission. However, some two-thirds of the fission reaction accumulation is fission products which are essential for the machines' operation. The thermonuclear machine could be designed for minimum activation if necessary.

2. Other hazards: Without a design, speculation is very unreliable.

3. Future energy demands: The supplies of deuterium in the world (oceans), in terms of energy, represent an inexhaustible supply of power compared to present consumption (about 10 billion years).

4. Time scale for thermonuclear development: This prediction is difficult, and I will assume that economic fusion power is of interest. One may argue as follows:

(1) Currently fusion has not reached the development of the late 1942 Chicago fission pile (14 years or more behind fission).

(2) Fission is not yet economic, but may be so by 1970 (W. K. Davis, private communication) (12 years).

(3) Fusion development seems harder than fission development (5 years).

(4) Fossil fuel shortage will be eased by fission reactors by the time fusion of practical interest so that the economic pressure for fusion development will be reduced (5 years).

If all this is added together:

Practical economic fusion not before the 1990's. It is possible, but unlikely, that this could move up to the 1980's with remarkable luck.

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Representative HOLIFIELD. The committee will stand adjourned until 2 o'clock when we will have Dr. Graves and the people from the Department of Defense before us.

(Whereupon, at 12:30 p. m., the committee recessed, to reconvene at 2 p. m., of the same day.)

AFTERNOON SESSION

Representative HOLIFIELD. The committee will be in order. It is our intention this afternoon to have testimony on the production of radiation and radioactivity by the detonating of nuclear weapons.

Our first witness on this subject is Dr. Alvin C. Graves, of the Los Alamos Laboratory. Dr. Graves, this committee is pleased to have you before us again. You have been before us many times over the past 10 or 11 years, and I know of no one that is better qualified to give us testimony on this subject than you, because you have had charge in a great many instances of these weapons tests.

So at this time, sir, the committee would be glad to hear your testimony.

STATEMENT OF DR. ALVIN C. GRAVES, LOS ALAMOS SCIENTIFIC LABORATORY [3]

Dr. GRAVES. Mr. Chairman, we are teachers at heart and are used to talking at the blackboard. So if you will pardon me, I will feel much more at home if I can talk at the board.

I would also like very much to express my regret to you that I was unable to prepare a written statement for you in time to be submitted for the record beforehand. On the other hand, I do have a written record here now, and if I may leave this with you, you can make such disposition of it as seems appropriate to you.

Representative HOLIFIELD. Thank you.

Dr. GRAVES. In this, the fifth topic of the hearings before your committee, you have asked me to cover the production of radiation and radioactive materials by nuclear-weapon detonations.

Although Dr. Mills in his discussion this morning went into the fission and fusion processes to some extent, I would like to go over some of the material that he covered, because the production of radia-

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tion and radioactive material is in this set of processes and, unless we cover them carefully, we will not be able to make the production of radiation understandable.

Moreover, in view of the desire to have this hearing not only for our own purposes but for purposes of informing the public, I hope you will bear with me if I speak to some extent on material which is well known to you.

Representative HOLIFIELD. Would you like to have the prepared testimony put in as you have prepared it, Doctor Graves?

Dr. GRAVES. If you would.

Representative HOLIFIELD. It will be accepted on that basis.

(The statement referred to follows:)

THE PRODUCTION OF RADIATION AND RADIOACTIVITY BY DETONATING NUCLEAR WEAPONS

(By Alvin C. Graves)

In this, the fifth section of the hearings before your committee, you have asked me to cover the production of radiation and radioactive materials by nuclear-weapons detonations. Although earlier witnesses have presented you with general background material on atomic radiation and its effect, I feel it appropriate to risk some repetition in my discussion since the origin of radiation and radioactive materials in detonations is in the fission process and in reactions with neutrons produced in the detonation. In view of your desire to use this investigation to inform the public on the subject of fallout from nuclear-weapons tests, I should like to request your indulgence if I speak to some extent on material which is already well known to you.

1. Description of nuclear-weapon detonation

Nuclear weapons differ from normal high-explosive bombs in three important respects. In the first place, their energy may be made orders of magnitude greater; second, their detonation is accompanied by intense thermal and nuclear radiation; and, third, there remains when the detonation is completed extremely large amounts of radioactivity. In describing the detonation of an atomic weapon completely, one should discuss in great detail blast effects, which differ in some important ways from blast effects of normal explosions, thermal radiation, and light which, as mentioned above, are novel features of nuclear detonations, initial nuclear radiations consisting of gamma, neutron, alpha, and beta radiations and, finally, delayed radiations. However, for purposes of this hearing, I propose to place my major emphasis on the last two of the above topics.

The release of energy in nuclear detonations deposits a very large amount of energy in a very small region of space in an extremely short time. For example, the complete fission of 1 pound of uranium (a little over a cubic inch) would produce an amount of energy equivalent to 9,000 tons of TNT, and the fusion of a pound of deuterium would produce energy equivalent to 26,000 tons of high explosive. Hence, the initial effect is a rise of temperature of bomb materials to a very high temperature of many million degrees as contrasted with perhaps 5,000 degrees in the case of ordinary high explosives. This extremely high temperature, not very different from that at the center of the sun, is accompanied by tremendous pressures such that, at a very few microseconds, pressures of the order of millions of pounds per square inch exist, and an expansion of vaporized bomb materials begins to take place. At 0.7 millisecond, the fireball for a 1-megaton detonation will have a radius of 220 feet, and at about 10 seconds will have reached its maximum diameter of about 7,000 feet. This growth in size is accompanied by a decrease in temperature and pressure, formation of a shock wave which produces the familiar blast effects and, at the same time, the fireball begins to rise and to engulf large quantities of surrounding materials, air, dirt, or water, depending on the particular situation in which the bomb was detonated. While the ball of fire is still luminous, fission products, fissionable material, bomb casing, and other materials will be present in the form of vapor, whereas, as the fireball increases in size and cools, these vapors will condense and be absorbed in or on other particles such that the cloud becomes a mixture of gaseous and solid radioactive particles. The cloud from a weapon in the megaton range would rise initially at a rate like 250 miles per

hour and, after a minute, would have risen perhaps 4 miles or more. It is the extremely rapid rise of fireball which makes meaningful a distinction between immediate or prompt radiation and delayed or residual radiation. The amount of radiation which can reach a point from distances like 4 miles is small and, hence, establishing an arbitrary division by defining all radiation delivered in the first minute as prompt radiation and all after 1 minute as residual radiation tends to correspond to a distinction between that radiation which is delivered from the fireball and that which is delivered from fallout, or from deposited or induced radioactive materials. Because of its connection with the problem of radiation, the following table is given as an illustration of the rise of a cloud from a 1-megaton detonation.

Rate of rise of cloud from 1-megaton detonation

Height (miles)	Time (minutes)	Rate of rise (miles per hour)
2	0.3	300
4	0.75	200
6	1.4	140
10	3.8	90
14	6.3	35

With different detonation yields and atmospheric conditions, different times and rates of rise would, of course, be observed.

2a. The fission process

For our purposes, an atom may be represented as composed of a massive central nucleus and a number of relatively light electrons rotating about it. The revolving electrons are important in ordinary chemical reactions, but in nuclear reactions it is the nucleus which is of importance. In most reactions in which a neutron is involved, the resulting nucleus will have nearly the same mass as the original nucleus and, hence, the number of possible product nuclei is very small. In the fission process, where the fissioning nucleus splits into two unequal pieces, both of which are very different in mass from that of the original atom, the number of possible product nuclei is very large indeed. It seems, for example, that U–235 may be split in something like 40 different ways, and that over 200 different nuclides, counting primary fission fragments and decay products, may be formed. Since these products are highly radioactive, for reasons that I will discuss, the resulting radioactivity is that of a mixture of many radioactive nuclei with different periods and different modes of disintegration.

The simplest atomic nucleus is that of hydrogen, which has an atomic mass of 1, contains 1 unit of electrical charge, and is known as the proton. All other atomic nuclei contain a number of protons and neutrons. The neutron also has an atomic mass of 1, but has no electrical charge. Hence, a neutron can be converted to a proton by loss of an electron. In nature there appears to be a strong tendency for the number of neutrons and protons in nuclei to be equal. For example, the helium nucleus contains 2 neutrons and 2 protons. The nucleus of 1 species of lithium contains 3 neutrons and 3 protons; boron, 5 neutrons and 5 protons; carbon, 6 and 6; nitrogen, 7 and 7; oxygen, 8 and 8; and so forth. Hence, uranium, with its 92 protons and 143 neutrons, is very neutron rich, and fragments resulting from fission are unstable such that, to become stable, they must either lose neutrons or convert neutrons into protons. Both processes occur. A number like 2, 3, or 4 neutrons is emitted, essentially instantaneously. And then, at various times from a small fraction of a second to many years, a nuclear neutron is converted into a nuclear proton by the emission of an electron. A fraction of 1 percent of emitted neutrons are delayed. That is, although about 99¼ percent of neutrons will be emitted within about 10 to 12 seconds of fission, ¾ percent will be emitted at times as long as several minutes. Periods for delayed neutrons vary from a fraction of a second to almost a minute.

Reference to the fission yield curve of U–235 indicates that production of masses 95 and 139 should be a common mode of fission of that element. A reasonable distribution of uranium protons suggests that these fragments might be Sr–95 and Xe–139. These fragments would both be unstable and would decay by the following processes:

$$Sr^{95} \rightarrow Y^{95} \rightarrow Zr^{95} \rightarrow Cb^{95} \rightarrow Mo^{95} + 4e^- + \text{gamma rays}$$
$$Xe^{139} \rightarrow Cs^{139} \rightarrow Ba^{139} \rightarrow La^{139} + 3e^- + \text{gamma rays}$$

Hence, in that particular mode of fission, 4 radioactive decays would occur in the light element and 3 in the heavy, with the emission of 7 electrons with 7 different decay periods, and with the production of many gamma rays of many energies. In fission with slow neutrons, production of stable fission fragments appears to be rare, although short chains of 1 or 2 decay stages appear. Two of the longest known are $Kr^{97} \rightarrow Mo^{97}$ among lighter fission products, and $Xe^{143} \rightarrow Nd^{143}$ among heavier. Each chain involves 6 stages of decay. The average is about 3 stages, or slightly more. During most such processes, gamma rays of various energies are emitted. These phenomena, involving the conversion of one element into another with emission of gamma rays and electrons, are the cause of much of the radioactivity observed in fallout. Induced activity will be discussed in a later section.

2b. Fusion process

The fusion process is quite different from the above and, hence, has a different importance in radiation and fallout considerations. Among light elements, binding energy per nuclear particle tends to decrease with increasing mass as contrasted with an increase with increasing mass in heavy elements. Hence, among light elements energy is released when light elements are joined together to make heavy elements. Such reactions, because of this process of joining atoms together, are called fusion reactions and, because they can be initiated and sustained by high temperatures, are also known as thermonuclear reactions. The number of modes is always extremely limited, at least for low energies and low mass numbers, as contrasted with the large number of modes in fission. Two atoms of deuterium, for example, can fuse in two ways; one involves production of hydrogen of mass 3 or tritium and a proton; the second involves production of helium of mass 3 and a neutron. In this case, only tritium is radioactive. Moreover, tritium itself has a very high probability of fusing with deuterium so that, under many conditions, tritium will combine with deuterium to produce helium of mass 4 and a neutron. In deuterium fusion, the following reactions might occur:

$$_1H^2+{}_1H^2 \rightarrow {}_2He^3+{}_0n^1+3.2 \text{ Mev}$$
$$_1H^2+{}_1H^2 \rightarrow {}_1H^3+{}_1H^1+4 \text{ Mev}$$
$$_1H^2+{}_1H^3 \rightarrow {}_2He^4+{}_0n^1+17 \text{ Mev}$$
$$_2He^3+{}_2He^3 \rightarrow {}_2He^4+2{}_0n^1+11 \text{ Mev}$$

Hence, the net direct result of fusing deuterium nuclei is production of protons, alpha particles, and many neutrons, but essentially no radioactive particles.

2c. Induced activities

A secondary source of radioactive materials results from neutrons produced in both fission and fusion processes. It was mentioned above that, depending on the particular mode of fission, two or more neutrons may be emitted and that deuterium fusion is accompanied by production of many neutrons. In any detonation of a nuclear weapon, some of these neutrons will escape and react with nitrogen atoms in air to form carbon 14 which is radioactive. In case detonations occur under conditions such that dirt is exposed to neutrons, many other neutron reactions can occur. Normally, radioactive materials induced by such processes have relatively short lives and will not be of major importance. Carbon 14, however, has a long life of over 5,000 years. Since it emits no gamma rays and only relatively weak beta rays, and since the amount formed will not be large compared to that that is present in nature, it will not be a significant activity. Oxygen absorbs fast neutrons to some extent, but the isotope of nitrogen formed has a half-life of only 7 seconds and, after a few minutes, will have completely decayed.

An important contribution to residual activity will be due to 14.8-hour sodium 24 formed by neutron capture by sodium. It emits beta particles and two relatively high energy gammas of 1.4 and 2.8 Mev. 2.6-hour Mn–56 emits beta particles and several high energy gammas. With some soils this element might cause an appreciable activity for a few hours, but within a day its activity would have been reduced to less than a percent of its initial amount. Si–31, with a half-life of 2.6 hours, is formed in appreciable quantities but is chiefly a beta emitter. Al–28 has a half-life of only a few minutes and, hence, will have disappeared in the first hour. The chlorine present in salt produces 37-minute Cl–38 which emits betas and high energy gammas.

Other materials such as zinc, copper, iron, glass, and salt in foodstuffs might, under some conditions, become active enough to be significant. Normally, how-

ever, induced activities will have a short half-life and make a significant contribution for only a relatively short time or will have little energy to emit.

2d. Initial radiation

As stated earlier, initial nuclear radiation is defined as that radiation which is delivered during the first minute after a detonation. Although initial neutrons and gamma rays constitute only about 3 percent of bomb energy as compared with 33 percent appearing as thermal radiation, a considerable proportion of bomb casualties can be caused by initial nuclear radiations. Fifty percent of people shielded by as much as about 2 feet of concrete would be killed by initial gammas and neutrons 1 mile from a 1-megaton bomb. A much lighter shield would give complete protection from thermal radiation.

Initial radiation consists of almost all neutrons, a part of gamma radiation, some beta and alpha rays. Alpha particles come from two sources. They are emitted from fissionable materials, plutonium and various isotopes of uranium, or from thermonuclear or other reactions in which alpha particle emission takes place. Beta particles come from fission fragment decay, as well as from such processes as photoelectric and Compton effects. Alpha particles, besides being relatively rare, have a short range like an inch or so in air and, hence, are not important in initial radiation. Beta particles have somewhat greater range but still cannot reach the ground from an air-burst. Both neutrons and gammas penetrate air to considerable distances and both are highly injurious to living organisms. Hence, they constitute by far the most important part of initial radiation. To a great extent, neutrons are slowed down and captured in bomb materials themselves, but enough fast neutrons escape to constitute an appreciable hazard at considerable distances. Only about a percent of gamma rays produced directly in fission escape from the immediate region. However, this contribution and that of gamma rays produced in other processes, as discussed below, produce sufficient intensity at considerable distances to constitute a real hazard. Gamma rays present in initial radiation come from a number of different sources. They originate not only in fission but also in various other neutron reactions. First, neutrons may be captured with formation of a highly excited compound nucleus which may lose its excitation by emission of gamma rays. For example, when neutrons are captured by nitrogen to form C–14, two high energy gamma rays are emitted. These can be an important component of initial radiation. Second, neutrons may also react with nuclei with the emission of protons or alpha particles, and gamma rays are frequently emitted in such processes. And, finally, neutrons may be scattered inelastically by nuclei with the emission of gamma rays. That is, a fast neutron in a collision with a nucleus may transmit some of its energy to that nucleus leaving it in an excited state. The excited nucleus may then revert to its normal state by emission of a gamma ray.

Fission fragments themselves decay with periods from a millionth of a second or less to many years, and many of these decays during the first minute are accompanied by emission of gamma rays known as delayed gammas. Although instantaneous and delayed gammas contributing to initial gamma radiation are produced in about equal amounts, instantaneous gammas are absorbed so strongly that they constitute only about 1 percent of external dose.

Gamma rays lose energy or are absorbed in going through matter, by excitation and ionization. It is believed that the chemical decomposition of molecules caused by this effect is the major cause of injury to animals and plants. Therefore, quantity of gamma radiation or gamma ray dose is defined in terms of ionization produced. The dose-distance relationships for gamma rays and neutrons are exceedingly complicated because of the large number of effects involved, but rough analytical expression can be obtained. Since surface area of a sphere increases with square of radius, intensity—that is, gamma rays (or neutrons) per square centimeter—will decrease with square of distance. In addition, since gamma rays and neutrons are strongly absorbed, degraded in energy, and scattered in air and other materials, there will be an exponential decrease depending on the absorption properties of air and these materials. Due to a balance between various effects, gamma ray dose at a given distance is roughly proportional to the yield up to about 80 or so kilotons. But, above that, the dose increases sharply with yield such that at 100 kilotons the dose will be 50 percent higher than indicated by a direct proportion, and at a megaton will be high by about a factor of five. This increase is due to decreased air density because of long negative pressure phase associated with large yields, and is partly counterbalanced by increased dose at lower yields caused by slower cloud rise with con-

sequent increase in contribution from fission product gamma rays. The net effect is very complicated, but roughly gamma ray dose decreases by a factor of 10 every 780 yards, in addition to inversely as the square of distance, and neutron dose decreases by a factor of 10 every 560 yards. Appropriate consideration of all the above factors indicates that dose-distance relationship for gamma rays can be approximated roughly by the following formula:

$$N=\frac{8.4\times10^{8}W}{D^{2}}e^{-D/342}\text{ rems}$$

where W′ is yield in kilotons increased at yields of 100 kilotons or above, as indicated previously, and D is distance in yards.

Neutron dose-distance relationship may be expressed to a very poor degree of approximation by the following formula:

$$I=1.4\times10^{8}\frac{W}{D^{2}}e^{-D/238}\text{ roentgens}$$

This ignores variations because of variation in neutron capture in bomb materials for different designs, as well as differences because of high energy neutrons from fusion reactions, and so on.

2e. Residual radiation

About 0.11 pound of fission products are produced for each kiloton of fission energy yield and its radioactivity at one minute would be comparable with that of many pounds of radium. A megaton of fission energy would, therefore, result in activity at one minute comparable with that of very many tons of radium. During the first 24 hours there would be a decrease by a factor of more than 6,000, but it has been estimated that this activity, if uniformly spread over 10,000 square miles, would cause a radiation intensity after 24 hours of 2.7 roentgens per hour at a distance of 3 feet above the ground. Unprotected personnel in that area would receive more than 300 R after the first day. During the first day they would receive many times that amount. Of course, activity would not be spread uniformly over such an area. Actual fallout situations will be covered by later speakers.

An indication of the speed with which fission product activity decreases can be obtained by the rough rule that for every sevenfold increase in time there will be a tenfold decrease in activity. For example, after 7 hours the activity would be one-tenth of its value at 1 hour; after 49 hours, or about 2 days, it would be 1 percent of its value at 1 hour; and after about 2 weeks, it would be one-tenth of a percent of its value at 1 hour.

There is given below the estimated total gamma activity of fission products from a 20-kiloton atomic bomb, expressed in megacuries, at various times after the detonation.

Total gamma activity of fission products in megacuries

Time:	*Activity*	Time—Continued	*Activity*
1 minute	820, 000	1 month	2. 3
1 hour	6, 000	1 year	. 11
1 day	133	10 years	. 008
1 week	13	100 years	. 0006

3. Partition of energy from detonations

The partition of energy from a nuclear explosion as betwen blast and shock, thermal radiation, and nuclear radiation varies appreciably with device design and with condition of firing. As a general approximation, nevertheless, I shall give the division which could be expected from detonation of a device of 1-megaton yield, fired within the earth's atmosphere, but at such an altitude that comparatively little extraneous material would be available to be made radioactive by escaping neutrons (as for example, if fired at an altitude of a few thousand feet above the earth). To some extent such a partition is meaningless since eventually all forms of energy will be converted to heat. However, initially energy is partitioned about as follows. About half the energy would be released as blast or shock, one-third would occur as thermal radiation (heat or light), and some 15 percent would be in the form of nuclear radiation. Of this 15 percent roughly one-third would be initial radiation occurring within

1 minute after firing, while two-thirds would be residual radiation. Some residual radioactivity can be deteced for many years after detonation.

The blast or shock energy would have effects quite similar to those experienced in case of ordinary high explosive detonations. These would consist of an initial shock front (the first arrival of highly compressed air), a later region of high and low pressures behind the shock front, and a violent wind flow. The latter would initially be directed in an outward direction, but later in a reverse direction. In the case of a typical air burst, the distance at which a given overpressure occurs varies generally as the cube root of yield. The term "generally" is used since effects such as reflection and refraction of shock waves, dust loading, and the like, may increase or decrease pressures and materially change blast effects at a given point. As an indication of the order cf magnitude of the effect one might expect from blast phenomena, the burst of a 20-kiloton weapon at an altitude of several hundred feet would destroy beyond economical repair multistory reinforced concrete buildings at distances up to a half mile. A 1-megaton burst fired under comparable conditions would cause similar damage at distances up to 2 miles.

The one-third of weapon energy emerging in the form of thermal radiation is contained initially in a relatively small voume of air and incandescent gases resulting from vaporization of device components. This intensely hot spherical mass termed "fireball' is visible until thermal energy has been dissipated and temperature reduced to such an extent that visible light is no longer emitted. The initial fireball temperature is of the order of several million degrees and thermal radiation is made up of rays in the ultraviolet, visible, and infrared. As the fireball cools there is a shift to long wavelengths, but phenomena are complicated by absorption in outer portions of the fireball and in shocked air, by change from emission to absorption spectra, and by changes in position and chemical composition of materials involved. The extent of injury or damage to a person or material from thermal radiation is a function of total energy and of time duration of pulse received. From a given weapon, or from weapons of comparable yield, quantity of thermal radiation energy received per unit area is primarily a function of distance from burst. The amount decreases inversely as distance squared and exponentially because of attenuation in the atmosphere. The period over which thermal energy is given off from an explosion increases with yield—that from a kiloton device being limited to a few tenths of a second but, for a megaton device, the period may extend to several seconds. A 20-kiloton burst could be expected to ignite combustible materials at ranges up to 2 miles, while a 1-megaton burst could have similar effect up to 10 miles. The 20-kiloton burst could cause first degree burns to exposed skin surfaces at ranges of 3 miles, while a megaton burst could cause similar burns at 14 miles.

The partition of energy between various nuclear radiations as observed at a distance depends materially on device design and cannot be discussed in detail in unclassified language. It should be sufficient to point out that neutrons are efficiently converted to gamma rays in heavy materials, gamma rays are absorbed and converted to thermal energy in heavy materials, and neutron energy spectrum depends materially on whether, or on the way in which, fusion reactions are employed, and on the kind and amount of heavy materials used.

4. Types of burst and effect on fallout

For descriptive purposes, three types of burst can be distinguished. These are air, surface, and subsurface bursts. A high air burst is one in which the hot fireball does not touch the earth and, for a megaton device, is about 3,000 feet high or more.

Initial nuclear radiations are absorbed and scattered less in low density air associated with air bursts. Hence, there is less energy degradation and mean free paths are longer. Additionally, less dirt is stirred up to further absorb and scatter radiation. The result is that dose at a given distance is greater.

In the event of high air bursts, fission products are widely dispersed and very little is deposited on the ground under or near the point of detonation. This is the safest type of burst from the point of view of nearby regions as far as fallout is concerned. However, this puts the maximum amount of activity into the atmosphere and is the worst type as far as worldwide fallout is concerned. As height of burst is decreased toward the earth's surface, more and more dirt is mixed with the cloud and more of the activity is absorbed on larger, more massive particles and, hence, falls out locally.

In the case of subsurface bursts, much initial radiation is absorbed within a short distance of the explosion and dissipated in heating ground or water or, in the case of neutrons, in producing induced activities. As depth of detonation increases, less activity appears in the atmosphere and more is deposited locally. Under such conditions, the maximum local fallout and minimum worldwide fallout occurs. At sufficient depths, the detonation would be entirely contained and no fallout problem would exist. Ground water contamination concentration would be a maximum for the latter case and a minimum for high air bursts. Subsurface bursts under water have extreme contaminating effects, and effects on marine life and contamination of seafood can be serious.

5. Limitation on data

Although initially residual radiation decreases approximately as t–1.2, departures from the law becomes appreciable after the first half year or so. Better data on dose and gamma ray energy as a function of time is desirable.

In estimating dose to people one needs knowledge of shielding of typical structures and data for calculating probable dose reduction to individuals engaged in typical activities.

After long time intervals, when most activities will have decayed to such an extent as to be negligible, the activity remaining will largely consist of Cs–137 and Sr–90. More data is needed on uptake of these materials in plants, their conversion to food, and effect on biological systems.

In order adequately to discuss residual radiation for typical situations, much more data is needed as to particle size versus height in the cloud as a function of type of burst, and activity per particle as a function of particle size and position of particle.

Although much effort has been expended on determination of patterns of deposition of residual radioactivity, existing data leaves much to be desired, especially for large yields.

6. Recommendations

It is recommended that:

1. Additional effort be allocated to obtain data as indicated above.
2. Since the actions of individuals within seconds after a detonation could make a difference of thousands of fatalities and could decrease the seriousness of many casualties, it is recommended that increased effort be expended in informing the American people of the effects of weapons and in establishing personal civil defense.

Dr. Graves. I would like to give you a general description of a nuclear-weapon detonation.

Nuclear weapons differ from normal weapons in three important respects. In the first place, the energy from nuclear weapons can be many orders of magnitude greater. The President has spoken about fission weapons of the order of 100 times greater than ordinary weapons. He has spoken about thermonuclear weapons in the millions of tons. So we can speak about nuclear weapons having yields equivalent to millions of tons of high explosive without getting into a classified area at all.

In the second place, when a nuclear weapon detonates, its detonation is accompanied by a very large amount of radiation.

In the third place, when a detonation is over, a considerable amount of residual radioactivity remains on the ground in the neighborhood of the detonation.

In describing such a detonation completely, one should therefore discuss first of all a blast or shock wave. Then one should discuss the thermal radiation, heat, and light which are given out at the time of the detonation. One should then go on to discuss the radiation that proceeds from the detonation, and finally discuss the residual contamination which is left on the ground.

Because of the purposes of this hearing, I intend largely to restrict my comments to the latter two of these subjects.

The release of energy in a nuclear detonation deposits a very large amount of energy in an extremely small region of space. One kiloton—1,000 tons of high explosives—produces a certain amount of energy. That same amount of energy could be given out by the fission of something like a cubic inch of uranium which is about a pound.

So we have a tremendous amount of energy in a real small region of space.

Similarly, in the fusion reaction, the fusion of 1 pound of deuterium could produce an amount of energy equal to that of about 26,000 tons of high explosive. Hence, the initial effect of such a detonation is the rise of temperature of the materials themselves to a very high figure—many millions of degrees—whereas in a normal high explosive detonation, the corresponding temperatures are of the order of 5,000°. Hence, this very high temperature is one of the first effects of a nuclear detonation which is different from that of high explosive reactions.

This extremely high temperature which is not very different from that which exists at the center of the sun is accompanied by tremendous pressures, such that at a very few microseconds pressures of the order of many millions of pounds per square inch exist. An expansion, then, of the vaporized bomb materials begins to take place, and at seven-tenths of a millisecond—that is, seven ten-thousandths of a second—the fireball for a 1 megaton detonation will have a radius of 220 feet, and at about 10 seconds will have reached its maximum diameter of about 7,000 feet. This is for a 1-megaton detonation.

This growth of size is accompanied by a decrease in temperature and pressure, the formation of a shock wave which produces the familiar blast effects, and at the same time the fireball will begin to rise and to engulf large quantities of air and other materials, depending on the way in which the detonation takes place. (See pp. 97–98.)

While the ball of fire is still luminous, fission products, fissionable material, bomb casing, and other materials, are all present in the form of vapor. But as the ball of fire begins to cool, these materials condense; they form little globules in the fireball; they are absorbed on particles of dust and dirt which may be present, and eventually start to fall out in the process which will be discussed for you called fallout.

The cloud from a megaton weapon rises initially at tremendous velocity. Its initial velocity may be as much as 250 miles an hour. Consequently, in the first minute the fireball will have risen to a very great height—as a matter of fact, something like 4 to 4½ miles—and hence the radiation which reaches people on the ground in the vicinity can reasonably be divided into two parts: That part which comes in the first minute, which is sensibly that from the fireball, and that which comes after the first minute when the fireball is so far away that it can no longer have an effect and hence this radiation is from that material deposited on the ground, induced activity, or from fallout.

This distinction is somewhat arbitrary and yet it is a useful distinction, and we speak thus of initial radiation as that which occurs in the first minute, and delayed radiation that which occurs after the first minute.

It might indicate to you, because of this distinction, just how the cloud goes as a function of time. If I list the height of the cloud and the time at which the cloud reaches that height, we will say that

the cloud will be at 2 miles in three-tenths of a minute. It will be at 4 miles in three-quarters of a minute. It will be at 14 miles in about 6 minutes.

Representative VAN ZANDT. It is based on 1 megaton?

Dr. GRAVES. This is all for a 1-megaton bomb.

So when we say that the initial radiation is that which occurs within the first minute, we are saying after that the bomb itself is so far away from us that the radiations from the fireball are essentially negligible compared to other kinds. That is basically the distinction between what we call initial radiation and residual radiation.

Clearly this table will vary for different yields or different atmospheric conditions. But it is an illustration of the height to which the cloud will rise in a given time in order just to make clear the distinction between initial and residual radiation.

Dr. Mills this morning discussed for you the fission process. I would like to repeat, to some extent, the information that he gave, in different language, chiefly because I would like to put a little different emphasis on the remarks that he made.

In the first place an atom may be thought to consist of a central nucleus with a number of electrons rotating around it in various orbits. The simplest nucleus known is that of the hydrogen atom which has a mass of 1 and a charge of plus 1. The hydrogen nucleus is called a proton. All other nuclei consist of protons and particles which we call neutrons. The neutron is just like the proton except that it has no positive charge. It has the same mass but has no charge.

For some reason or other, there seems to be a tendency in nature for nuclei to consist of about equal numbers of protons and neutrons. For example, the helium nucleus contains 2 protons and 2 neutrons. One form of lithium nucleus contains 3 protons and 3 neutrons. Carbon contains 6 and 6. Nitrogen contains 7 and 7. Oxygen contains 8 and 8, and so on. This is a general tendency among nuclei. They are stable when they have roughly equal numbers of protons and neutrons.

In the uranium nucleus, which contains 92 protons and 143 neutrons, it is obvious that this is not the case, because now the number of protons is very much less than the number of neutrons.

In the fission process, therefore, when we split the uranium nucleus we tend to have parts which are very rich in neutrons, and hence in the process we would expect to find neutrons being emitted, or neutrons giving up electrons so that the neutrons are converted to protons. Hence in the fission process we would expect to find neutrons being emitted, we would expect to find beta particles (electrons) being emitted, and we would expect to find energy in the form of gamma rays being emitted.

This is the basic origin for the activity and the radioactivity that is produced in nuclear detonations.

In ordinary chemical reactions, the revolving electrons take part, and they are the things which hold the atoms together to form carbon dioxide, water, salt, and so on.

It is the forces that these electrons produce in one way or another which cause the normal molecular reactions to occur.

In nuclear reactions, it is the nucleus which plays an important part. In the fission reaction we have the uranium nucleus, around this we will have 92 electrons. They are not shown on this chart because they have no importance in the particular reaction we are discussing. (See p. 98.)

The uranium nucleus is struck by a neutron. When the neutron strikes the nucleus, it forms a new nucleus which is a form of uranium but is heavier by 1 unit. It will now be uranium 236.

Uranium 236, then, is unstable in a very strange way. It is unstable in that it tends to split to form two pieces. These two pieces are the fission fragments which we have been discussing this morning and which we will discuss from now on in considerable detail.

In addition to the fission fragments, this reaction also gives off a number of neutrons. The number of neutrons will vary, depending on what the fragments are. It may be 3, as shown here, it may be 2, or it may be 4. But it will vary from one nucleus to another or from one mode of disintegration to another.

You have heard at considerable length of the production of krypton, the production of strontium, the production of barium, the production of many things in this sort of process. What comes out depends entirely on how the split takes place. If you take a piece of paper and tear it in half, the two pieces of paper will never be identical if you repeat the process time after time. Similarly here, the way in which the nucleus splits, depends very much on chance. We may at one time have it split the way we have shown here, but at another time we may have a different set of nuclei.

It turns out that fission can happen in some 40 different ways. That is, we may have 40 different pairs of nuclei produced. These nuclei are all radioactive; they all decay, and in decaying go through a number of different stages of decay. All in all, we may find some 200 different radioactive species present in fission debris. That is, there are the original 40 pairs produced by these 40 modes, and all of their daughters and granddaughters, a total of something like 200 different species of radioactive atoms.

I have another chart which shows the way in which this is done. I have listed on the bottom here the masses of these nuclei. Then up the side are listed the percents formed. This says 10 percent of the fission that occur will produce these elements. One one-hundredth of a percent would produce these. One one-hundred-thousandth of a percent would produce these. (See p. 96.)

What we have done is to try to illustrate the relative frequency with which a given mode of fission will occur. You will notice that in fission there is a strong tendency to produce a light fragment and a heavy fragment, and that there is very little probability of producing symmetrical fission, in which the two fragments would be equal. This figure can vary with a particular fissioning material that you use. This chart is drawn for uranium 235. If we were going to draw it for plutonium we would have a slightly different chart. If we were going to draw it for fast neutrons, we would again have a different chart. But for a specific case this is an illustration of the way, or the many ways, really, in which fission could occur.

One would expect, then, from this particular chart to find that masses in the region of 95—somewhere along in here [point to chart] and masses in the region of 140 or something of that sort, would oc-

cur quite frequently. Hence we would expect that we might have a case where strontium 95 was produced as the light fragment and xenon 139 as the heavy fragment. You notice that this strontium 95 is not the strontium that we were talking about this morning. The strontium this morning was strontium 90. This is strontium 95. It behaves like any form of strontium chemically. You cannot distinguish it from normal strontium chemically. But it is different nuclearly. Strontium 90 has a half-life of 28 years. This form of strontium has an extremely short half-life, of the order of a very small fraction of a second. It decays to yttrium 95, and you will recall that strontium 90 also went to an isotope of yttrium, yttrium 90. Yttrium has a different half-life, and for yttrium 95 is very short. These two half-lives are so short we do not know what they are, but they are each a very small fraction of a second. Yttrium 95 then goes to zirconium, and this goes to columbium or niobium, whichever is the name you prefer, and this finally goes to molybdenum, which is stable.

You notice in this process what has happened in that one of the neutrons in the nucleus of strontium 95 has given up an electron and thus converted itself into a proton. The mass has stayed at 95 all the way through the process, but we have lost an electron in each one of these stages, or we have gained one unit of positive charge.

So when strontium becomes yttrium, we have lost no mass, we have lost a negative charge, and similarly here and here [indicating at blackboard] hence this process which I am describing is accompanied by the loss of four electrons, and at each stage gamma rays will be emitted. That is, this strontium will be in a very excited state. In order to lose its excitation it emits a gamma ray or several gamma rays, so that not only electrons but also gamma rays are emitted.

Similarly xenon 139 will also decay with the production of electrons and gamma rays. It goes first of all to cesium 139. Cesium decays to barium. Barium decays to a form of lanthanum, and this is the stable lanthanum, as I recall it.

Again, let me emphasize that in this particular mode of fission which is one which should be most prominent, we have a net effect of the production of seven electrons, numerous gamma rays, and a large amount of energy. This is just an illustration of what may happen.

Other chains that can occur can be different from this but they will have this similarity, that they will produce electrons and they will produce gamma rays, and in the fission process, neutrons will be emitted. Almost never in the fission process are stable isotopes produced. It just so happens that when a fission reaction occurs, the fragments themselves do not appear in the form of stable nuclei. They are always in an unstable form and try to lose neutrons or electrons to become stable.

However, we do find a type produced which will have only 1 stage, or perhaps 2 stages of decay before becoming stable. Then we will have some which produce very many more. We have several that will produce as many as six. For example, krypton 97 among the light elements, and xenon 143 among the heavy, produce the longest decay chains that we know of. Each one of these has six stages of decay. Each emits six electrons and many gamma rays.

Senator BRICKER. Does the breakdown in krypton follow the same line as it does in the 95 group?

Dr. GRAVES. Yes, it is exactly the same process. You will have six stages. Here you lose one electron—that is, you go from krypton to the next element and so on—you do not change the mass at all. There are a few cases, and only a few, where instead of losing an electron like this, you will lose a neutron. This is a very rare occurrence and is the origin of the delayed neutrons. Almost invariably the decay process is similar to the one given here.

Senator BRICKER. You say that krypton 97 gives off six products in the decay process. What are the additional ones?

Dr. GRAVES. I don't have them here. It ends up again in a form of molybdenum. I can list many of them.

Senator BRICKER. It is the same end result?

Dr. GRAVES. Yes, sir. Instead of being molybdenum 95 it is molybdenum 97. Then there will be niobium, zirconium, and yttrium 97.

Senator ANDERSON. How do you know whether it will be strontium 95 or strontium 97? You end up with strontium and molybdenum 95 which you say is relatively harmless.

Dr. GRAVES. It is stable.

Senator ANDERSON. When do you get to the strontium 90?

Dr. GRAVES. We started out with this element with a mass 95. If we start out with an element of mass 90 right here then we would have ended up with strontium 90. That also is a very prominent member. It happens quite frequently.

Senator ANDERSON. Let me ask it this way, Doctor Graves: Are there any statistics showing how frequently this fissioning results in strontium 90 and how frequently it results in strontium 95 or strontium 97?

Dr. GRAVES. That is what this chart shows.

Senator ANDERSON. That is what that chart shows?

Dr. GRAVES. This says that in about 5 percent of the cases the chain which includes strontium 90 occurs. It may vary from 3 percent to 5 percent, but it is about that number.

Senator ANDERSON. In other words, one-twentieth of the time, you would say?

Dr. GRAVES. Yes, sir.

Senator BRICKER. Is your molybdenum 97 found in nature?

Dr. GRAVES. Yes, sir.

Senator BRICKER. All of the end products of these decaying processes are natural elements?

Dr. GRAVES. Yes, sir.

Senator ANDERSON. Then if there is discussion of bombing and people talk about strontium 90, there is a possibility that several bombs might be exploded and no strontium 90 at all exists.

Dr. GRAVES. As long as fission occurs, the probability is 1 in 20 that roughly 5 percent of the fragments which are produced will be strontium.

Senator ANDERSON. It is a fairly regular pattern that happens in most explosions where a portion of it is strontium 90, a portion is 95, and a portion is 97, but it is a relatively small portion?

Dr. GRAVES. As a matter of fact, when scientists are discussing the things that we do not know, or the things that we disagree on, this is not one of them. Data of this sort—the real input data—is very well

known. The question is on what effect this will have on a human being. I do not want to discuss that. This part is known.

Senator ANDERSON. Cesium 139 has some members of its family that are destructive.

Dr. GRAVES. Yes. Strontium 90 as you recall gives out beta particles. It emits no gamma rays. Hence it is only important if it is inside the body as an ingested material. I am talking out of my field but if it goes to the body it goes to the bones and there it has its effect. Cesium is not that way. Cesium is a gamma emitter and hence it can be important as a part of the external dose. It is a relatively prominent fragment. Cesium is formed in something like 5, 6, or 7 percent of fissions.

Senator ANDERSON. That is what brings me to say that even though strontium 90 is not formed, there may be a strontium figure, which I assume is somewhere around 97, which will produce some cesium 137.

Dr. GRAVES. That is correct.

Senator ANDERSON. And that is somewhat dangerous.

Dr. GRAVES. Yes. As a matter of fact, all of these fragments, as I will discuss in a minute, contribute to a hazard which we call the residual hazard, or what is left over after the detonation. In fact, this is the hazard, the things that are produced here are the things which are giving out gamma rays, beta rays and so on, and which produce the biological changes with which we are concerned.

Senator ANDERSON. So practically up and down that scale you have products that could be harmful.

Dr. GRAVES. Products that are harmful.

Senator BRICKER. What is the half-life of cesium 139? Is it a fast decaying element?

Dr. GRAVES. It is relatively fast. There are only a relatively few that are quite long. Strontium 90 is one of them, being 28 years. Cesium 137 is also long. Those two are the main fragments which are left after times like years. Those are the ones that you have to worry about. The others are all decayed by then.

Senator BRICKER. They are gone in a day or two?

Dr. GRAVES. Not in a day or two, but inside of a year.

Senator ANDERSON. It is because of this half life of strontium 90 that runs 28 years that we talk about it and worry about it more than we do 95 or 97 or any other number?

Dr. GRAVES. Exactly.

Senator ANDERSON. We are worrying about the length of time. The immediate bursts go quickly and then we worry about the long life of strontium 90?

Dr. GRAVES. Yes. It happens that this is exactly the wrong half-life. If it were very short it would be gone so fast we would not worry about it. If it were 1,000 years it would decay slowly. 28 years being comparable with the human cycle is just the wrong period. It has the maximum activity and still present with us for a long time.

Representative HOLIFIELD. Do you intend to get into the factor of the amount of this material that goes into the stratosphere and the amount that is left upon the earth in the initial explosion and the rate of fall from the stratosphere of the amount that goes in? Had you intended to comment on that?

Dr. GRAVES. No. It was my understanding that would be taken care of by the next speaker.

Representative HOLIFIELD. Very well.

Senator PASTORE. How do we know it has a 28-year half-life?

Dr. GRAVES. One can measure it very accurately, Senator Pastore. What we do is to measure the activity today and tomorrow and the next day. One sees it dropping off. So if you have 100,000 disintegrations now—suppose that a year from now you had half as many disintegrations—you would know that half of it was gone. So by following the decay one can determine just how long it takes for half of it to disappear.

Representative COLE. What is the explanation of why some form of strontium has a longer half-life than others?

Dr. GRAVES. It depends on how nearly stable it is. One form of potassium is almost stable. It is just barely over the edge. If it is just barely over the edge it takes quite a time for it to fall off.

Representative COLE. Stability is a substance that has no half-life.

Dr. GRAVES. Exactly. If you were to plot the normal elements, plot the number of neutrons against the number of protons, there is a great tendency for the number of neutrons to equal the number of protons. So among the light elements this is a straight line at 45 degrees. They all have an equal number of neutrons and protrons.

If for some reason or another you produce an element which has a different number of neutrons it is very unstable. If it is closer to this line it is less unstable. Finally when it is nearly on the line it takes many, many years for it to decay. It depends how near it is to this line. If you get up to uranium or the heavy end, this line curves up and you find we have a tendency to have more neutrons. But within, you will have a case like this [drawing curve on blackboard]. Anything within this band is stable. There will be a lot of individual points but they are all within a band like this. Outside of this region they are unstable.

So it depends on how far outside this band they are as to what their half-life tends to be.

Representative COLE. That is not quite it. What is there about a strontium 90 that gives it a comparatively long half-life but strontium 95 a negligible half-life?

Dr. GRAVES. Strontium 95 is just much less stable than strontium 90. It is very far off stability.

Representative COLE. What is there about it that gives it a much less stability?

Dr. GRAVES. Strontium 90 has many less neutrons.

Representative COLE. The difference between 95 and 90?

Dr. GRAVES. Yes.

Representative COLE. Does strontium 95 have the same effect on the anatomy as strontium 90?

Dr. GRAVES. If it were present in you? No. Strontium 95 not only gives off beta rays but it gives off gamma rays, whereas strontium 90 gives off only beta rays. In addition, strontium 90 stays with you for a long time. So it can become fixed in your bones and it can irradiate the material of your bones. Strontium 95 has such a short life that it never becomes fixed in the body. You do not have time to eat it and then have it converted into body tissue.

Representative COLE. If it did stay longer it would have the same effect?

Dr. GRAVES. Or even worse because of the gamma rays it produces.

The fusion process is quite different from the fission process. Among the light elements, there is a tendency for the energy per nuclear particle—that is, per proton and neutron—to decrease with increasing weight as contrasted with the fission process, in heavy elements, where it increases with increasing weight. Hence whereas in the fission process we tend to gain energy by splitting a nucleus, among the light elements we tend to gain energy by combining nuclei—by joining them together—and hence by a process which we call fusion.

Again I may be repeating to some extent what Dr. Mills said this morning, but I would like to list for you the reactions in deuterium which are important from the point of view of fusion because this again illustrates the important difference between the fission process and the fusion process as far as production of radioactivity is concerned.

Whereas in the fission process I have said there might be as many as 40 different modes that can occur—40 different ways in which the fission process can occur—there are only two ways in which the fusion process of deuterium can occur. We may have deuterium joining with another deuterium atom to produce a different form of hydrogen. Deuterium which is a form of hydrogen has a proton and a neutron in the nucleus. The fusion reaction may form still a third form of hydrogen, which we call tritium, where tritium has 1 proton in the nucleus and 2 neutrons.

Tritium plus a proton or helium 3 plus a neutron may be produced.

In each of these cases energy will be produced. In the first case there will be 3.2 Mev. Did Dr. Mills explain this term?

Representative HOLIFIELD. No.

Dr. GRAVES. A million electron volts is the energy an electron would have if it fell through a million volts. If you take it from here and apply a million volts to it, when it gets down to here it will have this amount of energy. It is like saying it has so many foot-pounds of energy, or what-not, but it is a number giving us a measure of energy. In this case we would also produce energy and it would be 4 Mev.

The net result of this fusion reaction, is that we have helium which is stable. We have protons which are simply the nucleii of hydrogen atoms which are stable. We have neutrons. Then we have tritium which is unstable.

Tritium itself may unite with deuterium and if it does it will produce a form of helium—normal helium, as a matter of fact—at mass 4, and it will produce a neutron.

In this case, we have used up our radioactive atom tritium and produced stable helium. Again we have a large amount of energy produced. This produces 17 million volts of energy.

Then finally, under certain circumstances we may have these helium 3 nuclei uniting which will produce helium 4, plus 2 neutrons. This time we will have 11 million volts.

The point that I am making is that in each of these reactions we have energy produced. In the net of all of these reactions we have many neutrons produced, but we have no radioactive materials produced, with the exception of tritium, which is also itself used up in subsequent reactions with deuterium, and this is a very probable reaction.

This, then, is the difference between the fusion reaction and the fission reaction. Although we have also large amounts of energy produced, we have very small amounts of radioactivity produced directly. We may still have radioactive materials produced by action of these neutrons on substances that are present. I will discuss this when I discuss induced activities.

We have now discussed the production of radioactive materials in the fission process, the production of radioactive material, such as it is, in the fusion process.

The third source of radioactive materials is in the induced activities which are caused when these neutrons react with substances that are available.

Chairman DURHAM. Did you say what degree of heat you get in either one of those final chain reactions?

Dr. GRAVES. If you take a pound of deuterium and somehow or another burn it all—that is, burn the whole pound of it—you will get the equivalent of 26,000 tons of TNT. That is the amount of energy that you would get from 1 pound of deuterium. The temperature will depend on how much material you apply this energy to. But this is the amount of energy you would get out of a pound of deuterium burning completely.

Representative VAN ZANDT. That is about 1 cubic inch.

Dr. GRAVES. No. It depends on what form it is. It is a gas. It depends on its pressure. A pound of uranium, I said, was about a cubic inch.

Is that sufficient for your purposes, sir?

Chairman DURHAM. Yes.

Dr. GRAVES. Normally, the radioactive materials induced by neutron capture in dirt or in bomb materials or in air will have a very short half-life, or they will have a long half-life but will have such small energies in their decay particles that they will not be important.

Consequently, we may say that the induced activities are not going to be a tremendously important worry or concern in this problem. For example, nitrogen in the air can capture neutrons to become carbon. It becomes a form of carbon which is radioactive, but it is radioactive with a half-life of 5,000 years. Consequently, although it is around a very long time, and although it may take its place in our bodies, it has very little energy in the particles which come from it. It has very low activity, and the amount that we can produce is extremely small compared to the amount which occurs naturally in nature.

Therefore, radioactive carbon itself is not an important activity.

We may have others, however. Oxygen can absorb fast neutrons and in so doing it forms an isotope of nitrogen which has a half-life of only 7 seconds. Since the half-life is only 7 seconds, within a minute essentially all of this nitrogen will be gone. In 6 half-lives—(I guess that term was discussed this morning)—the amount is reduced to something like 1 percent. In 10 half-lives it will be produced to a tenth of a percent. So in 70 seconds, or a minute and 10 seconds, there will be only one-one thousandths of the nitrogen remaining.

Chairman DURHAM. Do you get a chain reaction, Doctor?

Dr. GRAVES. No, sir. One of the important components of any residual activity will be due to 14.8-hour sodium; 14.8 hours is suffi-

ciently long so that it will be with us for a fair length of time, and yet it is sufficiently short that we will be able to have enough activity to make itself noticed. It is formed when ordinary sodium captures a neutron. Ordinary sodium is present to a very considerable extent in the form of sodium chloride, or salt, particularly if we are in the neighborhood of salt water. We will have large amounts of this type of sodium formed. Again, however, it will disappear in time. Say the half-life is 15 hours—it is a little less than that—this means that in something like 90 hours there will be only 1 percent of it left; 90 hours is roughly 4 days, so that inside of a week there will be very little of the sodium left. Whereas, the fission products will be with us for some time.

Silicon 31 is another one that is formed in very appreciable amounts, largely because there is a considerable amount of silicon present. It is formed when normal silicon captures a neutron. It, however, again has a half-life of only 2.6 hours and hence it decays very rapidly.

Aluminum 28, which again is quite abundant (aluminum is quite abundant—when it captures a neutron it forms aluminum 28) has only a half-life of a few minutes and hence will completely disappear within the first hour.

Chlorine produces a 37-minute activity. This emits betas and high energy gammas but again 37 minutes is so short that it will not be with us for very long.

The only other things that one should mention is that materials of construction, materials which are commonly used, such as zinc, copper, iron, glass, or the salt in foodstuffs, might under some conditions become active enough to become significant.

The net effect of what I have said, I think, is that although induced activities are present, although one can measure them, one can anticipate them, their effect will never be more than a few percent of that of the fission products themselves.

So that in our worries, or in our concern over the activities accompanying atomic detonations, I think it is fair to say that the induced activities are not the important source of radioactive materials or radiation.

Now, if I may leave the subject of radioactive materials and their production——

Representative HOLIFIELD. Before you leave that, Doctor, how do you explain the persistent radioactivity of the coral in the lagoons where these detonations have taken place?

Dr. GRAVES. Largely because there is deposited on this coral fission fragments themselves. When you detonate an atomic weapon on the surface—any surface—you bring up into the cloud large amounts of dust particles. These dust particles are in the cloud along with all of the fission fragments and all the fissioning nuclei, and they are stirred up together and all of the fission fragments must condense from the gaseous form onto something and they condense on whatever particles are there. If we have particles of coral they will condense on coral. In Nevada they will condense on dust particles. So what we have is a coral on which has been deposited fission activity.

Representative HOLIFIELD. So the long-lived activity of materials other than fission materials is caused by the adherence of the fissionable materials to the spectrums of matter, whatever they may be, whether it is dust or coral; is that right?

Dr. GRAVES. Yes.

Representative HOLIFIELD. The matter becomes a carrying element for the fissionable element.

Dr. GRAVES. It is important because it may bring the material down to the ground sooner because it is heavy. In that way it can have an important effect on fallout, as Mr. Kellogg will discuss next. It may also have some activity, like calcium in coral. The calcium of calcium carbonate may become active. Such activities almost invariably decay in such a short time that they may be ignored insofar as the fission-fragment activity is concerned.

Representative VAN ZANDT. Is there any uniformity as to the amount of particles lifted into the atmosphere from a megaton or kiloton of yield of the weapon?

Dr. GRAVES. Yes. You will get an argument from lots of people on this, but my number is that something like a megaton of energy will lift up a tenth of a megaton of dirt. This is rough. It depends a lot on what the dirt looks like and so on, but it is something of that order.

Representative VAN ZANDT. The greater the yield then the higher the particles are lifted.

Dr. GRAVES. That is roughly correct; yes. It depends not only on the yield but the atmospheric condition, as I mentioned before.

Representative HOLIFIELD. In evaluating the radioactivity in a bomb, we must take into consideration the amounts of matter which are contacted by the fireball of the bomb?

Dr. GRAVES. Yes.

Representative HOLIFIELD. It is obvious that, if a fireball is touching the ground or coral reefs or anything like that, it would produce a great deal more radioactive material than if exploded a mile high in the sky.

Dr. GRAVES. No, the difference is not so great. The amount of radioactive material produced will be very little different. It will be some because of the induced activity.

Representative HOLIFIELD. But the dispersion of it, because of its lack of attachment to matter which gravity would have an effect upon, would be completely different.

Dr. GRAVES. That will be completely different; yes, sir, and that is the importance. If you detonate it on the surface of the ground or below ground, then you will have a very strong tendency to have the radioactivity deposited locally. If you put it high up in the air you will have about the same amount of activity. Less will be deposited locally but more will be deposited worldwide.

Representative HOLIFIELD. Is it not true also that the higher you go with the bomb the less you take advantage of its energy release as far as destruction is concerned?

Dr. GRAVES. No, sir. As far as blast effects are concerned, there is a height which seems to be optimum for a given yield. This is not way high up in the air nor on the surface; if you are interested in maximizing blast damage you will pick a certain height. Similarly if you are interested in maximizing thermal damage you will not have it on the surface because on the surface one building will be shielded by another and hence thermal radiation will hit one and burn it completely but will not touch the other. Whereas, if you have it up high you will be able to get both buildings.

Representative HOLIFIELD. But in each case when you go above that set point, you lose efficiency.

Dr. GRAVES. Exactly.

Senator BRICKER. Doctor, you mentioned a moment ago that nitrogen would pick up one neutron and become carbon. That is carbon 14 with the long half-life?

Dr. GRAVES. That is right.

Senator BRICKER. That is the one of Doctor Libby's age determination?

Dr. GRAVES. Yes, sir.

As I mentioned earlier, initial nuclear radiation is defined as that radiation which is delivered during the first minute. I would like to spend a minute discussing initial radiation for you. Although initial neutrons and gamma rays constitute only about 3 percent of the energy of the detonation—that is, 3 percent of the bomb energy comes out as initial neutrons and gamma rays—a very considerable portion of the bomb casualties are caused by these neutrons and gamma rays.

You might compare this with thermal radiation, in which something like a third of the bomb's radiation will come out as thermal radiation. Let me say 33 percent. So here we have 3 percent coming out as neutrons and gamma rays and yet this 3 percent is an extremely important part of the effects that you observe in the detonation.

In order to indicate the magnitude of this effect, I might give as an illustration the following: 50 percent of the people shielded by as much as about 2 feet of concrete would be killed by the initial gamma rays and neutrons 1 mile from a 1-megaton bomb.

Let me put that on the blackboard for you. If I had a detonation of a 1-megaton bomb, and a mile away from that had a shelter whose walls were 2 feet thick, 50 percent of the people in that shelter would die. Whereas, with thermal radiation we could have a very much lighter shield at this same point and provide complete protection. That is why initial radiation is an important part of bomb detonations.

Representative VAN ZANDT. Doctor, how much concrete would be necessary to shield a person from a 1-megaton blast?

Dr. GRAVES. That is just on the edge. If it is 2 feet thick I say about 50 percent of the people would die. If we increase it to 3 feet thick, the chances are that practically everybody would live. This is just on the edge of being sufficiently safe.

Senator PASTORE. That is the question I was going to ask. Why would 50 percent die and not all of them die? Why would 50 percent be vulnerable and why 50 percent not? If you are hit, you are hit.

Dr. GRAVES. It is like when we have pneumonia. Some of us die and some don't. It is partly chance. It is partly how healthy we are. It is partly the condition we are in. This is just the mean lethal dose. This is right on the edge. If it gets much bigger, everybody will die; if it is much less nobody will die.

Senator PASTORE. How do you scientifically assert that? How do you prove that?

Dr. GRAVES. That is hard. The basis for it, however, is experiments with mice or rats, finding out what the mean lethal dose is for a long chain of animals. I would much prefer that you ask that sort of question of a biologist who knows it better. This is the basis for such

a statement. You go on the basis of experiments with animals to find it out.

Senator PASTORE. In other words, we have determined that with animals in blast situations, just as you have outlined, some have died and some have not?

Dr. GRAVES. That is right.

Chairman DURHAM. It is a proven and known fact, Doctor, is it not, that radiation dies in some individuals quicker than in others?

Dr. GRAVES. Some people are more susceptible to damage from radiation, just as in the case of pneumonia. I imagine that is your question.

Chairman DURHAM. The radiation itself dies more quickly in an individual.

Dr. GRAVES. The radiation will depend on the substance. Carbon 14 has a half lifetime of 5,000 years. It does not make any difference whether it is my tissue or yours. Sodium has a half-life of 14.8 hours. There is nothing we can do physically to change that half life. We can heat it up, cool it down, physically distort it in any way we want, but we cannot change the fact that in 14.8 hours half the sodium we have now will be gone. That is just the way radioactivity is.

But the effect of radiation on you may be slightly different from its effect on me.

Representative HOLIFIELD. The effect on a child, for instance, might be much greater than on an adult.

Dr. GRAVES. That is possible; yes.

Senator ANDERSON. Before you get away from the diagram you have there, you show deuterium, plus deuterium, and then doing some other things, and tritium coming in, and you said something about less fission in that sort of operation.

Dr. GRAVES. I said less radioactivity. There are no radioactive materials here except tritium. The tritium itself may be absorbed by deuterium to form normal helium.

Senator ANDERSON. Is there not radioactivity in all fission? In every fission process there is some radioactivity?

Dr. GRAVES. Yes. This fusion and this other is fission.

Senator ANDERSON. In order to have the fusion you are talking about there has to be some fission. I won't say how much.

Dr. GRAVES. This is a fusion reaction but it also can be maintained by heat. If we can somehow or another get this deuterium hot enough it can also be made to react. Hence it is carred a thermonuclear reaction as well as a fusion reaction. One of the ways of getting it hot enough might be to use fission.

Senator ANDERSON. You have not done it any other way, probably, so if you do use fission then any bomb that involves fission will have radioactivity in it.

Dr. GRAVES. I would prefer not to discuss that in an open meeting, Senator.

Senator ANDERSON. That is the trouble. I don't blame you.

Dr. GRAVES. I do not want to discuss how a bomb works.

Senator ANDERSON. I was trying to get around to the claim we have a clean bomb, which I do not believe we have. I did not know whether this was a good place to get it or not.

Dr. GRAVES. I will be happy to discuss that with you personally at any time.

Representative HOLIFIELD. These statements have been made publicly about these clean bombs. I would say this much, without going further into it at this time, that as long as statements have been made that there is a clean bomb some clarifying statement should be made in these hearings.

Dr. GRAVES. I have here a set of processes which involve production of no radioactive materials. I have here a process which involves the production of very large amounts of radioactive materials. As Senator Anderson has pointed out, there is a possibility of a combination of these two processes. If you have a large amount of energy coming from this fission process, there is nothing you can do but produce radioactive materials. If somehow or another you can arrange your geometry or your procedure such that you produce more of your energy from these processes (fusion) then you have less radioactive material for the energy you produce.

Senator ANDERSON. You have a cleaner bomb but you do not have a clean bomb.

Dr. GRAVES. Exactly.

Senator ANDERSON. That is exactly the point. Every time you try to get into a discussion of it—I am not complaining about your attitude on it—we find it is not the best thing to discuss in public, and you are glad to discuss it with me privately and I find it very profitable to discuss it with you privately.

But that does not help the people who read a news story that we have a clean bomb which some of us believe is a true statement.

Dr. GRAVES. I agree with you thoroughly. The statement should be a "cleaner" bomb.

Representative HOLIFIELD. People are being blamed about being confused. Some of the statements that have been made by AEC members and distinguished scientists along this field are directly attributable to part of this confusion that exists. It is the intent of these hearings to clear up some of this confusion, within the limits of declassification. I do not believe that the committee will want to accept everything that we try to do to clear up the confusion, as being classified.

Dr. GRAVES. I do not think one would ask you to do that either. What I would prefer would be to have a chance to discuss this with you privately and let you decide whether it is what you want to accept as classified or whether it is important for your discussions publicly.

Senator ANDERSON. May I say, Doctor, my sole purpose is to try to find out occasionally if there is a chance to clean this up. The only way is to ask witness by witness until we finally come down to a spot where it may be convenient to clear it up. It is not meant to be critical of you in the slightest.

Representative HOLIFIELD. No. This committee is not responsible for the phrase "clean bomb." We are not responsible for it. But there are millions of people throughout the world that may be hanging their hopes upon the fact that we have a humanitarian hydrogen bomb.

Dr. GRAVES. I am afraid the only comment one can make on it is that "cleanliness" is a little bit relative anyway. What you mean by "cleanliness" in this case is a question of degree.

Representative HOLIFIELD. You would not say in this case that cleanliness is next to godliness.

Dr. GRAVES. No. As Senator Anderson says, complete cleanliness is next to impossible to achieve.

Senator BRICKER. May I ask how much earth it would take of ordinary character or texture to give the same radiation shielding that 3 feet of cement would give?

Dr. GRAVES. Yes. It is almost directly proportional to the density. If you have well-tamped earth, something like twice as much earth would be about equally effective.

The initial radiation from all of these radioactive materials or from all of these detonations will consist of alpha particles, beta particles, neutrons, and gamma radiation.

The alpha particles can come from two sources. In the first place, the fissionable materials are radioactive in such a way as to produce alpha particles. So that uranium, plutonium, whatever materials of this sort are present, will produce alpha particles.

In addition, in the fusion reaction we have the production of alpha particles. Here is an alpha particle, here is one, and this would actually be similar to an alpha particle [indicating on blackboard] and there are other alpha particle reactions which are possible.

Neutrons can be absorbed in materials to produce alpha particles. So the two sources are in the heavy materials which are present and in the various alpha-particle-producing neutron reactions which can take place.

As Dr. Mills mentioned to you this morning, alpha particles have an extremely short range. They will scarcely get through the dead skin on the surface of your body, and hence as far as the initial radiation is concerned, are of almost no importance.

Beta particles have a somewhat greater range than alpha particles. They are also produced in large numbers in the various processes that occur. But again they have a short range, such that they cannot reach the ground from an air burst, and within a very short time, when the cloud rises, they will not be able to reach the ground from any sort of a burst, and hence they also are not of great importance as far as the initial radiation is concerned.

Other speakers later will talk about the importance of alpha radiation and beta radiation as far as the internal hazard is concerned and as far as contamination on the skin is concerned. I do not propose to go into that. I would rather limit my remarks to the discussion of gamma radiation and neutron radiation as they effect the initial radiation hazards.

Both neutrons and gamma rays can penetrate air to very considerable distances. They are both highly injurious to living organisms. Hence they constitute by far the most important part of initial radiation.

To a great extent neutrons come out in a very few milliseconds—the first few milliseconds of the detonation—when the bomb will be a very compact, dense material still, and hence these neutrons are very largely absorbed in bomb materials. Enough fast neutrons, however, are present to constitute an appreciable direct hazard at consideraable distances.

Similarly, the gamma rays which are associated with the fission process itself—gamma rays that come out in the initial stages of the fission process—are also present in the stage when the bomb is con-

densed, heavy material, and hence these gamma rays are highly absorbed.

Only about 1 percent of the gamma rays produced directly in fission escape the immediate region of the detonation. However, again this contribution and that of the gamma rays produced in other processes which I will discuss in a minute will produce sufficient intensity at considerable distances to constitute a very real hazard.

Let me mention some of the sources of the gamma radiation which is present in initial radiation.

In the first place it originates in the fission reaction—the one that we picture here.

Second, gamma rays can appear in various neutron reactions.

First, neutrons may be captured with the formation of a highly excited compound nucleus. This compound nucleus loses its excitation—that is, returns to its normal state—by emitting a gamma ray. For example, when neutrons are captured to form carbon 14, in the process of being captured, nitrogen 14 is formed in a highly excited state. This produced two gamma rays which are high-energy gamma rays and emits them. Nitrogen 14 captures a neutron to become nitrogen 15. The nitrogen 15 emits a proton to become carbon 14. But in this process the compound nucleus releases these two high-energy gamma rays.

In the second place, neutrons may react with nuclei, emitting protons or alpha particles, and gamma rays are frequently emitted in this process.

Finally, neutrons may bounce off other nuclei, and they may bounce inelastically such that the bombarded nucleus becomes excited and emits gamma rays.

The third important component or part of the gamma rays in initial radiation is from fission fragments. Fission fragments can be widely spread, and as they decay can give off gamma rays. Many of these gamma rays are given up in the first minute and hence are properly included in the initial radiation.

Although instantaneous and delayed gammas are produced in about equal amounts, the instantaneous gammas are absorbed so strongly that they constitute only about 1 percent of the external dose received. Hence 99 percent of the external dose is due to the so-called delayed gamma radiation. It will probably be of interest to discuss at least briefly the dose distance relationship for both gamma rays and neutrons, that is, how far from a given detonation would someone receive what dose.

Gamma rays lose energy in going through matter by both excitation of atoms and ionization of those atoms. In fact, it is believed that the chemical decomposition of molecules caused by this effect is the major cause of injury to animals and plants. Consequently, the dose of gamma rays is defined and is measured in terms of the ionization produced.

The dose distance relationships for gamma rays and neutrons are very complicated because of the large number of effects involved. Consequently, I would like to ask you to consider what I say from now on in this respect to be quite approximate. It is not possible to write down an exact analytical expression for the dose-distance relationship.

Representative HOLIFIELD. Dr. Graves, are you going to use chart 6 and chart 7 in your presentation?

Dr. GRAVES. I would like to discuss, if I may, the surface burst and the characteristics of an air burst as they relate to fallout. I will use those. Would you like to have me show them?

Representative HOLIFIELD. These are the charts which you furnished us with your statement.

Dr. GRAVES. I will use those; yes.

Representative HOLIFIELD. I think it would be illustrative if you do.

Dr. GRAVES. I will use those; yes.

It turns out that gamma rays going through air will decrease because of distance, and this decrease is a decrease as the square of the distance.

Gamma rays as they go through air will decrease because of many effects. They will be scattered. They will be absorbed. They will lose their energy. But it turns out that the gamma ray dose decreases by a factor of 10 every 780 yards. Consequently, we have a relationship for gamma ray dose which depends on the yield, depends on the distance squared, and then it drops as an exponential which is given by E to the minus D over 338.

$$\text{Formula: Dose}=\text{Const.}\ \frac{W}{D^2}\ e^{-D/338}$$

This says that dose drops by a factor of E every 338 yeards or by a factor of 10 every 780 yards.

Then there is a constant multiplier. This is an extremely rough formula. For yields from a kiloton up to something like 80 kilotons, it is about correct. If we get much above 80 kilotons, say 100 kilotons, one should multiply the yield by 1½, so it is about 50 percent off there. If one is talking about a megaton, then one should multiply this by a factor of five or something like that. Roughly as far as distance dependency is concerned, the formula can give at least approximate results.

One may do a similar thing for the neutrons. The neutrons again are a constant times the yield divided by the distance squared, and then an exponential term. The neutrons are absorbed more quickly than the gamma rays. The exponential will contain D over 242. If I put it in the same terms that I did for gamma rays, I would say that neutrons decrease by a factor of 10 every 560 yards. So that neutrons will not go as far as gamma rays.

$$\text{Formula: Dose}=\text{Const.}\ \frac{W}{D^2}\ e^{-D/242}$$

The residual radiation—what is left after the bomb has gone off, after the cloud has gone up in the air and after the fallout has occurred—can also be discussed briefly. In the first place, about a tenth of a pound of fission products are produced for each kiloton of fission energy. The radioactivity at 1 minute would be comparable with that of many pounds of radium. Consequently, a megaton of fission energy would result in activity at 1 minute comparable to that of many tons of radium.

I heard somewhere recently that the total world's production of radium since the beginning of time was something like 50 pounds, so that when we talk about the detonation of a megaton, we are talking

about the production of many times the activity of all the radium which has ever been produced. During the first 24 hours, there would be a decrease by a factor of more than 6,000 in the total activity present and then it will decay continuously. But it has been estimated that if all of these fission fragments from a megaton of energy were spread uniformly over 10,000 square miles, it would cause a radiation intensity after 24 hours of 2.7 roentgens per hour. Consequently, unprotected personnel in that 10,000 square miles would receive more than 300 roentgens of radiation after the first day. 300 roentgens is a lot of radiation. It is estimated that something like 450 is the mean lethal does. So 300 is approaching a lethal does.

Representative VAN ZANDT. Then it would be a uniform distribution over the area.

Dr. GRAVES. That is a uniform distribution over 10,000 square miles which would not happen.

Representative HOLIFIELD. But is it not true that there is no such thing as a uniform distribution?

Dr. GRAVES. Exactly.

Representative HOLIFIELD. This starts at many thousands of roentgens near the bomb and it gradually decreases.

Dr. GRAVES. Yes, sir. Again this will be discussed in the next section of the hearings. I don't want to discuss actual patterns on the ground, but I did want to give an idea of what we are talking about in a rough order of magnitude sort of way.

I would like also to give an idea of the rapidity with which the material will decay. So I will give a table of activity as a function of time. Again let me confine this to a 20 kiloton—a so-called nominal bomb.

At the end of 1 minute, the total activity will be equal to 820,000 magarcuries. A curie is the activity of 1 gram of radium. A magacurie would be the activity of a million grams of radium. So we are saying that at 1 minute the activity would be the equivalent of that of 820,000 million grams of radium.

At the end of 1 hour, this will be down to 6,000 megacuries. At the end of 1 day this will be down to 133 megacuries. At the end of a week it will be down to 13. At the end of a month it will be down to about 2. At the end of a year it will be down to 0.11. Then may I finish this off by saying that at 100 years it will be 0.0006.

This morning you asked Dr. Mills the question, how long would it take this to disappear completely. I could go on for a thousand years and a million years, and I would still get something here, but it gets pretty small. Roughly one can say that for a period of time equal to 7—like 7 days or hours—the activity decreases by a factor of 10. So if we say that the activity at the end of 1 hour is something, then at the end of 49 hours—that is 2 days—it will be down by a factor of 100. In 2 days we have lost a factor of 100. In 2 weeks we will have lost a factor of a thousand. This gives you some idea, I think, of the speed with which this residual radiation will disappear.

Representative HOLIFIELD. In order to be clear on this, Dr. Graves, we are talking about a 20,000-ton bomb.

Dr. GRAVES. Yes, sir.

Representative HOLIFIELD. In the case of a megaton bomb, what is your residual radioactivity danger, and let us go from that and extrapolate it to a 10-megaton bomb. I do not ask for the exact figures.

Dr. GRAVES. We can give them. Let me restrict my answer to those in which the energy comes from the fission process. I don't want to get mixed up in the fusion-fission business again. It will depend exactly on the ratio of yields, A megaton is 50 times as much as a nominal bomb yield. If I multiply by 50, I would get the corresponding numbers for a megaton.

Senator BRICKER. But the time would be the same.

Dr. GRAVES. Yes, sir. I would multiply each one of these numbers by the same factor.

Senator BRICKER. The same ratio of decrease?

Dr. GRAVES. That is right.

Senator ANDERSON. In order to help the record out without making it too damaging, would it not be fair to say, Dr. Graves, that an average megaton bomb at the present time at least would probably not be all fission?

Dr. GRAVES. That is correct.

You have asked that I discuss with you to some extent the partition of energy from a nuclear detonation. I would like to preface my remarks by saying that there really is no correct partition of energy. In the initial stages, we have heat—thermal energy—and in the final stages we have thermal energy. Everything else is thermal initially and ends up as thermal. In between it can be converted into nuclear radiations, shock energy, or thermal radiation. So we are talking about something which is changing and the percentages that I gave are not something which you can say will happen after 2 weeks or at any specific time.

This is a rough estimate [pointing to chart] of the way the energy from a typical air burst will be partitioned. Roughly half of the energy will come out as blast or shock wave. Roughly a third or 35 percent of it will come out as thermal radiation. The initial nuclear radiation—that is, that which comes out in the first minute—is only about 5 percent. Whereas, the residual nuclear radiation, that which comes from deposited fission fragments and from induced activities is about 10 percent. As I say, these numbers sort of interchange back and forth. Blast and shock eventually go into heating up the air and become thermal radiation again. Similarly the residual nuclear radiation, the neutrons and gamma and beta rays come out and heat up the air a little bit, and hence become thermal radiation. So this chart is correct at some time, but the partition of energy changes from the first instance to longer times. Moreover, it depends to some extent on the yield of the weapon, the amount that comes out as thermal radiation might vary depending on what the conditions of the weapon itself are. (See p. 97.)

Representative HOLIFIELD. Dr. Graves, will you explain to me what you mean by "thermal radiation"? Do you mean the heat that goes with the explosion?

Dr. GRAVES. Yes. It is the same sort of energy that you detect when you stand in front of a radiant heater or if you open the door of a furnace. You feel something hot on your face. Those who have been out to Nevada have felt this.

Representative HOLIFIELD. If we talk about the total radiation of the bomb, we are talking about this 15 percent that you speak of here. That becomes 100 percent when you talk about total radiation.

Dr. GRAVES. This 15 percent is the total nuclear radiation. This thermal radiation is something quite different. It is just something because the materials are hot.

Representative HOLIFIELD. In testimony before another congressional committee, Dr. Libby said that in the case of one of these megaton bombs, and I believe it was a 10-megaton bomb he was testifying about, that 25 percent of the radioactivity was the part that caused the downwind pattern of 7,000 miles in the testing grounds of radioactive fallout; 75 percent of it went up into the troposphere and stratosphere. Is that according to your understanding?

Dr. GRAVES. What he was talking about in that case would be the nuclear radiation itself.

Representative HOLIFIELD. Yes.

Dr. GRAVES. Let me say the residual radiation.

Representative HOLIFIELD. Yes.

Dr. GRAVES. We have a large amount of fission activity and induced activity. He was restricting his comment to that. He said that of that, 25 percent in his example was deposited locally, and 75 percent went to the stratosphere. This will change depending quite a bit on how you detonate the device itself. It will change. In his particular example that is how he estimated.

Representative HOLIFIELD. The fact that 75 percent went into the stratosphere did not necessarily mean we were rid of that, because there is a factor of return to the earth, is there not, over a period of years?

Dr. GRAVES. Yes.

Representative HOLIFIELD. Eventually you would get 100 percent of the radioactivity, although your first part would decay.

Dr. GRAVES. You gain a lot if you can put it up into the stratosphere and let it stay up say 100 years. Instead of talking about 820,000 megacuries, you are talking about 0.00006 megacuries. That is a difference. If it is going to decay, let us let it decay 100 miles up.

Representative HOLIFIELD. Has it not been said that it will return in a period of 10 years?

Dr. GRAVES. That is right.

Representative HOLIFIELD. So we cannot talk about 100 years or 1,000 years; we must talk about 10 years.

Dr. GRAVES. Exactly. That is the reason strontium's 28-year half-life is important. If it were a half-life of 1 year, we would not worry about it. It has a half-life long compared to the time it stays in the stratosphere. Hence, still a lot of it is left when it comes down.

Representative HOLIFIELD. So you can say that most of the strontium 90 that goes into the stratosphere would eventually return to the earth, although it would be diffused much greater than that in the local fallout, as you term it.

Dr. GRAVES. Yes.

Representative COLE. Mr. Chairman, I would like to inquire of Dr. Graves, if you are permitted to answer, whether nuclear radiation is a desirable element of a military weapon——

Dr. GRAVES. No one has told me I can't answer that, except I don't know the answer.

Representative COLE. A weapon is for the purpose of destruction. With a nuclear weapon, that is the dazzling light, the heat, the blast and radiation. Those are the four destructive parts of the weapon.

Dr. GRAVES. That is right.

Representative COLE. What I would like to know is whether you feel you can tell us that the radiation phase of a weapon is a desirable factor from a military standpoint?

Dr. GRAVES. I am not told I cannot answer that, but I just don't know the answer. It would seem to me this might also depend on the particular tactics or strategy that are under consideration.

I am not expert on military matters, but if you are interested in establishing a beachhead, you want to be sure you can use that beachhead, so you would not want to put so much activity on the beachhead that you cannot land and utilize it. It would depend on the circumstances of the use of the weapon.

Representative COLE. And whether the nuclear radiation phase is a desirable element would determine the nature of the radiation you seek to create.

Dr. GRAVES. The military situation would certainly be an important consideration.

Representative COLE. What I am thinking is that if nuclear radiation does not have an important military significance in weapons use, I should think that the tendency and your tests and weapons improvement would be to make these weapons as free of nuclear radiation as possible, or in other words, to make them as clean as possible.

Dr. GRAVES. The purpose of our weapons test is to try to find out what we can do with these weapons, such that if someone asks for a weapon which is clean, we will be able to say we know what we can do to satisfy that particular requirement, or if they ask for a weapon with this characteristic or that characteristic. Hence, the weapons test is to give us the knowledge on which we can make an appropriate design to satisfy that requirement.

Representative COLE. Make them either clean or dirty as the military requirements indicate.

Dr. GRAVES. Yes, sir.

Representative COLE. I would hope that some of the other witnesses from the military might discuss that later.

Representative VAN ZANDT. Is it not true that nuclear radiation is contaminating?

Dr. GRAVES. Yes.

Representative VAN ZANDT. Do we not have contaminating weapons in our stockpile at the present time?

Dr. GRAVES. Yes. You mean weapons that are fission. Sure.

Representative VAN ZANDT. Contaminating.

Dr. GRAVES. Yes.

Representative VAN ZANDT. It is common knowledge that military strategists will employ contamination, in the prosecution of a war. They seek to isolate an area through contamination. In addition they might contaminate water for the purpose of producing moisture that would be radioactive.

Dr. GRAVES. I am not an expert on military strategy or tactics so I am afraid I cannot help you.

Representative COLE. It may be true that all of our weapons contain contamination. The question is whether that contamination is in there deliberately or inevitably.

Dr. GRAVES. As I say, if the weapon is designed to make use of the fission process, then that contamination is there inevitably whether the military wants it or not. As I answered you, the object of our tests is to find out whether it must be in there inevitably so that if it should not be wanted, then we would know what to do about it.

Senator ANDERSON. Doctor, in order to clear up any question when you got mixed up with fission or fusion, and my question whether it would be fission along with fusion, the page proof of the booklet, The Effects of Nuclear Weapons, issued by the Atomic Energy Commission, at page 17 has this paragraph. It has been declassified. We have just determined that:

> In order to make the nuclear fusion reactions take place, temperatures of the order of a million degrees are necessary. The only known way in which such temperatures can be obtained on earth is by means of a fission explosion. Consequently, by combining a quantity of deuterium or a mixture with a fission bomb, it should be possible to initiate one or more of the thermonuclear fusion reactions given above. If these reactions accompanied by energy evolution can be propagated rapidly through the volume you have the hydrogen isotope or isotopes, a thermonuclear explosion may be realized.

Therefore, I hope I was not getting outside the properly declassified section when I asked you if, so far at least, we did not have to have a fission explosion in order to have the following fusion explosion.

Dr. GRAVES. I am sure that what we have said earlier was perfectly all right.

Senator ANDERSON. I think it was all right.

Representative HOLIFIELD. Therefore, the conclusion we can reach is that there is a dirty bomb and there is no such thing as a clean bomb, and I am using the word "clean" in the absolute sense and "dirty" in the absolute sense.

Dr. GRAVES. There are dirtier bombs, and some that are less dirty.

Representative COLE. That is correct. It is not a question of a clean bomb versus a dirty bomb. There are varying shades of gray and white in between. It is not one or the other, is it?

Dr. GRAVES. No, sir.

Representative VAN ZANDT. Doctor, can this clean or dirty state be produced in the way you use the weapon?

Dr. GRAVES. Why don't I get into that discussion now? (See p. 99.)

This is the picture of the type of situation which results from a surface burst. In a surface burst of an atomic weapon or a nuclear detonation of any sort, there are two effects that should be mentioned. In the first place, the neutrons that escape from the detonation can strike the ground and hence produce induced activities. In the second place, large amounts of dirt will be mixed up with the cloud, radioactive particles will be plated out on these particles, and hence will tend to fall out more rapidly since these dirt particles will be heavy. So we have, then, here a typical mushroom. It will be very dirty. It will look dirty. You will have nearby fallout of the very heavy particles. At somewhat greater distances we will have fallout of the somewhat lighter particles. Then at very great distances, we will have fallout of the very fine particles.

If this is a relatively small device, very little of the activity will go up into the stratosphere. There is a stable layer in the atmosphere. Again this can be talked about more authoritatively by some of the other people who are present who will be speaking to you later, but there is a very stable layer in the atmosphere which tends to contain such explosions. For the normal small type of device the active material does not get above that, and hence it will all fall out close by, or at least on, say the first pass of the material around the world.

Whereas, from large bursts where the clouds go to very great altitudes, the activity can get up into the stratosphere and this may fall out as much as 10 years later, as Senator Anderson has pointed out.

The difference is that in this particular case we will have very much dirt mixed up with the mushroom. We will have very much close-in and near-out fallout and probably relatively less of the worldwide fallout.

Representative HOLIFIELD. Dr. Graves, before you leave that, you are assuming, of course, perfect test conditions when you make that statement.

Dr. GRAVES. No, sir, I am talking only about the case of a detonation which has occurred on ground. Whether it is a test condition or not.

Representative HOLIFIELD. But as far as the deposit of radioactivity is concerned, you are talking about what you would call safe weather conditions after this explosion, you would have a different situation, would you not?

Dr. GRAVES. I have not been discussing what would happen if you had rainfall or snowfall or that sort of thing, that is right.

Representative HOLIFIELD. That is why you are so careful in postponing your tests, in order to be sure that you have an element of as much certainty as possible with regard to weather conditions.

Dr. GRAVES. Yes.

Representative HOLIFIELD. So that you can get the maximum of protection.

Dr. GRAVES. In the case of testing weapons we try to avoid a situation where the device is detonated on the ground because we don't want to have this very heavy local fallout. We would like to avoid this situation if we can. We try therefore to use towers and make them as high as we can, or we use air bursts as in this chart, or we use balloons for holding the device up. All of this is to avoid getting this mixture of dirt into the cloud itself. (See p. 99.)

Representative HOLIFIELD. It was the purpose of my question to allow you to explain that point.

Representative VAN ZANDT. To take it one step further, suppose that the bomb was detonated under adverse conditions that included a lot of precipitation, what effect would it then have on the fallout?

Dr. GRAVES. Depending on where the precipitation was. Suppose there were a lot of precipitation right at the mushroom, instead of dirt, you would have globules of water which would fall fast. Hence you bring the fallout down faster.

Representative VAN ZANDT. In other words, you add moisture to the particle and make it heavier, and it falls to earth much quicker.

Dr. GRAVES. Exactly. In the case of an air burst, we detonate it high up in the air, and we have two effects. In the first place, there

is relatively little dirt mixed into the cloud and hence there is very little local fallout, very little at the intermediate distances. Practically all of it is spread around over great distances.

In the second pace, since the detonation occurs high up in the air, very few of the neutrons will be subject to capture by chlorine and sodium, calcium or silicon, so that there will be very little induced activity. Most of the activity will be in the form of fission particles and carbon-14 produced by capture of the neutrons in the air.

Then one should realize that there is a possibility of difference here. If you want somehow or another to avoid worldwide fallout the best way to do it is to do it as a surface or subsurface burst. For example, if for some reason we wanted to do our best to avoid fallout over the whole earth, this does it, because this puts most of it down locally. If we want to avoid local fallout, we do it high in the air.

Representative COLE. Dr. Graves, the record does not show which is this and which is that.

Dr. GRAVES. I am sorry. If we want to avoid local fallout and emphasize worldwide fallout, we do it by means of an air burst, the higher burst. If we want somehow to minimize worldwide fallout and maximize local fallout, then we do it by means of a surface burst or a subsurface burst.

Representative HOLIFIELD. That would be in the small kiloton range, would it not?

Dr. GRAVES. It could be either.

Representative HOLIFIELD. If you are going to the megaton range with the fireball touching the earth and with the height of the mushroom cloud, do you not defeat your purpose?

Dr. GRAVES. I am not saying what the purpose is.

Representative HOLIFIELD. I was thinking of a military purpose. The purpose of either having fallout locally or worldwide.

Dr. GRAVES. If we have a megaton weapon to detonate for some reason, if we want to minimize worldwide fallout we must maximize local fallout. Hence we will do it by some such system as this—get as much dirt mixed up in the cloud as we possibly can. If we want to minimize local fallout, no matter if it is a megaton or not, then we go to the air burst because then we minimize the amount of dirt mixed up in the cloud.

The subwater or the underground shots just make this statement more so. If we really want to maximize local fallout, then we detonate these things so deep underground or underwater that nothing goes in the atmosphere, and then it is all local fallout and no worldwide fallout. So if we go from a high air burst down to a case where the fireball touches the ground, and then to where the fireball is on the ground, and then to where it is away below the ground, we are essentially going from a case where there is almost no local fallout, where there is more and more and more local fallout, until finally it is all local fallout.

Essentially it is not fallout at all. It is underground where it stays.

Representative COLE. If that is so, Doctor, if local contamination is a desirable objective of our military, could not that be accomplished by regulating the place where the explosion occurs, rather than to have that contamination contained within the weapon itself?

Dr. GRAVES. All of this is assuming that there is contamination in the weapon. If there is no contaminating material in the weapon, then we won't have fallout anywhere.

Representative COLE. It is impossible to have an absence of contamination in the weapon?

Dr. GRAVES. Exactly.

Representative COLE. A while ago I asked to what extent nuclear radiation within the weapon itself was a desirable military objective. You said it would depend on the local conditions as to whether they wanted to contaminate an area. Now my question is, Cannot that local contamination be accomplished by regulating the height or place where the weapon explodes, rather than having the nuclear radiation contained in the weapon itself?

Dr. GRAVES. The answer is "Yes."

Representative COLE. If that is so, why is not in theory at least the goal of the military to achieve a weapon as free of nuclear radiation as possible and still obtain the blast and heat effect?

Dr. GRAVES. That is the point I was trying to make. If you have a military situation in which you want to contaminate a region very badly, you can't do it by taking a weapon with very little radioactivity in it and detonate it in any way to put fallout down. You have to have the fission products there in order to contaminate the region. Am I misunderstanding you?

Representative COLE. You were not misunderstanding me. I am afraid I am not completely understanding you.

Dr. GRAVES. We have agreed that we cannot produce an absolutely clean weapon. This I think, is clear. On the other hand, there are degrees of cleanliness. If you want to contaminate an area, then let us not pick the lesser degree of cleanliness and try to get activity which is not in the weapon itself.

Representative COLE. In order to have local contamination you must have fissionable products in the weapon itself.

Dr. GRAVES. That is right.

Representative VAN ZANDT. Dr. Graves, earlier this afternoon you said that an air burst at a certain altitude was found to be most effective. Were these conditions that you are now describing taken into consideration in arriving at that conclusion?

Dr. GRAVES. Yes. I said that the height of burst depends on the effect you are trying to produce. If you want to produce the maximum local fallout, then clearly the best place to do it is at or below the surface, or somewhere along in there. If the effect you are trying to produce is the maximum amount of worldwide fallout then the best thing to do is to do it high up in the air.

Chairman DURHAM. You would eliminate most of it by going deep enough in having the explosion?

Dr. GRAVES. If you go deep enough you will contaminate everything right where the explosion occurred. It might be pretty deep for a pretty big weapon, but that is certainly correct.

Representative HOLIFIELD. Of course, this does not take into consideration the strategy or the policy of the military in using these weapons, nor does it take into consideration the policy of an enemy nation in attacking us.

Dr. GRAVES. That is right.

Representative HOLIFIELD. We have no guaranty that an enemy nation will be considerate enough to handle this in a way which will do the least damage, do we?

Dr. GRAVES. No, sir.

Mr. Chairman, I have nothing further that I would like to stress particularly. As I say, I am very happy to have a chance to appear before you and if you have additional questions, I will be glad to answer them.

Representative HOLIFIELD. We appreciate your presentation, Dr. Graves. Will you be with us for the rest of the day in case there are additional questions?

Dr. GRAVES. I would be happy to stay.

Representative HOLIFIELD. Is it according to your schedule to stay? We are not asking you to stay, because we know the important work you are doing.

Dr. GRAVES. No, sir; I have all day today.

Senator HICKENLOOPER. At least while I have been in the meetings, Dr. Graves, there has not been any discussion or comparison of the impact of cosmic rays as compared to other radiating activity or any radiation which might come in from outer space, and how the intensity increases or decreases on that.

Representative HOLIFIELD. That is scheduled for later.

Senator HICKENLOOPER. That is fine. I did not know whether it was scheduled to be discussed.

Representative HOLIFIELD. Mr. Cole.

Representative COLE. Mr. Chairman, I am very hesitant to ask Dr. Graves to continue longer, but since Dr. Graves has been in charge of our weapons tests both in the Pacific and continental United States for the past 6 years or since he is here, it seems to me that it would be of public interest to hear from him the procedures which he and those in charge of the tests followed in order to minimize the consequences of the tests, if he can do so within a few minutes. It is a rather large order, because I am somewhat familiar with the extent of your efforts. But I would like at least to give you an opportunity of telling the public of just what you and your staff do in having the tests occur under conditions which will cause the least damage.

Dr. GRAVES. May I start with the statement of the work which has gone into planning the present series, Mr. Cole, because in my opinion the main safeguards that are introduced into the tests of weapons occur during the planning stages.

In planning the present series, we were faced with the necessity for testing a relatively large number of devices, larger than we had tested before. Hence our planning was conditioned on that particular basis. Since we had to test more devices, we had to be even more careful in this set of tests than we have been in the past. As a result, we went back to the individual laboratories and to the Department of Defense, and told them that some of the tests that they were requiring to be done seemed to us to make the overall operation difficult and asked that they consider their particular requirements to see if somehow or other they could not raise the height of burst, change from towers to balloons, decrease the planned yield, or do such things.

In fact, we also suggested in several cases that the tests be combined so that the data could be obtained from a single test or a single detonation, rather than from two.

We also devised a new method of determining the relative hazards of a particular test by estimating the number of megacuries—the numbers that I placed on the board over there—which would be deposited locally. So that if we have a case where a hundred megacuries is going to be deposited locally, we can be sure that test will be less dangerous than one in which two hundred megacuries would be deposited.

In this way we were able to compare one test with another. We could decide which particular test we should worry about. We could decide whether this series was going in fact to be worse than the next series or the past series or not.

Once we have finally come up with a plan whereby the total amount of fallout is minimized, then we have to come to face with problem of carrying on the tests such that even the fallout that does occur will not hurt anybody. In order to do this, we have assembled in Nevada as competent a meteorological group as one can find anywhere. This meteorological group tells us long in advance what the weather will be like, such that we can control where the fallout will occur.

The test area is in the midst of a bombing range which consists of very many square miles where there is no one. If we can arrange things such that the fallout occurs in this bombing range, then we will be sure of not hurting anyone. In order to guarantee that it will fall in the bombing range, we have to know what the weather is going to be like. The postponements that have occurred for the last 10 days or so have all been due to 2 facts. In the first place, we have had unusual high winds. We have had a jet stream coming through which could carry the material to great distances, and that we do not like. We would rather have the fallout occur on our bombing range. In the second place, we have had a lot of precipitation. I know those of you who have been out there with us realize that we have been on a desert, but you may be surprised to find out that a few days ago our meteorologists told us there would be scattered showers in the region in the desert. We said let us not shoot while we have showers. I went into town that night to tell the newspaper people why we had this postponement, and I almost drowned getting back out. There was a flood. The highway was covered with water, and we had 6 inches of water in our ditches in the desert.

The next day they told us we might have some snow flurries. We had snow out on the desert. All these conditions are not feasible for tests because they sweep down the radiation and put it on the ground in regions that we can't control.

Consequently, I would like to give a very considerable hand to this group of meteorologists who we have out there who may make us mad because they make us postpone, but they keep us out of trouble. They tell us the weather with great accuracy and permit us to be sure that the weather will not give us a fallout situation that we would not like.

I am often asked the question, why is it that we don't do all of these shots by high airburst. We discussed here the possibility of minimizing local fallout by using high airbursts. The conditions of testing are largely the conditions pertinent to the information we want to

get. We are not interested in producing there, information such that if someone says to make a weapon, then the laboratories will have the information they need to make that weapon. It turns out that there are experiments that must be performed to get some information in which the position of the device must be known with extremely great accuracy.

I may say that we have an experiment proceeding in which the position of the device to better than a half inch is required. Clearly this sort of accuracy is impossible from an air burst. It is even impossible with a device suspended from a balloon.

There are other cases where we don't care where the device is to a half mile or something of that sort. We just don't care. In such cases, I think we would be very wrong if we did not arrange somehow or other to detonate these devices high up in the air by an air burst.

So what we do is to then say our laboratories have certain requirements. They require us to test certain devices. They require us to get certain information from those devices. Our job in Nevada and in the Pacific is to devise a plan whereby these tests can be made and these experiments done safely.

Representative COLE. There is only one other question. What has been your experience with respect to radiation damage that has been the consequence of tests during the time you have been Director, limiting it to the tests in Nevada.

Dr. GRAVES. Again a part of the planning activities before this last series of tests was to accumulate all of the known data, all of the data from previous tests, to accumulate all of the measurements which have been made, and to interpret these as best we could into the total dose in all of the communities around the Nevada test site. These doses do not mean that the people in these communities got those doses, but it does mean that a certain fence post would have been exposed to that particular dose, and if someone stayed near that fence post he would have gotten that dose.

We looked at it and the best we were able to determine is that in the region which is now inhabited, the highest accumulated dose would have been something like 4.5 roentgens in the 6 years we have been there. This is something less than 1 roentgen per year. I am speaking from memory so don't hold me too firmly. I think there was one place with 4.3 roentgens. I don't remember now the name of the town. There was one with 4 roentgens. But the great majority were less than 4 roentgens. I am eliminating one place known as Riverside Cabins, where no one is actually living now. The people who were living there were living there for 1 year, as I recall it. The infinite dose to those cabins if somebody had lived there forever would have been something like 7 roentgens. This was the only place that I know of that received more than this 4.3 I have discussed.

Representative HOLIFIELD. Thank you very much, Dr. Graves. We appreciate your testimony.

Our next witness is Dr. Frank Shelton, of the Armed Forces special weapons project.

Dr. Shelton, will you come forward and take the right hand chair.

STATEMENT OF DR. FRANK SHELTON, TECHNICAL DIRECTOR, ARMED FORCES SPECIAL WEAPONS PROJECT [4]

Dr. SHELTON. The material presented to you by Dr. Graves was coordinated with me, and I concur in the statements that he has made to you. To a large extent I feel that the material was gathered and in part reflects the material in The Effects of Nuclear Weapons, the publication which some of you have before you. That being the case, the material as presented I would consider representing an official Department of Defense and Atomic Energy Commission position regarding the accuracy of the material.

I believe that is about all I have to say.

Representative HOLIFIELD. Dr. Shelton, you know that this page proof was only delivered to us I think late Friday, and we have not had a chance to even begin to read it.

Dr. SHELTON. Yes. I was informed this morning that the AEC had made copies available last week.

Representative HOLIFIELD. Therefore, we are not in a position at this time to question you on that particular book. You have no other statement to make?

Dr. SHELTON. I might mention a few words about the organization which I represent. The Armed Forces special weapons project, among other people, helped to prepare the book you have before you. It is the organization within the Department of Defense which would have primary responsibility during the weapons tests to obtain weapons effects data, principally for the Department of Defense. However, we have obtained a good deal of material at the request of the Atomic Energy Commission. We have, for example, been the organization which would gather together the capability of the country to measure the fallout from such an operation as the last Pacific one.

Representative HOLIFIELD. The only thing I can say then, unless there are some questions, is that the staff and the members will have to look at this before we present any questions to you. It is possible that we will call you back later.

Dr. SHELTON. Yes. What I had in mind saying was that I thought Dr. Graves had worked up a very complete presentation for you. I thought it would be somewhat redundant for me to elaborate further. Finally, that much of the material that he presented to you is material which we have already coordinated on and is the official position on the part of the Department of Defense.

Chairman DURHAM. Doctor, give us the size of the composition of your organization, and how you operate. Do you operate in connection with the Los Alamos Laboratories and all the rest of the laboratories throughout the country?

Dr. SHELTON. The Armed Forces special weapons project, as I said, is an agency of the Department of Defense. Essentially we work for the three services, the Army, the Navy and the Air Force, and it is our job to obtain the effects data that they require in the employment

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[4] Technical director of the Armed Forces special weapons project. He has been active in the atomic-energy field since 1952. During the spring of 1955, he served as technical adviser to the Military Effects Test Group at Operation Teapot, and in 1953 participated in Upshot-Knothole. Dr. Shelton was born in 1924. He received his bachelor of science, masters, and doctor of philosophy, all in physics, from the California Institute of Technology. Prior to joining the Armed Forces special weapons project, Dr. Shelton was with the Sandia Corp. in the weapons effects field. (Submitted by Department of Defense.)

or anticipated employment of nuclear weapons. To get that information we must participate on the full-scale weapons tests. To do the job, we have called upon many of the Government laboratories, and a number of the civilian laboratories to actually go into the field and to obtain the data.

The Armed Forces special weapons project would then have as its job the assembling of that group of people. We actually coordinate that effort, and guide those people in the field to obtain the required data.

Chairman DURHAM. You tell the weapons division at Los Alamos or Livermore the type of weapons you want?

Dr. SHELTON. It is usually not that way. More nearly the case is that they are detonating a particular type of weapon in the process of development. In a more normal case, we would be aware of that type of weapon and need for information from that type of weapon. We do not tell them the type of weapon.

Chairman DURHAM. You suggest through the military channels what type of weapon you desire. Isn't that the procedure. That is, for whatever size mission?

Dr. SHELTON. Yes, sir. There are broad characteristics required by the Department of Defense in the weapons which eventually would be stockpiled.

Chairman DURHAM. What part do you take in evaluating the tests after they have already been completed?

Dr. SHELTON. When a test has been completed and we have obtained the data in the field, AFSWP continues to monitor the agencies that have taken that data, and in the final production of the reports. One of our largest functions is production of final test reports, on the effects, and not the develcpment of weapons.

Chairman DURHAM. Do you go into the hazard at all of the radiation fallout or does that rest entirely in the laboratories?

Dr. SHELTON. Our large participation in fallout has been pretty well confined to the local fallout. For instance, in the last Pacific tests, we did document many thousands of square miles of the ocean. It was the AFSWP that documented the local fallout in the last Pacific tests.

Chairman DURHAM. Do you have any contact with NATO or the Far Eastern organization SEATO?

Dr. SHELTON. In addition to obtaining the information on weapons effects, we do transmit to the services that type of information in various publications. Those various publications are distributed to the American portion of NATO. Of course, there is the classification problem again. Typically one of our best known publications is one called Capabilities of Atomic Weapons. This is a compilation of the effects as we best know them. That is distributed to all of the Armed Forces of the United States.

Representative HOLIFIELD. Dr. Shelton, do you have or can you present to the committee an unclassified list of the tests which we have held and an estimate of the radioactive mission of those tests? Is that permitted?

Dr. SHELTON. You are asking for a compilation of the tests in chronological order, and you are asking for the amount of radiation produced on those tests?

Representative HOLIFIELD. I am asking you if that can be given to the committee on an unclassified basis or not?

Dr. SHELTON. I have discussed this subject, prior to coming here, with members of the AEC. We feel that we could not do that. We could do it in a closed session.

Representative HOLIFIELD. I understand that.

Dr. SHELTON. We feel that it would reveal information of a sensitive nature.

Representative HOLIFIELD. In this book which I have not had a chance to read yet, you do not go beyond the description of explosion of one weapon, let us say. There is no extrapolation of what would occur in a war if 100 weapons of a megaton or 5 megatons each were exploded on the United States?

Dr. SHELTON. What you have said is essentially correct. The phenomena as presented are over varying yields, but presented as one shot at a time and the effects that you can expect. The only effect, of course, that would be additive in a real large sense would be the fallout. In the normal employment of weapons one would not typically overkill his target by repeatedly blasting weapons in the same spot. We did not treat the fallout except on a one-shot basis.

Representative HOLIFIELD. I notice on page 419, you have a description of a 1-megaton surface burst with a pattern of radioactivity, and the number of miles involved, the number of hours involved for downwind distribution at the rate of 15 miles per hour wind. It would be possible, by extrapolation, however, to take that particular description and apply it to 100 or 200 weapons if you know the meteorological conditions as of a certain time.

Dr. SHELTON. What you have said is essentially correct. Although the chart given is for the dose rate at 1 hour, for a 1-megaton weapon, one finds the prescriptions given here, the mechanics of getting the same pattern for any other yields. We were not so explicit in giving you the pattern for other winds. As you saw, it was a uniform 15-mile-per-hour wind at all altitudes. I believe Dr. Kellogg, who is to follow us, will give you varying winds, but we feel that the fundamental areas involved are represented by the chart and the prescriptions given for other yields.

Representative HOLIFIELD. As a matter of policy, you do not believe that we should keep from the American people the effects of atomic or hydrogen weapons, do you?

Dr. SHELTON. Indeed not, sir. We have gone out of our way, for instance, in presenting the book that you are talking about and go just as far as we could go. In fact, many of the statements in the book required careful consideration of classification and the final determination that in the interest of the public that was the proper thing to do. We have withheld perhaps only a small percentage of all that is now known about nuclear weapons which is in the sensitive area relating to design of weapons.

Representative HOLIFIELD. You are presenting this book for the committee's action if they so desire of including it in the record, are you not?

Dr. SHELTON. The advanced copies of The Effects of Nuclear Weapons were distributed to the committee by the AEC. Any portions of that book for the record are perfectly all right. I noticed that the book

as I have it here does not have the preface, and I believe that would have explained the role of AFSWP in the preparation of the book. There is a preface and acknowledgment that I presume will go into the final publication.

Chairman DURHAM. Your division prepared this entirely?

Dr. SHELTON. Our division prepared this book in cooperation with Dr. Glasstone, and with the cooperation of the Atomic Energy Commission. We were asked to help prepare the book, and we did.

Representative HOLIFIELD. General Starbird, do you wish to add anything to what Dr. Shelton has given to us today?

STATEMENT OF GEN. ALFRED D. STARBIRD, DIRECTOR, DIVISION OF MILITARY APPLICATION, ATOMIC ENERGY COMMISSION [5]

General STARBIRD. I have nothing specific to add, sir.

Representative HOLIFIELD. You have not prepared formal testimony?

General STARBIRD. I had prepared, Mr. Chairman, some testimony originally when I thought I was to be the first witness. That testimony has been made available to you. It is entitled, "Testimony Before the Joint Committee on Atomic Energy on the Production of Radiation and Radioactivity From Nuclear Weapons." Knowing that Dr. Graves would be available, I consulted with him. I do know that the information contained in the statement that I mentioned has Dr. Graves' concurrence. I find that he has covered verbally everything that is in the statement and in somewhat more elaboration. I would like to request, therefore, that the statement be added to the record.

Representative HOLIFIELD. Thank you. It will be accepted without opposition.

(The statement referred to follows:)

TESTIMONY BEFORE THE JOINT COMMITTEE ON ATOMIC ENERGY ON THE PRODUCTION OF RADIATION AND RADIOACTIVITY FROM NUCLEAR WEAPONS

TOPIC V

1. In this, the 5th topic of the hearing before your committee, we have been asked to cover the Production of Radiation and Radioactivity with Nuclear Weapons. From earlier witnesses you have received general background information on radioactivity and its effect, together with a description of the two basic nuclear processes (fission and fusion). For this topic we have been asked to discuss the nuclear process employed in weapons, to describe the several different physical effects which the explosion of a nuclear weapon would give, and to discuss briefly the division of radiation energy which would result from various conditions of firing. We have prepared a written statement covering these matters and I shall follow closely that statement in my oral presentation.

Description of nuclear weapons

2. An explosion is the release of a large quantity of energy in a short interval of time and within a limited space. The release of this energy is accompanied by a very great increase in temperature so that the products of the explosion become extremely hot gases. The expansion of the air heated by a nuclear

[5] Date and place of birth: April 28, 1912; Fort Sill, Okla. Education: Bachelor of arts, U. S. Military Academy, 1933; civil engineering, Princeton Engineering College, 1937. Work history: Troop duty, Fort Belvoir, 1933–37; company officers' course, Fort Belvoir, 1937–38; instructor of the Engineering Department at the USMA, 1938–41; Operations Division, War Department, General Staff, 1941–44; engineer combat group commander, Third Army, 1944–45; Operations Division, War Department, General Staff, 1945–46; Deputy Chief of Staff, USARPAC and Joint Task Force 7, 1946–49; Weapons System Evaluation Group, Office of Secretary of Defense, 1949–50; area engineer, Corps of Engineers, Pierre, S. Dak., 1950; Secretariat, SHAPE, Paris, France, 1950–53; Office, Chief of Engineers, 1953–55; Director, Division of Military Application, AEC, 1955. (Submitted by the Atomic Energy Commission.)

detonation causes the formation of a blast wave. When the head of the wave (the shock front) passes a given point it results in an abrupt rise in pressure causing some of the destructive effects of the explosive.

3. The nuclear bomb is similar to the more conventional high explosive bomb in that a portion of its destructive action is due to the blast or shock discussed above. However, apart from the fact that the nuclear bomb can be many thousands of times more powerful than the largest TNT bomb, there are other more basic differences. Firstly, a fairly large portion of the energy from a nuclear explosion is emitted in the form of light and heat. This emission is referred to generally as the "thermal radiation." It can cause fires or skin burns at considerable distances. Secondly, the explosion is accompanied by highly penetrating, but invisible, rays called the "initial nuclear radiation." Finally, the substances remaining after the nuclear explosion are in large part radioactive, emitting similar nuclear radiations over an extended period of time. This later radiation, arbitrarily taken as that which occurs later than 1 minute after the bomb's initiation, is commonly referred to as the "residual nuclear radioactivity."

4. Earlier nuclear weapons made use only of the fission process in the achieving of this high energy nuclear detonation. In this process neutrons are caused to enter the fissionable nuclei of atoms of either uranium or plutonium. Under certain prerequisite conditions the fissionable material is split (or fissioned) into fission products by an almost instantaneous chain reaction. During the fissioning, great quantities of energy are released. Neutrons and gamma rays escape from the fissioning material and bombard surrounding elements, forming some radioactive isotopes. The fission products which result from the explosion constitute a very complex mixture. This mixture may consist of about 170 different types of fission debris which are isotopic forms of some 35 different chemical elements. (See chart I.) This fission debris initially is highly radioactive and decays over a period of time by the emission of beta particles and gamma rays. From each kiloton of fission yield approximately one-tenth of 1 pound of radioactive fission products can be expected to occur. At 1 minute after the explosion, when the residual nuclear radiation has been postulated as beginning, the radioactivity from the fission products of a kiloton of fission energy yield is comparable to that of some 100,000 tons of radium. The radioactivity decays rapidly. For example, there are given below the estimated total gamma activities of the fission products from a nominal atomic bomb, expressed in megacuries, at various times after the detonation.

Total gamma activity of fission products in megacuries

Time:	*Activity*	Time—Continued	*Activity*
1 minute	820,000	1 month	2.3
1 hour	6,000	1 year	.11
1 day	133	10 years	.008
1 week	13	100 years	.0006

5. Later, means were found of using the fusion process to secure weapons of higher yield than were practical from the purely fission designs. You will remember that a fusion process is the uniting or fusing of very light elements to form heavier elements and that great quantities of energy are given off in the process. To initiate a fusion process tremendous heat is required. The term "thermonuclear" results from the fact that such weapons use heat to maintain the nuclear reaction. In contrast to fission, no fission product radioactivity results directly from fusion. Fusion, however, is accompanied by the escape of neutrons, some of extremely high energies, and these can induce radioactivity in materials with which they come in contact. Naturally, too, in a thermonuclear weapon the fission portion of the reaction forms radioactive debris in the same manner as in a purely fission weapon.

6. The partition of energy from a nuclear explosion as between blast and shock, thermal radiation and nuclear radiation varies somewhat with the design of the device and with its condition of firing. As a general approximation nevertheless, the division can be considered as that shown by chart II. The chart portrays specifically the result which could be expected from the detonation of a device of yield of 1 megaton, fired within the earth's atmosphere, but at such altitude that comparatively little extraneous material from outside of the device is available to be made radioactive by escaping neutrons (as for example, if fired at a few thousands of feet in the air).

7. You will note that in this case some 50 percent of the energy would be released as blast or shock, some 35 percent would occur as thermal radiation (heat or light), while some 15 percent would be in the form of nuclear radiation. Of

this 15 percent, roughly one-third would be "initial radiation" occurring within 1 minute after firing, while two-thirds would be "residual radiation." The highest intensity of this residual radiation naturally would occur during the seconds immediately after the first minute. It would decay rapidly thereafter but some small residual radioactivity could be expected for many years or even thousands of years after the detonation. (See table in paragraph IV.)

8. The 50 percent of the energy translated into blast or shock would have effects quite similar to those to be expected from a high explosive detonation. (See chart III.) These would consist of a shock front (the head of the blast wave), a later region of high and low pressures behind the shock front, and a violent wind flow. This latter would initially be in the direction outward away from the explosion, but later in a reverse direction. The damage caused by the shock or blast would be, of course, a function of the weapon's yield, of distance from the firing and of the strength of the receiver. In the case of a typical air burst, the distance to which a given overpressure (or blast effect) extends varies generally as the cube root of the yield. The term "generally" is used for the reason that there are other factors such as reflected or refracted shock waves and pressures which under many conditions can reinforce or interfere with one another in such manner as to change materially the blast effect at a given point. As an indication of the order or magnitude of the effect one might expect from this phenomenon the burst of a 20-kiloton weapon at an altitude of several hundred feet could be expected to destroy beyond economical repair a multistory reinforced concrete building at distances up to one-half mile. On the other hand, a 1-megaton burst fired under comparable conditions could be expected to give similar damage to the reinforced concrete structure at distances up to 2 miles. Although large pressure differences result in injury to the human body, persons are more likely to be injured by flying objects, crushed or buried under buildings, or thrown against fixed structures than to be injured directly by wave overpressures.

9. The one-third of the weapon's energy emerging in the form of thermal radiation is contained initially in a relatively small volume of incandescent gases resulting from the vaporization of components of the device and of the adjacent atmospheric or other materials. (See chart IV.) This intensely hot spherical mass termed the "fireball" is visible for a perceptible period of time until the thermal radiation has been dispersed over such volume that visible light is no longer emitted. The initial temperature of the fireball is of the order of several million degrees and the thermal radiation covers a broad spectrum of wavelengths and includes ultraviolet, visible, and infrared. These radiations travel outward at tremendous speeds. The extent of injury or damage to a person or material resultant from thermal radiation is a function mainly of total energy received, but secondarily of the rate of absorption. From a given weapon, or from weapons of comparable energy yield, the intensity of thermal radiation received is a function primarily of distance from the burst. The amount varies inversely as the square of the distance, provided there were no attenuation by the atmosphere. The period over which thermal energy is given off from an explosion increases with the yield—that from a kiloton device being limited to a few tenths of a second but for a megaton device the period may extend to several seconds. A 20-kiloton burst could be expected to ignite combustible house materials at ranges up to 2 miles, while a 1-megaton burst could have similar effect up to 10 miles. The 20-kiloton burst could cause first degree burns to exposed skin surfaces at ranges of 3 miles, while a megaton burst could cause similar burns at 14 miles. Adverse weather conditions can vary the distance at which these effects occur.

Nature of the nuclear radiation from a weapon

10. The 15 percent of the weapon's energy which becomes nuclear radiation is the result of several actions and interactions. Some of these, of course, occur immediately after firing while others are much later in the chain. To name the primary of these actions (chart V):

(*a*) Firstly, the initial fission or thermonuclear reactions emit high gamma and neutron fluxes.

(*b*) Secondly, radioactive debris products result from the fissioned atoms. The radioactive debris products condense into particles of various sizes and may fallout locally or at a distance depending upon their size and the altitude from which they fall, as well as meteorlogical conditions. The individual radioactive isotopes regain their nuclear stability by giving off beta particles, which in a large fraction of the cases is accompanied by emission of gamma radiation. The average time for the atoms of a particular isotope

to reach stability varies from a fraction of a second to thousands of years.

(c) Thirdly, neutrons contribute to the residual radiation by inducing activity in various elements of the materials in the bomb, atmosphere or in substances which may be in the explosion environment.

11. Essentially all neutrons escaping a bomb are released from the fission or fusion reaction of the bomb's nuclear material. They emerge almost immediately after initiation of the firing. Though they represent only a very small portion of the total energy yield of the explosion they can possess a sizable kinetic energy. They can penetrate relatively long distances through the atmosphere (of the order of several thousand feet near sea level) and can induce radioactivity in the atoms they encounter. The distance at which the neutrons from a nuclear explosion can in themselves constitute a hazard is a function of the type of reaction, of the size of the explosion, and of the materials surrounding the bomb. From thermonuclear reactions, neutrons of higher energy are released than from the fission process. On the other hand, the number of neutrons escaping to travel to great distances depends on the thickness and type of material which surrounds the nuclear constituents of the bomb.

12. Gamma radiation, like neutrons, is released in large quantities from the initial explosive mass and can pentrate considerable distances through the air. Further, additional quantities of gamma radiation can result from the interaction of escaping neutrons with particles they encounter, and from the subsequent decay of radioactive elements throughout their life. A sizable portion of the residual radiation from a nuclear explosion is released ultimately as gamma radiation. This release is of decreasing intensity with time, but can continue for many years.

13. Two other forms of residual radioactivity alpha and beta particles, are found normally in bomb debris. The alpha particles are of very short range and result only from the unfissioned portion of the original fissionable material. If the fissionable material is available in sufficient quantity and is taken internally into the body, the alpha radiation from it could (with long residence in the body) cause extensive damage. However, for various reasons which will be discussed under a later topic, this unfissioned material is generally less of a hazard than some of the fission products. Beta particles (electrons) have a limited range. Depending upon the initial energy, the range may be only a few centimeters in atmosphere at sea level. Such particles result from the decay of fission products. They can constitute a hazard, but only when deposited on the body's surface or internally.

Type of weapon bursts and their effect on radioactivity

14. The portion of a bomb's energy that emerges as radioactivity (as contrasted to that portion which emerges as blast or thermal) is a function primarily of bomb design. Naturally, if the fission yield is high in contrast with the fusion a greater relative quantity of fission debris products will result. Naturally, too, the higher the energy of the escaping neutrons and the greater the amount of material close to the explosion, the greater will be the induced activity.

15. The conditions of the firing of the weapon, however, can have an important effect both on the amount of total residual nuclear activity and importantly on the distance at which that activity can be felt. In this connection, I shall mention briefly and generally the changes in residual radioactivity distribution which could result from weapons or devices fired on the earth's surface; at several hundreds or several thousands of feet in the air; underground; or underwater.

(a) In the case of a surface or near surface burst (chart VI), a large quantity of the surface material could be drawn up into the fireball to be mixed there with the radioactive fission products while those products are still in gaseous form. Escaping neutrons encountering this drawn up material could induce radioactivity therein. At the same time, however, when the cooling radioactive gases condense they would form in part larger particles which would trap (or scavenge) the material's residual radioactivity. These larger particles would fall out rapidly and relatively close to the firing point from such a burst, thus making local fallout heavy.

(b) In the case of an air burst at several hundred or several thousand feet altitude (chart VII) where the fireball does not touch the ground, the debris from the fission process would be unchanged in amount from that of a low altitude burst. However, the induced activity caused by the interaction on particles sucked into the fireball from the surface would be lacking. The total radioactive debris then would be less than in the case of the surface burst. The scavenging material from the earth's surface also would be lacking. The particles into which the debris condenses would generally be

smaller than in the case of a surface burst and would drift to the earth's surface at a distance from the firing.

(*c*) In the case of an underground or an underwater burst it is theoretically possible to place the detonation at such depth that little or no radioactivity would reach the atmosphere. Naturally, for such firings the induced activity would be relatively high, but would be confined in the earth or water along with the bomb's debris fragments. In the case of underwater shots, the movement of their residual radioactive particles is affected by ocean currents. In the case of underground burst, however, movement could only be through transfer of the surrounding material to the outside or by leaching. Great depths would be required to confine weapons of multikiloton or multimegaton yield if total initial radioactivity is to be confined.

Measurements and limitation on the data

16. In each of our several test series we have devoted a great deal of effort to the securing of information on the effects to be expected from nuclear weapons. Certain information can be readily secured and other is most difficult to measure or estimate.

17. We have available detailed information on the blast and shock effects to be expected from weapons of various sizes, on various type structures, under different conditions of firing, and different ranges. These data present rather precise estimates of what could be expected from a future detonation or detonations. The same is true, but to a somewhat lesser extent, with respect to thermal radiation.

18. With regard to nuclear radiation we have devoted intensive effort and with all means available to date to the securing of essential information. We can estimate with a fairly high degree of reliability the initial radiation which will result from a certain firing. With respect to residual radiation, because of the time over which it will occur and the many factors (including atmospheric) which have effect, the giving of precise forecasts is much more difficult. I shall not attempt to cover these factors, nor their reliability, in view of the fact that many of the later witnesses will give expert testimony in this regard.

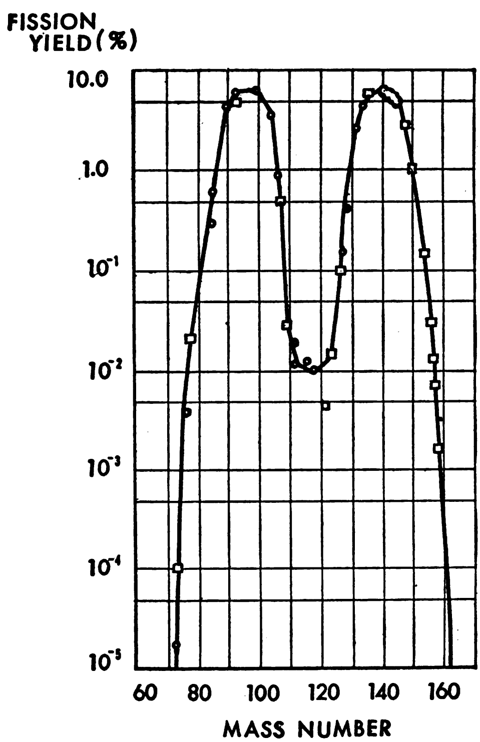

CHART I.—Fission yield versus mass number of fission products.

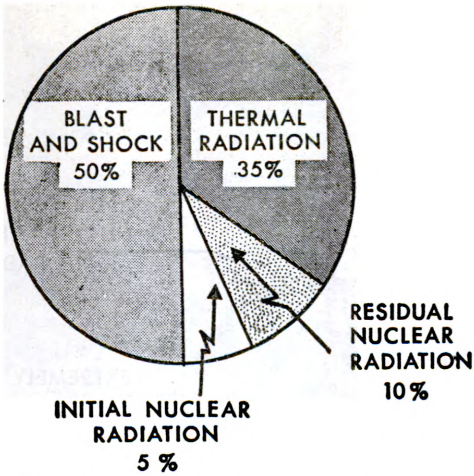

CHART II.—Distribution of energy in a typical air burst.

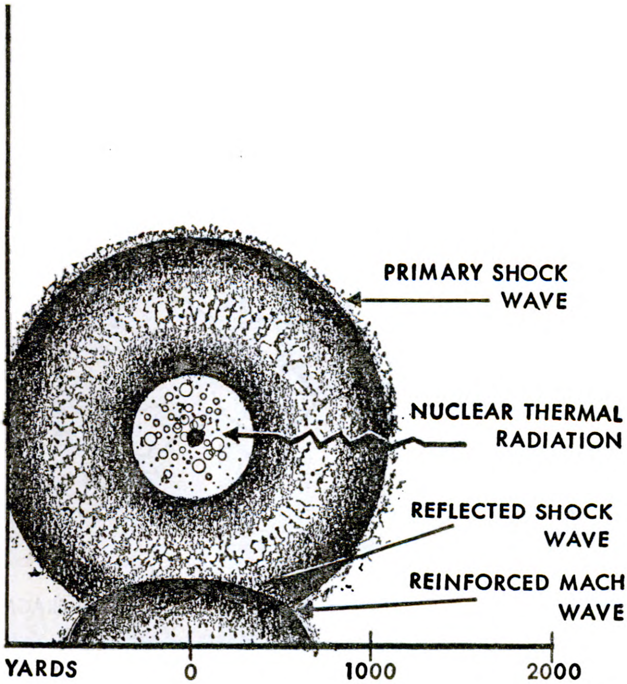

CHART III. Sectional view development of an atomic air burst.

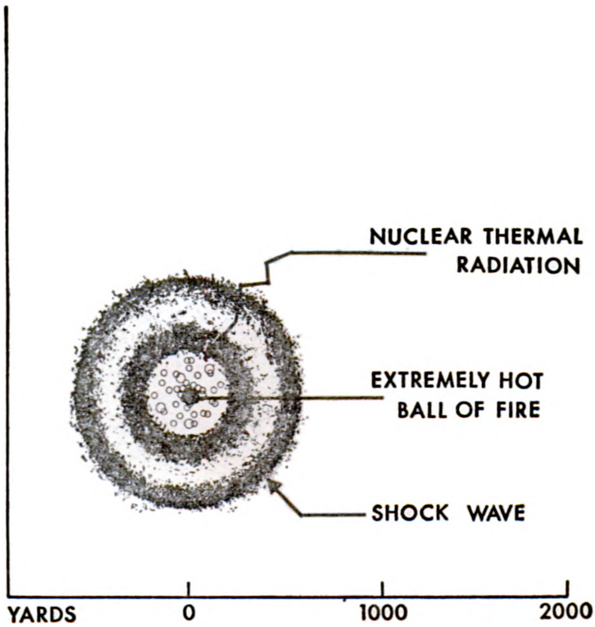

Chart IV

Sectional view development of an atomic burst.

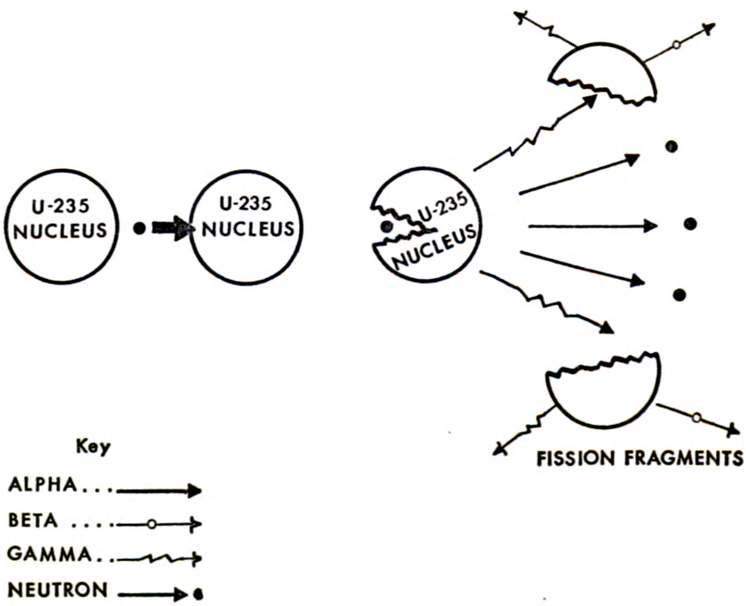

CHART V.—Fissioning of U-235 nucleus.

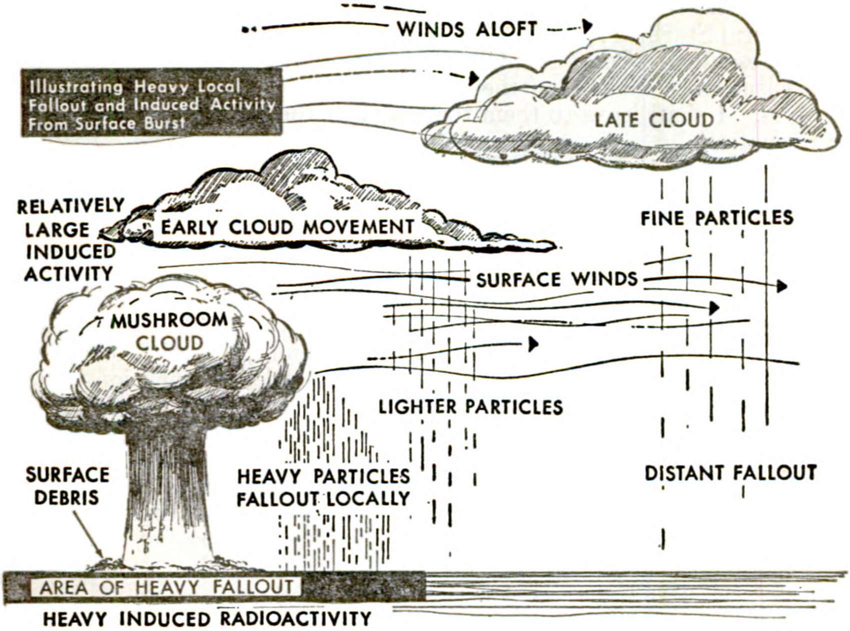

Chart VI.—Surface Burst

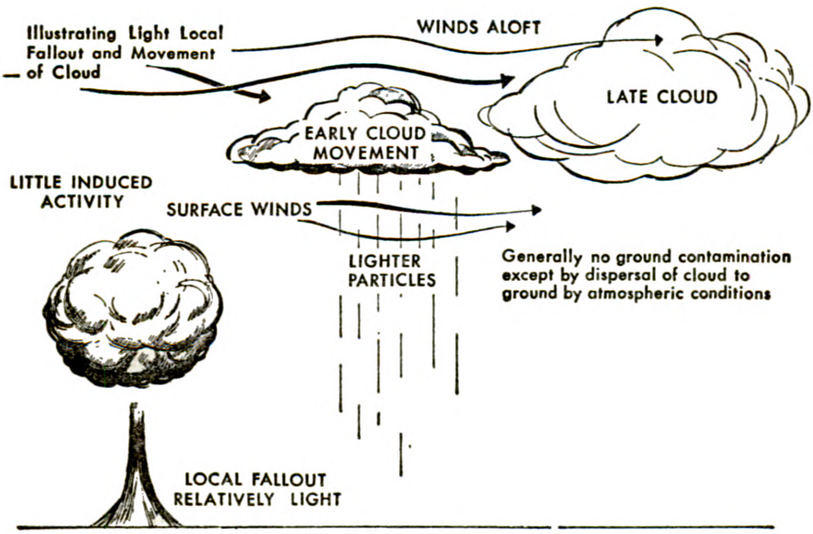

Chart VII.—Airburst

Representative HOLIFIELD. Are there any questions of Dr. Shelton or General Starbird?

Representative COLE. Mr. Chairman, I should like to have either of the witnesses comment on the question which I propounded to Dr. Graves. I shall leave to them the realm of the classified or security information.

I would like to know to what extent internal nuclear radiation or induced radiation is an important factor in determining weapons development.

General STARBIRD. The radiation that is in the weapons basically that were discussed, sir, was inherent in the original design of weapons. It would be possible that that radiation could have some military advantage. But our major effort of our laboratories over a past period of several series has been to find ways and means of decreasing the contamination that would result from a nuclear detonation.

Representative COLE. Is it a fair conclusion, then, that your goal is to obtain a weapon with the greatest yield and the least amount of radiation, whether internal or induced?

General STARBIRD. That is one of our major goals, sir.

Representative COLE. Is it to any extent a goal to obtain a weapon with a minimum of blast and heat and a maximum of radiation?

General STARBIRD. I know of no such goal, sir.

Representative COLE. That is enough.

Chairman DURHAM. Your preference, of course, would be no radiation?

Representative COLE. If you can get the same blast.

General STARBIRD. To carry out the objective I mentioned, the fulfillment of that objective would be to get none.

Representative VAN ZANDT. Does a nuclear weapon offer a field commander the capability of contaminating an area with radiation?

General STARBIRD. As Dr. Graves mentioned, a greater amount of radioactivity can be placed locally. I think, sir, this would be the best answer. However, I cannot comment on whether that would be an objective.

Representative VAN ZANDT. Then radiation does play a part in the prosecution of a nuclear war.

General STARBIRD. It certainly plays a part, sir.

Representative HOLIFIELD. As a matter of fact, you cannot have a nuclear war as far as you know now without radiation, can you?

General STARBIRD. I know of no way.

Representative HOLIFIELD. Looking at this strictly from a military standpoint, why would you, without any assurance that the enemy is doing likewise, want to decrease one of the powerful elements of the weapon—there being 3 elements, 1 blast, 1 heat, and 1 radiation—looking at it strictly from a military standpoint. Why would you deny yourself one of those elements?

General STARBIRD. One would like to have weapons that he has the greatest amount of control over.

Representative HOLIFIELD. But if you did have a weapon where you had a maximum control over the factor of radioactivity would you have any assurance that a ruthless enemy would not take advantage of this other powerful element of the weapon in an onslaught against us?

General STARBIRD. I feel I am not really the one to answer that, sir. My field right now is in an assignment with the Atomic Energy Commission. To answer your point from a personal viewpoint, I don't know of anything that we could do to guarantee that the other man would do likewise.

Representative HOLIFIELD. Are there any questions of General Starbird?

Senator ANDERSON. Did I understand you a moment ago in answer to Congressman Cole's question to say that you were trying as far as possible to reduce the radiation in weapons?

General STARBIRD. This is an objective, sir, of the Commission's program, that is, to find ways and means of reducing the contamination from a weapon.

Senator ANDERSON. I am not talking about the Commission's program now. I am talking about the military program. Do I understand it to be your testimony that the military is now engaged in trying to reduce the radiation? If so, I would like to whisper to you here on the side the question that you can either comment on or not.

General STARBIRD. No, sir, I think I can comment. Certainly the military has indicated to us a great interest in weapons that have a lesser contamination for the yield involved.

Senator ANDERSON. I was not talking about interest. I was talking about program.

General STARBIRD. We are the ones, sir, as you know, who actually do the development work and the actual research for the production of the nuclear weapons. Generally to carry out an express requirement or expression of interest by the Department of Defense.

Senator ANDERSON. To whom can I direct a final question and see if it is classified?

General STARBIRD. Mr. Marshall is Director of Classification.

Representative VAN ZANDT. General, is it not true that in the employment of special weapons the field commander has to have a variety of them? He may call upon a weapon where he wants no contamination left after the burst, or he may employ a weapon where he wants to contaminate a great area. So it is up to you people to develop various types of weapons to give him the versatility he must have in the prosecution of a nuclear-type war. Am I correct in that assumption?

General STARBIRD. It is true that a versatility of weapons increases the strength of the military force, sir.

Senator ANDERSON. I have no further questions.

Representative HOLIFIELD. Thank you very much, gentlemen. As I said to Dr. Shelton, we will want to look at this book and possibly call you back later.

General STARBIRD. Thank you, sir.

Representative COLE. Would the Chair indulge me for just a moment in order that I may fill in a gap—not from these witnesses—when Dr. Graves said at the motel in Nevada the highest dosage was 7 point something roentgens.

Dr. GRAVES. That is correct.

Representative COLE. Tell us, since you are here, Doctor, what are the rules of the Commission with respect to safe dosages of workers in the Commission's laboratories, and so forth.

Dr. GRAVES. I have forgotten the title, but I think it is the American Commission for Radiation Protection, or something of that sort, originally stated that the workers in radioactivity could take one tenth of a roentgen per day forever without suffering injury. In the Commission laboratories it was determined that we should reduce that by roughly a factor of two and so it has been reduced to three-tenths of a roentgen per week.

In our test operations we have said that we will permit our people to take this three-tenths of a roentgen per week. However, they went for a quarter of a year, which would be 13 weeks, and hence our test criterion has been something like 3.9 roentgens per quarter or per period of 13 weeks. This means that our people in the test series we have tried to keep below 3.9 roentgens. We have not always done that. As a matter of fact, there have been a number of cases where people have gotten 3, or 4, or 5 times that much. For people offsite, we would like to do better than that. It is our feeling that if people are not willingly engaged in this activity, we should not ask them to take as much as we do. So we try to say that people offsite should not get more than 3.9 roentgens per year instead of per quarter. The present criterion is, I guess, 3.9 roentgens per series.

Representative COLE. I would like you to comment on the lethality of the 7 or so roentgens given to the motel.

Dr. GRAVES. The lethal dose is around 450 roentgens. This is very much less than that. Again this is not a subject on which I am an expert. So I don't want this to be taken as expert testimony in your record. To my knowledge, in order to be able to examine the blood or tissue or blood of someone and find out they have been exposed to radiation, the minimum dosage you can detect by some changes in the body you can see immediately is something like 20 or 25 roentgens. If it is less than 20 or 25 roentgens, there is no test we can make on an individual to show he has had radiation. If we get up above 20 or 25 roentgens, you begin to notice that there have been changes made in the blood. Some cells have become broken up or deformed in one way or another. You can detect some small changes above 25 roentgens. At about 75 roentgens or 100 roentgens a person would become ill, nauseated, and have some radiation sickness, and would recover, and presumably be all right. It is around 400 roentgens when you begin to find a few people die. Around 450 about half of the people would die, or something of that sort. So this 7 roentgens is considerably less than the amount that one can detect by any means that we know of for detecting radiation.

Representative COLE. Is it a fair conclusion, then, that the persons who may have been exposed to that dosage of 7 roentgens were not harmed in any way.

Dr. GRAVES. That is not a fair statement, because then you get me in trouble with people who are worried about long-term effects.

Representative HOLIFIELD. The Chair might say we are going into the pathology feature of this, and we have a list of distinguished witnesses that will testify on this point.

Dr. GRAVES. Yes. I want to be sure you understand I am not an expert in this field. I do not mind talking about it, but do not take my testimony as expert in that particular connection.

Representative VAN ZANDT. Mr. Chairman, may I ask this question. Doctor, how many roentgens did your body absorb in the Los Alamos accident?

Dr. GRAVES. I had about 200.

Representative COLE. From outward appearances you look rather healthy.

Dr. GRAVES. Thank you.

Representative COLE. At this time some several years later.

Dr. GRAVES. That was in 1946, so it has been 11 years. But this really is not important. You may have one person take 200 roentgens as I did and be perfectly happy for 10 years. But does it give me a greater probability of having cancer or does it give me a greater probability of this, that or the other, we just do not know. The danger is not that this will happen to you. The danger is that it is more likely to happen to you. Maybe the more likely is not very much more likely, but it is still more likely.

Representative VAN ZANDT. Doctor, how did this dose of radiation affect you?

Dr. GRAVES. I was nauseated for the first day. I was in the hospital for 2 weeks. I never did feel very sick but I was quite—I did not have very much ambition, I was tired, I got tired climbing steps and so on, and this lasted for perhaps 6 months. At the end of 6 months I was back to work, and I can't tell any difference now.

Representative VAN ZANDT. Did it affect your hair in any way?

Dr. GRAVES. I lost the hair on one side of my head. I did not have to shave for a while, which was a byproduct that was useful.

Representative VAN ZANDT. How about your eye?

Dr. GRAVES. I have a radiation cataract in one eye. The other eye is perfectly all right.

Representative HOLIFIELD. What was the white corpuscle count at the end of 6 months?

Dr. GRAVES. At the end of 6 months it was back to normal. You can't tell anything. You can examine me with a microscope or anything else, and you can't tell any difference now. At the time my white blood count dropped from about 8,000 or 9,000, which was normal, down to around 2,000. Again I don't have these numbers in front of me, so I don't remember exactly. But at the end of perhaps a week or 10 days the count began to increase again, and got back to normal. As a matter of fact, it got above normal. By 6 months it was back to normal, and stayed there ever since.

Representative HOLIFIELD. Dr. Graves, I think I express the feelings of every member of this committee that have known about this for so many years, that we are glad you are in as good health as you are today, and we want to again express our thanks to you for the tremendous contribution you have made to the security of our Nation.

Representative COLE. Mr. Chairman, I just want to concur in what you have said with respect to the attitude of the committee toward Dr. Graves' work. But since we have engaged in some rather personal questions of him with respect to consequences of his exposure, I would like to inquire if since that occurred you have increased your family in any way, and if so, whether the progeny is apparently normal and health. Mr. Chairman, I do not ask it facetiously. Here is a man

who has been exposed to a degree of radiation probably greater than any person that we know. He has told us the consequences to him of his own body. Since radiation exposure has been said to involve a question of sterility and so forth, unless he would rather not answer, I would like to give him the opportunity of indicating.

Dr. GRAVES. I had one daughter before the accident. I have had a daughter and son since the accident. The daughter and son as far as can be told are perfectly normal kids. We love them very much.

Representative VAN ZANDT. From a heredity standpoint, do they show any extraordinary amount of energy as a result of your brush with atomic energy?

Dr. GRAVES. Speaking as a parent they are very intelligent children.

Representative HOLIFIELD. Thank you very much. Our next witness is Dr. W. W. Kellogg of the Rand Corp. and he will speak to us on the subject of atmospheric transport, storage, and removal of particulate radioactivity.

Dr. Kellogg, how long is your presentation?

STATEMENT OF DR. W. W. KELLOGG, RAND CORP.[6]

Dr. KELLOGG. I have a report for the record which is somewhat long, and I was not planning to give it all now. It has a lot of documentation in it. I was going to abstract it to the committee orally. I could do it in 30 or 40 minutes. Is it too late to do that?

Representative HOLIFIELD. We will accept your prepared statement for the record. We will be glad to have you summarize it.

(The statement referred to follows:)

ATMOSPHERIC TRANSPORT AND CLOSE-IN FALLOUT OF RADIOACTIVE DEBRIS FROM ATOMIC EXPLOSIONS

(By Dr. William W. Kellogg, RAND Corporation)

INTRODUCTION

It is well known that the radioactive debris from an atomic explosion is carried high into the atmosphere, and that eventually all of it reaches the ground. However, there are a variety of things which can happen to these particles on their way to ground, and their paths can be quite complicated. The purpose of the present report is to describe and document part of this process of radioactive fallout.

In order to limit the discussion, fallout here will be taken to mean "close-in fallout," the fallout which occurs during the first day or two following the explosion, and which deposits radioactivity within a few hundred miles of ground

[6] Born: February 14, 1917, at New York Mills, N. Y. Educated: Brooks School, North Andover, Mass.; Yale University, bachelor of arts, 1939; University of California, Berkeley, graduate studies in physics; UCLA, master of arts in meteorology, 1942; UCLA, doctor of philosophy in meteorology, 1949. Occupations: Prep school science teacher (Brooks), 1939–40; teaching assistant, physics, University of California, 1940–41; U. S. Air Force, pilot weather officer, separated with rank of captain, 1941–46; research assistant, research associate, and assistant professor (in succession), Institute of Geophysics, UCLA, 1946–52; research scientist, the Rand Corp., Santa Monica, 1947–present. Affiliations: American Meteorological Society (committee on admissions, upper atomsphere committee); American Geophysical Union (upper atmosphere committee); Society of Sigma Xi; member, meteorological committee on the biological effects of atomic radiation, National Academy of Sciences-National Research Council; member, working group in internal instrumentation of the earth satelite program; member, ad hoc panel for measuring radioactivity in air of the United States National Committee for the International Geophysical Year; formerly member, upper atmosphere committee, NACA (now defunct). (Submitted by Witness.)

zero. The intermediate scale of fallout (that which occurs in the first few weeks) and the worldwide fallout will be treated by others.

Although the purpose is to tell what we know about fallout, an effort will also be made to point out the areas of uncertainty in our knowledge. Fallout is a process which is affected by many different things, and the atmosphere by its very nature behaves in an erratic and random way. Thus, it is fair to say at the outset that, no matter how well we could document our observations of fallout, there would still be an area of uncertainty due to the randomness of the process. This aspect should be borne in mind in considering the evidence which follows.

DESCRIPTION OF THE PROCESS OF CLOSE-IN FALLOUT

There is a fundamental difference between the fallout from an atomic device detonated at the ground and the fallout from one detonated so high that the fireball does not touch the ground. In the case of the surface burst, large quantities of surface material are broken up, melted, and even vaporized, and some of this material comes in intimate contact with the radioactive fission products. Then, after the atomic cloud has stopped rising and the violent updrafts associated with the explosion have subsided, the larger and heavier particles start falling back to the ground. The result is an area around ground zero and extending downwind which is covered in a more or less systematic way with particles contaminated by atomic debris.

In the case of an air burst in which the white-hot fireball never reaches the surface, the radioactive fission products never come into close contact with the surface material; they remain as an exceedingly fine aerosol. At first sight this might be thought to be an oversimplification, since there have been many cases in which the fireball never touched the ground, but the surface material was observed to have been sucked up into the rising atomic cloud. Actually, however, in such cases a survey of the area has shown that there has been a negligible amount of radioactive fallout on the ground. Though tons of sand and dust may have been raised by the explosion, they apparently did not become contaminated by fission products.

The explanation for this curious fact prabably lies in a detailed consideration of the way in which the surface material is sucked up into the fireball of an air burst. Within a few seconds from burst time, the circulation in the atomic fireball develops a toroidal form, with an updraft in the middle and downdraft around the outside. Most of the fission products are then confined to a doughnut-shaped region, and may be thought of as constituting a smoke ring. When the surface debris is carried into the fireball a few seconds after the detonation, it passes up along the axis of the cloud, through the middle, and can often be seen to cascade back down around the outside of the cloud. In its passage through the cloud, it has passed around the radioactive smoke ring but has never mixed with it.[1]

There has not been a large number of surface shots in the United States test series, and most of these have been set off in the Pacific area, where complete documentation of the fallout has been difficult because the greater part of the material came down in the open ocean or in the water of the lagoons. During the last Pacific test, however, a method of surveying the ocean to determine the distribution of the fallout was used which has given us some fairly complete and quantitative data on the pattern of the fallout from some larger yield devices.[2] A reanalysis of the fraction of the debris which came down within the first few hundred miles from the various Operation Redwing surface shots by Tucker,[3] based on the ocean and atoll survey made jointly by the Scripps Institute of Oceanography, the Naval Radiological Defense Laboratory, the Evans Signal Laboratory, the New York Operations Office of the AEC, the Chemical Warfare Laboratories of the Army Chemical Center, and the Air Forces Special Weapons Center, reveals that from a large yield surface burst about 85 percent falls down in roughly the first 24 hours; for a barge shot in the water of a lagoon the fraction is between 65 and 70 percent. According to Tucker, the accuracy of the estimates

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[1] Kellogg, W. W., R. R. Rapp, and S. M. Greenfield: Close-In Fallout, Jour. Met., vol. 14, No. 1, pp. 1–8, 1957.

[2] Van Lint, V. A. J., L. E. Killion, J. A. Chiment, and D. C. Campbell: Fallout Studies During Operation Redwing, Field Command, AFSWP, Operation Redwing Preliminary Report, ITR–1354, October 1956 (Secret, R. D.).

[3] Tucker, B. L.: Fraction of Redwing Radioactivity in Local Fallout, RAND Corp., Report in preparation, May 1957 (Secret, R. D.).

here is probably no better than 20 or 30 percent, so the good agreement which he obtained for various kinds of shots may be fortuitous.[4]

The one other piece of evidence on the fraction falling out from a surface shot comes from Operation Jangle. The Los Alamos Health and Safety Division had a number of stations downwind to record the fallout, and the Air Force surveyed a larger area by flying an instrumented aircraft at low altitudes over the desert. Two analyses have been made of the resulting fallout pattern in order to estimate the fraction of the debris which was represented, one by Lulejian[5] and the other by Rapp.[6] The results are as follows:

	Percent
Lulejian: Beyond 10 miles from ground zero and within 200 miles	60±20
Rapp: Beyond 4 miles from ground zero and within 200 miles	77
Rapp: Total fallout out to 200 miles	87

It should be noted that the famous March 1, 1954, test of the Castle series in the Pacific, which received some publicity because of the fallout on some nearby inhabited atolls,[7] was not well enough documented to enable one to get a good estimate of the percentage of fallout. In order for such an estimate to be made it is clearly necessary to be able to lay out the *complete* fallout pattern. This was not possible here, since the islands on which the fallout occurred occupied only a part of the pattern, and were probably not in the region of maximum fallout. This event will be discussed more below.

As pointed out above, if the height of burst is raised, the amount of surface material which can become intimately mixed with the fission products becomes less. As a result, the fraction which takes part in close-in fallout decreases with increasing height of burst. A tower shot does not exactly follow this trend, however, since the material in the tower itself and in the cab at the top of the tower apparently provides some radioactive fallout. The fraction falling out from a tower shot appears to be quite variable, as can be seen from the following tabulation prepared by Kenneth Nagler and Dr. Lester Machta of the United States Weather Bureau, based on a detailed analysis of the actual fallout from a number of tests in Nevada, all of which had yields in the range of 12 to 18 kt.

	Percent
300-foot tower	17.8
	12.3
	8.9
	7.8
	7.0
Average	10.8
500-foot tower	5.4
524-foot airburst (especially uncertain)	1.0

It should be noted that the particular airburst cited here produced a fireball which almost touched the ground. Higher airbursts, as mentioned above, produce no significant close-in fallout.

So far the discussion has been concerned with the *total amount* of radioactive material taking part in the fallout. The *distribution* of this material on the ground depends on a number of parameters—wind structure, yield and height of burst, and kind of surface. The yield and height of burst predominantly determine the distribution of radioactivity with size of particle, and the height and size of the cloud at time of stabilization. The kind of soil taken into the fireball presumably has an effect on the particle size distribution too. In order to make a calculation of where the debris will go, all these factors must be taken into account in one way or another. The various ways of handling this complicated situation are treated in the next section.

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[4] In ref. 2, Appendix E, similar estimates are made which are less than the ones quoted. However, it appears that a different "normalization factor" was used to convert from kt yield to megacurries of fission product activity at one hour, and this was combined with an inappropriate decay rate to convert from the time of observation to the reference time of 1 hour. Further, Tucked introduced a correction for the radioactive sodium from the ocean water which was activated by neutrons from the explosion, and which contributed to the observed radioactivity.

[5] Lulejian, N. M.: Radioactive Fallout from Atomic Bombs, Air Research and Development Command, C3–36417 (with supplement), November 1953 (Secret, R. D.).

[6] Greenfield, S. M., W. W. Kellogg, F. J. Krieger, and R. R. Rapp: Transport and Early Deposition of Radioactive Debris from Atomic Explosions, Report of Project Aureole, Rand Corp., R–265–AEC, July 1954 (Secret, R. D.). See chapter 4.

[7] Cronkite, E. P., V. P. Bond, and C. L. Dunham: Some Effects of Ionizing Radiation on Human Beings, United States Atomic Energy Commission, July 1956.

Before proceeding further it might be well to mention something about what happens to these radioactive particles after they are on the ground. The largest particles involved may be a millimeter or more in diameter, but these constitute only a small fraction of the total debris. Both observation of the particles, collected in many ways in the Pacific and in Nevada, and theoretical calculations of the way in which they must fall indicated that the majority of the particles taking part in the close-in fallout have diameters between about 50 and 400 microns (1 micron is 10,000 cm.).[8 9 10 11]. According to G. R. Hilst, of the Hanford Atomic Products Operation, particles of less and about 50 microns diameter are difficult to erode by wind action because they tend to sift down and cling between the larger particles of the soil, and particles larger than about 500 microns diameter are difficult to erode because the wind cannot easily lift them. The particle size range in which radioactive fallout lies is the size which can be most easily lifted by the wind and redeposited somewhere else. Under high wind conditions this could further complicate the prediction of where the debris would go.

COMPUTING FALLOUT PATTERNS

Clearly, the direction that a particle takes on its way to the ground is determined by the wind. It is not the wind at one level alone which must be considered, but the cumulative effect of all the winds between the ground and the initial altitude of the particle. There have been a number of mehods developed to make some sort of best guess about where the debris will be deposited, and these all have one element in common: The wind field from the ground up to the atomic cloud must be analyzed and integrated.

In order to understand the matter of fallout computation, it is necessary to see what is involved in an integration of the wind field. Figure 1 shows, in schematic form, how such an integration can be done vectorially. Let it be assumed for the moment that a particle starting from 50,000 feet, for example, has a constant rate of fall. In such a case it will spend the same amount of time in each layer of a given thickness, say 5,000 feet. The direction of its travel while in a given layer will be in the direction of the mean wind in that layer, and the distance it travels while in the layer will be proportional to the length of the corresponding wind vector. Then it falls down into the next layer and again travels with the mean wind in that layer. In order to determine the total distance and direction which this particle traveled on the way to the ground it is only necessary to add the successive wind vectors for each layer head to tail, and the resultant vector will represent the total travel.

In practice, meteorologists have found it convenient to add the vectors starting from the ground and working upward, as shown in figure 1b. Now the integrated wind, or total particle travel, from any given altitude can be immediately determined by drawing a vector from the origin to the head of the arrow corresponding to the correct altitude. In other words, a family of integrated winds can be produced in this way, and the direction and rate of travel of all particles can be estimated by inspection of the diagram. Recall that it was assumed here that the particles fell at a constant rate. This is not the case in actuality, and so the simple vector addition described here must be modified in the more sophisticated analyses of fallout.

There have been four main approaches to the construction of a fallout analysis, depending on the amount of time available for the computation and the degree of completeness required. It should be emphasized that these various approaches do not compete with each other, since they are each tailored to answer a different set of questions about the fallout, and they differ greatly in the amount of labor required to carry them out. In order of increasing complexity, they are—

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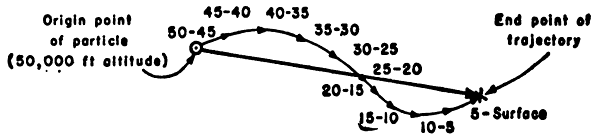
[8] Rainey, C. T., J. W. Neel, H. M. Mork, and Kermit H. Larson: Distribution and Characteristics of Fall-Out at Distances Greater than 10 Miles from Ground Zero, March and April 1953, U. C. L. A. School of Medicine, Operation Upshot-Knothole, WT-811, February 1954 (Secret, R. D.).

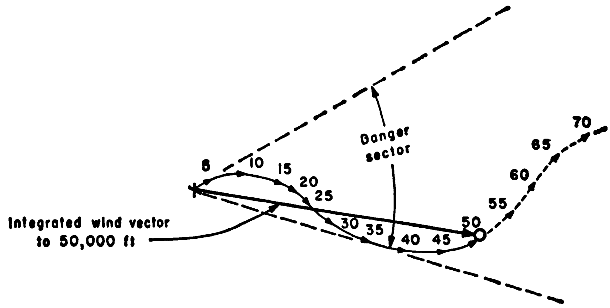
[9] Heidt, W. B., Jr., E. A. Schuert, W. W. Perkins, and R. L. Stetson: Nature, Intensity, and Distribution of Fallout from Mike Shot, U. S. Naval Radiological Defense Lab., Operation Ivy, WT-615, November 1952 (Secret, R. D.).

[10] Stetson, R. L., E. A. Schuert, W. W. Perkins, T. H. Shirasawa, and H. K. Chan: Distribution and Intensity of Fallout, U. S. Naval Radiological Defense Lab., Opertaion Castle, WT-915, January 1956 (Secret, R. D.).

[11] Wilsey, E. F., R. J. French, and H. I. West, Jr.: Fallout Studies, Army Chemical Center, Operation Castle, WT-916, February 1956 (Secret, R. D.).



(a) Actual particle trajectory



(b) Usual method of plotting and integrating the wind field

FIGURE 1.—Schematic representation of a wind field and the analysis of a falling particle's trajectory.

1. *The danger sector.*—An inspection of the integrated wind plot shown in figure 1b shows that all the particles starting in a vertical line over the origin would travel within the sector indicated by the dashed lines. This can be called the danger sector, since it is the sector within which the debris will fall, more or less, assuming a perfectly constant wind field. Certain refinements can be made to the simple sector presentation with little effort, such as delineation of the times at which the particles starting over ground zero will reach a given point on the ground; and the finite initial size of the atomic cloud can be taken into account graphically. This approach has been described in detail in several readily available reports.[12 13 14] Since it is quick and convenient, it is the method which has been recommended by the Weather Bureau, the Air Weather Service, and the FCDA for use in weather stations in an emergency. In order to further expedite the computation, the Weather Bureau has recently instituted the inclusion of the integrated winds from each upper wind station in the routine teletype message. These go by the code name of "UF winds," and are available from about 70 weather stations within the United States twice daily. Note, however, that the danger sector method does not provide a way for telling the actual levels of activity and does not distinguish the parts of the sector which are more intensely contaminated, though there are some methods for roughly estimating where these will be.

2. *The idealized pattern.*—Several of the earlier workers in the field of fallout noted the fact that the majority of the patterns (in Nevada) had a characteristic cigar shape.[5 15] It was therefore tempting to attempt to characterize fallout patterns in general in terms of a family of simple elliptical shapes, with a circular

[12] Air Weather Service Manual 105–33, Radioactivity Fall-Out and Radex Plots, Hqs., Air Weather Service, June 2, 1952.

[13] Construction of Fallout Plots from Coded Messages Provided by the U. S. Weather Bureau," Federal Civil Defense Administration, Battle Creek, Advisory Bulletin No. 188, May 25, 1955 (and supplements).

[14] Training Manual for Computing and Coding Civil Defense Fallout Winds, U. S. Weather Bureau, Washington, April 1955.

[15] Laurino, R. K., and I. G. Poppoff: Contamination Patterns at Operation Jangle, U. S. Naval Radiological Defense Laboratory, Rept. 399, April 1953 (Secret, R. D.).

section around ground zero. The Armed Forces special weapons project (AFSWP) and others have, over a period of years, developed rather elaborate sets of scaling and shaping laws, designed to fit these idealized patterns to a wide range of yields and, to a limited extent, wind conditions. These methods are described in detail elsewhere.[16] A recent report of the Air Force Special Weapons Center by Boyd and Baker has summarized and made comparisons of the various methods.[17] They all have the common characteristic that the only input required is the weapon yield (a surface burst is assumed) and some sort of an integrated wind, sometimes called the effective wind. For certain planning purposes these idealized fallout patterns are quite useful, since they give a good idea of the area covered by a given dose contour, and for a simple wind structure the orientation and shape may be quite representative. However, as our experience with actual fallout patterns grows, it becomes clear that the simple wind structure required to lay down a symmetrical pattern like the idealized ones is not necessarily the expected one, particularly in the Tropics or in Nevada in summer. Therefore, for prediction purposes such a method may be of little value; moreover, the way in which it is presented gives an erroneous impression of the accuracy of the plot, since the dose rate contours are actually specified.

3. *Analog method.*—A very common technique in weather forecasting, one which all meteorologists use either subconsciously or consciously, is the use of analogs. Essentially, this means a sorting over of cases which have occurred in the past to find a situation analogous to the current situation, and presuming that the same processes will follow the same course again. There have not been enough surface bursts to build up a good file of analogs, but an artificial set can be calculated, using the sort of detailed calculations to be described in the next section. Such a "catalog" of fallout patterns has already been produced by the Rand Corp.[18] To make use of this collection of analogs, the meteorologist must find a wind field in the catalog which by proper manipulation can be more or less matched to the current wind field, and he can then take advantage of the fact that the resulting fallout pattern has already been computed in great detail. If the yield does not match, then certain scaling laws can be applied to the analog to make it the correct size. Naturally, the same wind field never occurs exactly the same way twice, but the matching can be done quite successfully over a wide range of conditions and yields.

4. *Fallout models.*—In attempts to describe as closely as possible what actually happens in the fallout process, several agencies have developed techniques in which the particles in the initial atomic cloud are traced down to the ground, and in which their combined effect is then calculated for each point in the fallout field. The result is a polt of the expected dose rate at any given point for a given time. In order to perform such an elaborate computation the following factors must all be considered:

Wind field—in some of the computations it is not only possible to consider the variation with height, but the variation with time and space. Under certain conditions, as will be shown, such variations are quite important.

Initial distribution of particles in space—although the size and shape of the atomic cloud can be observed photographically, the distribution of the radioactivity inside the cloud is not well known. Assumptions about this vary from model to model.

Size distribution of the particles—since the larger particles will in general fall faster than the smaller ones, it is necessary to specify how much of the total radioactivity is associated with each range of particle size. Furthermore, the size distribution probably differs in different parts of the cloud, a feature which some of the models attempt to take into account.

Rate of fall—the rate of fall of a particle depends on its size, density, and shape. Thus, the rates of fall of a given size particle at each altitude must be specified in each model.

Turbulent diffusion—in at least one of the models which has been tried the spread of the trajectories due to random turbulence has been taken into

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[16] Capabilities of Atomic Weapons, Armed Forces Special Weapons Project. TM 23–200/OPNAV Instruction 003400. IA/AFL 136–4/NAVMC 1104, Washington, 1955 (Secret, R. D.). (See sec. 13.)

[17] Boyd, R. E., and D. Baker: Comparison of Methods Used in Scaling Residual Contamination Pattern Resulting from Surface Detonations of Nuclear Weapons, Hqs., Air Force Special Weapons Center. Kirtland AFB. AFSWC–TN–56–1. April 1956 (Secret, R. D.).

[18] S. M. Greenfield, R. R. Rapp, and P. A. Walters: A Catalog of Fallout Patterns, Rand Corp., Rept. RM–1676, April 1956.

account. However, most of the models choose to neglect this effect, since it does not appear to be very important for the early deposition.

Each of the computational models must specify all of the above factors, and there have been some rather large differences between the assumptions, due to our lack of very definite information about the true facts of the matter. In addition, different computational techniques are used to analze the model, some using high speed digital computers, some using special electronic or optical analog computers, and some using a graphical "hand" computation.

In January 1955, the Armed Forces Special Weapons Project (AFSWP) organized a symposium on radioactive fallout, and all the various agencies which had studies the question of fallout were invited to apply their respective fallout models to two specified sets of wind conditions, known as condition A and condition B. The results, as published in the AFSWP report on the symposium [19] are shown in figures 2 and 3. The winds used are tabulated in table 1 and table 2. For details of the actual computational schemes used, one should refer to the fallout symposium report [19] or to the reports of the various agencies, some of which have become unclassified.

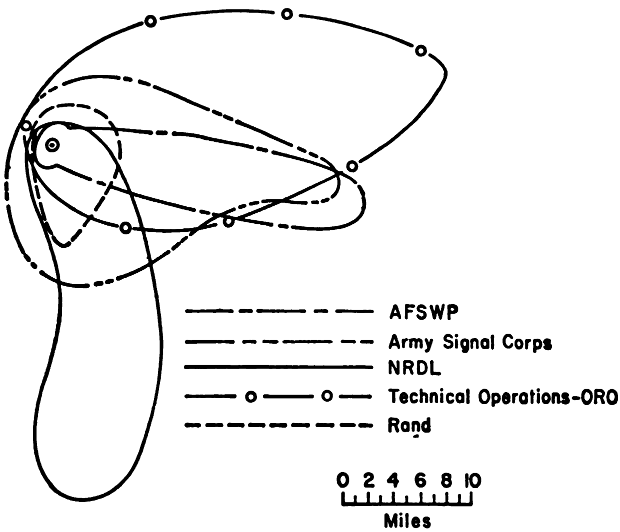

Figure 2.—AFSWP comparison of fallout computations (Ref. 18). Cases for "Condition A," 1 megaton yield, showing contours for 1,500 r dose accumulated by 48 hours.

Table 1.—*Condition A—Wintertime situation of an abrupt, approximately 90°, shear at a height of approximately 40,000 feet*

[Dodge City, Kans.—37°46′ N. 99°58′ W.—1500 Greenwich meantime—Dec. 28, 1953—Elevation: 2,625 feet]

Height (feet, mean sea level)	Wind direction (degrees)	Wind speed (knots)	Height (feet, mean sea level)	Wind direction (degrees)	Wind speed (knots)
Surface	260	7	45,000	255	43
5,000	247	12	50,000	255	55
10,000	273	19	55,000	260	47
15,000	307	13	60,000	272	54
20,000	008	16	65,000	289	40
25,000	045	41	70,000	285	36
30,000	036	52	75,000	285	38
35,000	357	29	80,000	285	45
40,000	243	47			

[19] Fallout Symposium, Armed Forces Special Weapons Project Report 895, January 1955 (secret, R. D.).

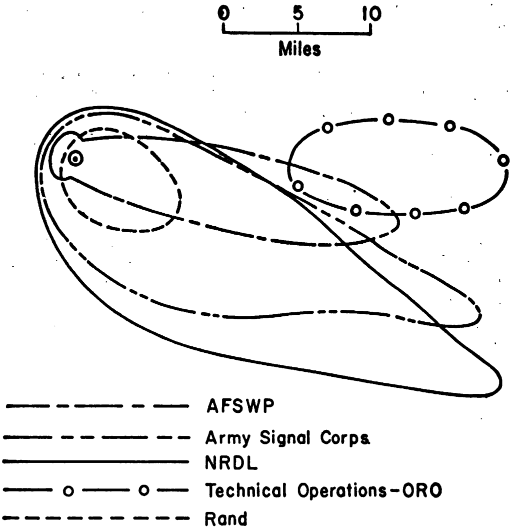

FIGURE 3.—AFSWP comparison of fallout computations (Ref. 18) cases for "Condition B," 1 megaton yield, showing contours for 1,500 r dose accumulated by 48 hours.

TABLE 2.—*Condition B—Gradual shear of approximately 90°*

[Washington, D. C. (Siver Hill)—38°50′ N., 76°57′W.—0300 Grenwich mean time—Sept. 28, 1952—Elevation: 289 feet]

Height (feet, mean sea level)	Wind direction (degrees)	Wind speed (knots)	Height (feet, mean sea level)	Wind direction (degrees)	Wind speed (knots)
Surface	Calm	0	45,000	292	23
5,000	358	11	50,000	290	28
10,000	009	20	55,000	290	20
15,000	325	14	60,000	268	11
20,000	282	19	65,000	276	21
25,000	263	34	70,000	293	7
30,000	263	47	75,000	293	8
35,000	273	37	80,000	285	10
40,000	308	27	85,000	250	11

The significant thing to note is the discouragingly poor agreement between the various results. It is possible that some of the agencies have modified their models in the past 2 years, and that there would be better agreement if the exercise were repeated now, but it is highly unlikely that the agreement would be anywhere nearly exact. It would seem that we simply do not know enough yet about the process of fallout to be able to reconstruct a fallout model (no matter how sophisticated in conception) on which everyone would agree.

PREDICTION AND RECONSTRUCTION OF FALLOUT PATTERNS

As stated in the previous section, there have been a number of methods develeped for the computation of fallout patterns. Naturally, these were developed with the observed fallout from a handful of surface and tower bursts in hand, and all claim (to a greater or lesser degree) to give results which agree with reality.

The real question of agreement with reality is, however, obscured by the fact that reality is hard to define, even in retrospect, when all the facts are collected. First, the wind field is poorly observed, and the variations in the wind field with time and space are difficult to take into account in reconstructing what

happened. The meteorological literature has a number of studies of this variability and of the uncertainties in observation.[20 21 22] A good rule of thumb, derived from experience with the tracking of constant-level balloons, is that, over a good upper air network of the sort which covers the United States, the path of a particle cannot be determined from an analysis of the wind field to better than 20 percent of the length of the trajectory. Thus, after going 100 miles, the uncertainty in the position of a drifting particle is about 20 miles, even when we have all the upper-air data which we can lay our hands on.

Furthermore, the fallout itself is poorly observed, due to the great distances that have to be covered, the irregularities of the terrain (in Nevada) or the uncertainty of where it went after landing in the ocean (in the Pacific). Thus, even if our computation were, in principle, a perfect one, we would still not have a clear picture against which to compare it.

When the meteorologist is faced with the problem of *predicting* a fallout pattern, the uncertainties of a wind prediction are added to the uncertainties in the computational model. The longer the time lag between prediction and the event, the greater will be the uncertainties.[23] For times of up to 12 hours, it appears that persistence is about as good as a forecast, and after about 2 to 3 days a forecast is not much better than a climatological mean.

Without belaboring this point, it should suffice to show two interesting examples of predicted and reconstructed fallout patterns. One is from a burst of roughly 30 kilotons on a tower in Nevada, the Open or Civil Defense shot of May 5, 1955. The patterns shown in figures 4 and 5 were prepared by Kenneth Nagler, of the United States Weather Bureau, and show the patterns which were predicted 2 hours before shot time by 2 methods of models. The two models, one of the Weather Bureau and the other of the Los Alamos Scientific Laboratory and the University of California Radiation Laboratory, were used. The first involved a hand computation by an elaborate graphical analysis, the other involved a high speed digital computer (IBM–701). There were some differences in the two models, but these were not basic ones—that is, they both used the general approach described in the previous section. It will be noted that both methods predicted patterns extending due north from the shot point, following the direction of the H–2 hour predicted wind. The observed pattern, shown in figure 6, was reconstructed from the available road monitoring and from a few aircraft measurements by Nagler. The fallout started out northward, and then curved to the eastward, reflecting a gradual shift in the wind direction from south to west that took place in the hours following the shot. Also shown in figure 6 is an attempt to reconstruct the pattern, using the Weather Bureau's model and taking into account the change of wind with time and space. The result agrees with the obesrved pattern better, but still not perfectly.

Another example of a fallout pattern which changed its direction during the later stages of the fallout is the March 1, 1954, Castle shot on the Bikini atoll, referred to earlier. In this case, the fallout apparently started out in a direction east-northeast, but a continued veering of the wind caused it to curve more to the east and east-southeast, until one side of it lay across some neighboring atolls. A study of this event by Rand in which the fallout was computed with the shot-time wind alone, and then again with the variable (true) wind, shows clearly how the pattern must have curved as it progressed.[24]

It is interesting to note that both of these examples demonstrate the effect of the changing wind with time, an effect which is often very hard for the meterologist to specify. A study of the statistics of this change of wind with time has been made by Frank Cuff, department of meteorology, University of Utah.[25] Referring to the integrated wind (see above) from the ground up to various altitudes in Nevada, he found the mean absolute bearing changes shown in table 3.

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[20] Neiburger, N., L. Sherman, W. W. Kellogg, and A. F. Gustafson: On the Computation of Wind from Pressure Data, Jour. Met., vol. 5, No. 3, pp. 87–92, 1948.

[21] Rapp, R. R.: The Effect of Variability and Instrumental Error on Measurements in the Free Atmosphere, New York University Meteorological Papers, vol. 2, No. 1, June 1952.

[22] Kochanski, A. B.: Wind, Temperature, and Their Variabilities to 120,000 Feet, Air Weather Service Technical Report, 105–142, May 1956.

[23] Ellsaesser, H. W.: Errors in Upper-Level Wind Forecasts, Air Weather Service Technical Report, 105–140/1, December 1956.

[24] Greenfield, S. M., and R. R. Rapp: Fallout Computations and Castle-Bravo—A Case Study, Rand Corp., RM–1855, January 1957 (secret, R. D.).

[25] Cuff, R. D.: A Study of the Time Variability of Integrated Winds Near Las Vegas, Nevada, thesis for M. S. Degree, Dept. of Meteorology, Univ. of Utah, March 1957.

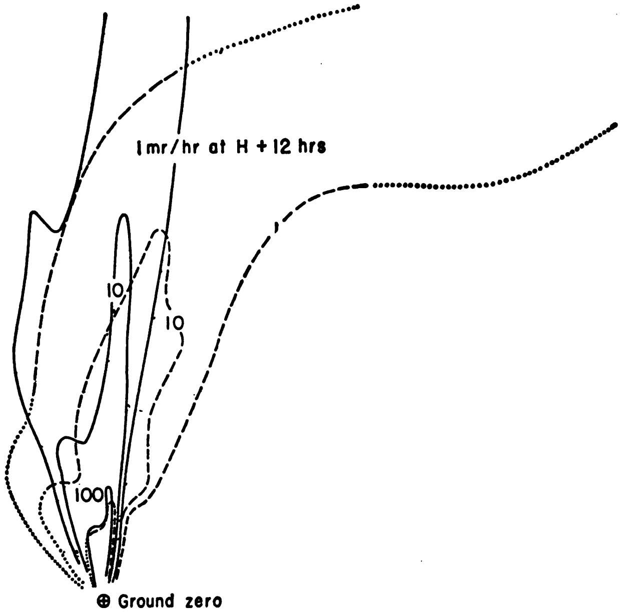

FIGURE 4.—The observed fallout distribution (dashed lines) and the pattern computed by the Weather Bureau using winds predicted at H–2 hours. May 5, 1955.

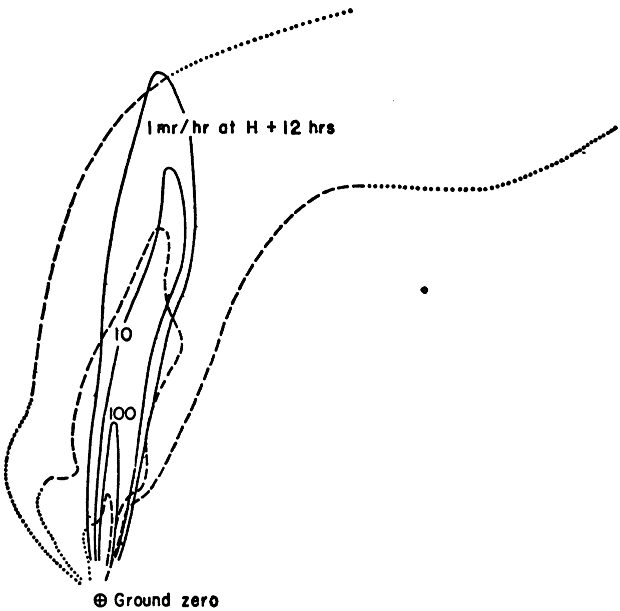

FIGURE 5.—The observed fallout distribution (dashed lines) and the pattern computed by LASL–UCRL using winds predicted at H–2 hours. May 5, 1955.

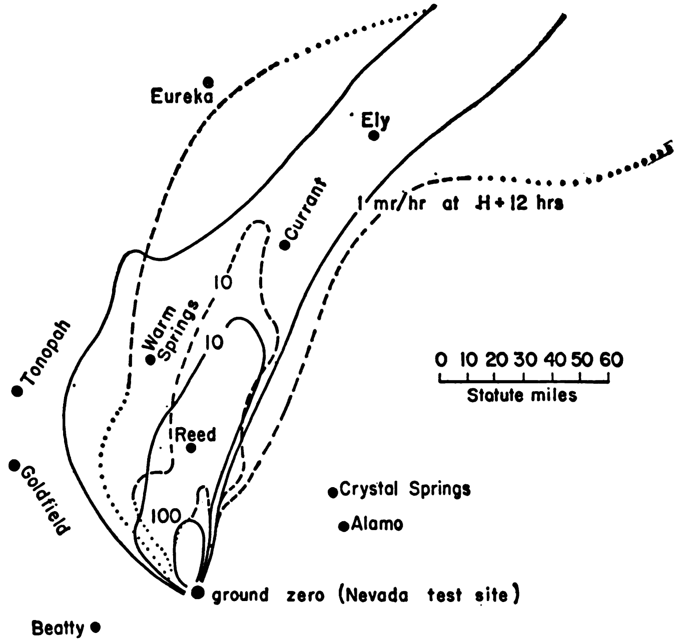

FIGURE 6.—The observed fallout distribution (dashed lines) and the pattern reconstructed by the Weather Bureau using a hand computation with time and space variation of winds (solid lines). May 5, 1955.

TABLE 3.—*Mean absolute bearing change of integrated winds*

Time interval (hours)	Integrated wind from surface to—		
	20,000 feet	40,000 feet	50,000 feet
3	12°	7°	6°
6	22°	15°	13°
12	33°	28°	

It will be noted that the bigger the thickness of the atmosphere considered in forming the integrated wind the smaller is the shift of the wind. This probably reflects the fact that wind shifts at one level may sometimes be partially canceled by opposite wind changes at another altitude. Another lesson to be learned from this study is that the statistics of the wind at one level cannot be relied upon to give reliable information about the statistics of the integrated wind, which must combine the effects at many levels.

A recent study of the predictability of fallout for the Nevada test site has been made by Jack Reed of the Sandia Corp.[26] Here the variability of the wind, the forecasting accuracy, the length of the forecast period, etc., are all considered in order to given an estimate of the degree of confidence with which the fallout can be put into an uninhabited "safe sector." This approach to the problem is one which should be taken more often in meteorology, since it demonstrates that any weather forecast should have a prabability assigned to it—a probability which is always less than one.

THE DYNAMICS OF FALLOUT

So far a great deal has been said about the final fallout pattern and how it is computed. A very important feature of the pattern from a practical standpoint

[26] Reed, J. W.: Estimating Safety Probabilities From Fallout Forecasts for Nevada Test Site, Sandi Corp. report SC–4073 (TR), February 1957.

is the *time* at which the fallout reaches various parts of the pattern. Clearly, the fallout cannot all occur at once, since it takes some time for the particles to reach the ground, and while they are falling they are carried with the wind. Thus, the fallout around ground zero starts very quickly, whereas the fallout miles away may not start for hours. (For example, the island of Rongelap did not receive its fallout until some 4 to 6 hours after shot time.[7])

Recall that, for a surface burst of more than a few kilotons yield, most of the radioactive debris is in the mushroom cloud. When the yield is several megatons, this mushroom cloud rises into the stratosphere,[1] and so even the relatively infrequent larger particles, of 1,000 microns diameter and over, take from 30 to 40 minutes to fall back to the ground. It appears that there are some few radioactive particles which escape from the mushroom while it is rising and are left behind in the stem cloud, and these will, of course, find their way to the ground sooner, in the downwind direction.

In order to demonstrate the time of arrival of radioactivity at points relatively close to ground zero, the Naval Radiological Defense Laboratory[9,10] and the Army Chemical Corps[11] have designed equipment which records the fallout as a function of time. Though their respective instruments were designed independently, they both work on essentially the same principle: A small tray or container is uncovered for a certain period of time (say, 5 minutes), then covered again. Automatically the next sampler is uncovered for its sampling period, and so on. It should be mentioned that both sets of instruments remained closed for the first minute after shot time, to allow the shock wave to pass the sampling station.

A large number of such fallout versus time measurements were made at the time of the Castle shot 1, and a few had been made earlier at the Ivy Mike test by NRDL. When all the results using 5-minute sampling times (20 cases) are plotted up one is impressed, first of all, at the erratic nature of the results. This is probably due to the fact that the samples are made with small areas and small time intervals, and therefore do not give results which are entirely representative of the fallout at that location.[37]

The next thing which one notices about the results is that the majority of them show *no fallout for the first 30 minutes;* the average time of arrival for all stations which received any fallout was 28 minutes. These stations were located at distances from ground zero ranging from 8 to 30 miles. In visualizing these distances, recall that the Ivy Mike cloud had a radius of about 5 minutes of 10 miles, and at 10 minutes it was nearly twice this. For the Castle shot 1 the radius at 10 minutes was about 30 miles, and still growing. Thus, all the stations represented were literally in the shadow of the great mushroom cloud—though none were in the initial part of the stem.

The few stations which did apparently receive fallout earlier may have had something wrong with their mechanism (as would appear to be the case where two nearby stations give completely opposite results), or they were in a direction from ground zero which allowed them to be dusted by the material from the crater area which was born by the low level winds. This latter explanation appears to be reasonable, since we know that a certain small fraction of the radioactivity produced does reside in the stem cloud at relatively low altitudes.

It is therefore tempting to visualize the fallout as a slowly descending blanket, with a diameter roughly the diameter of the mushroom cloud. The blanket starts its fall as soon as the atomic cloud stabilizes (about 4 to 6 minutes after burst time) and touches the ground over a large area simultaneously. While this mushroom material undoubtedly represents the major fallout, some material from the stem may reach the ground sooner, and the direction of this immediate fallout from the stem would be determined by the mean wind in the lower levels, below, say, 20,000 feet.

Following this early arrival of the radioactive debris the fallout pattern is laid out in a more or less orderly way and spreads in the direction of the integrated winds. To illustrate how the pattern grows with time, figures 7 and 8 show the growth of a hypothetical 1 megaton pattern under 2 very different wind conditions. One shows how it grows under a condition where the winds are moderately strong and all in the same general direction. The other shows how one grows under a low wind condition. In the first case the debris is spread rapidly in a ribbon across the country. In the second case the debris continues to fall in the vicinity of ground zero for many hours. Neither of these wind conditions is particularly unusual, and there are naturally an infinte number of possible intermediate cases.

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[37] See ref. 10.

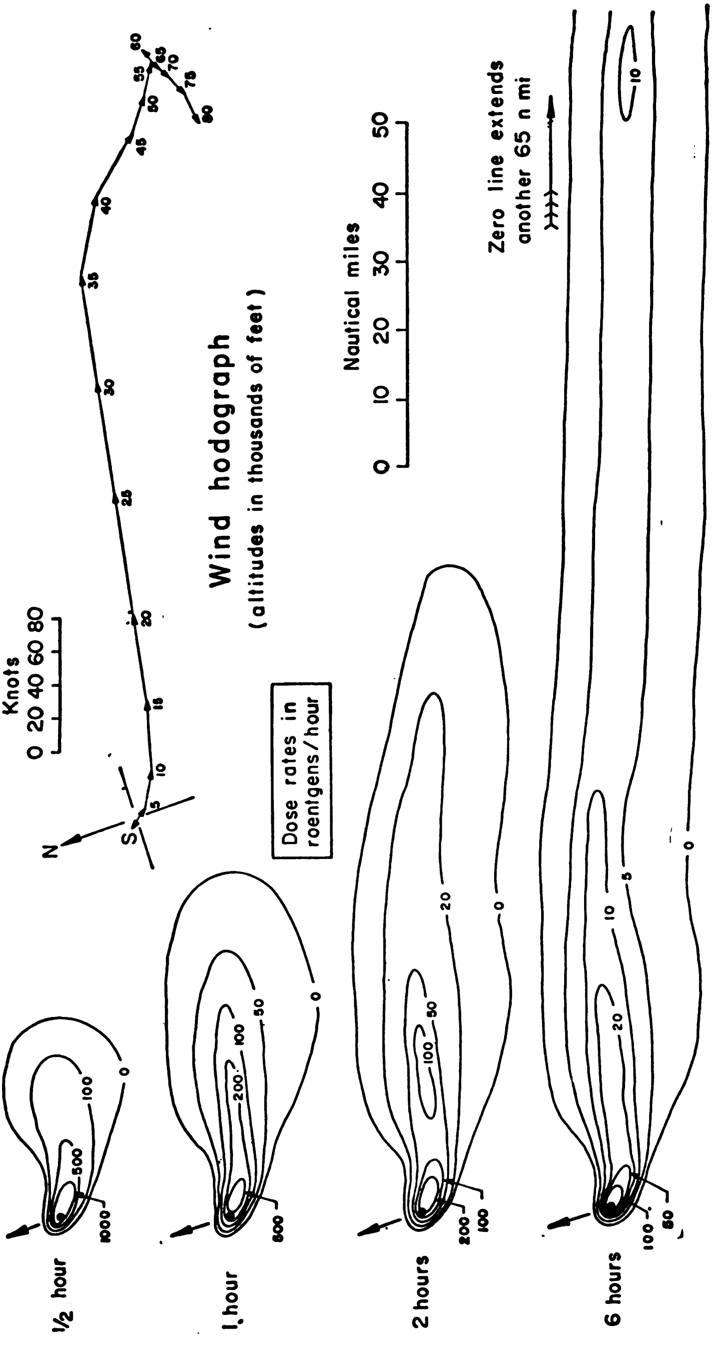

FIGURE 7.—Calculated fallout from a 1 MT surface burst with a two-thirds fission yield under a "high wind" condition. Winds are those for San Francisco on June 15, 1954.

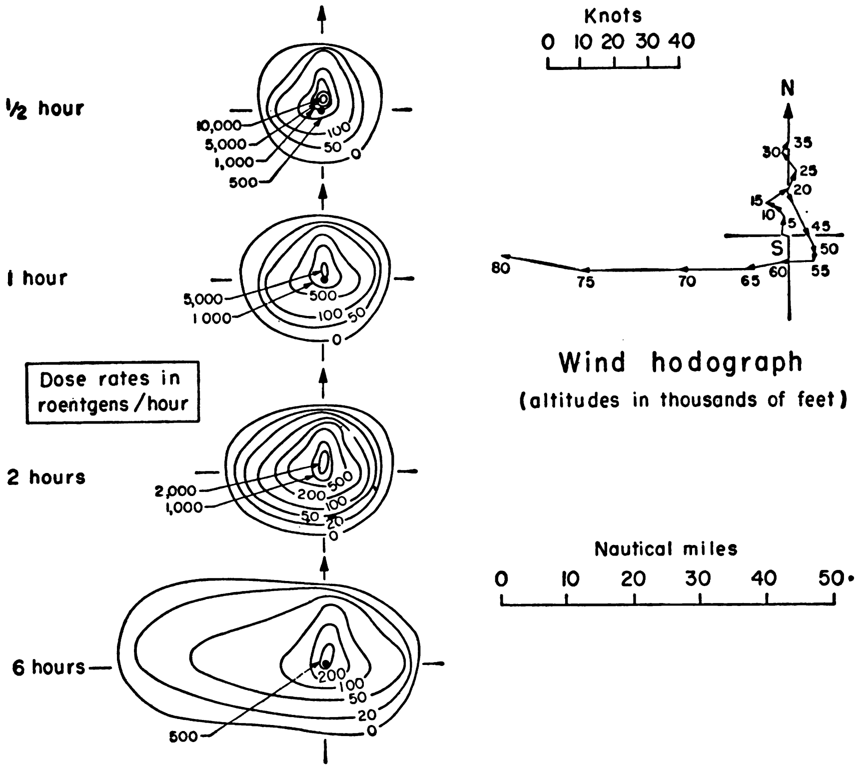

FIGURE 8.—Calculated fallout from a 1 MT surface burst with a two-thirds fission yield under a "low wind" condition. Winds are those for Atlanta on June 15, 1954.

FALLOUT FROM A BOMBING CAMPAIGN

No discussion of fallout would be complete without some discussion of the results of a bombing campaign, in which many bombs are set off against a target system with a large number of widely dispersed aiming points. Such a target system might be, for example, the industrial complex of the United States, or its system of airbases, providing targets which are located in a more or less random manner over the entire country.

The natural laws governing the fallout from such a campaign are the same as those governing the fallout from one burst. The difference lies in the fact that now the fallout patterns overlap in places and reinforce each other. Furthermore, where the ground zeros are fairly close to each other the fallout is more or less independent of the wind direction, since it makes little or no difference which bomb causes fallout on a given spot—such an area is "blanketed."

A number of studies have been made of such campaigns,[28 29] and a technique has been developed by Greenfield for estimating on a probabalistic basis the results of fallout from multiple-bombs dropped randomly in a large area.[30] One such study, in which the hypothetical fallout was computed for an attack on the United States under a rather typical meteorological situation, was performed by Charles K. Shafer, headquarters, Federal Civil Defense Administration, Battle Creek. It was done in connection with the FCDA's Operation Sentinel.

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[28] Davidson, H. D., J. B. Green, J. B. Phelps, C. D. Stolzenbach: Fallout as a Threat from Attack by Manned Bombers, Operations Research Office, Johns Hopkins University, ORO–R–17, appendix C, September 1956 (secret).

[29] Rapp, R. R.: Fallout Computations for Operational Studies, Rand Corp., RM–1753, July 1956 (secret, R. D.).

[30] Greenfield, S. M.: Radioactive Contamination from a Multibomb Campaign, Rand Corp., RM–1607, January 1956 (secret, R. D.).

Though this represents just one particular combination of events, it is instructive to see what would have happened under this hypothetical attack, according to Shafer.

In this exercise about 250 nuclear (or thermonuclear) weapons with "damage zones" ranging from 3 to 5 miles were dropped on cities, industrial targets, and airfields through the United States. The combined fallout pattern from all these bombs is shown in figure 9. The details are contained in an unpublished report by the FCDA, and the following are some of the general conclusions which were drawn with regard to the effect of such an attack on the United States population:

	Dead	Injured	Uninjured
1st day	36,000,000	57,000,000	58,000,000
7th day	51,000,000	42,000,000	58,000,000
14th day	61,000,000	31,000,000	58,000,000
60th day	72,000,000	21,000,000	58,000,000

These numbers are based on 1950 population figures. Those dead on the first day were presumably killed by the immediate effects of the bombs, i. e., mostly blast and thermal effects. The subsequent rise in fatalities reflects the delayed effects of radiation damage, coupled in many cases to external injuries. While one should not take these actual numbers too literally, their orders of magnitude and the trends shown here are fairly realistic. In particular, the indication that fallout might account for a large number of deaths—nearly as many died by the immediate effects—is pertinent. In actuality, many of the "uninjured" ones would be caught by the fallout as they tried to move about. Clearly, however, such figures can only be illustrative, since the behavior patterns of the population would have a tremendous effect on the casualties due to radiation. While the meterorologist can predict to some extent the fallout patterns, he can hardly be expected to predict whether or not the population will be trained and provided with adequate shelters before such an attack.

Dr. KELLOGG. I am with the Rand Corp. at Santa Monica, Calif., at the present time. I am head of what we call the geophysics group. The geophysics group is composed to a large extent of meteorologists and atmospheric physicists, and we have been interested in studying the subject of radioactive fallout for a number of years.

I feel somewhat inadequate for the job of presenting all the material which I want to present today, partly because it is a complicated question, and partly because I feel that there is still some difference of opinion among meterologists on the details of this question of close-in fallout. I hope that I can not only reflect sort of a consensus of opinion, and my own opinion, but also indicate where we feel that we need to have more information on the subject of close-in fallout.

Before I start, I would like to make three rather general statements which in a sense are threads of the whole thing which I am presenting.

The first is one which I think you have already sensed from the testimony of Dr. Graves, to the effect that radioactive fallout is a major effect from an atomic explosion. I therefore feel that we should make all the pertinent facts available to the public and to the military on this question because one can't consider an atomic explosion and its effects without considering radioactive fallout.

The second point I would like to make very briefly is this.

Chairman DURHAM. May I ask a question at that point? Has your company been doing this all the time—making the information available to the public and also to the military?

Dr. KELLOGG. We have tried to very hard. In the January issue of the Journal of Meterology, for example, the work on close-in fallout

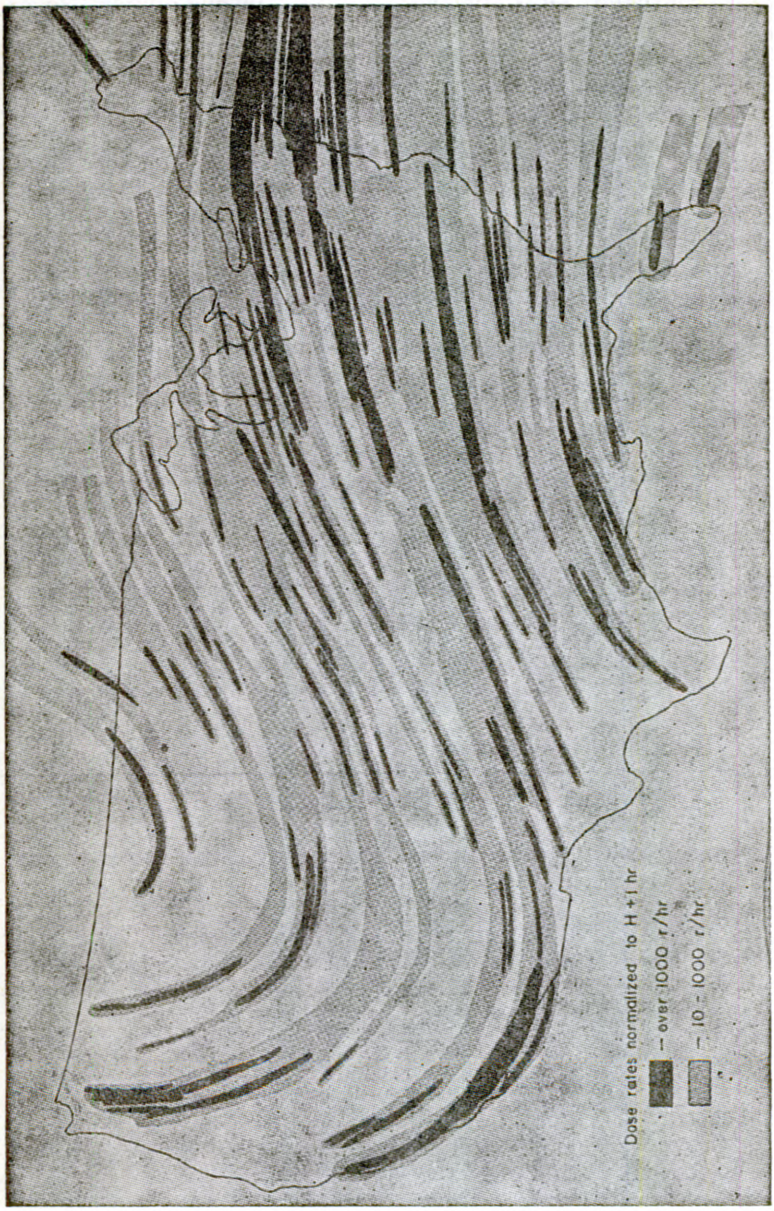

FIGURE 9.—Fallout condition computed by the FCDA for Operation Sentinel.

which our group has done was summarized. It is an article which appeared in what you might call the official United States meteorological journal by Dr. Rapp and Mr. Greenfield and myself. There have been other outputs from our group that have to do with other facets of fallout which we have published as fast as we could get them into the open. We have made an effort in this direction.

The second point I was going to make has to do with feeling that I think every meteorologist would share with me, that we are often charged with an impossible job of predicting exactly how the atmosphere will behave over a long period of time in the future. I think one of the points to be made is that there is always an element of uncertainty in a meteorological forecast. This should be kept in mind when we speak of the prediction of fallout patterns.

The third point that I think should be made in connection with close-in fallout is that when we speak of close-in fallout, as Dr. Graves pointed out, we must try to figure out how much of this material does come down in close-in fallout, because this determines what is left over to go worldwide. So I will spend a little bit of time defining what we know about the fraction which falls out close in, because this will have bearing on the worldwide problem, too.

Representative HOLIFIELD. Your company has made a study of this under a contract with the AEC or with the Defense Department?

Dr. KELLOGG. In 1953, our company accepted a contract with the AEC to study various aspects of fallout. This ran for 3 years. I believe it was 3 or 3½ years. We no longer operate under this contract. Our work is now primarily for the Air Force. Under our prime contract with the Air Force, we are continuing our work on this matter.

Representative HOLIFIELD. Your reports have been made to the Atomic Energy Commission for the 3 years, and you are now making reports to the Air Force; is that right?

Dr. KELLOGG. That is correct; yes. Most of our work is now for the Air Force, but we maintain close liaison with the AEC in our work.

Representative HOLIFIELD. Are you under any directions in regard to security in testifying before us today on this matter?

Dr. KELLOGG. No. I am happy to say that although I wrote the testimony which is here, and submitted it to both AEC and the Department of Defense, it has been cleared. Essentially everything I wanted to say they have allowed me to say.

Representative HOLIFIELD. Does your testimony bring up to date your findings?

Dr. KELLOGG. Yes.

Representative HOLIFIELD. You may proceed.

Dr. KELOGG. Dr. Graves pointed out that there is a big difference between an air burst and a surface burst and the fraction that falls out. Since he has covered this so ably, I will not go over this in detail. You understand about this business of the surface material being mixed in the fireball when the detonation is on the surface.

In the case of a tower shot, which is sort of an in-between case, the fraction which comes down is variable, in the complete report, in order to demonstrate how variable this is, I have taken some numbers which were given to me by Mr. Nagler, of the Weather Bureau, who has made a very careful analysis of the fraction which fell out from

five detonations, all of about the same yield. All had about 15 kilotons yield, and all were on the same height of tower.

The interesting thing was that they varied over a rather wide range in the fraction which came down. This, I think, is probably due to the fact that the meterological conditions and the way in which the tower was made, the amount of material around the test device, and so forth, all varied.

Representative HOLIFIELD. Do you mean by that that the fallout was uneven and in some places there was a heavier dose than others?

Dr. KELLOGG. This is true, of course. It is not just laid down uniformly. The thing which I was speaking of at the moment was the total fraction which comes down in the first 24 hours in the part of the country which is carefully monitored so we can keep track of it, in other words.

Representative HOLIFIELD. There was a great variation in the reading of your instruments?

Dr. KELLOGG. When all the information from all the instruments was in and analyzed, and the Weather Bureau or the Health and Safety Division was able to analyze this, they were able, as it were, to count up all the radioactivity over the entire area, and get a total budget. They knew how much went into the atmosphere. They were able to see how much came down. They could say that this was some fraction of the amount produced.

Representative HOLIFIELD. But that was in the nature of a fraction of the total fallout, and it was not a measurement of the degree of the dose in that area, or was it both?

Dr. KELLOGG. It was both. In order to determine the fraction which fell out, the thing which is measured is the dose.

Representative HOLIFIELD. What was your variance between the high dose and the low dose?

Dr. KELLOGG. Later on I can show you a chart—I am glad I came prepared with a chart—showing an example of just how it does vary in the test area.

Representative VAN ZANDT. Doctor, we have been talking about the fallout coming down and going up. For a kiloton yield, how many pounds of radioactive debris does it lift into the stratosphere?

Dr. KELLOGG. I don't know the number. You mean for a surface burst.

Representative VAN ZANDT. Yes; for a surface burst.

Dr. KELLOGG. I don't know exactly. I think Dr. Graves mentioned something about a ton per kiloton or something like that.

Representative VAN ZANDT. Dr. Graves said the figure he used would be debatable. The reason I ask the question is that Dr. Libby some months ago made a statement in which he said that for every 20,000 tons of TNT yield 2 pounds of radioactive fallout is lifted into the heavens. Then he went on to explain that within a matter of weeks most of it will have fallen out. Then he talked about the megaton yield, and how it was lifted into the stratosphere, and that it may remain there from a few seconds to 10 years. Would you concur in such a statement?

Dr. KELLOGG. These numbers, I think, are a little confusing, about the 2½ pounds per kiloton. I don't know what he was referring to Perhaps he meant radioactive debris.

Representative VAN ZANDT. That is correct.

Dr. KELLOGG. The important thing seems to be that the fraction of the total debris which is produced does not depend on the yield as much as it depends on the height of the burst. What I mean to say is this. We have a case in Nevada of a surface shot for which we could measure the dose around the countryside and make an estimate of the fraction of that low-yield device in Nevada which came down. The various estimates are produced here. It looked as though something like 80 or 85 percent of the material from that low-yield surface burst came down somewhere in the first 24 hours. Then in the Pacific, although it has been very hard until recently to estimate what this fraction was, during the last test a system for monitoring the oceans has been developed and by an analysis of this ocean monitoring again we are able to make a rough estimate of the fraction which comes down in 24 hours.

Again, although the estimates vary, a good estimate seems to be around 80 or 85 percent for surface bursts. This is over a very wide range of yields.

Representative COLE. When you speak of a surface burst, do you include a tower test?

Dr. KELLOGG. No; I do not. A tower seems to produce less fallout fractionwise. An air burst produces virtually no close-in fallout. Mind you, my subject is close-in fallout, so I will stick to this amount that comes down in the first 24 hours.

Representative HOLIFIELD. You may proceed.

Dr. KELLOGG. The meteorologists who are concerned with a study of fallout are naturally interested in how to keep track of the debris. I don't propose to give a lesson on how to compute fallout patterns. I think, though, that it would be constructive for the committee to know that these are four main schools of thought on how to predict or reconstruct fallout patterns.

The four main schools of thought—and I will show a chart in just a moment—all require one input, and that is the wind information. In order to tell the direction the fallout goes, the wind must be observed, and if it is a prediction, the wind must be predicted. This is an essential ingredient to any fallout calculation, obviously. In actually doing this, the wind all the way up from the ground to the height of the atomic Cloud has to be taken into account, since the particles start up high when the cloud stabilizes, and start to fall, and they spend a certain length of time in each layer as they wall. So the distance which they travel on their way to the ground will, of course, be the cumulative effect. We refer to this cumulative effect in terms of the integrated wind, as we measure it.

This integrated wind, or cumulative wind, up to some altitude is so essential to a fallout calculation that the Weather Bureau, at the request of the FCDA, has recently gone to a system of teletype messages twice a day from about 70 stations in which a kind of integrated wind appears in the regular wind transmission. This is to make the integrated wind immediately available in any Weather Bureau station in the country.

Representative COLE. Would you explain what you mean by integrated wind? I do not understand.

Dr. KELLOGG. Yes. I If I can take just a moment, I can draw a picture. Dr. Graves mentioned that we have all been teachers at one time or another, and we reach for a blackboard whenever we can.

The use of the word "integrated" is perhaps a little too fancy. It really means we are just adding winds together to get some sort of resultant wind.

If we can represent the wind at any one level by a vector, a particle traveling through this layer will travel in the direction of the vector, and it will go a distance proportional to the length of the vector. If it travels through this layer and falls down into the next layer, it will find itself then traveling with the wind at that layer. So it will start curving and follow that new path. Perhaps the wind at the next layer down will be different again. After we have added the winds at a number of layers together, we might have a curving path something like that, representing the horizontal projection of the particle's trajectory. We have added vectors, and we have gotten the resultant as the particles travel through a number of layers and finally reach the ground. This is what we would call the integrated wind, or the effective wind, the path which the particle finally took.

Representative COLE. Does that mean the mean of the wind influences?

Dr. KELLOGG. This is the total effect of the wind on the particle as it fell from the place it started to the ground. You might think of it as the "mean." I think that it would be fair to call it the mean effect.

This is an essential ingredient to any fallout calculation. I have a chart here which will show the four main schools of thought for computing fallout that I mentioned. Very briefly I will go over these various schools of thought.

Here I have sketched in a little vector addition such as I have on the board. This is the kind of thing which is very easy to compute. In general, no matter where the particles came from in the atmosphere, one of the integrated winds will be a line connecting the origin of the vector plot with the end of one of these vectors. Just by inspection of this little diagram you can see that no matter where the particle started from, it has got to be in this sector between the dashed lines. So this is just the simplest kind of fallout calculation. It merely says there is a "danger sector," and somewhere in there there will be fallout.

Chairman DURHAM. You are talking to what height?

Dr. KELLOGG. Our usual radiosonde wind flights go to 60 to 80 thousand feet, and with a big effort they can be made to go higher. This is usually high enough to establish where the danger sector will be. If very large yields were to be involved one might be interested in winds still higher than our usual radiosondes can go.

The next school of thought, if I can refer to it as that, is known as the "idealized pattern." I have not seen the new book which AFSWP has prepared, which you have in your hand. AFSWP has been one of the chief exponents of the idealized pattern. Essentially it started with the observation in the early days of fallout that fallout patterns often look sort of cigar-shaped, and it was tempting to try to characterize all fallout patterns as a simple elliptical shape with a circle around ground zero. Then various rules were established for shaping them, making them fatter or skinnier or longer or shorter, depending on the yield or the wind.

The idealized pattern is a very useful method where one wants to characterize fallout for planning purposes. But it has not found much

acceptance where one is interested in a prediction, because the predicted patterns are apt to be more unideal.

Representative COLE. Ideal from what standpoint?

Dr. KELLOGG. They can be characterized by a simple ellipse like this.

Representative COLE. I still don't understand what is intended to be the ideal.

Dr. KELLOGG. Idealized in the mathematical sense, I guess, in that you can characterize it in a sort of perfect shape.

Another method for predicting fallout patterns and one which appeals to meteorologists—because every meteorologist when he makes a forecast of weather looks at the present weather pattern and searches his mind (or his files if he is well organized) to try to find something like it in the past, and then he will say to himself: "What happened in the past will probably happen again, so I will use this back pattern or analog as a prediction tool. I will simply see what happened the day following that previous case which was like the case today."

An analog method could be used for predicting fallout if we had a big collection of fallout patterns, and the winds that went with them, and then we would just match winds and scales taking into account the yield and we would be able to have a fallout prediction. However, we have not had very many actual fallout patterns to look at. So we really have not been able to build up a real file of analogs. The only file of analogs that we can draw on is one which is computed theoretically. As a matter of fact, the Rand Corp. has published, unclassified, something which we call "the catalog of fallout patterns," on the basis of which one can begin to use an analog method for prediction.

The most complete characterization of fallout is usually started with what is known as a "fallout model." A few agencies have taken the bull by the horns and have set up very complicated computing schemes for tracing each particle down to the ground from each level, each particle size, and adding up the effects on the ground. Of course, no one computing scheme could actually trace each particle, but there are shortcuts, and various agencies have developed practical computing schemes based on some kind of a fallout model, which reproduces the fallout as accurately as it can be done by theoretical methods.

Representative HOLIFIELD. Let me ask you this question. There are a number of these patterns in the AFSWP book.

Dr. KELLOGG. Idealized patterns.

Representative HOLIFIELD. There is an idealized pattern here. There are different kinds of patterns in here. I notice that in 1954 high yield explosion at Bikini that it gives a long pear shaped pattern. It starts out with a 5,000 roentgen yield and it goes at the end of 60 miles to 3,000, and at a little over 100 miles it is 2,000 roentgens, at 130 miles it is 1,000 and at 160 miles it is 500. That is at 36 hours.

Dr. KELLOGG. Mr. Chairman, these are probably cumulative doses, aren't they?

Representative HOLIFIELD. Yes, over the 36 hours. I was going to ask you about that. Dr. Graves spoke today about receiving 200 roentgens. As I remember the description of that accident, it was just for a moment. The question I want to ask you is this: Would 200 roentgens received in an instant be equivalent to 200 roentgens received over a longer period?

Dr. KELLOGG. This is a biological question. I prefer not to answer this in any detail. My biologist friends tell me that there is a certain amount of leeway there in the time in which one could accept it. If you get it within a relatively short time like a few hours, it is equivalent to getting it all at once. This is something which I think the biologists should comment on.

Representative HOLIFIELD. Very well.

Dr. KELLOGG. In order to give you a feel of what actually happens under fallout conditions, I have three charts which I can go through very quickly. They were prepared following the open or civil defense shot on May 5. This is a chart which was prepared by Mr. Nagler, who has made a detailed study of the fallout from a number of the tests in Nevada.

This first chart (p. 114) shows the fallout from the May 5, 1955, civil defense or open shot, which was roughly 30 kilotons on a tower. This is a map showing a few of the landmarks, Goldfield, Tonopah, Warm Springs, and so forth. You notice the scale of miles here, 60 miles as the total scale. In red is the observed fallout as deduced from an extensive system of road monitoring and from a few aircraft observations in that area. You see it goes out several hundred miles.

This little red line here is 100 milliroentgens per hour at 12 hours. The next red line is 10 milliroentgens per hour at 12 hours, and the outside one is 1 milliroentgen per hour at 12 hours. These blue lines here were a noble attempt to reconstruct the fallout taking into account all the wind observations at the time, and a careful synoptic analysis of the fallout. Here you can see the blue and red lines following fairly close to each other, the 10 and 10 and the 100 and 100. You can see the close-in fallout was reproduced quite well. In fact, the general curvature of the pattern toward the east was reproduced very well. It is important to note the curvature toward the east, because this is the kind of thing I was talking about when I said that a meterological forecast is a tough thing.

Representative COLE. Before you take the chart down, does the blue line indicate the forecast of the weather people with respect to the wind?

Dr. KELLOGG. No. This is a reconstruction. The next chart (p. 113) shows a forecast.

Representative VAN ZANDT. Did this fallout move as a mass and did it continue to move as a mass, or did it break up eventually?

Dr. KELLOGG. In this case it continued to move as a mass. In other words, it started falling here close to ground zero first, and then it was laid down in a fan shape curving to the east. It occurred earlier close in and later and later as you go along in the pattern. Heavy particles were landing close in, and lighter particles which drift longer landed further out.

This next chart (p. 113) is a prediction. This is the Weather Bureau's prediction, using the winds predicted at 2 hours before shot time. I think this is the kind of thing that one would expect. Very good verification in close. After all, this is where it is important. But then it was pretty hard apparently, in this case to predict the later shift, which must have occurred as much as 12 hours later.

Representative COLE. Is your scale the same in this chart?

Dr. KELLOGG. Yes. The scale is exactly the same. It is the same map. The red lines are exactly tracing the red lines you saw before.

Just to show the Weather Bureau is not the only outfit making predictions, this was the Los Alamos Scientific Laboratory and the University of California Radiation Laboratory prediction. They also have a forecast team of meterologists. Here again the prediction made at 2 hours before the shot time showed the early fallout within the hundred milliroentgen per hour at 12-hour line to be fairly well verified, at least in the direction in which it went. Again they did not get the curvature of the later fallout.

Chairman DURHAM. What would be your observation as to the accuracy there in the predictions by the Weather Bureau and the other outfit? It looks to me they are off quite a bit in the prediction, because the observation line there cuts back pretty quick, and the other continues on up.

Dr. KELLOGG. The early part, as I say, in both cases was fairly well verified, but neither of them anticipated the shift of the wind which occurred later on. I think this is what one would expect. The meteorologists don't fool themselves as to how well they can predict the wind. There are a number of studies of this matter. One of the best ones recently was by the Air Weather Service, which gives actual wind statistics and forecast statistics. Recently, Jack Reed, a meterologist with the Sandia Corporation, has made a study of this situation and applied it directly to Nevada, the purpose being to try to assign some kind of probability to a forecast made a certain number of hours before shot time. Meteorologists recognize that any forecast is a kind of probability. It is an educated guess. This effort by Reed is a very noble effort to actually assign the right kind of probability to such a forecast, so that the people who have to use the forecast can know with what certainty the forecast was made.

Representative COLE. How do you account for the fact that the forecast of the direction of the fallout was reasonably accurate, but the forecast of the breadth or width of the fallout was quite inaccurate?

Dr. KELLOGG. I don't know the details. I did not sit down and go through exactly the assumptions that were made in each of these models by these people. I can only say that it would have something to do with the model they took, that is: How big a cloud they assumed, which would determine how wide the pattern was; how the radioactivity was distributed with height; the fraction which fell out, which we were talking about earlier—any of these might not have been predicted accurately. As I mentioned earlier we are uncertain about this fraction when we are firing in a tower. Any of these assumptions could have had an effect on the width of the predicted pattern.

Representative COLE. But the analyzers did know in advance the estimated yield of the test, did they not?

Dr. KELLOGG. Yes. Even though you may know the yield for a tower shot, you may not know the fraction of this yield which takes place in the early fallout with any accuracy, nor how it is distributed with height and particle size. These could have accounted for this changing shape.

Representative VAN ZANDT. Both groups had the same data as far as weather was concerned in that part of the world.

Dr. KELLOGG. Yes. They both used the same wind prediction.

Chairman DURHAM. In other words, they are pretty good up to a hundred miles, but beyond that it is not too accurate?

Dr. KELLOGG. That is the way it looks here, and that is the way it would always tend to look where we have a difficult forecast situation. This is probably a very light wind condition. The distant parts are really rather unimportant. I might mention here, as I recall, that the farthest extension of the 100 milliroentgen per hour line on this chart represents a "lifetime dose"—that is, the accumulated dose you would receive if you stood out in the open from the time it came down to infinity—of 9 roentgens. At this point on the 10 milliroentgen per hour line it is much less. It is a fraction of a roentgen.

Representative HOLIFIELD. You have not given us the strength of that particular weapon.

Dr. KELLOGG. About 30 kilotons on a 500 foot tower.

Representative VAN ZANDT. Doctor, are you in a position to tell us how long you actually followed that cloud?

Dr. KELLOGG. I am sure it was followed. I don't remember the details. An attempt is made for radiological safety purposes to trace where the cloud went by meteorological analysis. This is something which we have done quite a bit of at Rand. I don't remember whether we analyzed this one particularly or not. It is possible to do it by just analyzing the winds. It is also possible to do it by monitoring it by aircraft.

Representative VAN ZANDT. Is that your field, monitoring the cloud by aircraft?

Dr. KELLOGG. No, that is not.

Representative COLE. Would you repeat the dosage at 10 miles out? Would you state again what that is?

Dr. KELLOGG. Let up put this first chart (p. 114) back up which has the scale of miles. Let me make sure I have the right numbers here.

This point here—the furthest extension of the 100-milliroentgens-per-hour, 12-hour line—represents an infinity dose of 9 roentgens. That is about 30 miles from ground zero.

Representative COLE. Tell me what you mean by an infinity dose of 9 roentgens.

Dr. KELLOGG. By reconstructing the pattern and also by certain instruments which note when the thing starts, we can tell when the debris arrives at the ground. It doesn't all arrive at once, but it arrives within a relatively short period of time. Then, if we had an instrument which just simply counted roentgens, and it was hung on a post 3 feet above the ground and stayed there from then to doomsday, it would fiinally accumulate 9 roentgens. That is what we mean by infinity dose. Of course, most of this 9 roentgens would be accumulated in the first few days.

Representative COLE. What is the influence which determines the period of accumulation other than wind?

Dr. KELLOGG. The wind determines when it starts. The total dose then depends on the amount which comes down, and when it came down.

Representative COLE. Suppose it got there at this point and there was no wind at all.

Dr. KELLOGG. It would hardly get there if there were no wind at all.

Representative COLE. I still do not understand what you mean by a 9 roentgen perpetuity dosage.

Dr. KELLOGG. I can draw it perhaps as a time plot. (At the blackboard). This is the dose rate in roentgens per hour on the vertical scale. Here is the time on the horizontal scale. We said this was for a point about 30 miles out from ground zero. Suppose there was a 10-knot wind, so about 3 hours after shot time we begin to get fallout. The dose rises at 3 hours, and suppose it falls for the next hour and then stops. That is probably what would have occurred, fallout occurring for about an hour while the cloud is passing by. Then decay starts. This is radioactive decay at the rate that Dr. Graves gave, according to the time-to-the-1.2 power law, where time is measured from shot time. If this were the rate at 3 hours, and if we go to 7 times that, or 21 hours, it would be down by a factor of 10. After another 7 times 21 hours, whatever that is, the dose rate would be down to a hundredth. If we counted up the total number of roentgens, that is, multiply the dose rate times the time for each time interval and sum over all the intervals, we would get a cumulative dose. If we calculated this out on the tail of the curve to an infinite length of time, we have what we call an infinity dose. As you can see from here, most of this infinity dose is obtained in the first day or so in this case.

Representative COLE. I think I understand. At least I do better than I did before.

Dr. KELLOGG. I admit it is a difficult concept at first, but it is one which the people who are working with fallout sometimes use. They prefer to use an infinity dose instead of a dose rate at some time. It is merely a matter of what you want to talk about.

Representative HOLIFIELD. Dr. Kellogg, how far are you on your summary? I understand that you can be with us tomorrow, and, if it is very long, I want to carry you over until tomorrow. If it is short, since it is 5:30 and the members have to get back to their offices——

Dr. KELLOGG. I would like to take a little bit longer, 10 or 15 minutes, if that is all right for you, so perhaps tomorrow would be better. What I have to present still, I think, is fairly pertinent.

Representative HOLIFIELD. We do not want to cut you out of any time. I suggest that we start with you tomorrow and, in the meantime, it will give the staff some time to look at your prepared presentation, and we may have some more questions for you.

Dr. KELLOGG. This was a good stopping point, anyway.

Chairman DURHAM. I might say this is my first lesson in meteorology.

Representative HOLIFIELD. The Chair will announce that the committee will resume its hearings tomorrow morning in room 457 in this building. There will also be a 2 p. m. session tomorrow. Wednesday, we will come back to this room again. The meeting stands adjourned.

(At 5:25 p. m., Monday, May 27, 1957, a recess was taken until Tuesday, May 28, 1957, at 10 a. m.)

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

TUESDAY, MAY 28, 1957

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION OF THE
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The special subcommittee met, pursuant to recess, at 10: 05 a. m., in room 457, Senate Office Building, Hon. Chet Holifield, chairman of the subcommttee, presiding.

Present: Representatives Holifield, Durham (chairman of the Joint Committee), Price, Dempsey, Van Zandt; Senators Pastore, Hickenlooper, and Bricker.

Present also: Professional staff members James T. Ramey, executive director; George E. Brown, Jr., Hal Hollister, staff technical adviser, and Paul Tompkins, consultant.

Representative HOLIFIELD. The committee will be in order.

Today we open our second day of the hearings on the nature of radioactive fallout and its effect on man. Yesterday we began our hearings with an introductory statement by Dr. Charles L. Dunham, Director of the Division of Biology and Medicine of the Atomic Energy Commission, in which he provided some perspective on the general radiation problem as a basis for the beginning of the hearings. He was followed by Dr. Mark Mills, associate director of the Livermore Laboratory of the University of California, who provided us with technical background information on radioactivity and radiation and the hazard aspects of controlled fusion and fission reactions.

Yesterday afternoon, Dr. Alvin C. Graves, chief of the testing operations of the Los Alamos Scientific Laboratory, provided more detailed information on the production of radiation and radioactivity by the detonation of nuclear weapons. He described the effects of immediate bomb detonations, together with local fallout and worldwide fallout. He also gave some indications of the nature of the fallout from so-called clean and dirty weapons.

Dr. Graves was followed by Dr. Frank Shelton, of the Armed Forces special-weapons project, and Gen. Alfred D. Starbird, Director of the AEC Division of Military Applications, who provided a few comments on Dr. Graves' testimony and the new book, The Effects of Nuclear Weapons.

Incidentally, General Starbird indicated that the printed statement entitled "Testimony Before the Joint Committee on Atomic Energy on the Production of Radiation and Radioactivity From Nuclear Weapons—Topic V" was his statement, which he submitted for the record. The chariman and committee were under the impression that

this statement, distributed prior to Dr. Graves' testimony, was the testimony of Dr. Graves, since they were somewhat similar. We were given a carbon copy of the statement which Dr. Graves used as a basis for his oral presentation, and are having it reproduced for the information of the subcommittee. It is hoped that the Commission will identify their statements in a better fashion hereafter.

Our last witness yesterday was Dr. W. W. Kellogg, of the RAND Corp., who began his testimony on the atmospheric transport, storage, and removal of particulate radioactivity, particularly local fallout debris.

Dr. Kellogg did not complete his testimony yesterday, and we are happy to have him with us again today to resume his testimony.

Dr. Kellogg, will you please come forward? You may proceed where you left off:

STATEMENT OF DR. W. W. KELLOGG, OF THE RAND CORP.—Resumed

Dr. KELLOGG. Mr. Holifield and members of the committee, perhaps I should start by recapping just very briefly what happened yesterday, and perhaps making it a little bit clearer. I would like to very briefly mention the fact that the scientists who have been interested in studying fallout have been in fairly close touch with each other, and last March about 60 of us assembled in Santa Monica, Calif., to try and iron out some of the points of difference that we had.

Chairman DURHAM. You say 60 people, Doctor. Whom do you mean?

Dr. KELLOGG. Sixty people representing the various agencies, such as the AEC, the Air Force, the Army, the Navy, and various civilian agencies who have all been connected in one way or another with studies of fallout.

Chairman DURHAM. Was there a report made by that group?

Dr. KELLOGG. The report is being prepared. It will be classified when it comes out, because we wanted to discuss specific tests. Therefore, it is classified. However, there was one thing which we could all agree on at the end of the session, and that was that we had a long way to go before we could all get a consistent and clear picture of this process of radioactive fallout.

In particular, I think it should be mentioned that the fraction of fallout from a surface burst, which I gave yesterday as around 80 percent, in my opinion, was discussed at great length at this meeting, and it was shown that the evidence which we have for this fraction which falls out from a surface burst is not clear, and that we really are not certain about the fraction which falls out. We know that it is roughly 50 percent, plus or minus some number of percent, and the 80 percent which I gave you is sort of a best guess that RAND has made. I do not think that I should say this is a certain number by any means. I wanted to make that clear.

Representative HOLIFIELD. The 80 percent goes into the stratosphere?

Dr. KELLOGG. The 80 percent is what falls down in the first 24 hours or so. It would leave 20 percent to stay in the air.

Representative HOLIFIELD. This only refers to bombs, where the column does not go into the stratosphere?

Dr. KELLOGG. No, sir. This is more or less independent of yield, I would say. We have an observation of a low-yield weapon, and we have some other observations of higher yield weapons; and the fraction which falls out in early fallout does not seem to be very sensitive to yield.

I think the important difference, and one which I think will be taken up by Dr. Machta, who is following me, is that when we have a low-yield weapon the cloud does not go very high; therefore what is left is at a relatively low level in the troposphere, which is the lower part of the atmosphere. When we have a high-yield weapon, then what is left is higher in the atmosphere, in the stratospere.

The fraction of the debris taking part in the close-in fallout depends on whether it was a surface burst or not. How high it goes depends on the yield.

Representative HOLIFIELD. For the purposes of clarification, will you please explain what you mean by the troposphere?

Dr. KELLOGG. Yes, sir. The troposphere in middle latitudes where we are now is the atmosphere from the ground up to about, say, 30,000 to 40,000 feet. It is the part of the atmosphere that contains clouds, contains the weather as we know it.

Representative HOLIFIELD. And in the Tropics, it would go up to about 50,000?

Dr. KELLOGG. 50,000 to 60,000 feet in the Tropics; yes, sir.

Representative HOLIFIELD. And above that is the stratosphere?

Dr. KELLOGG. That is right.

I will not belabor this point. I just wanted to indicate there is some uncertainty about this number.

Yesterday we showed charts from the May 5, 1955, open shot in Nevada, a shot of roughly 30 kilotons on a 500-foot tower. The motive for showing the fallout from this particular burst was to give the committee a little bit of feel for the irregularity of one of these fallout patterns, and to show that various ways of predicting the fallout had been developed; that the predictions close in where the fallout is heavy enough to be important is fairly accurate, whereas further out, where the fallout is relatively unimportant the predictions are less certain.

I think in Nevada the fallout patterns are particularly irregular because of the terrain features which break it up. The rugged terrain also makes it hard to observe the fallout pattern.

So the red line which I showed on that chart representing the "observed" fallout is only roughly known. If we had a complete map, we would see it marching up across the mountains and down into the valleys, and we know these have an effect upon fallout. It would not be a smooth line as I showed it.

Representative HOLIFIELD. Would you say the area is such adjacent to the explosion point of these weapons to give the maximum opportunity for local fallout?

Dr. KELLOGG. That appears to be the case; yes, sir.

The hundred milliroentgen per hour line at 12 hours, which was shown on the chart, only went out about 30 miles in a due-north direction, and this early fallout, of course, can be forecast fairly well.

There are two further topics which I would like to take up, which I did not have a chance to yesterday.

One has to do with the growth of the pattern in time. I think we have not really discussed the growth in time of the pattern. We have

tended to look at these patterns as if they were laid on the ground with a big rubber stamp, whereas I think you all understand that the pattern develops over a period of time because the debris arrives at different places at different times in a more or less orderly process.

In particular, the question of when the fallout occurs close in to ground as zero, I believe, requires a little bit of attention, since in an emergency this would have an important bearing on what plans one would take to get out of the fallout.

In preparation for this hearing I collected all the data I could get from the tests which had a bearing on this question of when the fallout begins relatively close in to ground zero; and by relatively close in, I do not mean just a few thousand feet. I mean at distances ranging from 8 to, say, 30 miles, from ground zero for large yields, where the atomic cloud is quickly overhead. That is, this is the fallout which is in the shadow of the mushroom cloud.

It appears from a collection of data, which are summarized in the report which I presented for the record, that the fallout close in does not generally begin for about 30 minutes. The times of arrival actually vary from 20 to about 40 minutes. I think this is significant, and I think the explanation is rather easy to see. If one visualizes the fact that most of the debris is carried up in the mushroom cloud, then when the mushroom cloud stabilizes it takes a while for this material to get back down to the ground. So this delay seems reasonable, and I have documented it with the data which we have from the atomic tests.

The second part, having to do with the growth of the pattern in time, I think, can best be illustrated by this chart which, incidentally, is taken from the unclassified article in the Journal of Meteorology, written by Dr. Rapp, Mr. Greenfeld, and myself. So this is familiar to meteorologists who have read the journal article.

This chart shows a succession of fallout patterns at various times: First, a half hour, 1 hour, 2 hours, and 6 hours. It is from a 1-megaton explosion, and it is assumed there is two-thirds of a megaton of fission products represented in this fallout pattern. (See p. 116.)

The dose rates represented are in roentgens per hour at the time of the pattern.

I believe this is instructive in showing the way in which the pattern grows, in showing that it is most intense when it first comes down, and then decays after it is on the ground.

Close in we have the inside line, the thousand roentgens per hour at a half hour. The next line is 500. In other words, judging from the numbers which we heard yesterday, if this dose were to be imagined to be steady, at roughly half hour you would get a lethal dose in something like an hour.

Representative HOLIFIELD. For how many miles?

Dr. KELLOGG. The scale of miles is here. The 500 roentgen line at a half hour extends out, I would judge, about 12 miles.

Representative HOLIFIELD. In other words, in one-half hour everything within 30 miles downwind—is it 12 miles or 30 miles?

Dr. KELLOGG. Twelve miles.

Representative HOLIFIELD. Twelve miles downwind would receive a lethal dose of 500 roentgens?

Dr. KELLOGG. Yes.

There is one thing which was not taken into account in this calculation because it was done before we had really studied the time-of-arrival business. It appears that at about half an hour this blanket of fallout material descends on the ground, and so that at half an hour it is sort of nip and tuck whether the fallout has arrived yet or not. This half-hour picture might really refer to something like 40 minutes, but I do not think we know enough to make a clear distinction.

Senator BRICKER. What about the wind currents at that time?

Dr. KELLOGG. The wind current was a fairly strong west wind, and the wind did not change much with altitude in this case. So it is being laid out in an easterly direction at a fairly rapid clip.

Senator BRICKER. What do you mean by "fairly strong" winds—20 miles?

Dr. KELLOGG. In the original article there is what we call a hodograph showing the winds. I did not reproduce it on the chart, and perhaps I should. I can tell you roughly what they were.

Senator BRICKER. That is what I want, just roughly.

Dr. KELLOGG. They roughly range from about 40- to 80-knot winds in this case. Fairly windy day at high altitudes.

Representative HOLIFIELD. Would you explain at this point why you have on the chart there "1 megaton yield" and then "two-thirds megaton fission yield"? Would you give us a clarification on that?

Dr. KELLOGG. This represents the fact—and it is merely for illustration purposes only, of course—that this was a thermonuclear device in which part of the yeld came from the thermonuclear reaction.

Representative HOLIFIELD. Part of the blast yield?

Dr. KELLOGG. Part of the blast yield.

Representative HOLIFIELD. Or the heat yield?

Dr. KELLOGG. That is right.

Representative HOLIFIELD. Therefore, you figure roughly you would lose a third, that the megaton would not be completely a 1 megaton of fission?

Dr. KELLOGG. That is right.

Representative HOLIFIELD. One megaton of radioactive fission, but it would be two-thirds of a megaton in the fission products, and the other third in blast and heat?

Dr. KELLOGG. No, sir, that is not quite the way to say it, sir.

The total energy yield and thermal yield in terms of the work put out by the explosion was 1 megaton. However, only two-thirds of a megaton of fission products were released. And the conversion there from megatons of work to megacuries is the thing which Dr. Graves discussed yesterday, how to do this. As I say, this is for illustration only.

Representative PRICE. Mr. Chairman?

Representative HOLIFIELD. Mr. Price.

Representative PRICE. You will probably cover this before you get through with the chart. With an increasing yield, how does the figure over there on the half-hour basis expand, or how does this work if you drop a 10-megaton bomb?

Dr. KELLOGG. The way in which this pattern changes with yield, seems to be in such a way that the area within any one of these contours goes up with the yield. The areas go up linearly with the yield.

Representative PRICE. What would that 500 figure be on a 10-megaton bomb?

Dr. KELLOGG. There would still be a 500 line on the 10-megaton bomb, but it would be 3 times bigger in any dimension. It would have an area 10 times bigger.

Representative PRICE. It would have an area, say, of about 40 miles?

Dr. KELLOGG. Yes; that is about right. I would rather point to the 1-hour pattern because there is a question as we raise the yield, this question of whether this material gets back down to the ground in half an hour. I expect it would not for a 10-megaton yield. It probably would for 1-megaton yield. That is because of the difference in altitude.

Let's look at the 1-hour pattern, and talk about that. I prefer that to the half-hour pattern, particularly if we wish to discuss the way in which it scales with increasing yield.

Representative PRICE. On the 1-hour one, you have 500 roentgens still extending pretty close to 12 miles?

Dr. KELLOGG. Yes; it is. It looks like it was about 10 miles.

Representative PRICE. So, if you had a 10-megaton drop, the 500-roentgen area would be at least 40 miles?

Dr. KELLOGG. That is true; yes. The square root of 10 is a little over 3.

Representative HOLIFIELD. Now, in areas in the case of warfare where cities are close together that are primary targets, you would also face the factor of lapping, would you not?

Dr. KELLOGG. Yes.

Representative HOLIFIELD. And that would increase the radioactive fallout to the extent that the degrees of lap would add?

Dr. KELLOGG. That is true.

If I may, I would like to show a chart in which the fallout from a multiple-bomb campaign is shown. (See p. 119.)

Dr. KELLOGG. Now that we have had time out for the scene moving, I would like to say that this chart was prepared by Mr. Charles Shafer of the Federal Civil Defense Administration headquarters in Battle Creek, Mich., and he has very kindly let me borrow this chart, which was prepared for the FCDA's Operation Sentinel.

Representative HOLIFIELD. Would you make an estimate as to the pictorial authenticity of this, based upon scientific information?

Dr. KELLOGG. Yes. This kind of a fallout chart has been done as an exercise many times.

Chairman DURHAM. It was actually——

Dr. KELLOGG. Mr. Shafer is in the audience, and if you press me on the details, I will perhaps ask permission to have him help me.

Representative VAN ZANDT. It would be nice to have Mr. Shafer up here.

Representative HOLIFIELD. Yes, let's have Mr. Shafer up here at this time.

Dr. KELLOGG. Fine. I would like very much to have him up here.

Representative HOLIFIELD. Mr. Charles Shafer. We are happy to have you before the committee this morning. The committee would like to hear briefly the substantiation from a scientific standpoint of this chart which you have prepared, and its meaning.

STATEMENT OF CHARLES SHAFER, METEOROLOGIST, UNITED STATES WEATHER BUREAU (ON ASSIGNMENT TO FCDA) [1]

Mr. SHAFER. This particular fallout analysis is purely hypothetical, which is obvious. It is based upon one of the techniques which Dr. Kellogg explained to the committee yesterday, the stylized pattern technique, and it is virtually identical to that which is described in the AFSWP paper, copies of which were entered into testimony yesterday.

The meteorology used on this particular fallout analysis was the wind existing from 80,000 feet altitude, down to the surface of the earth during the period November 20 to 21, 1956.

In actuality, if such an attack or an attack of this magnitude——

Representative HOLIFIELD. Will you please repeat the date? Some of us did not hear that.

Mr. SHAFER. November 20 to 21, 1956, a 24-hour period.

This represents an attack of total yield of approximately 2,500 megatons, a very large attack. It is an estimate. It is an analysis based upon many assumptions such as Dr. Kellogg described yesterday. In an actual attack of this magnitude, certainly the levels of radiation would vary considerably from those indicated on this chart, simply because we do not know all of the information we need to know in order to do such an analysis as this is on a forecast basis.

Representative VAN ZANDT. What type of attack was it?

Mr. SHAFER. This was an attack of thermonuclear weapons.

Would you like to have some information on the size and the distribution of weapons?

Representative VAN ZANDT. As long as you do not enter the classified field.

Mr. SHAFER. There is no classification involved in it.

The weight and distribution: The weapons were of three sizes: In the range of 5-megaton yields; 10-megaton yields; and 20-megaton yields. There were 250 bombs, with a total yield of about 2,500 megatons. There were 144 areas of attack. Fifty-three of the areas were basically population and industrial centers; 59 areas were basically military installations; and the remaining 52 areas contained both military and population objectives.

Representative VAN ZANDT. How many minutes or hours were involved?

Mr. SHAFER. In the attack?

Representative VAN ZANDT. Yes, in the attack.

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[1] Waverly, N. Y., graduated from State Teachers College at Albany in 1939 and did graduate work in mathematics and meteorology at American University and New York University.

Assigned to Federal Civil Defense Administration by the Weather Bureau in April 1955 to assist in radiological defense problems resulting from phenomena of fallout. Began meteorological career as weather observer at Raleigh, N. C., prior to World War II. Flight weather forecaster at Dayton Army Air Field and consultant to Accelerated Service Testing Branch of U. S. Air Force. One year scholarship at College of Engineering of New York University. Subsequent to this Mr. Shafer was detailed to International Civil Aviation Organization at Montreal for a special research project, then to Washington National Airport as an international forecaster.

Next he was assigned to Weather Bureau-Air Force-Navy Analysis Center and in 1952 was loaned to CAA and transfered to Europe to plan and supervise the rehabilitation of the Greek Weather Service. In 1954 Mr. Shafer received 3 months of specialized instruction in long-range forecasting and worked as supervising meteorologist at the Military Weather Bureau unit at Suitland, Md.

Mr. Shafer transferred from the Weather Bureau to the Radiological Defense Operations Office of FCDA and will work with other governmental agencies in developing a national radiological monitoring network. (Submitted by Federal Civil Defense Administration.)

Mr. SHAFER. Approximately 2 hours for the delivery of the attack, sir. The fallout analysis was carried on for 24 hours. We assumed that there was sufficient fallout debris to render the analysis valid to carry it out for 24 hours.

One thing I should mention is that the analysis of the east coast is probably erroneous. It would have been better to have terminated the line at the coast, simply because debris in the ocean area is subjected to considerable mixing by ocean currents, and there would have been considerable dilution. However, from our analysis this is an indication of what would have come down. It would not have remained in that location, as it would have been more apt to do on the ground.

Representative VAN ZANDT. Will you mention some of the targets, and give the geographical distribution of them.

Mr. SHAFER. Yes. We gave the Air Defense Command credit for quite a few knockdowns on this. This represented the results of the aircraft which were able to penetrate our borders and deliver their weapons as indicated.

The heaviest concentration, the heaviest complex of detonation happens to be in the Detroit area, because we made some assumptions not only with regard to enemy intent in getting to target, we also assumed there were some knockdowns of aircraft and some unintentional weapon detonations.

In the Detroit area, for example, there are some 70 or 80 megatons of detonation of weapons within 15 or 20 miles of one another. Consequently, there is a tremendous overlap from Detroit eastward across Buffalo and across central New York State—the factor Mr. Holifield mentioned a while ago, the factor of overlap.

Chairman DURHAM. What is the yellow?

Mr. SHAFER. The yellow shading, sir, represents the areas of most intense radiation hazard, and area where the dose rate normalized back to 1 hour after detonation would have exceeded 3,000 roentgens per hour.

The red areas are where the radiation levels would have exceeded 1,000 roentgens per hour. The blue areas are where the radiation levels would have exceeded 100 roentgens per hour. The green levels are where the radiation would have exceeded 10 roentgens per hour.

Would you be interested in the effects of this particular attack as far as people are concerned?

Representative HOLIFIELD. Yes. What was your estimate on the loss of human life?

Mr. SHAFER. May I read that, sir, so I will have it correct?

Without evacuation of the target cities to escape primary weapons effects—that is, to escape blasts, thermal and initial radiation effect——

Representative HOLIFIELD. I did not get that.

Mr. SHAFER. Without target evacuation, target area evacuation to escape primary weapons effects, and assuming present-day shielding which exists in the United States, that is, homes, or home basements, or the basements of large administration buildings, the bomb-damage assessment on this particular attack indicated a total loss by death of about 82 million people, based upon current United States population, and about 24 million surviving casualties, 60 days subsequent to the attack. This left about 60 million relatively uninjured, but doubtlessly suffering some radiation effects.

In this particular analysis, we only carried it down to the 10 roentgens per hour. So that even in the white areas, where the levels are relatively low, there could be some radiation effect. Of the total fatalities, some 50 percent were a result of radiation factors.

Now, by assuming a 50-percent effective evacuation of the target areas——

Representative HOLIFIELD. At that point I think you should tell us where you have your evacuation centers.

Mr. SHAFER. On this first analysis, in which there were about 82 million fatalities, there was no evacuation assumed, simply a duck-and-cover operation upon the sound of the warning bell that an attack was imminent.

Representative HOLIFIELD. All right. Now, you are going to assume evacuation. Where are you going to evacuate them to, under the same conditions, to protect them from fallout on a map of that type?

Mr. SHAFER. We had to make two assumptions on this particular phase of the analysis.

Assuming a 50 percent of effective evacuation beyond the D zone of blast damage, that would be from 5 to 15 miles beyond ground zero, depending on weapon size; assuming a 50-percent effective evacuation, and assuming the existence of a radiation shelter program in the United States, assuming the existence of shelters to which evacuees could go, the fatalities decreased to about 31 million. So there was a net saving of over 50 million people under a radiation shelter program, and an assumption of 50 percent effective evacuation beyond the primary effects of the weapon.

Senator PASTORE. And that hypothesis is predicated upon an attack of 2,500 megatons within a period of 2 hours, and you are talking about evacuees within that period?

Mr. SHAFER. The evacuation immediately prior to that, sir. Evacuation beyond the D zone of blast damage. Evacuation for, perhaps 5 to 15 miles to shelters prior to the attack, perhaps 1 hour, or perhaps less than 1 hour prior to the attack.

Representative HOLIFIELD. Of course, you have not only made two assumptions which cannot obtain under present conditions, one, because you do not have a warning time; two, because you do not have a shelter system; but you made a third assumption that you know where point zero of the falling bombs is going to be. All three of those assumptions are in the realm of impossible prediction.

Mr. SHAFER. Actually, in the analysis, we only assumed a 50-percent evacuation of the target areas attacked in this particular exercise; that is correct.

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. In 2 hours' time how many bombs were dropped.

Mr. SHAFER. 250 bombs on 144 areas.

Representative VAN ZANDT. 144 areas?

Mr. SHAFER. That is right.

Representative VAN ZANDT. Your chart indicates that they dropped bombs on the Northwest?

Mr. SHAFER. That is correct, sir. Seattle——

Representative VAN ZANDT. And California?

Mr. SHAFER. Correct.

Representative VAN ZANDT. And then moved east. How about Arizona and New Mexico?

Mr. SHAFER. Yes, sir. We have weapons detonated at Phoenix, at some of the airbases near there, and at Tucson.

Representative VAN ZANDT. How about the Salt Lake City and Denver areas?

Mr. SHAFER. That is correct, sir. Hill field in Utah, and also Salt Lake City itself, sir. And Denver, at the airbase outside of Denver, and Denver city itself.

Representative VAN ZANDT. In other words, you covered all the densities of population in the United States?

Mr. SHAFER. Many of them, that is correct.

Representative VAN ZANDT. Whether military or otherwise?

Mr. SHAFER. Military, civilian, and industrial targets were the primary objectives. However, there are some random detonations also at areas that would not meet those criteria.

Representative VAN ZANDT. You said 50 percent of the casualties could be charged to radiation?

Mr. SHAFER. That is correct.

Representative VAN ZANDT. Are we to assume the other 50 percent were killed by blast effect?

Mr. SHAFER. That is correct, blast and thermal effect.

Representative PRICE. Mr. Chairman?

Representative HOLIFIELD. Mr. Price.

Representative PRICE. What type of bombs were you assuming?

Mr. SHAFER. Thermonuclear-type weapons, and obviously they were not the relatively clean type of which you were speaking yesterday.

Representative PRICE. Exclusively H-bombs?

Mr. SHAFER. That is correct.

Representative VAN ZANDT. In your imagination, did you provide for an aerial burst?

Mr. SHAFER. These are all surface detonations.

Representative VAN ZANDT. All surface detonations?

Mr. SHAFER. Yes. You will notice there is quite a variation of the pattern since we used the actual existing meteorology of a particular day.

For example, in Florida the winds were much less strong, than over here [indicating], say across Colorado and across the northern plains. As I recall the wind speeds in the Florida peninsula were in the matter of the neighborhood of perhaps 15 miles per hour, the mean effective wind from 80,000 feet to the surface, whereas they exceeded 50 miles per hour as we went further north.

Representative VAN ZANDT. What period of time did the deaths occur?

Mr. SHAFER. The first 60 days. Our computations went for the first 60 days, sir. There may have been some deaths beyond then, because, as pointed out yesterday, the delivery of radiation exceeds 60 days. However, certainly the bulk of the problem would be indicated.

In this particular exercise we dealt only with the immediate survival problem. We did not take into account the soil contamination, the uptake of strontium 90, and the long-term haul problem that certainly would be very much with us from an attack of this magnitude.

Chairman DURHAM. How about water? Did you take that into consideration?

Mr. SHAFER. No, we did not take contamination of water into account here. Certainly there would be considerable contamination of reservoirs from fallout, from rainfall, moving the deposition from its point of contact and into reservoirs.

Representative HOLIFIELD. You might say, then, this represents the immediate effect, say, the within-24-hour effect?

Mr. SHAFER. As far as this analysis of the fallout is concerned; yes. The mortality of fatality statistics which I gave you indicated the first 60 days.

Representative HOLIFIELD. Yes.

Mr. SHAFER. As far as contamination of water is concerned, certainly southern Lake Michigan would have had a considerable amount from this. Many cities depend upon their drinking water from Lake Michigan.

Senator PASTORE. It is my prayer that these questions always remain hypothetical, but why did you take the figure of 2,500 megatons?

Mr. SHAFER. Solely to study the problems connected with a large-scale attack with ground-burst weapons. There is nothing significant about the figure 2,500. This is one of an infinite number of attacks which could have been assumed. I might advise the committee that we have studied attacks with much less and much greater megatonnage than this. We recently completed an agency exercise based upon a 20-megaton surface detonation on all of the 314 targets in the United States. The megatonnage was some 2½ times that indicated on this analysis, but it was very specialized. It was certainly a situation that no enemy would be able to accomplish, to get 314 intended weapons upon their intended targets.

Representative HOLIFIELD. This, of course, does not give us an estimate of the amount of radiation that would go into the world's atmosphere and stratosphere, and which would fall over the following 10 years around the world on areas which are not attacked by an immediate detonation of weapons?

Mr. SHAFER. That is correct, sir.

Chairman DURHAM. Then it was assumed in the preparation of the map that this could be accomplished in the 1960's?

Mr. SHAFER. This was not intended to represent capability at any particular time, although we assumed that such an attack could be increasingly within enemy capability during the 1960's.

Representative VAN ZANDT. Was it based on the Russian capabilities?

Mr. SHAFER. This was based solely upon our assumption for this exercise.

Representative VAN ZANDT. Was it based upon Russia's capability of getting 100 percent of her planes through to the target?

Mr. SHAFER. No, sir. We assumed that this represented those aircraft which were able to penetrate our borders and deliver their weapons, 250 weapons. I do not know how many aircraft that would require. If the Air Defense Command were effective in knocking down 50 percent of the invading bomber aircraft, this would imply 500, perhaps, in the original attack.

Representative VAN ZANDT. Would you take a bomb burst over Los Angels, and trace it as it moves east, giving us some idea of the destruction in human lives?

Mr. SHAFER. That would be rather difficult, but I will attempt it, sir.

There were four weapon detonations in the vicinity of Los Angeles, the large area, the large population center, the country. The blue-dashed line at the end of my pointer represents the area which would be affected by fallout at the end of 1 hour. That is 1 hour subsequent to the detonation. The levels in the yellow area would exceed 3,000 roentgens per hour. Unless survivors in that area had very good shelter, shelter with a shielding factor of perhaps 5,000, everyone would be dead. It would be a writeoff area.

Almost the same generalization in the red area. Not so much in the blue. Certainly not in the green. And when we get out to the white, a relatively no-hazards area at all.

By the end of 3 hours, the debris would have moved down almost to the extreme southeastern portion of California, almost to the Colorado River, the border between Arizona and California. By 7 hours, it would be over to central Arizona. However, at this time you would have to decrease the levels of radiation indicated on here by a factor of 10, as was mentioned yesterday, the radiological decay, the radiation decay which takes place. So by this time, in the yellow areas the dose rates are no longer 3,000 r per hour, but are down to 300 r per hour. In such a field as that, one could still be exposed to a lethal dose in less than 2 hours' time.

The debris spread on eastward to New Mexico by about 12 hours. However, by this time there have been considerable overlaps from attacks on San Diego, and from attacks on Arizona; so it becomes rather impossible to trace the debris from one single bomb.

Here is one [indicating] in the data which can be traced a bit more easily. This represents 6 hours of time; some 12 hours down to Grand Canyon; 18 hours, perhaps, over to the Colorado border; and by 24 hours into the Denver area. But it would be of no significance at Denver, since Denver has been struck by 3 weapons some 24 hours before, and the addition at this time is a relatively no additional value. Is that what you wanted, sir?

Representative VAN ZANDT. That is it; thank you.

Representative HOLIFIELD. Mr. Shafer, will you please give us your background, whom you work for, and so forth, so we can have that on the record?

Mr. SHAFER. Yes, sir. I am a meteorologist from the United States Weather Bureau. I have been assigned to FCDA, the Federal Civil Defense Administration, for the past 2 years, to assist them in their radiological defense problem.

Representative HOLIFIELD. How long have you been in this work?

Mr. SHAFER. In the Weather Bureau?

Representative HOLIFIELD. Yes.

Mr. SHAFER. I have been in the Weather Bureau 16 years, sir.

Representative HOLIFIELD. In presenting this, do you feel that this is supported by a majority of the meteorologists; that this sort of a hypothetical portrayal would be supported by most of them?

Mr. SHAFER. Most of them would not take strong exception to this. As indicated yesterday, there are 4 different techniques, or ways of

doing this, and each of the 4 yields some minor variation. As far as presenting the magnitude of the problem, and, for planning purposes, to indicate what we are up against, this is reasonably accurate. For actual operations in the event of an attack, for actual survival operations, this would not be adequate. We could only accomplish an analysis such as this by complete monitoring or by a combination of monitoring and meteorological techniques.

Representative HOLIFIELD. Are there any further questions?

Thank you very much, Mr. Shafer.

Mr. SHAFER. Thank you, sir.

Representative HOLIFIELD. Dr. Kellogg.

Dr. KELLOGG. Are there more questions about this?

Representative HOLIFIELD. I think we will not need the charts.

Dr. KELLOGG. This closes my presentation. The analysis by Mr. Shafer was the last thing I wanted to present to the committee.

Representative HOLIFIELD. All right. Before you leave the stand, as an experienced student in this field, would you have anything to add to this presentation of Mr. Shafer on this particular point?

Dr. KELLOGG. No; I think he covered it very well.

Representative HOLIFIELD. From your scientific background and professional standing, would you concur, in general, with the remarks that he made?

Dr. KELLOGG. Yes. The techniques which he used are, I think, fairly well accepted by those of us who are planning fallout calculations.

Representative HOLIFIELD. Are there further questions of Dr. Kellogg?

Thank you very much, Dr. Kellogg, for your appearance before the committee. Your testimony has been very valuable.

Our next witness will be Dr. Lester Machta, of the United States Weather Bureau, and he will continue the testimony on atmospheric transport, storage, and removal of particulate radioactivity.

Dr. Machta has previously testified before congressional committees, and is looked upon as one of the real experts in this field.

STATEMENT OF DR. LESTER MACHTA, METEOROLOGIST, UNITED STATES WEATHER BUREAU [2]

Dr. MACHTA. Thank you, Mr. Chairman. I thank you for the opportunity of continuing the meteorological presentation. The story which I would like to tell pertains to a meteorological prediction of global fallout, and how this is verified by actual observation, and, finally, what this may mean for us in the future.

The radioactive debris which has not fallen out locally is carried by the atmosphere to distances far removed from the point of the

[2] Meteorologist, U. S. Weather Bureau; associated with atomic energy and meteorology since coming to Washington in 1948, now Chief of the Special Projects Section. Born in New York, N. Y., in 1919, graduated cum laude from Brooklyn College in 1939. His meteorological training includes graduate work at New York University (master of arts, 1946) and at Massachusetts Institute of Technology (doctor of science, 1948). During the war he taught meteorology in both a civilian and military capacity for the Air Force. Member of Sigma Xi, Pi Mu Epsilon, the American Meteorological Society, and the American Geophysical Society. Recently been given a gold medal for exceptional service by the Department of Commerce. Publications in the meteorological literature are numerous and, in recent times, include papers on atomic energy and meteorology. Has been a member of many important Government committees, including the Advisory Committee passing on the meteorological safety of tests in Nevada. Has been instrumental in making the worldwide measurement of radioactivity part of the International Geophysical Year program. (Submitted by U. S. Department of Commerce.)

explosion before entering man's environment or body. Why are we interested in the details of this transport? Aside from the academic meteorological implications, there are at least two aspects of atmospheric transport which are of importance to these hearings.

First, if the radioactive debris remains suspended sufficiently long, radioactive decay can reduce the hazard of the particles. Unfortunately, evidence points to residence times in the atmosphere which are too short to allow for appreciable decay of such substances as strontium 90 and cesium 137 with their 27- or 28-year half lives. Second, and more important, is the question of the uniformity or nonuniformity of deposition over the earth. We will return to this point later.

Senator BRICKER. Is the half life of cesium 137 practically the same as strontium 90?

Dr. MACHTA. I believe so, sir. The radioactive debris which has not fallen out locally can be carried either by tropospheric or stratospheric winds. I should like to show a number of placards.

The first placard (1) shows the earth as a sphere bounded by a layer of air in which our weather takes place, known as the troposphere; the upper layer, which is separated from the troposphere by the tropopause shown here as the line, is the stratosphere. The break in the tropopause in the area in which J appears is believed to be frequently associated with the jet stream, a current of high-speed west-to-east winds. The everyday weather—cloudiness, precipitation—occurs only in the troposphere. We are well informed about the tropospheric air motions, but know much less about the stratospheric air motions. Two features are, perhaps, of importance. First, there is considerable turbulence and, hence, mixing in the troposphere. On the other hand, the stratosphere is rather smooth and, as far as we know, much less vertical mixing takes place there.

Second, in both the troposphere and stratosphere, the prevailing winds blow from west to east or from east to west, so that the transport occurs much more rapidly in an east-west or west-east than in a north-south direction.

The fate of the radioactivity that is left over after local fallout has ceased depends upon whether it has been left behind in the troposphere or forced into the stratosphere. Thus, as has been mentioned many times, high-yield explosions with their tremendous force throw the mushroom heads into the stratosphere, while the lower yield tests such as we conduct in Nevada remain in the troposphere. High air bursts of almost any yield can throw their debris into the stratosphere, also.

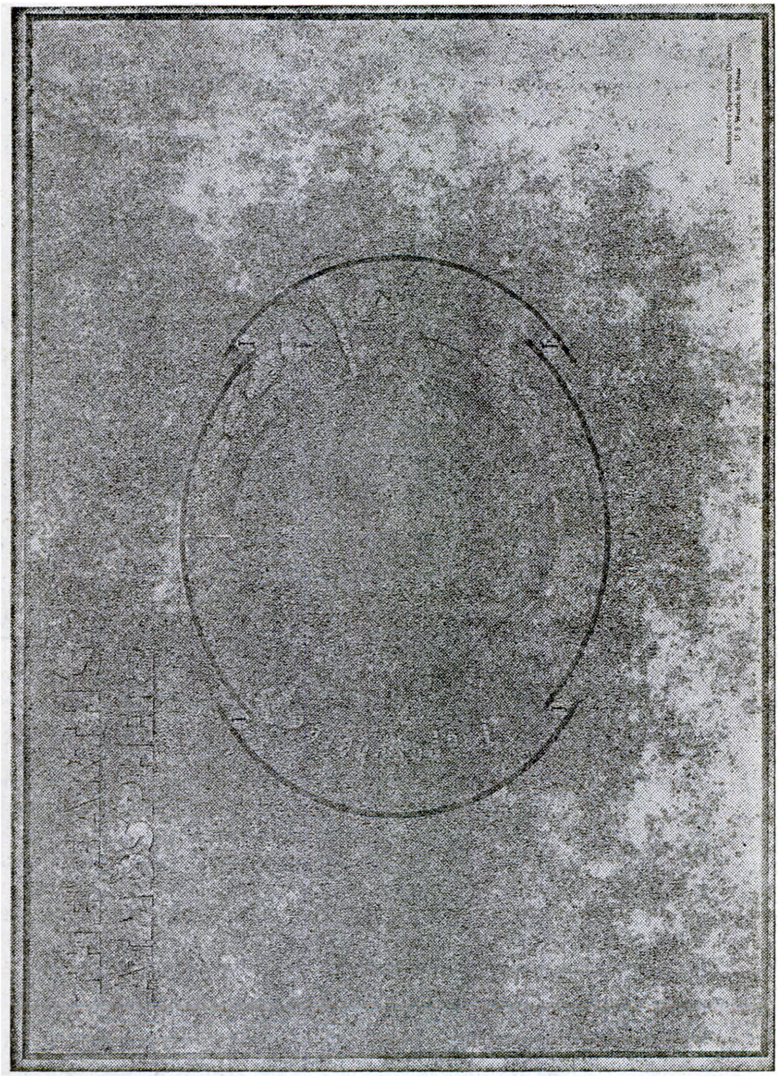
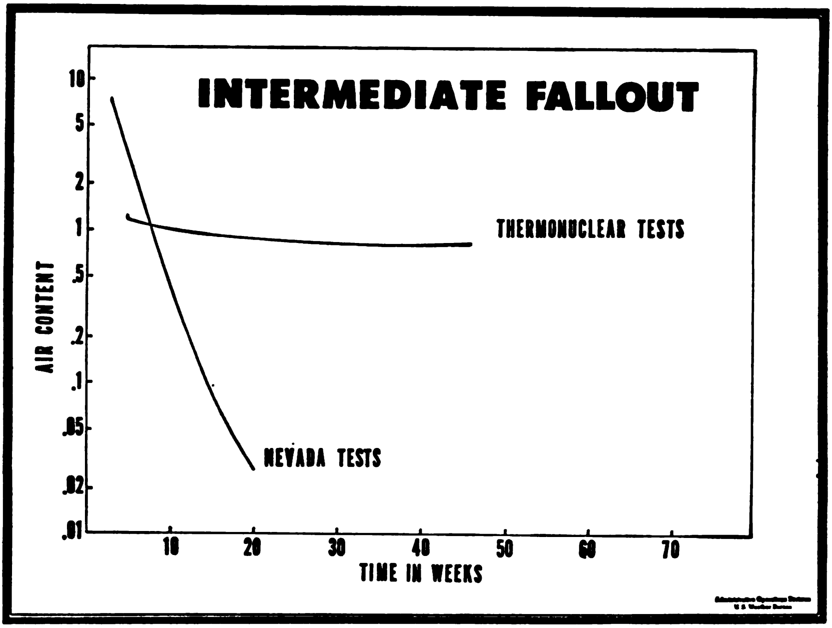


FIGURE 1

FIGURE 2



The next placard (2) shows the decrease with time of the atmospheric radioactivity from tropospheric and stratospheric sources. We see that for a Nevada A-bomb-type shot, in which air content is plotted against time in weeks after the explosion, the atmospheric radioactivity decreases rapidly with time after the test, so that within a matter of weeks, or at most a few months, the level of radioactivity is not appreciably above natural backgrounds. Precipitation and turbulence quickly remove the particles from the atmosphere. On the other hand, from the large-scale explosions, the thermonuclear tests, which throw their debris into the stratosphere, one finds practically no change with time. Although the same processes of removal are still active in the troposphere, the curve fails to show the decrease because there is a continual feeding of new radioactive debris from the stratosphere downward.

TROPOSPHERIC FALLOUT

Let us first look at the fallout from tropospheric debris. This placard (3) shows isolines of deposition from one of the Nevada test series. It is a Mercator map of the entire world, and the very heavy shading indicates the area around Nevada.

The brightness of the red coloring is proportional to the amount of fallout. I use the word "tropospheric" and "intermediate" interchangeably in this discussion. This picture illustrates the prevailing west-east flow by the fact that most of the radioactivity lies in the same belt of latitude as the original latitude of the explosion. The fallout is carried primarily to the east by the prevailing winds, and decreases in intensity as we get farther from the test site.

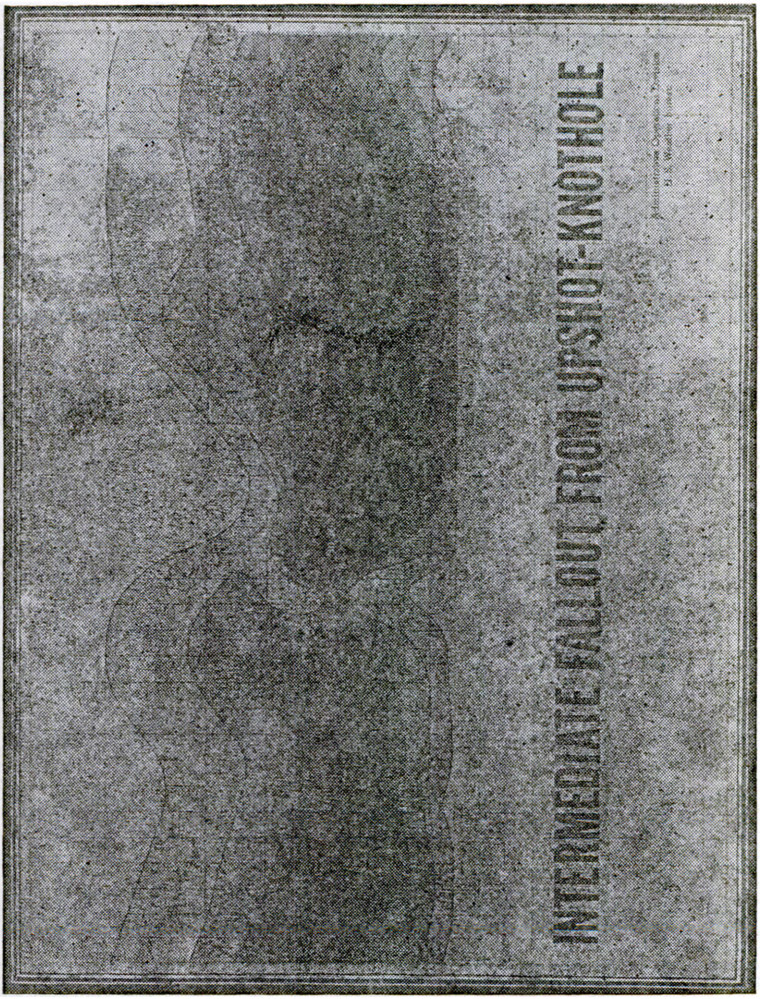


FIGURE 3

Representative HOLIFIELD. Before we leave that, Dr. Machta, I want to reread one of the lines you have given.

Dr. MACHTA. Yes, sir.

Representative HOLIFIELD (reading):

> This picture illustrates the prevailing west-east flow by the fact that most of the radioactivity lies in the same belt of latitude as the original latitude of the explosion.

Judging from the shading on your map there, and from this statement, then, there is a deposition in the Temperate Zone, assuming that is where these tests occur, where most of the people live, which is higher in intensity, although in different gradations, than it would be in either of the polar zones?

Dr. MACHTA. That is exactly correct, sir.

Representative HOLIFIELD. So when we talk about average global fallout, although it is a theoretical equation, it is an unreal evaluation in terms of the phenomena which actually occur?

Dr. MACHTA. I would like to take this up later. My main presentation actually deals with the nonuniformity of the fallout, and this is one of the aspects which gives rise to nonuniformity, namely, that the tropospheric fallout remains in the same latitude belt that the explosion takes place. But this is only one of the aspects.

Representative HOLIFIELD. My observation is, although it is only one of the aspects—my observation still is——

Dr. MACHTA. Is correct.

Representative HOLIFIELD. Is correct?

Dr. MACHTA. Yes, sir.

Representative HOLIFIELD. Thank you.

Dr. MACHTA. The next placard (4) provides the tropospheric fallout, the cumulative deposition for the first 35 days from the Castle-Bravo, the March 1, 1954, thermonuclear detonation in the Marshall Islands, showing once again that the fallout lies largely in the belt of latitude in which the explosion takes place. In the case of the large explosions, it is likely that the stem of the nuclear cloud provides most of the radioactive fallout in this period.

What fraction of the radioactive debris is deposited in the first few weeks or months—aside from the local fallout? For the typical Nevada tower shot, perhaps 75 percent, and for the high-yield ground explosion in the Pacific Proving Grounds, somewhere between 1 and 5 percent. These figures are very uncertain. Thus for the high-yield explosions, the delayed fallout is more important than the tropospheric fallout. This is because about 80 percent falls out locally, and when we add to this 5 percent more, we still have of the order of 15 to 20 percent, on the average, in stratospheric fallout that is still left after local and tropospheric fallout has ceased.

Thus, for the high-yield explosions, the delayed fallout is more important than the tropospheric fallout. It is evident that the tropospheric fallout is not uniform over the globe, as you just pointed out, sir.

STRATOSPHERIC FALLOUT

Finally, those particles which do not fall out locally or in the first 1 or 2 months, remain suspended in the atmosphere for a prolonged period—a matter of years, on the average. This has been termed

FIGURE 4

delayed or stratospheric fallout. These particles originate exclusively in the stratosphere. We do not know their size. The only thing we can say is that they must be quite small in order to remain airborne for such long periods of time. However, whether they are only carried downward from stratosphere to the troposphere by air motions or whether they also sink due to their weight, is not yet known for sure. From evidence I have seen, I would gues that there is a slow settling of the particles as fast as a mile or so per year—the principal removal is by downward atmospheric motions.

The radioactivity which is inserted into the stratosphere can be transported by 1 or 2 atmospheric processes; first, mixing, and second, direct transport. To understand these, consider an analogy—how a blob of ink in a bathtub can be transported, where the bathtub is considered to be the stratosphere. In mixing, one can imagine that the bathtub is stirred so that the ink quickly covers the entire water of the bathtub. In the second way, direct transport, one may imagine that a cup is dipped into the bathtub and part or all of the blob of ink is bodily lifted from one part and inserted into another part of the tub. It is quite evident that the first process, mixing, tends toward uniformity, whereas the second simply transports a blob from one place to another without materially changing the concentration. Our knowledge of the stratosphere is too limited to be sure of the comparative importance of these two processes. We are fairly sure that the vertical mixing—the exchange in the vertical—is very slow in the stratosphere due to the smoothness of the airflow.

I believe that the main movement of radioactive particles in the stratosphere is the result of direct transport. Mixing is so slow that the stratospheric distribution is nonuniform, even after 2 years.

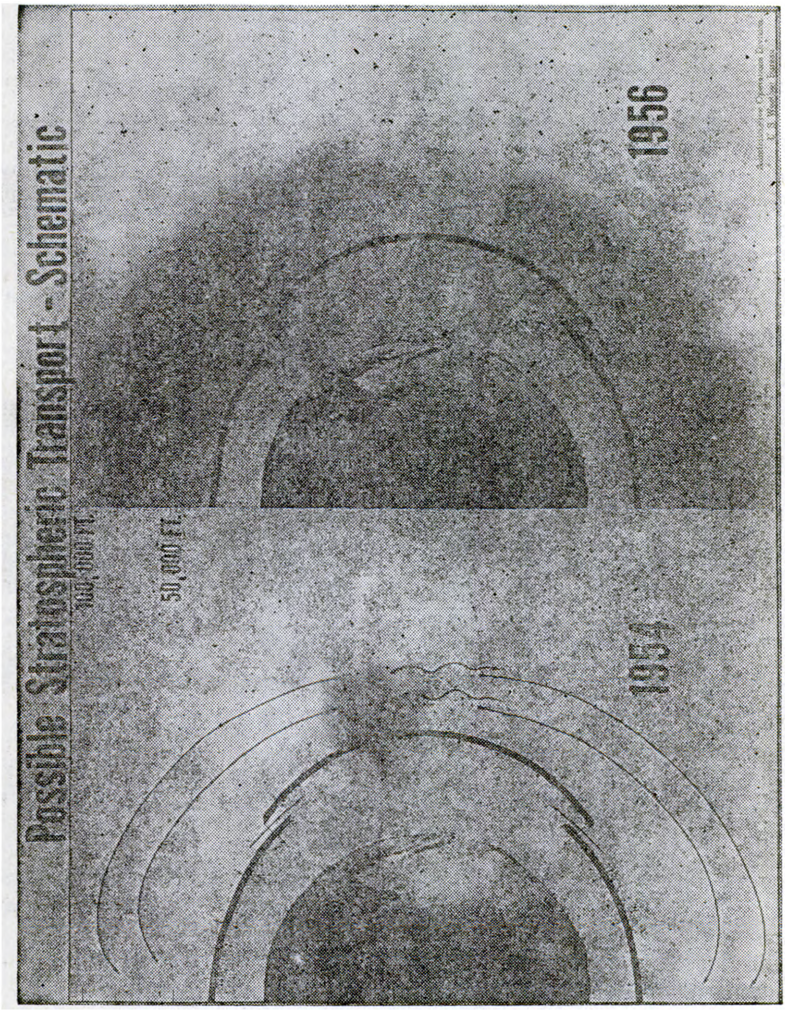
There are several meteorological hypotheses of direct transport or circulation in the stratosphere. I believe that a recent one proposed by Dr. Brewer of England best fits the facts available to me. Brewer is referred to as having said that there is—

> * * * slow poleward circulation of stratospheric air from the equatorial regions. During the course of this circulation, the air may be carried to heights of the order of 30 kilometers—100,000 feet—the height being greater in the winter hemisphere * * *.

The next placard (5) will show my interpretation of this circulation on the left-hand side. We see half of the globe and the tropopause as in earlier pictures. The H-bomb nuclear clouds in the Marshall Islands at 11° north, are shown schematically as one large red cloud. The wiggly arrows pointing southward indicate the possibility of southward mixing of a small part of the initial nuclear clouds into the Southern Hemisphere. In each hemisphere, we see the slow poleward circulation proposed by Brewer. I conclude that the bulk of the radioactivity in the stratosphere as in the Northern Hemisphere as illustrated on the right-hand side of the placard (5) by the heavy shading. The uncertainty in Southern Hemisphere stratospheric content is reflected by the question marks. I also believe that the bulk of debris is in the northern portions of the Northern Hemisphere rather than uniformly spread throughout the hemisphere.

Having now transported our radioactive debris to a different part of the stratosphere, let us ask where and how the debris may leave the stratosphere to enter the troposphere. Here, again, we have a

FIGURE 5


Possible Stratospheric Transport - Schematic
100,000 FT.
50,000 FT.
1954
1956

number of theories but no positive direct evidence. Ordinary vertical mixing processes will remove debris from the stratosphere through the tropopause and into the troposphere at all latitudes. However, it is believed that the break in the tropopause which one frequently finds in the vicinity of the jet stream, the region of very high west to east speeds in the temperate latitudes of both hemispheres, is a place of preferential exchange of air between troposphere and stratosphere. It is possible that it is in this area that much of the radioactive debris enters the troposphere. It has been also suggested that the formation of new and higher tropospauses, which may also occur with the passage of storms in the temperate latitude, will leave behind a considerable amount of stratospheric air to be incorporated into the troposphere. The tropopause of the polar regions of both hemispheres is often very indistinct, especially in winter and it has been suggested that removal can preferentially occur here. On the other hand, the equatorial tropopause is noted for its persistent intensity and a minimum of transport may occur through it. Meteorological theories, therefore, recommend more stratospheric removal in the temperate or polar than in the equatorial regions of the earth. The lengths of the arrows, which you see on the left-hand side of the placard (5) across the tropopause and through the tropopause break suggest the relative removal rates.

Once radioactive particles enter the troposphere, their stay is short. The ordinary weather processes and settling out of particles removes them from the atmosphere in a matter of weeks or a few months, according to best opinions. On the next placard (6), we see some of the removal processes. Impact on vertical surfaces, such as trees or forests, on grasses of wheatfields, on sides of houses, side of hills, and so forth, removes particles from the troposphere. Further, and by far the most important removal mechanism, appears to be scavening by falling precipitation shown on the right-hand side of the drawing.

THE UNIFORMITY OF FALLOUT

With these introductory meteorological remarks out of the way, let us now return to our question of uniformity of fallout over the globe by particles which are part of the stratospheric or delayed fallout.

The next placard (7) shows on the left hand side the individual soil sample strontium 90—that is the ordinate shows the amount of fallout—fallout values plotted against latitude from pole to pole, as of 1956. That is, we have a sample from one place with a certain latitude, having a given value, and it appears at the point on the graph which you see in front of you. The scatter of points is great, but the general sense of the points is given by the solid black line which is intended to represent a rough average latitudinal profile of strontium 90 fallout.

Mr. Eisenbud tomorrow will describe the limitations and uncertainties as well as the details of the soil sample results. The spread of points around the line reflects the peculiarities of atmospheric removal, that is areas of unusual rain or lack thereof, and idiosyncrasies of soil sampling and analyses which will be described later, by Mr. Eisenbud. One should view the picture broadly, since the uncertainty in individual values may be large.

FIGURE 6

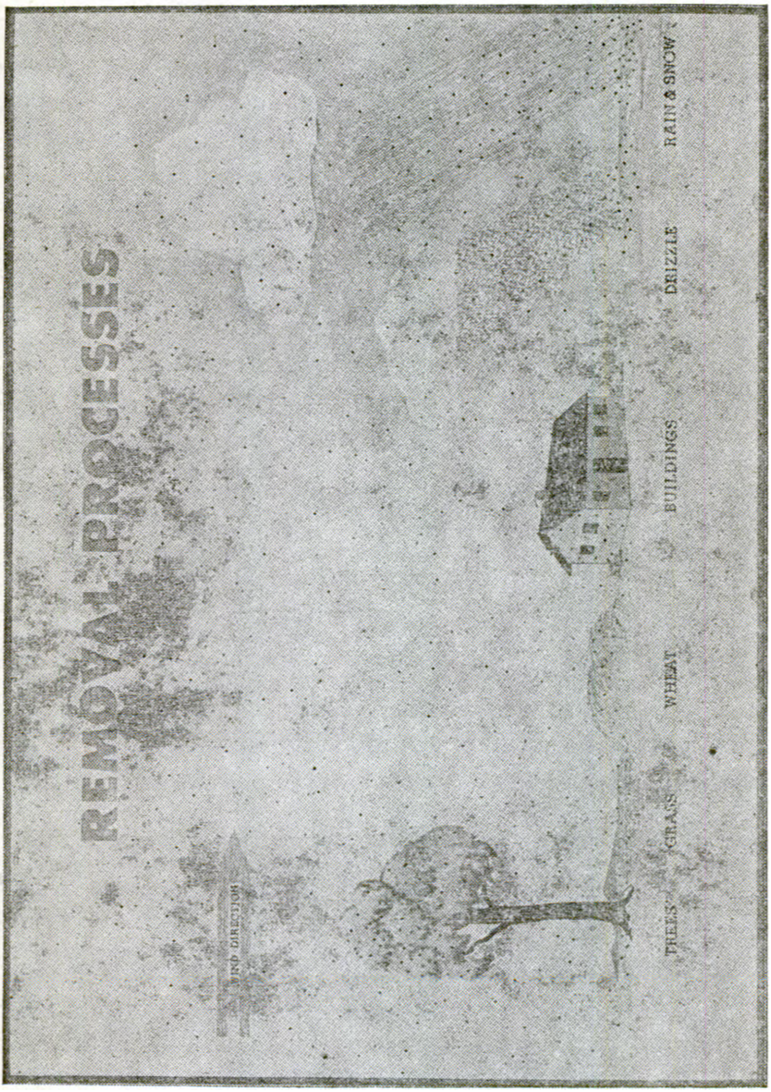
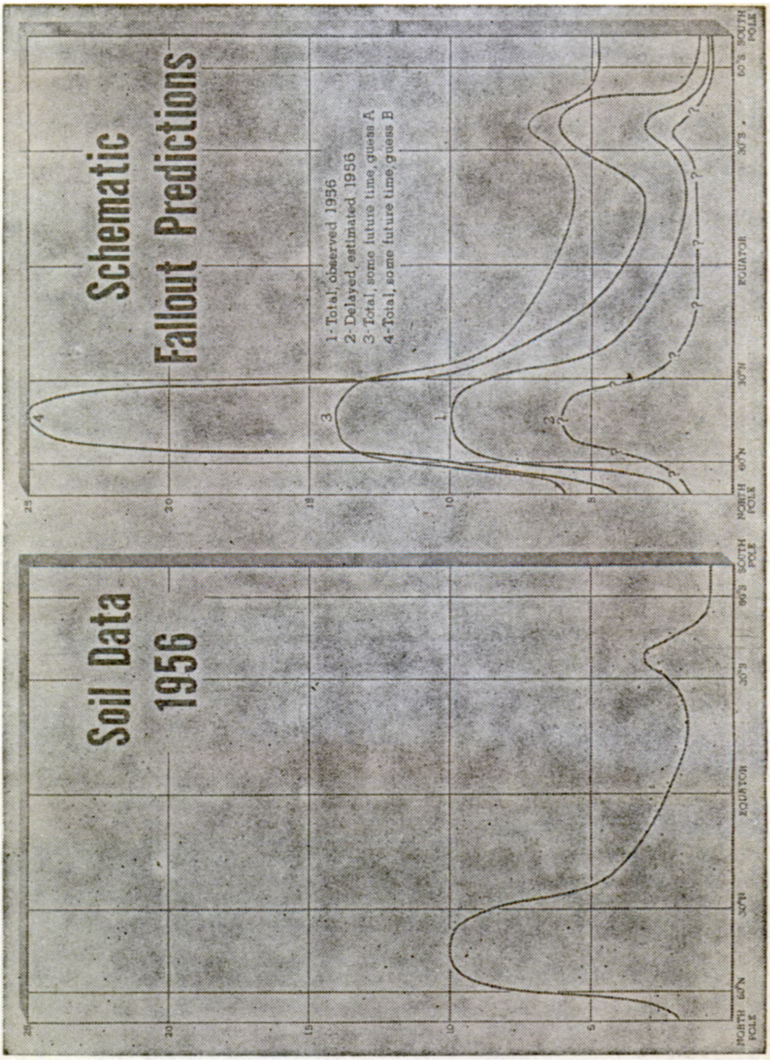

REMOVAL PROCESSES
WIND DIRECTION
TREES
GRASS
WHEAT
BUILDINGS
DRIZZLE
RAIN & SNOW

FIGURE 7


Schematic Fallout Predictions
1- Total, observed 1956
2- Delayed, estimated 1956
3- Total, some future time, guess A
4- Total, some future time, guess B
Soil Data 1956
25
20
15
10
5
NORTH POLE
60°N
30°N
EQUATOR
30°S
60°S
SOUTH POLE

While this picture fits the meteorological description which I have just offered, namely greater fallout in the north temperature latitudes or between about 25° and 60° N., it would also fit a picture based on the assumption that the greater amount of fallout in the temperate latitudes of the Northern Hemisphere comes from our small Nevada and the small Russian tests. The peak coincides almost exactly with the latitudes of these tests. However, the evidence suggests that the bump is the result of preferred fallout from stratospheric sources as well as from the small Nevada and Russian tests.

Senator BRICKER. One question at that point, Doctor.

Dr. MACHTA. Yes, sir.

Senator BRICKER. I noticed in the public press it is generally believed that the recent rainfall in Washington which brought down some radioactive particles was from the Russian tests. Could you confirm that?

Dr. MACHTA. To the best of our knowledge, the debris remains suspended in the atmosphere for a period of months, and this reported fallout very well could have been the result of the Russian tests.

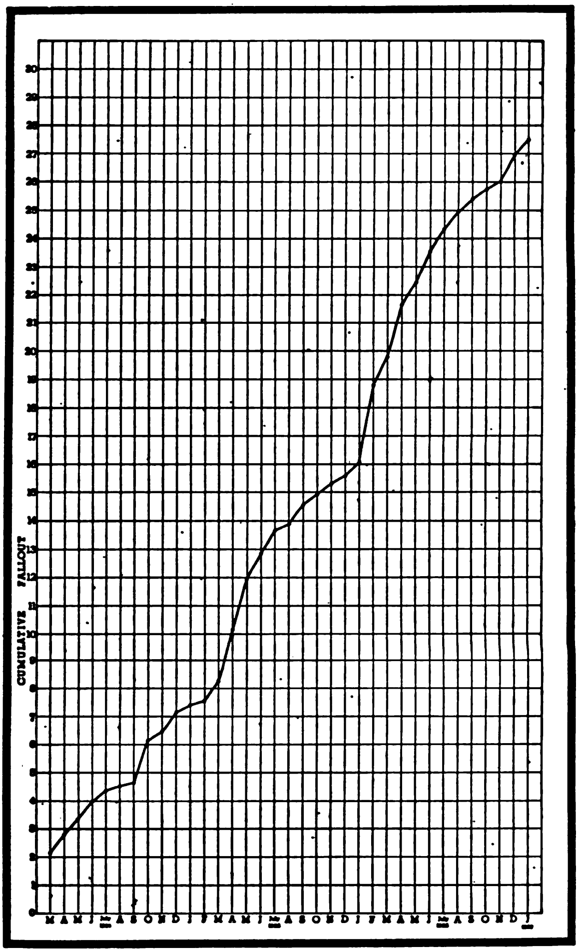
First, although the interpretation is uncertain, the magnitude of the bump calls for more fallout than could have been produced by our Nevada and the smaller Russian tests which have occurred up to the time at which this drawing is applicable. Second, we have a measure of the contribution of tropospheric fallout alone in the United States. In the fall of 1953, strontium 90 fallout was about 5 units compared with 25 or 30 units in the fall of 1956. By 1953, we had conducted 31 of 45 Nevada tests, several Pacific tests series nad the U. S. S. R. had detonated a number of nuclear bombs. The tropospheric fallout since 1953, in my opiinon, could not account for the 20 or 25 additional units of fallout which occurred betwen 1953 and 1956, considering the Nevada and tropospheric Russian fallout.

This graph shows the cumulative amounts of fallout in the vertical plotted against time in months along the horizontal, and shows how in New York City the strontium 90 fallout has been building up with time.

For a third argument, the curve of fallout versus time derived from collections by an exposed pot in New York City is shown in the next placard (8). The changes in slope at various times are evident. It has been suggested that the gentle slope is due to continual drip from the stratosphere and the cliffs due to the additional temporary tropospheric fallout.

Although this thesis is questionable, since the steep rises might be due to more rain at the time, and small tests were held in the U. S. S. R. during the periods of gentle slopes, one can get an idea of the ratio of tropospheric to delayed fallout using this concept. The gentle slope times the period of record yields the stratospheric fallout. The result is that about 50 percent of New York City fallout or 14 units is delayed and 50 percent tropospheric. Finally, in the case of rainwater collections at New York, shown on the same placard (8), it has been possible to analyze for a radioisotope of a short half-life from which a separation of young tropospheric and old stratospheric debris can be made. We find that most of this fallout must be attributed to delayed fallout. Unfortunately, the only radioisotope available,

FIGURE 8



New York City Pot Data

strontium 89, which has only about a 60-day half-life, is not measured as accurately as we would desire.

I have belabored this point because, in the final analysis, the evidence for nonuniform stratospheric deposition should come from fallout data itself and not from meteorological theories. I believe the evidence to be strong enough to indicate nonuniform delayed fallout, so that in future calculations, it should be taken into account. For example, consider the right hand side of the placard (7). The black solid line labeled (1) is a direct copy of the rough latitude profile shown on the left hand side. This is what we see on the ground from all sources in 1956 as best as I can interpret between the points.

The dashed black line (2), below the observed curve, is an estimate, little more than speculation, of the distribution of stratospheric fall-

out up to 1956. The difference between the dashed line and the solid line is what the Nevada tests, the smaller Pacific and the smaller Russian tests have contributed. The lower curve is what has dripped out of the stratosphere. The bump in the north temperate latitude is not as pronounced in the stratospheric fallout curve.

Two cases are now considered. The same amount of fallout has been added to the 1956 observed curve in both cases, an amount of stratospheric fallout about equal to the total already deposited. That is the amount given by the curve on the left, or from all sources. This is not to be construed as indicating that this is necessarily the amount yet to be deposited, but rather it is used for illustrative purposes only. The green curve labeled (3) shows a future profile if the same amount of fallout is added at all latitudes. This is the case of adding everything uniformly to the earth. The red curve (4) with the much more marked peak, shows a profile with pronounced nonuniform future fallout, as I would envisage it. The ratio of the peak value of the nonuniform to the uniform curves is almost 2; the nonuniform peak is about 3 times the world average.

Representative HOLIFIELD. If your theory is right on that, then this would have a direct effect upon the tolerance levels which we are talking about from a worldwide standpoint?

Dr. MACHTA. I am afraid I am not competent to talk about tolerance levels, only about meteorology.

Representative HOLIFIELD. It would more than double, then, the previous estimate as to contamination, would it not?

Dr. MACHTA. May I continue to give my viewpoint on what this actually does in the next paragraph?

Representative HOLIFIELD. All right.

Dr. MACHTA. In my view, these profiles represent the two extremes of distribution of future fallout if the present strontium 90 fallout is doubled. The true distribution should lie between the extremes.

I do not know, in fact, whether the upper curve is correct or the lower curve, but I think there is a possibility the upper one is correct, and we should say the truth lies between the two.

Representative HOLIFIELD. Is this an area where additional research is needed?

Dr. MACHTA. Yes; and I believe the AEC is conducting such research to try to pin this down.

Chairman DURHAM. How about your lower curve on the ground. That is correct, then?

Dr. MACHTA. I did not understand the question.

Chairman DURHAM. Your lower curve, is that accurate?

Dr. MACHTA. No. This is my speculation of what has come out of the stratosphere, too.

Chairman DURHAM. That is speculation too?

Dr. MACHTA. That is correct; and you may see it is estimated as of 1956.

There is one more additional calculation, and this is that nonuniform removal processes also result in nonuniform fallout patterns. Evidence suggests rainfall as being one of the major processes by which the particles are removed from the atmosphere. While agriculture occurs primarily in rainy areas, there are also rainy areas associated with the Icelandic and Aleutian low-pressure systems which

produce enormous amounts of rain over oceans and which are not associated with any agricultural areas. Thus, it appears as though the amount of rainfall in the milksheds and other agricultural areas of the temperate latitudes of the Northern Hemisphere contain very little more rainfall than the average rainfall in the latitude belt in which they exist. However, the rainfall and fallout relationship, is by no means perfect. It is my view, for example, that air masses in which rain has just begun produce more fallout than the air which has not been cleaned by previous rain.

SUMMARY

In summary, I would like to list our knowledge with regard to pertinent meteorology and the uniformity of fallout.

These facts are quite certain, and everyone agrees to them:

1. Much of the long-lived radioactive debris from high-yield tests remains airborne in the stratosphere for years, not weeks or months.
2. Tropspheric fallout lies mainly in the band of latitude of the test and is, therefore, not uniformly spread over the earth.
3. The observed soil data in 1956 reveals nonuniform strontium 90 fallout over the globe. On the average, there is more fallout in the north temperate latitudes.

These points can be presented with varying degrees of confidence:

1. After 2 years, debris in the stratosphere from our Castle test is still not uniformly distributed in the stratosphere. The upper air program of the Atomic Energy Commission can check this thesis in the near future.
2. The stratospheric removal rate and the stratospheric distribution of radioactivity with time depends on the latitude and height of the injection. For example, a contaminant introduced just above the tropopause at 50,000 feet mean sea level at, say, 45° north will come out of the stratosphere much more quickly and in a less dilute form on the ground in the temperate latitude than injections in the Marshall Islands at, say, 80,000 feet. In other words, where and when it will come out depends on where you put it in.
3. Delayed fallout has not been deposited uniformly over the earth. On the average, there is more delayed fallout in the north temperate latitude, even though the main injection was in the Tropics, that is the Marshall Islands.

Finally, we know comparatively little about these points and the conclusions are speculative:

1. The degree of nonuniformity of delayed fallout to date or in the future is unknown. One can put reasonable bounds on the peak value in the temperate latitudes given the amount of delayed fallout.
2. I do not know whether there is also a preferential region of stratospheric fallout in the Temperature Zone of the Southern Hemisphere, but I suspect there might be.
3. The extreme local variability in fallout due to rainfall and other meteorological differences is not known for certain. I would guess that areas as large as milksheds, for example, would not have more than 2 or 3 times the average fallout for the latitude. On the other hand, the lack of rainfall and other removal processes can result in almost zero fallout.

Due to the uncertainty in defining the degree of uniformity, I would recommend that predictions of future fallout be assigned a range of

values based on the 2 extremes just described, in the belief the truth should be between the 2 of them.

Representative HOLIFIELD. Thank you very much, Dr. Machta for that very important presentation. Would you please remain for questions?

Are there any questions at this time of Dr. Machta?

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. VAN ZANDT.

Representative VAN ZANDT. Doctor, describe the jet stream to us. Is it spotty, or is it worldwide?

Dr. MACHTA. By and large the jet stream completely emcompasses the earth. There are areas in which the speeds are greater. However, it is not the jet stream, as such, which is of importance to us, we think. What is important are the places where there are breaks in the tropopause because it is in these areas where horizontal mixing can take materials from the stratosphere and bring them into the troposphere and not have to undergo vertical mixing.

Representative VAN ZANDT. Are they associated with seasons?

Dr. MACHTA. They are usually associated with the jet stream. In the wintertime in the United States, for example, they are located 25° or 30° north. In the summertime, they are much farther north along the United States-Canadian border. The altitude of the jet is about 35,000 feet, give or take a few thousand.

Representative VAN ZANDT. You have had experience with these tests in the Pacific, have you not, Doctor?

Dr. MACHTA. Some experience; yes.

Representative VAN ZANDT. Can you recall at any time immediately after the test when any phenomena developed at high altitudes, as far as weather is concerned?

Dr. MACHTA. In the very local area in which the tests took place, I think there may have been a few showers, which perhaps may not have otherwise occurred. They never extended more than a few tens of miles, I believe, from the area of the tests. I actually was not present at any of the Pacific tests.

Representative VAN ZANDT. On the other hand, other than showers, was there any development of extraordinary winds?

Dr. MACHTA. None to my knowledge, sir.

Representative VAN ZANDT. Some years ago, I think you were the coauthor of a paper that concerned a study made at the request of, I think, this committee regarding the effect of atomic tests on the weather. Would you comment on that paper at this time?

Dr. MACHTA. Well, the research which we have conducted since then does not lead us to believe that our conclusions were in any way erroneous. We still find no connection between the testing of nuclear weapons and the weather. We must admit we are still ignorant on a number of features, and there always exists a remote possibility that something we are not aware of does produce weather. We know of none such.

Representative VAN ZANDT. Doctor, if my memory serves me correctly, you said at that time, that, based on data available, we are simply passing through another cycle as far as world weather is concerned. Do you still hold this view?

Dr. MACHTA. I think the fact we have gone from a drought in the Texas area to what you now observe is sort of evidence there are cycles in the atmosphere.

Representative PRICE. Mr. Chairman?

Representative HOLIFIELD. Mr. Price?

Representative PRICE. Dr. Machta, you made a very interesting negative statement in here. I wonder if you would state it more positively. If you do, it seems to me it would be one of the most salient points of your statement, one of the most interesting points.

On page 10, under speculation No. 3, you say:

> I would guess that areas as large as milksheds, for example, would not have more than 2 or 3 times the average fallout for the latitude.

Stating that positively, you would say they have about 2 or 3 times as much?

Dr. MACHTA. I do not feel the data which are available would allow one to be positive that there are areas in which the fallout is necessarily 2 or 3 times as great.

In the United Kingdom, for example, the fallout readings in 1955 varied, I believe, from about 3.3 up to 10, and they found a very definite, positive correlation with rainfall. The value of 3.3 occurred in a dry area, and the value of 10 occurred in a rainy area. However, what the average should have been for the overall area is not certain.

Representative PRICE. Of course, these milkshed areas you are talking about are the areas where you pick up the most calcium, and where strontium 90 will do the most harm to the human race.

Dr. MACHTA. Yes. I do not think I used the word "milkshed" to indicate the rainfall is unusual in milksheds. I just wanted to indicate areas of that magnitude and size as having on the average not more than 2 or 3 times the total amount of rainfall for the general latitude band. I do not believe I can be any more specific than to indicate an upper bound.

Representative HOLIFIELD. Will the gentleman yield?

Representative PRICE. Yes.

Representative HOLIFIELD. The point of your testimony there is that, of necessity, a milkshed must be located in an area where there is enough rainfall for the production of hay and other material which the animals eat, and you relate that to the fact that rainfall does bring down stratospheric, at least tropospheric radioactive particles. It is upon that sequence of suppositions that you make the statement there would be more radioactivity in that area rather than in a desert-like area where there is no rainfall?

Dr. MACHTA. The data quite clearly prove that this is so. In Brawley, in the Imperial Valley Desert, the values are running much lower than they are for the rest of the United States.

Representative HOLIFIELD. That comes from scientific readings?

Dr. MACHTA. These really are actual measurements of the soil contents, sir, as reported by Dr. Libby and others.

Representative HOLIFIELD. Mr. Durham.

Chairman DURHAM. Dr. Machta, what you said here this morning not only applies to the United States, but has application worldwide?

Dr. MACHTA. This is entirely so, sir.

Representative HOLIFIELD. Do you have any evidence or information regarding the readings of fallout in Japan?

Dr. MACHTA. I would rather that this question be answered by Mr. Eisenbud tomorrow, since he is aware of the details and of the limitations of the data, sir.

Representative VAN ZANDT. Dr. Machta, at this point, may I inquire as to whether or not the information you gave us this morning, is in any way, shape, or form coordinated with the information that may have been passed on to you by foreign countries?

Dr. MACHTA. I believe that one of the points plotted here is taken from a published report from the United Kingdom. Mr. Eisenbud, again, is in a better position to advise you of the information in foreign countries than I am.

Representative HOLIFIELD. We will go into that in detail with him, then, if you prefer.

Dr. MACHTA. Yes.

Representative HOLIFIELD. Because there are several important questions in that area.

Dr. MACHTA. I think he would be much more competent in answering these.

Representative HOLIFIELD. Are there any further questions?

Senator BRICKER. Mr. Chairman?

Representative HOLIFIELD. Mr. Bricker.

Senator BRICKER. Doctor, there are scientists who disagree with you about the intermingling in the atmosphere and the uniformity of fallout over the world ultimately, are there not?

Dr. MACHTA. I am not aware of any that disagree. I may point out that we had a meeting a number of weeks ago with the Meteorology Panel of the National Academy of Sciences Committee on the Biological Effects of Atomic Radiation, at which we discussed this, and I believe that the tenor of conclusions raised by that committee is what I have presented in my paper. I am not aware of other meteorologists who would take exception to my statement.

Representative PRICE. Mr. Chairman?

Representative HOLIFIELD. Mr. Price.

Representative PRICE. Dr. Machta, how did you make this study?

Dr. MACHTA. You mean what led me to the conclusions indicated here?

Representative PRICE. Not only to the conclusions, what is your actual operation, the method of sampling?

Dr. MACHTA. Of sampling the earth?

Representative PRICE. Yes.

Dr. MACHTA. I believe again this is what Mr. Eisenbud will discuss tomorrow. He can verify and talk of the validity of the data. I am sorry. I have simply been given the information, and I am interpreting it in terms of meteorology.

Representative HOLIFIELD. In general, you can say it is from sampling of the soil in different areas of the earth's surface?

Dr. MACHTA. Yes.

Representative HOLIFIELD. And from atmospheric samples?

Dr. MACHTA. Yes, sir. This is in general what it is. The details can be presented later.

Senator PASTORE. Would you say the same as to your conclusion or speculation as to the gathering of this cloud in the Northern Hemisphere, that is, even from the shots that were at Marshall Islands?

Or do you know it is there up in the stratosphere, this deep concentration you talked about?

Dr. MACHTA. In the stratosphere?

Senator PASTORE. Yes. I mean, in your statement here, you rely a great deal on Dr. Brewer of England.

Dr. MACHTA. Yes.

Senator PASTORE. To what extent do you rely on him? What do you know of your own knowledge scientifically as to the existence of this contaminated material in the stratosphere in concentrated form in the Northern Hemisphere, even though the injection was in the tropical part of the earth?

Dr. MACHTA. The answer to this question, sir, is that I do have information which I have not presented to the committee, which is classified, and which leads me to believe it very strongly.

Senator PASTORE. I do not want to get into classification now. I want to get how much of this is speculation, and how much of it is facts.

Dr. MACHTA. The hypothesis of Dr. Brewer is speculation, sir. However, I do have other classified information which leads me to believe that the picture which he has drawn is in fact the truth.

Senator PASTORE. You mean predicated upon the fact that you know it is there?

Dr. MACHTA. Yes, sir.

Senator PASTORE. Then, insofar as Dr. Brewer is concerned, you are merely speculating as to what brings it there?

Dr. MACHTA. Correct, sir. He was offered the mechanism which accounts for the facts at my disposal. I try to proceed as a meteorologist would, sir.

Representative HOLIFIELD. As a layman, let me ask you this question: Is it not possible that we can obtain radioactive particulate from the stratosphere to sample?

Dr. MACHTA. This is already being done by the Atomic Energy Commission, sir.

Representative HOLIFIELD. So we are not speculating on what there is in the stratosphere. We know what there is in the stratosphere, as far as our samples indicate?

Dr. MACHTA. No, sir; I do not think the answer can be given that way. The AEC upper air program as yet does not have results which can clearly prove or disprove the hypothesis I have presented. It will have this information in the very near future.

Senator PASTORE. We have Dr. Brewer's thesis or theory that there is an element or a phenomenon that even though this injection was in the tropical part of the earth, it has directed itself toward the polar regions.

Dr. MACHTA. Correct, sir.

Senator PASTORE. Now, in order to develop that theory, you have to know what is up there?

Dr. MACHTA. Correct, sir.

Senator PASTORE. Otherwise, the theory means nothing?

Dr. MACHTA. Yes.

Senator PASTORE. Therefore, you do know it is there?

Dr. MACHTA. From other information that is classified. It is not the information I was just describing to Mr. Holifield. That will be unclassified.

Senator PASTORE. We know it is there.

Dr. MACHTA. Reasonably well. I will not say positively, but information available to me and to others—incidentally not myself alone—would suggest that is where it is located.

Senator PASTORE. If we did not know it was there, Dr. Brewer's theory would not amount to anything?

Dr. MACHTA. That might be so, sir.

Senator PASTORE. Well, it is so, sir.

Dr. MACHTA. Well, there are other possibilities. For example, it conceivably could have——

Senator PASTORE. Otherwise you would have a hypothesis upon a hypothesis that leads to nothing.

(Conference between Senator Pastore and Representative Holifield.)

Senator PASTORE. Well, all right.

Representative HOLIFIELD. We understand, sir, that you are under certain prohibitions in making testimony in public session, and I am sure this matter can be resolved in executive session.

Dr. MACHTA. Surely.

Representative PRICE. Mr. Chairman?

Representative HOLIFIELD. Mr. Price.

Representative PRICE. In reading a recent speech delivered by one of the Atomic Energy Commissioners on variations of strontium 90, he said:

> For air-fired megaton weapons our present indication is that the fallout is almost worldwide; and for reasons of simplicity, and in the absence of better information at the present time, we work on the model that this is a uniform distribution over the entire world of the material that falls from the stratosphere.

The way I read your statement, you do not quite agree with that.

Dr. MACHTA. In my view, sir, the information which is just now available, and which has been presented here, and which probably has been developed subsequent to the statement you have made, suggests otherwise.

Representative PRICE. This is a very recent speech. This speech was delivered in the latter part of April of this year.

Dr. MACHTA. The data were given to me only about last week or so, sir. This is the data in which the particular model I formulate has been put together, sir.

Representative PRICE. This was delivered before the spring meeting of the American Physical Society by Dr. Libby. It is at variance with the statement you made here this morning.

Dr. MACHTA. I believe the entire Sunshine program—"Sunshine" referring to the stratospheric or lifelong hazard from radioactive debris—is one which is in continual development, and this is not the only area in which modifications of the model and theories have been worked out as new data are obtained.

Representative HOLIFIELD. Are there further questions?

If not, we will excuse you, and thank you very much, Dr. Machta, for a very challenging presentation.

Your article on World Wide Travel of Atomic Energy Debris will be inserted at this point.

[Reprinted from Science, September 14, 1956, vol. 124]

WORLD-WIDE TRAVEL OF ATOMIC DEBRIS

L. Machta, R. J. List, L. F. F. Hubert[1]

For centuries meteorologists have thought of exploring large-scale atmospheric circulations by means of tracers. The literature describes how man has successfully tracked fluorescent particles to a distance of 100 miles,[2] used radioactive tracers across the United States,[3] and followed volcanic ash and forest fire smoke over distances of the order of 1000 miles.[4] Only the dust from a major volcanic eruption, such as Krakatao, has been tracked on a truly global scale.

During two of the nuclear test periods in the Pacific Proving Grounds of the U. S. Atomic Energy Commission, sufficient radioactive debris was thrown into the atmosphere to be deposited in both hemispheres. Measurements of the deposited radioactivity were obtained from exposed sheets of gummed film. The details of the network and the sampling and measurement techniques have been described by Eisenbud and Harley.[5] It should be noted, however, that the deposition of particles on the adhesive surface depends either on the presence of precipitation or, in dry weather, on turbulence to assist the impaction of the particles on the horizontal surface of the paper. It is thus possible to have a cloud of radioactive particles pass two stations simultaneously and have only the station with rain note the presence of the particles overhead. The gummed-film method of collection is recognized as being as crude as it is simple.

The nuclear explosions are treated in this article, the Mike shot on 1 November 1952 and the Bravo shot on 1 March 1954. The shots were similar in that both are described as having had energy in the megaton range, both were detonated at or near the earth's surface on a coral island, and both had atomic clouds that penetrated into the stratosphere. To the meteorologist, the main difference of interest between the two events is the season.

WINDS

The winds acting on the two atomic clouds at the time of detonation are illustrated in Fig. 1. The wind structure has been estimated, when necessary, from observations at nearby locations and times. On both days the tropopause was found at an altitude of about 55,000 feet, and it separated winds blowing from different directions. The easterly winds above the tropopause increased in speed to the highest altitude of the available wind information for the Bravo shot, while for Mike the easterly winds decreased in speed and ultimately changed to westerly winds. The easterly winds in the trade-wind layer, the moist maritime air mass lying near the sea, extended up to about 20,000 feet during the detonation of the Mike device, while for the Bravo shot they were below 10,000 feet. Between the trade-wind layer and the tropopause, one normally finds westerly winds. During the Mike shot these westerlies were temporarily interrupted and became southerly winds, while for the Bravo shot they were toward a more normal bearing.

In Fig. 2 is found the approximate area covered during the early days by that part of the nuclear cloud from the Mike shot which was located below the tropopause. The shaded areas in Fig. 2 have been deduced from meteorological considerations alone, and, in many cases, are subject to considerable uncertainty. Shading was discontinued when the meteorological data no longer warranted any reasonable estimate of the path. The light winds and sparsity of upper-wind observations have made tracing the upper tropospheric portion of the Mike cloud

[1] The authors are on the staff on the U. S. Weather Bureau, Washington, D. C.
[2] R. R. Braham, B. K. Seely, W. D. Crozier, Trans. Am. Geophys. Union 33, 825 (1952).
[3] R. J. List, Bull. Am. Meteorol. Soc. 35, 315 (1954).
[4] H. Wexler, Weateherwise 3, 129 (1950).
[5] M. Eisenbud and J. H. Harley, Science 124, 251 (1956).

particularly uncertain. For this reason, the time of passage across the North American mainland is unknown. Tracing was discontinued on 7 November. The tradewind portion of the nuclear cloud appears to have split south of Japan, the upper portion (near 20,000 feet) curving around a Pacific high cell and entering the United States about 9 November.

The estimated meteorological path of the Bravo cloud is shown in Fig. 3. The upper tropospheric portion of the nuclear cloud was traced to the Central American area by about 5 March, and an offshoot extending northward into the United States at about 20,000 feet was detected approximately 1 week later.

Differences between the paths of the Mike and Bravo clouds are evident from Figs. 2 and 3. In part, the differences are seasonal and in part due to the specific meteorology for the shot days. Thus, in November the mid-tropospheric westerly winds are not as strong as they are in March, and they are located farther north, on the average. Further, in November one finds an anticyclonic circulation not far from the Marshall Islands which is not typically present in March. The shallowness of the trade-wind layer during the Bravo shot is an example of a feature unusual for the region during any season.

There has been no attempt to track the stratospheric portions of the atomic cloud because of the sparsity of wind observations at these altitudes. Evidence from numerous isolated high-level winds, not necessarily obtained during the periods of the two nuclear tests, suggests a path that would travel around the earth at about the same latitude as the point of origin. It is interesting to note that in no case was it imperative to rely on stratospheric transport of the nuclear debris to account for the earliest arrival at any point, for the transport of the nuclear cloud in the troposphere appeared to account for the first observations of radioactivity.

An attempt to determine the earliest arrival time at the ground at each point of observation has been undertaken. The results, which are shown in Figs. 2 and 3 as the number of days after the shot day, should in many cases be viewed with caution. First, in many of the stations in the Southern Hemisphere, the deposited activity was so low that it made the arrival date almost meaningless. Second, despite elaborate precautions, it is likely that some gummed films were contaminated during handling. Finally, as noted in the second paragraph the apparent arrival time of the cloud at many stations coincided with rainfall, suggesting that the nuclear cloud may have been overhead some time earlier but that precipitation was required to bring its activity to earth.

FALLOUT

It is noted that, in accordance with the meteorological estimates, the fallout over the United States progressed roughly from west to east during the Mike shot. Fallout from the Bravo event did not appear at the West Coast stations in the United States until 2 weeks after one of the cloud protuberances entered the central United States. Of perhaps greatest interest, although also of greatest doubt, are the comparatively early arrival times in the Southern Hemisphere. Thus, for example, a literal interpretation of the chart reveals that every station in the Southern Hemisphere showed an earlier arrival time than did the United States West Coast stations for the Bravo case. Also of interest are the comparatively late arrival times for the mid-Pacific stations west of the Hawaiian Islands during the Mike fallout. These stations were south of one branch of the nuclear cloud and north of the other.

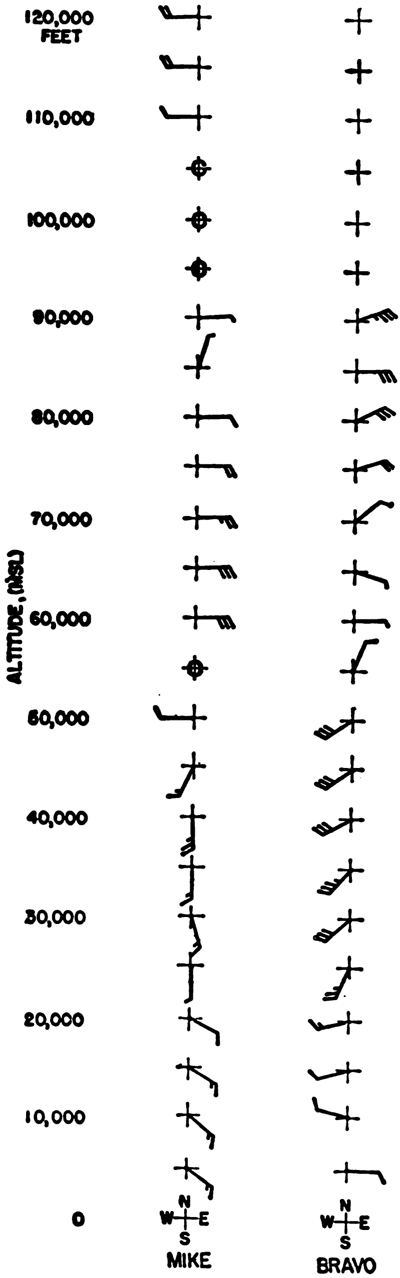


FIGURE 1.—Upper winds at shot time. Arrows blow with the winds, and barbs indicate wind speed; full barb, 10 knots; one-half barb, 5 knots.

The actual fallout at each station and an analysis of the data are shown on Figs. 4 and 5. The units are cumulative decayed beta activity for the first 35 days following each event and are approximately equivalent to millicuries per 100 square miles (the values have not been corrected for the efficiency of the gummed film.) Several features that differentiate the two maps should be noted. First, an average value for all United States and Canadian stations was obtained for the Mike shot, as opposed to values for individual stations during the Bravo shot. Second, the isolines located between points on the West Coast of the United States and points in the Western Pacific Ocean are also based on fallout observations obtained from transport vessels for Bravo. Finally, as is evident, the network was expanded between the two events, primarily in an attempt to locate stations in rainy areas. In many cases, when the period of record is incomplete or the data are suspect, parentheses have been placed around the number. No attempt has been made to reconstruct the isolines for the fallout that occurred within the first 24 hours of the shot.

The comparatively small values obtained at the Southern Hemisphere stations especially during the Mike shot, are immediately evident from the fallout maps. The northern part of the Northern Hemisphere, however, received equally small depositions. The distribution of fallout for the Pacific stations appears to be consistent with the features of the meteorology described, although the branching of the cloud south of Japan in the Mike pattern is based only on scanty observational evidence.

It is apparent that radioactive debris produced by nuclear explosions does not possess all the desired attributes of a tracer for studying global circulations. Information concerning the magnitude and distribution of the radioactivity that remains airborne after the initial fallout is not available. The debris, being particulate, is washed out of the atmosphere and cannot be strictly treated as a conservative property. Thus, for example, the depositions in the Southern Hemisphere may have been low because most of the debris was rained out as it passed southward through the Intertropical Convergence Zone. In addition, the most effective sampling program for the debris provides only the crudest measure of the fallout. Yet, despite these limitations, it appears that the meteorologist can obtain useful information by operating such a network of gummed films during nuclear test periods. Although it is not proposed that special nuclear tests be undertaken for meteorological purposes, it seems reasonable to expect even greater value from future tests using an expanded network and having detonations at other locations and times.

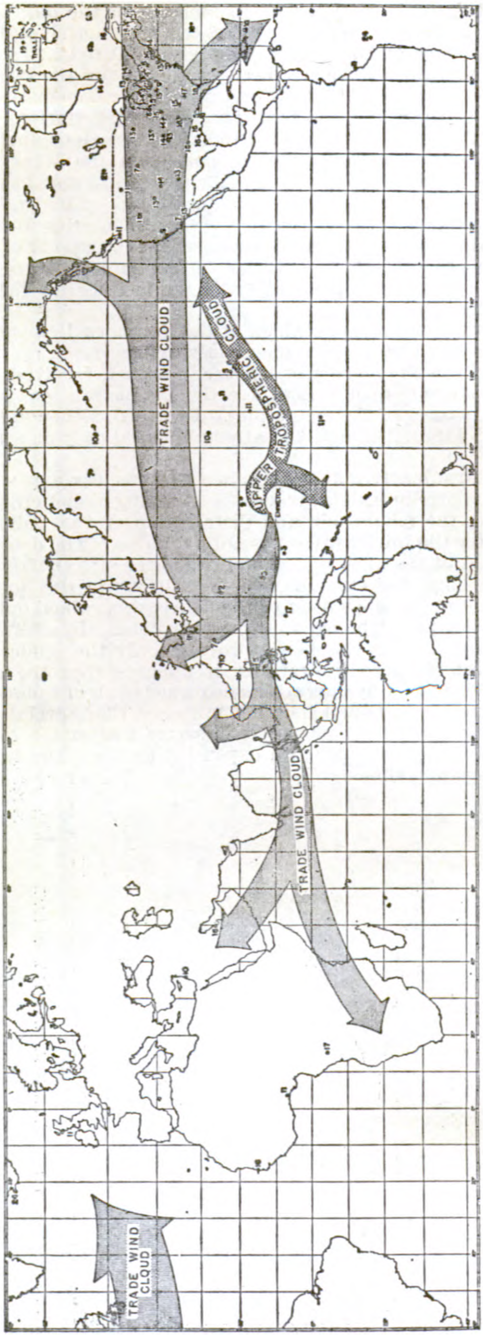


FIGURE 2.—Early history of the Mike cloud. The figures indicate the number of days between detonation and the first ground observation of fission products.

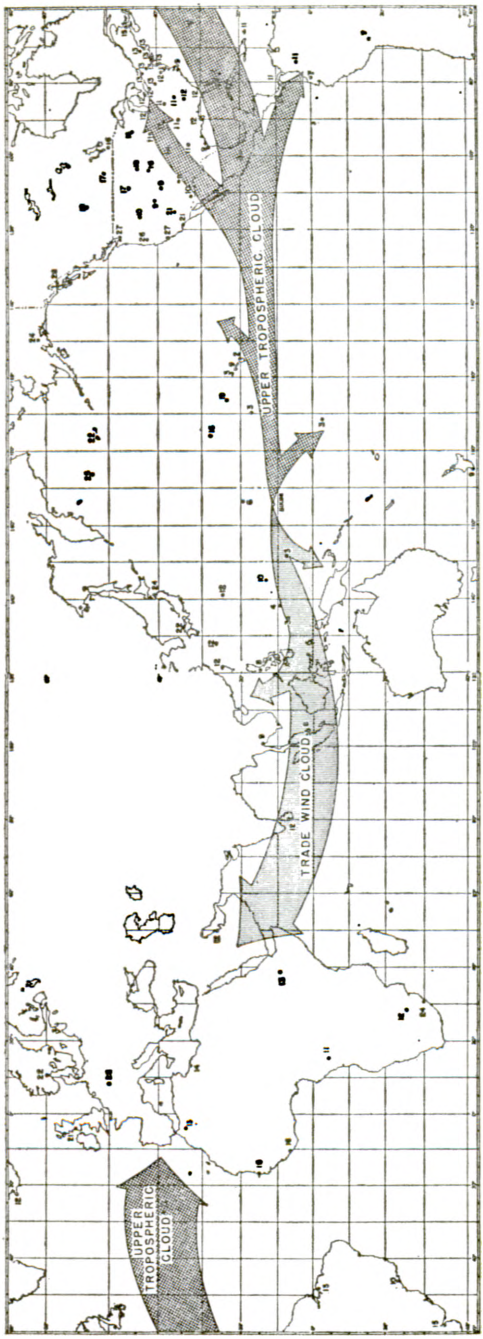


FIGURE 3.—Early history of the Bravo cloud. The figures indicate the number of days between detonation and the first ground observation of fission products.

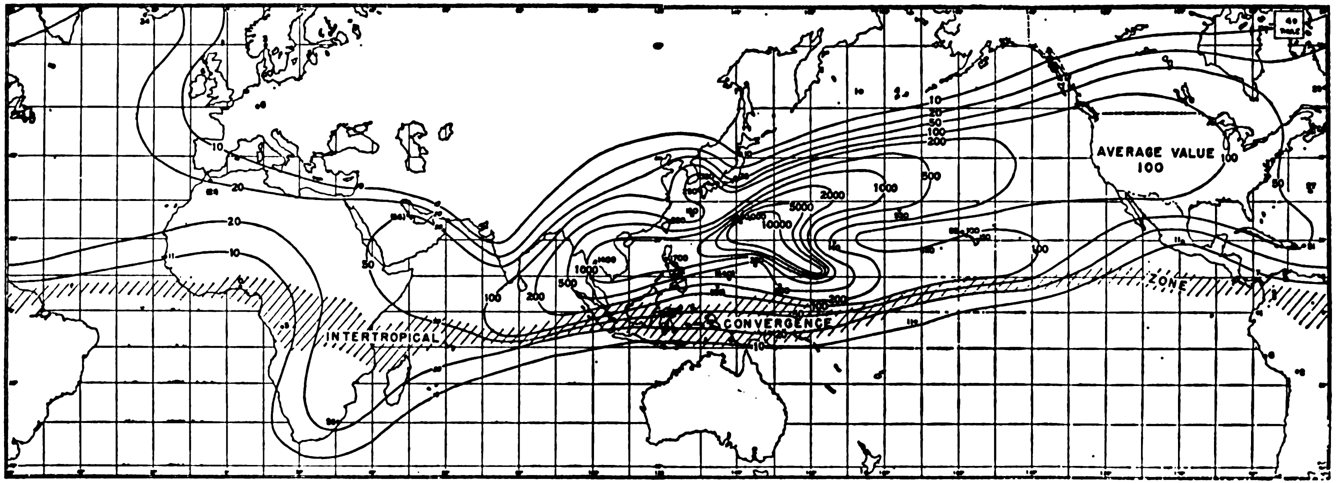


FIGURE 4.—Total radioactive fallout from the Mike cloud in the period from 2 to 35 days after detonation, in millicuries per 100 square miles. Hatching indicates the approximate November position of the Intertropical Convergence Zone, the belt of low pressure that tends to separate Northern and Southern Hemisphere air near the surface of the earth.

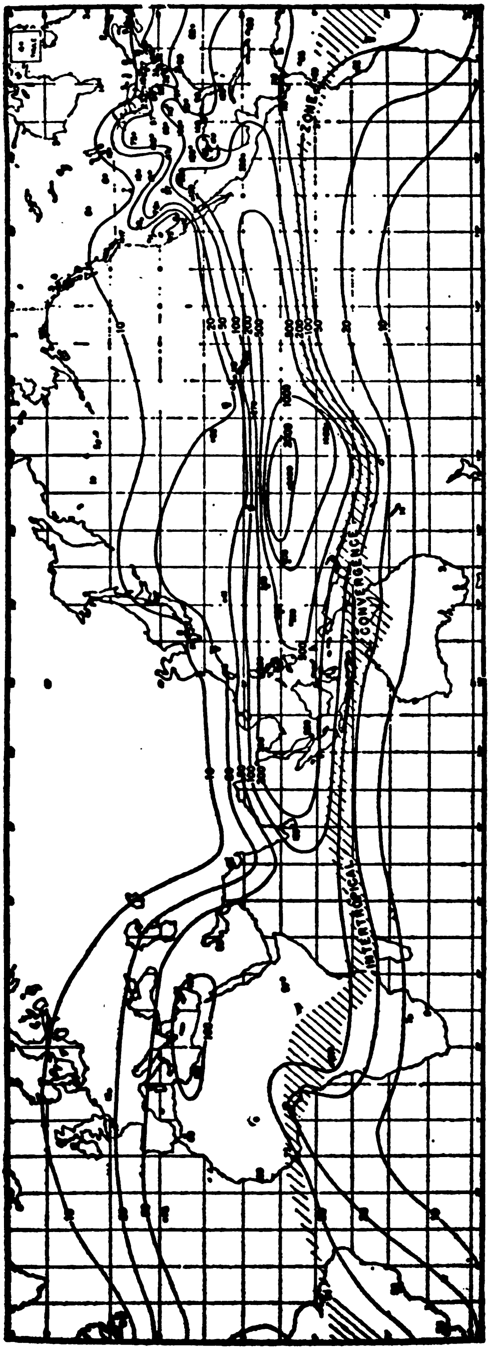


FIGURE 5.—Total radioactive fallout from the Bravo cloud in the period from 2 to 35 days after detonation, in millicuries per 100 square miles. Hatching indicates approximate March position of the Intertropical Convergence Zone, the belt of low pressure that tends to separate Northern and Southern Hemisphere air near the surface of the earth.

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Our next witness is Dr. Gordon Dunning from the Division of Biology and Medicine of the Atomic Energy Commission.

Dr. Dunning will testify on the topic of Local Fallout: The Mechanisms by Which It Can Affect Man and the Measures He Can Take to Minimize Exposure.

Dr. Dunning, we note that you have a distinguished biography, and your biography will also be inserted with your remarks.

The committee will be in order. The acoustics in this room are very bad, and we will ask our guests to be as quiet as possible so all can hear. We will ask the witness to speak up a little bit louder than usual in his testimony so that all the people can hear.

STATEMENT OF DR. GORDON M. DUNNING, DIVISION OF BIOLOGY AND MEDICINE, ATOMIC ENERGY COMMISSION [3]

Dr. DUNNING. Mr. Chairman, since there is so much relevant material on this subject, if it pleases the committee, I would like to submit the rather voluminous written report for the committee.

Representative HOLIFIELD. Without objection, this report and a letter from David T. Shaw, Assistant General Manager of the AEC will be received for printing in the record, and you may give such summary as you feel necessary.

(The documents referred to follow:)

RADIATIONS FROM FALLOUT AND THEIR EFFECTS

Gordon M. Dunning, Division of Biology and Medicine, United States Atomic Energy Commission, Washington, D. C.

FORMATION OF RADIOACTIVE PARTICLES

At the time of detonation of a nuclear weapon, about 60 different isotopes are formed, representing some 35 elements. Most of these give rise to decay chains consisting of several isotopes so that there may be 170 isotopes produced eventually.

In terms of activity, a 1-megaton detonation (1 million tons) TNT equivalent energy produced by fission of atoms will result in about 300,000 megacuries of radioactivity. measured 1 hour after the burst. In addition there may be present induced radioactive isotopes resulting from the reaction of neutrons released at the time of detonation, with natural materials such as soil and water. (A fusion reaction produces no radioactive substances directly but may cause induced activity because of its release of neutrons.) The total radioactivity of the products of a fission reaction will greatly exceed that of the activity induced in the soil or water. In the case where the fireball clears the ground, there will be a relatively small percentage of the total fission product activity deposited around ground zero and the neutron-induced activity probably will be much greater. However, none of the neutron-induced isotopes that might be produced in appreciable quantities have long half-lives.

Shortly after a nuclear burst, some of the radioisotopes combine with oxygen to form negative radicals while the halogens form halides which combine with the strongly electropositive elements to form compounds. The noble gases such as radiokrypton and radioxeon remain in the atomic state until they decay to a

[3] Date and place of birth: September 11, 1910; Cortland, N. Y. Education: State Teachers College, Cortland, N. Y., 1929–33; New York University (6 weeks), 1933; State Teachers College, Cortland, N. Y., 1934–36; M. S. (Sci. Edu.), Syracuse University, 1941; doctor of education, 1948. Work history: Teacher, Middletown, N. Y., 1937–41; U. S. Army (Lt. Col.), 1942–46; instructor, New York Agricultural and Technical Institute, Alfred, N. Y., 1947–48; teacher, Phy. & Phy. Sci., Indiana, Penn., 1948–51; AEC, Biophysics Rea. Anal. Div. B&M, 1951–53; AEC, Biophysicist, Division of Biology and Medicine, 1953–55; AEC, Radiation Effects Specialist, Division of Biology and Medicine, 1955—. (Submitted by the Atomic Energy Commission.)

daughter isotope which can form an oxide or halide. With the rapid cooling of the fireball, there is condensation of the isotopes and inert materials.

In the case of an air burst there will be available only small quantities of relatively fine particles of dust in the air and debris from the bomb casing to act as a transport vehicle for the radioisotopes. When the fireball intersects the ground the intense heat melts or vaporizes large quantities of soil and transports them aloft to act as carriers for the condensing radioisotopes. A characteristic toroidal motion sweeps this debris in and around the fireball where the melting temperature is reached and the particles come in contact with the fission products still in gaseous form. Subsequent cooling results in the radioactive isotopes becoming associated within and on the surface of the particles. It has been estimated that from 50 to 90 percent of these particles are between 50 and 1,000 microns in diameter. Of these, probably less than half of the larger particles falling out near the site of the detonation will possess any activity, since most particles will not reach sufficiently high temperatures to incorporate the radioactive materials, and dry, relatively cool, soil is a poor scavenger.

The high yield weapon detonated at the Pacific proving ground in the fall of 1952 resulted in a crater in the coral nearly a mile in diameter and 175 feet deep. Although a minor factor in the crater production might have been the compression of the coral by the blast, probably more than a hundred million tons of material were dislodged and thrown into the air. The exact results might not be reproduced for a detonation over continental land areas or built-up cities but in general the effects would be similar.

DISTRIBUTION OF RADIOACTIVE PARTICLES

For nominal bombs (in the range of 20-kiloton yield) the atomic cloud will not rise above the tropopause. (The tropopause marks the level below which is the turbulent airflow of the troposphere and above which is the relatively stable nonturbulent air of the stratosphere). The cloud from a high yield weapon will penetrate into the stratosphere as illustrated by the photograph on page 196 of the detonation during Operation Ivy in the fall of 1952. Two minutes after the explosion the cloud had risen to 40,000 feet and 10 minutes later neared its maximum height of over 100,000 feet. The smaller particles carried into the stratosphere will settle only very slowly until they reach the troposphere where the turbulent air and rainfall will carry them much more rapidly to the earth's surface.

The stratospheric storage is uniquely significant since the mixture of radioisotopes present there is enriched in strontium 90, the element of most concern for long-term hazards. This is because strontium 90 has a gaseous precursor krypton 90 with a half life of 25 seconds. Thus, at the time when conditions are optimum in the fireball for the oxides and halides to become associated with molten inert particles, only a fraction of strontium 90 has formed and the gaseous krypton parent is largely carried into the stratosphere. This results in the nearby fallout (within several hundred miles downwind) being partially depleted in strontium 90 while at more distant areas will be enriched.

The activity placed in the stratosphere circles and recircles the earth, first at the same general latitude as the burst and then slowly spreading laterally. At the same time there will be a slow diffusion into the tropopause. Initially, there will be more deposition in the same hemisphere (northern or southern) in which the burst occurred but after many months the rate of deposition may become more generally uniform over the entire earth's surface. In terms of strontium 90 about 10 to 20 percent of the activity remaining in the stratosphere may descend each year.

The distribution of the nearby fallout (up to several hundred miles downwind) from high yield weapons detonated near the earth's surface will be determined principally by particle size, initial position in the stem and cloud, and by the wind structure at various altitudes. The particle sizes and the distribution of these particles within the stem and cloud are principally functions of the yield of the bomb, the nature of the surface over which the burst occurs and the quantity of material vaporized. There are uncertainties in our knowledge but figure 1 presents one generalized concept of such an initial distribution. Although the cloud may be 100 miles in diameter the activity probably is not uniformly distributed, but rather is more concentrated near the central and lower portions of the cloud.

The influence of the wind structure at various altitudes on the ground distribution of the nearby fallout is qualitatively represented in figure 2. The last sketch

in figure 2 illustrates the effects of the "shearing" action of the winds when they travel in different directions and/or speeds at the various altitudes through which the particles must fall. Due to these wind conditions, it is possible to obtain fallout patterns ranging from one looking like an ink blot around ground zero at one extreme, to other situations where the fallout material is spread in a long thin finger. In general, the pattern may be expected to approximate an ellipse.

It is clear that such variables as wind conditions and the yields of nuclear bombs and their positions of detonation above different types of surface make it possible to predict fallout patterns precisely. In the case of nuclear weapons testing these variables are either known or can be predicted with good accuracy. However, in civil defense planning, certain assumptions concerning these variables must be used in estimating not only a single fallout pattern, but also possible overlapping patterns in the event of multiple detonations.

RADIATIONS AND FALLOUT

In describing and evaluating the effects of fallout patterns, it is necessary to consider the characteristics of the radiations emitted from the radioactive material. These are of three types: Gamma rays, beta particles, and alpha particles. Gamma rays are the emissions of principal concern, because of their greater penetrating power. The most energetic beta particles travel only a few yards in air and are of concern only when the fallout materials remain in contact with or in very close proximity to the skin, or when tne emitting materials find their way into the body. The amount of alpha-emitting isotopes associated with fallout material is considered to be of relatively minor consequence.

EXTERNAL GAMMA EXPOSURE

The gamma radiation dose that one may actually receive and the biological effects are dependent upon a number of factors, as follows:

1. Radiological decay

The decrease in radioactivity of fallout material roughly follows the relationship of $(\text{time})^{-1.2}$. This means that, for every sevenfold lapse of time after a nuclear explosion, there will be a tenfold reduction in dose rate. For example, if fallout occurs 1 hour after a detonation, such as might occur for 20 or 30 miles around ground zero of a high-yield weapon, the dose rate will be one-tenth of its initial value by the seventh hour. An additional tenfold reduction would require 7 times 7 hours or approximately 2 additional days of waiting. The theoretical[1] dose accumulated from the first to seventh hour after detonation would be approximately the same as that from the seventh hour until 1 week later. Further, this first-week dose would be about twice as great as the entire remaining dose possible for the lifetime of the activity (fig. 3). This rapid decay suggests the benefits of protection in the early periods after fallout and, where possible, delay of entry into a contaminated area.

In localities downwind where initial fallouts might not occur until, say, 24 hours after a detonation, the situation would be somewhat different, in that the radioactive decay would be slower. For example, consider the cases where fallout occurred at (*a*) 1 hour, and (*b*) 24 hours, after a detonation. One day after fallout the dose rate in the first case would be one-forty-fifth of its initial activity (1st hour), but in the second case the dose rate would have decreased to only slightly less than one-half of its initial activity (24th hour).

The above estimates are based on an assumed radiological decay of $(\text{time})^{-1.2}$. This is reasonably accurate for early periods of time after detonation, but the decay may start to vary significantly from the theoretical curve after several months have elapsed (fig. 4). At times later than shown in figure 4 the decay curve would be expected to flatten out due to the presence of long-lived cesium 137 (27-year half life).

2. Weathering and shielding effects

The magnitude and time of occurrence of weathering and shielding makes it impossible to establish a single establishment of a precise rule of effects covering all situations, impossible, yet, these factors are operative in determining the total exposure received from fallout.

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[1] Calculations of theoretical doses are based on (*a*) the radioactivity decreasing according to $(\text{time})^{-1.2}$, (*b*) there is no loss of activity by weathering effects, and (*c*) the person is out of doors for the time considered.

One example of weathering effects was after the March 1, 1954, fallout on the Marshall Islands in the Pacific. Figure 4 shows the gamma dose rates on the island of Rongelap over a period of about 2 years. In the first 10 days when the winds were light and there was no rainfall, the decrease in activity was roughly consistent with known radiological decay rate. The break between the 10th and 25th day undoubtedly represents the effects of rain which was known to have occurred in that period. Figure 4 suggests, however, that any further reduction in contamination by rainfall was slight.

An example of the effects of winds, occurred after one of the nuclear detonations at the Nevada test site in 1953. Strong winds blew almost at right angles across a narrow band fallout field on the 2d and 3d day after the detonation. The gamma dose rates at 3 feet above the ground on the 4th day were less than predicted by the relationship of $(\text{time})^{-1.2}$ by factors ranging from 3 to 6, while the activity of the soil samples collected on the first day and taken into the laboratory did decrease approximately as $(\text{time})^{-1.2}$. This effect of winds would not be expected to be as great for large contaminated areas of nonsandy soils.

Calculations of shielding and attenuation factors for different types of materials and theoretical calculations for various structures are plentiful (references through 11) (table 1), but more information based on actual field experience is needed. Limited data were obtained during Operation Teapot (spring 1955) where film badges were placed inside and outside of buildings for several days. The ratio of out-of-doors to indoors doses ranged from 1.3 to 7 with 1-room frame buildings providing the least attenuation factor and multiroom concrete block buildings the greater values. This program will be expanded during Operation Plumbbob as will the program of estimating personnel exposure by having a large number of people living around the Nevada test site wear film badges during and following the test series.

3. Gamma energy spectra

The relative biological effectiveness of differing energy photons and their varying depth-dose curves has been shown for X-rays (12). Similar results have been obtained for gamma rays as illustrated by one set of experiments (13) using burros where there was a shift of LD 50/30 values (lethal dose to 50 percent of the exposed animals who died in 30 days) from 684 roentgens with cobalt 60 (1.25 Mev mean energy) to 585 roentgens with Zr–95—NB–95 (~0.7 Mev mean energy). The gamma energy spectra from the mixture of isotopes in fallout is quite complex and is further complicated by the presence of scattered radiation, with its lesser energies, mixed with the direct radiation. Figure 5 illustrates the estimated gamma spectra at 3 feet above the ground following the detonation of March 1, 1954, at the Pacific Proving Ground (14).

4. Geometry of the source

The geometry of the source can make a significant difference in depth-dose curves and resultant biological effects. This may be illustrated by one experiment using swine where the LD 50/30 values for external dose decreased from 500 to 350-400 roentgens when the exposure was changed from unilateral to bilateral (the radiation exposure was first on one side only, then from opposite sides of the subject) (12). With a fallout field, the source probably would be more radial, thus a roentgen as measured in air would have more biological effect than one where the source is unilateral such as from the immediate radiations at the instant of a burst (although there is some scattered radiation), or from X-ray machines which have been used frequently with unilateral beams in developing data on biological effects of radiation.

5. Biological repair factor

It has been recognized that, in general, the longer the period over which a given radiation dose is delivered, the less is the resultant biological effect, except for such aspects as the genetic effects and life shortening. In situations of heavy fallout and relatively large potential radiation doses, the biological repair factor may be considered in estimating incapacitating and lethal doses. Since past experiments usually have been designed for other purposes, the data from these do not readily elucidate the rate of repair or the proportions of reparable and irreparable damage resulting from differently timed doses. Varying relationships have been demonstrated, depending upon the species or even the strain of animal, as well as the criteria selected for study, such as skin damage, life shortening, and LD 50 values. Our present knowledge does not permit establishment of a precise overall relationship for timed doses versus biological effects;

yet there are sufficient convincing data to permit an attempt at estimating the effect of this phenomenon.

Blair, Smith, Sacher, Davidson (15, 16, 17, 18, 19) and others have made extensive analyses of existing data on the effects of time-spaced doses for several species of animals. Generally, the recovery rate for larger and longer lived mammals, such as dogs, is significantly less than for mice. One estimate places the half-time recovery for man as long as 4 weeks (the time for one-half of the biological damage to be repaired) (19).

Since the estimated rate of biological recovery for man is relatively slow, this factor would have its greatest influence where a given total radiation dose was delivered over long periods of time. This would be the case where the fall-out occurred at later times after detonation rather than close-in areas where the fallout is essentially complete in about an hour after the burst, and about one-half of the total possible dose is delivered in the first 24 hours.

NEARBY FALLOUT FROM HIGH YIELD WEAPONS

As an exercise during the National Association of Civil Defense Directors meeting in Washington, D. C., on April 15–17, 1957, it was assumed the 4 bombs were dropped simultaneously as follows: 20 megaton on the Union Station Washington, D. C., 5 megaton on the National Airport, 20 megaton on Baltimore, Md., and 10 megaton on the Patuxent River Naval Air Station. The map on page 195 shows that combined fallout from these 4 bombs. The isodose rate lines are in units of roentgens per hour at 1 hour after detonation. By this time essentially all of the fallout would have occurred in these nearby areas.

Recalling that the radioactive decay is rapid for this fallout that occurs early after detonation, it becomes evident that if adequate protective areas are available it would be wiser for people to remain in place, rather than be exposed out of doors during the period of highest activity. Likewise, if a delay in movement is possible there will be more of an opportunity to evaluate the situation, and to then affect an orderly evacuation.

Since each situation will be unique, no rigid criteria will be proposed here for permissible exposures or for mandatory evacuation, since there may be other factors present as potentially hazardous as radiation. Rather, table 2 was developed to illustrate the kind of thinking and planning possible for civil defense. Three levels of exposure to civil defense workers are shown. The lowest of 25 roentgens is much higher than is permitted in peacetime, yet most personnel will retain their full working capacity even with exposures up to 100 roentgens.

Table 2 suggests several points relative to rescue. One of these, is that higher permitted radiation exposures to rescue crews would allow earlier entry into the contaminated area to affect first aid and general rescue work. Also, in the case of relatively little protection to the populace, there would be a saving in radiation exposure to them. On the other hand, people better sheltered, as illustrated in column V, would receive less total expoure if they stayed in the protected areas until the out-of-doors activity had decreased, and at the same time a delay of entry into the contaminated area would result in less radiation exposure to the rescue crews who might then be used again for other missions.

DISTANT FALLOUT PATTERNS FROM HIGH YIELD WEAPONS

The discussion above suggests the wide variability possible in distant fallout patterns from high-yield weapons and the great variation in radiation dose that one may receive due to shielding and weathering effects. Therefore, the following analysis is intended to be only a generalized one to illustrate the parameters and how they may operate in determining the radiation doses.

Consider the case of fallout from a high-yield weapon where people continue to live in an area without any special measures to protect themselves. Assume (*a*) for the first week following the fallout, the measured gamma activity decays according to $(\text{time})^{-1.2}$, for the second week $(\text{time})^{-1.3}$, and for the third week and thereafter $(\text{time})^{-1.4}$, and (*b*) the shielding factor afforded by normal housing will reduce the out-of-doors daily dose by 25 percent, and (*c*) the half-time of repair of biological injury is 4 weeks. Probably all of these assumptions are conservative, i. e., they overestimate the hazard. Based on these assumptions, figure 6 shows the dose rates at time of fallout or entry into an area that might produce an "effective biological dose" (the term given to the radiation exposure according to the above assumptions) of one roentgen (20). This

graph may be extrapolated to other readings. For example, if a fallout begins 5 hours after detonation and the dose rate at that time is 10 r. per hour, about 67 r. (effective biological dose) will be accumulated provided personnel continues to live normally in the contaminated area. This is computed as follows:

$$\frac{10}{0.15}=67$$

It is frankly recognized that in any single curve, such as that shown in figure 6, there are inherent a number of uncertainties. Criteria based on deliberate analyses of the relevant data, however, may be more valid than those determined under the duress of an emergency situation. Such a simplified graph might provide radiological monitors with a quick, even if rough, estimate of the potential hazards and thus assist in making decisions on questions such as evacuation.

Using figure 6, the idealized fallout diagram on page — was constructed to illustrate a possible pattern from a single high-yield surface burst (20).

The two innermost isodose lines shown were selected to suggest regions where (*a*) a significant percentage of personnel might be expected to die (400 r.) and (*b*) a few percent to become ill (100 r.), assuming continued occupancy of these areas with no special protective measures. These percentages would, of course, rise within the encompassed areas. The 50 r. effective biological isodose line has no unique significance, but suggests the magnitude of dose which might call for emergency measures against radiation exposures even in the face of other possible hazards. Table 3 shows the approximate areas encompassed by the three isodose lines. For areas where the fallout occurs a few hours or more following detonation, many days or weeks will be required to accumulate the major portion of effective biological doses, so that spot decisions involving additional hazards might not be necessary.

The question is frequently asked as to the time one must spend within a shelter or remain outside of a contaminated area. The answer depends upon a number of parameters, such as the criteria established for maximum permissible dose, as well as length of stay within the area of contamination . With knowledge of the magniture of the radiation levels present and an assumed rate of decay, $(t)^{-1.2}$, it is possible to plan and execute a short stay, even in a highly contaminated area. Planning for continuous occupancy requires more extensive analysis. The following data may aid in such evaluation.

The fall out map (idealized fallout diagram on page 196) and table 3 suggest the degree of radiation exposure received in continuous occupancy under normal living conditions beginning with the time of initial fallout. For those entering the contaminated zone 4 months after the first fallout, however, and then living there indefinitely, the area encompassed by the 50 r. effective biological isodose line will have shrunk from about 25,000 to 2,500 square miles. At such time (4 months after fallout), an area of about 1,000 square miles within the 50 r. isodose line might have the highest residual contamination, amounting to about 3 times the does rates at the periphery. The 0.3 r. per week, out-of-doors, isodose-rate line might extend to about the same position as the line marked "50" on the map.

As one attempts to extrapolate such data to 1 year after fallout, the analysis becomes still more difficult and uncertain. The data suggest, however, that if return is postponed to 1 year after fallout, the 50 r. effective biological isodose line will have disappeared. On the basis of these conservative estimates, the 1,000 square miles of highest contamination might have an out-of-doors dose rate of about 4 r. per week after 1 year. Similarly, personnel might accumulate a dose of about 100 r. for the first year following their return, and an additional 90 r. over the next 3 years, independent of the biological recovery factor. It is to be expected that this factor would be relatively great for such long periods of time, thus reducing the effective biological dose below 50 r. The 0.3 r. per week, out-of-doors, isodoserate line might encompass an area somewhat larger than the line marked 400 on the map (20).

For such effects as genetic, it is the total dose received that is important, since biological repair does not enter in such calculations. According to the conservative estimates of weathering and shielding used above, possibly several hundred roentgens might be delivered in the areas of heaviest contamination, from the end of the first year after the fallout occurred until the radioactivity had decreased to essentially zero. However, the foregoing analyses are based on passive factors only, not taking into account the actions of persons themselves in reduc-

ing contamination. If, for example, a permanent return into an area were postponed for 1 year after fallout, the radiological situation probably would have been adequately appraised, and decontamination operations initiated. (This subject will be discussed by others.) Moreover, with the return of a populace into a known contaminated area, more than normal precautions might be expected in regard to occupancy of the more protective types of buildings and reduction of time spent out of doors.

Of course, greater degrees of contamination could result from multiple, overlapping, fallout patterns. There is a need for continuing studies of these problems.

ENVIRONMENTAL CONTAMINATION

Radioactive contamination of an area will, of course, influence agricultural pursuits. An evaluation of these problems involves complex and difficult studies which will not be attempted here. In terms of civil defense, however, there is one phase that should be noted here.

The relatively heavy fallout that occurred on some of the Marshall Islands in March 1954 provides the most direct data. Since the time of this fallout there have been 10 radiological and biological surveys of these islands. All of these data are summarized in a report prepared by the Atomic Energy Commission and in press with the Government Printing Office (21).

There are strikingly wide variances in the degree of gross contamination in the soils and in the plant and animal life. Likewise, relatively large ranges in values were found for the individual isotopes in the plants and animals. Any conclusions, therefore, must be of only the most tentative and generalized nature.

The data do suggest that, in terms of strontium 90, the isotope of principal concern, this activity built up in the plantlife over the first year after fallout and then started decreasing slowly. By using very rough approximation, and extrapolations, the data suggest that, if plantlife had been growing in the area of great contamination, i. e., where the gamma dose rates extrapolated to H plus 1 hour would have been 2,500 roentgens per hour, it might have contained 10–30 microcuries of strontium 90 per kilogram of calcium, at 1 year. The corresponding values for the soils are several times higher. If an assumption is made that there is a discriminatory factor of about 4 for the Sr-Ca ratio in plants versus bones, the above data suggest possible levels of strontium 90 in the bones of animals from continuous consumption of this food of a few to several microcuries of strontium 90 per kilogram of calcium. The maximum permissible body burden for adult atomic-energy workers is 1 microcurie of strontium 90 per kilogram of calcium.

There is some confirmatory evidence for this crude evaluation. A variety of native animals were left on the island of Rongelap after the fallout in March 1954. They have been collected and sacrificed serially in time. Even after 2 years of continuous occupancy, it was reported that there were no pathological changes that could be ascribed to radiation (22). Their bones showed from about a one-tenth to a few tenths of a microcurie of strontium 90 per kilogram of calcium. Since the areas of highest contamination were about 12–14 times greater than Rongelap, an extrapolation would suggest values in the same range as above, i. e., a few to several microcuries of strontium 90 per kilogram of calcium if animals had lived in the area of great contamination.

The Pacific island soils have higher calcium content than most soils in the United States, and of course there are differences in the type of plantlife and in the climate. However, theoretical calculations suggest that the same fallout in the United States might result in something like 100 microcuries of strontium 90 per kilogram of calcium in the soils with the highest contamination. With assumed discriminatory factors from soil to bones of 10 or more, the implied eventual body burden of strontium 90 is of the same magnitude in the Pacific.

The uncertainty of these data, however, would not deny the possibility that for a similar fallout in the United States there might eventually result a body burden of 10 or more microcuries per kilogram, if people were to subsist entirely on food from the area of highest contamination. With maintained values 2 to 3 times this amount, it might be expected that a few percent might die of bone tumors after a latent period of 15 to 20 years. It would be expected, however, that the strontium 90 content in the food supply would slowly decrease with time. Any measures taken to reduce the uptake of strontium 90 into the food supply, and any supplemental foods from less contaminated areas would lower the strontium intake.

For civil-defense purposes, a full evaluation of the whole environmental contamination problem is needed, especially for the cases of multiple, overlapping, fallout patterns from many nuclear detonations which might occur under wartime conditions.

EXTERNAL BETA EXPOSURE

The second principal emission from the fallout material is beta particles. These are essentially high-speed electrons, of which even the most energetic travel only a short distance into the skin. (See the next section for discussion on internal exposures.) If large enough radiation doses are delivered by these beta particles, the skin may first show erythema (reddening) and then proceed to more serious damage. If a sizable fraction of the body should suffer serious skin damage from these beta radiations, the results would be similar to those from thermal burns, i. e., serious injury or death.

There is little doubt that "beta burns" can and have occurred. In the case of the Marshallese who were in the fallout from the detonation at the Pacific on March 1, 1954, most of the more heavily exposed showed some degree of skin damage, as well as about half of them showing some degree of epilation due to beta doses (22). However, none of these effects were present except in those areas when the radiation material was in contact with the skin, i. e., the scalp, neck, bend of the elbow, between and topside of the toes. No skin damage was observed where there was a covering of even a single layer of cotton clothing. In fact, the beta radiations emanating from the radioactive material on the ground should have been adequate to produce detectable skin damage (based on the amount of contamination present), yet this was not observed.

These findings indicate the obvious benefits to be expected from (*a*) remaining inside during the time of actual fallout to reduce the possibility of direct body contamination, or, if out of doors, to keep the body covered, and (*b*) early removal of the body contamination, since higher doses are delivered during early times after fallout.

The Marshallese were semiclothed, had moist skin, and most of them were out-of-doors during the time of fallout. Some bathed during the two-day exposure period before evacuation, but others did not, therefore, there were optimal conditions in general for possible beta damage. The group suffering greatest exposure showed 20 percent (13 individuals) with deep lesions; 70 percent (45 individuals) superficial lesions; and 10 percent (6 individuals) no lesions. Likewise, 55 percent (35 individuals) showed some degree of epilation followed by a regrowth of the hair. However, during this same period of time they received a whole-body gamma dose of 175-roentgens—a value approaching lethality for some of those exposed. These data, together with others, indicate that the external gamma radiation would be the controlling factor for making such decisions as to evacuation, although recognizing that any beta exposure would be an additional body insult.

INTERNAL EXPOSURES

The principal factor in evaluating long-term hazards from ingestion and inhalation is the doses delivered to the bones by isotopes of strontium. This subject will be discussed in detail by others.

The principal hazards from intake of relatively large amounts of radioactive fallout for several weeks immediately following a nuclear detonation are doses to the:

(*a*) gastrointestional tract, from the gross fission product activity,
(*b*) thyroid, from isotopes of iodine, and
(*c*) bone, principally from isotopes of strontium and barium-lanthanum.

The solubility of the fallout material is a major factor in determining the resultant fate, and thus radiation doses, within the body. The solubility varies, depending among other factors upon the surface over which the detonation occurred. The fallout material collected in soil samples at the Nevada test site has been quite insoluble, i. e., only a few percent in distilled water and roughly 20 to 30 percent in 0.1 N HCl. However, it would be expected that the activity actually present in drinking water supplies would be principally in soluble form. The water collected from a well and a cistern on the island of Rongelap about 21 months after the March 1, 1954, fallout, was found to have about 80 percent of the activity in the filtrate, but there was an undetermined amount that settled to the bottom. Other data suggest the material to have been about 10 to 20 percent soluble in water.

Figure 7 shows relative doses to the body organs, based on the assumptions that (*a*) 90 percent of the material is insoluble (when calculating doses to the

gastrointestinal tract), (*b*) all of the isotopes of iodine are soluble (when estimating doses to the thyroid), and (*c*) 25 percent of the ingested strontium isotopes and 7 percent of the barium-lanthanum reached the bones. It may be seen that ingestion of a given amount of fission product activity on the fourth and fifth days may result in nearly 2½ times the dose to the thyroid as to the lower large intestine. For a continuous consumption of fallout material from the 1st hour to the 30th day the ratio of doses is about 1.7. Table 4 indicates the amount of ingested fission product activity to produce 1 rad dose to the lower large intestine.

Analyses of past data strongly indicate the quantity of fallout material taken in for times immediately following a detonation: (*a*) by inhalation is very much less than by ingestion (unless of course one does not eat or drink), and (*b*) may come from surface contamination of the food rather than by the soil-plant-animal cycle.

How much intake is actually permitted depends upon many factors including the essentialness of the food and water to sustain life, and one's philosophy of acceptable biological risks and damages in the face of other possible hazards such as mass evacuation. By using table 4 and figure 7, an estimate may be made of the radiation doses that might result from the ingestion of a given amount of fission product activity. In determining how much actual ingestion, and thus the radiation doses that might be permitted, reference may be made to table 5 which suggests the biological effects from certain doses.

Such evaluations as attempted here are necessary and valuable for planning purposes, but once the fallout occurs the emergency of the situation may preclude immediate analysis of the food and water suppiles. Further, the abstinence from food and water because it might be contaminated could not be continued indefinitely. Therefore, the following three commonsense rules are suggested:

1. Reduce the use of contaminated food and water to bare minimum until adequate monitoring can be performed; use first any stored clear water and canned or covered foods; wash and scrub any exposed foods.
2. If the effects of lack of food and water become acute, then use whatever is available but in as limited quantities as possible. Whenever possible select what seems to be the least likely contaminated water and/or foodstuffs.
3. Since it is especially dsirable to restrict the intake of radioactivity in children, give them first preference for food and water having the lowest degree of contamination.

In an area of heavy fallout one matter to consider is the relative hazards from the external gamma exposure versus internal doses from ingestion of the material. One of the best evidences on this point was the fallout that occurred on the Rongelapese in March 1954. Those in the highest exposure group received 175 roentgens whole body external gamma exposure yet their body burdens of internal emitters were relatively low (22). These and other data suggest that:

If the degree of contamination of an area for several weeks immediately following a nuclear detonation is such that the external gamma exposure would permit normal and continuous occupancy, the internal hazard would not deny it.

This is based on such reasonable assumptions of (*a*) about 50 percent reduction of gamma exposure from out-of-doors doses afforded by living a part of each day in normal family dwellings, (*b*) washing and/or scrubbing contaminated foods, and (*c*) excluding areas where relatively little fallout occurred, but into which may be transported highly contaminated food and/or water. After longer periods of time during which the gamma dose rates in an originally highly contaminated area have decreased to acceptable levels, it probably would be necessary to evaluate the residual contamination for the bone seeking radioisotopes, especially strontium 90.

NUCLEAR WEAPONS TESTING

Since 1951, the United States has conducted 11 series of nuclear tests, 5 at the Nevada test site and 6 at the Eniwetok Proving Ground, for a total of more than 63 test detonations. A sixth series is currently underway at the Nevada test site. The fallout on the inhabitants of some of the Marshall Islands in March 1954 (which will be discussed by others) and fallout on some Japanese fishermen, have been the major effects off the testing areas. The only other off-site damage has been in the United States where the blast wave has caused minor structural damage for which about $45,000 has been paid in claims (23), and fallout that occurred on some horses and cattle grazing within 20 miles of ground zero causing skin burns for which about $15,000 was paid.

At the Eniwetok Proving Ground, where the larger devices are tested, the warning area covers nearly 400,000 square miles. This area is under constant sur-

veillance during the time of testing both by surface ships and by aircraft. Starting 2 days prior to a detonation, the search is intensified in the sector of probable fallout. If any transient ship is located in the warning area, it is advised to leave and the detonation is delayed until it is clear.

Fully manned weather and fallout prediction units are an integral part of the task force conducting the tests. Since the larger detonations in the Pacific require additional information on the upper air, new types of high-altitude balloons and missiles are used. Nine weather stations are established by the task force during the test series on islands around the site, in addition to the eight regular weather stations in operation on other islands.

After each detonation, aircraft track the radioactive air out for several hundred miles. Other aircraft, with special monitoring equipment fly over land and sea areas to measure any residual contamination.

Through the cooperation of the United States Public Health Service, trained monitors were present during Operation Redwing (spring 1956 series) on the populated islands of Wotho, Ujelang, and Utirik.

As would be expected, the delineation of fallout patterns in the wide expanses of the Pacific is difficult. For the immediate monitoring, aerial surveys are conducted as mentioned above, automatic equipment are placed on land areas, and a variety of ships, skiffs, and buoys are utilized. Following each test series, large-scale radiological and biological surveys are made. Data from these surveys have been summarized by the Commission in a document soon to be published by the Government Printing Office (21).

The Nevada test site covers an area of about 600 square miles, with the adjacent 4,000 square miles being a United States Air Force gunnery range (24). Surrounding these areas are wide expanses of sparsely populated land. For general safety, as well as security, the Nevada test site is closed to the public. Aerial and surface surveys are made to insure that no persons or animals wander into the area. Each nuclear detonation is publicly announced ahead of time.

As a part of the test organization there is an advisory panel of experts in the fields of biology and medicine, blast, fallout prediction, and meteorology. A series of meetings is held before the firing of each shot to weigh carefully all factors related to the safety of the public.

A complete weather unit is in operation at the Nevada test site, drawing upon all of the extensive data available from the United States Weather Bureau and the Air Weather Service, plus six additional weather stations ringing the test site. These data are evaluated for the current and predicted trends up to 1 hour before shot time. A shot can be canceled at any time up to a few seconds before the scheduled detonation. In the past, more than 80 postponements have been made due to unfavorable weather conditions.

Several measures have been used to reduce the radioactive fallout off the test site. First, of course, only small nuclear devices are tested at Nevada. Since the greater the height of the fireball above the surface the less is the fallout in nearby areas, the test towers have been extended to 500 feet, and during Operation Plumbbob (spring 1957) there will be at least one 700-foot tower. Also, a new technique of using captive balloons is being developed. Extensive tests are being conducted to determine the feasibility of detonating nuclear devices so far underground that all of the radioactive material will remain captured and thus, of course, completely eliminate any fallout.

Prior to each nuclear detonation a warning circle is established for aircraft, designed to provide control of aerial flights within the area of predicted path of the atomic cloud. A representative of the Civil Aeronautics Administration is assigned to the test organization and assists in establishing the controlled area. This may typically extend about 150 miles in radius and be in force for a period from about H minus one-half hour to H plus 10 hours. All aircraft are required to check through the Civil Aeronautics Administration before flying in this area.

After each nuclear burst, aircraft from the test organization track the cloud until it is no longer readily detectable. Behind this come other aircraft to plot the fallout pattern on the ground. This survey is repeated on D plus 1 day.

The off-site monitoring program during Operation Plumbbob (spring 1957) illustrates the extensive system organized not only to take numerous radiological measurements but also to provide close liaison with the citizens of nearby communities. The Atomic Energy Commission and the United States Public Health Service jointly organized a program wherein the areas around the test site are mapped out into 17 zones. A technically qualified man has been assigned to live in each zone. His duties consist not only of normal monitoring activities

but also, prior to and during the test series, of learning the communities and families in his zone, getting to know the people and having them know him. In addition to the 17 zone commanders, as they are called, there are 8 mobile monitoring teams on call to go to any locality to assist if needed or to travel to areas outside the 17 zones.

Four additional monitoring programs are also in operation. One of these projects is primarily of research nature yet provides radiation monitoring data out to 160 miles or more from the test site. A second program is a unique system of telemetering, whereby instruments are placed in about 30 communities around the test site and connected to commercial telephone wires. The operator sits at the control point and, by placing a normal telephone call, receives back signals that are translated in a matter of seconds into gamma radiation dose rates. A third project consists of automatic instruments located in another 15 communities that permanently record the gamma dose rates continuously from the beginning to the end of the test series. A fourth program consists of aerial surveys with special gamma detection instruments.

Extending outward from the test site across the country are 38 United States Public Health Service monitoring stations established in cooperation with the Atomic Energy Commission, and 11 AEC installations (see tables 6 and 7). In addition, through the cooperation of the United States Weather Bureau 93 stations in the United States make gummed paper collections of fallout (table 7). These gummed-paper collections are also made worldwide at 73 other locations by arrangement with the Department of State, United States Weather Bureau, United States Air Force, and Navy (table 9).

RADIATION EXPOSURES TO THE PUBLIC

The data and their evaluation concerning strontium 90 produced by nuclear weapons testing will be discussed by others at this hearing.

The external gamma exposures through September 1955 may be described briefly as follows:

"* * * With respect to the gamma dose, the average value for the United States is higher than it is for the rest of the world. The range of values in the United States is relatively narrow, 6 to 49 millirads, except for Salt Lake City (160), Grand Junction (120), and Albuquerque, N. Mex. (110). The representative dose for eastern United States is about 15 to 20 millirads, with slightly higher values in the Middle West and lower values on the west coast.

"The cumulative gamma dose at the foreign stations is in the range of 4 to 23 millirads, except for some of the Pacific islands, where the range is from 13 to 150 millirads * * *" (25).

These are infinity does, i. e., the maximum possible exposures one might receive if he were out of doors for the lifetime of the radioactivity, there were no weathering effects, and the activity decayed according to $(\text{time})^{-1.2}$. The actual radiation exposures will vary with changes in these conditions, but roughly may approximate one-half of the infinity dose.

In summarizing, the data on radiation exposures from fallout, the National Academy of Sciences-National Research Council report said (26):

"* * * it may be stated that United States residents have, on the average, been receiving from fallout over the past 5 years a dose which, if weapons testing were continued at the same rate, is estimated to produce a total 30-year dose of about *one-tenth of a roentgen;* and since the accuracy involved is probably not better than a factor of 5, one could better say that the 30-year dose from weapons testing if maintained at the past level would probably be larger than 0.02 roentgens and smaller than 0.50 roentgens. * * *

"The rate of fallout over the past years has not been uniform. If weapons testing were, in the future, continued at the largest rate which has so far occurred (in 1953 and 1955) then the 30 year fallout dose would be about twice that stated above. * * *"

Gamma radiation exposures near the Nevada test site are generally higher that the average for the United States. The map on page 195 shows the estimated gamma exposures accumulated from all tests at the Nevada test site. Table 10 lists all of the communities that have received sufficient fallout to result in an estimated 0.2 roentgens or more to the inhabitants. In addition to this list, the highest fallout level noted to date in an inhabited place around the Nevada test site occurred in 1953 at a motor court near Bunkerville, Nev., where about 15 people might have accumulated 7 to 8 roentgens if they had continued to live there indefinitely.

The National Academy of Sciences-National Research Council Report recommended: (26)

"* * * That for the present it be accepted as a uniform national standard that X-ray installations (medical and nonmedical), power installations, disposal of radioactive wastes, experimental installations, testing of weapons, and all other humanly controllable sources of radiations be so restricted that members of our general population shall not receive from such sources an average of more than 10 roentgens, in addition to background, of ionizing radiation as a total accumulated dose to the reproductive cells from conception to age 30. * * *

"* * * That individual persons not receive more than a total accumulated dose to the reproductive cells of 50 roentgens up to age 30 years * * * and not more than 50 roentgens additional up to age 40 * * *."

The National Committee on Radiation Protection and Measurement (27) has recommended that, "The maximum permissible dose to the gonads for the population of the United States as a whole from all sources of radiation, including medical and other manmade sources, and background, shall not exceed 14 million rems per million of population over the period from conception up to age 30, and one-third that amount in each decade thereafter. Averaging should be done for the population group in which cross-breeding may be expected." (27)

Since natural background radiation is roughly 4 roentgens per 30 years, the value for manmade sources becomes about 10 million man-rems for a population of one million. This particular unit was selected because of genetic considerations, that is, radiation doses to relatively large populations. The average exposure to only those communities around the Nevada test site that experienced the greatest amount of fallout (0.2 roentgens or more) is 0.6 roentgens for the 6 years since the regular nuclear tests were started. The round numbers are 58,000 man-roentgens for 100,000 people. If the area considered around the Nevada test site is enlarged to include 1,000,000 people the average exposure is about 0.1 roentgens for the 6 years, or at a rate of about one-half roentgen per 30 years. This is one-twentieth of the recommendation of the National Committee on Radiation Protection and Measurement for maximum exposures.

The highest measured concentration of fission product activity in the air off the Nevada test site was at St. George, Utah, during the spring 1953 test series, amounting to about 1.3 microcuries per cubic meter of air averaged over a 24-hour period. It was estimated that the radiation dose to the lungs from this activity was less than that delivered every month by naturally occurring radioactive isotopes in the air that we breathe.

The highest measured concentration of activity from fallout material in water off the controlled area was at upper Pahranagat Lake, Nev., in the spring of 1955 amounting to 1.4×10^{-4} microcuries per milliliter at 3 days after the detonation. This is one-thirty-sixth of the operational guide—an amount that is considered safe for continuous consumption.

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Table 1.—Rough estimate of reduction in gamma radiation within structures

Type structure	*Percentage of out-of-doors level*
One-story frame house:	
First floor	~50
Basement (center)	~10
Basement (side)	<10
Multistory reinforced concrete:	
Lower floors (away from windows)	<10
Basement	~0.1
Shelter (equivalent to 3 feet of earth)	~0.1

TABLE 2.—*Radiation exposure*

Permissible dose to rescue crew (roentgens)[1]	Time of initial contact with populace (hours after detonation)	Dose to populace while waiting rescue (roentgens)[2]	Total radiation dose to populace (roentgens)[3]	Dose to populace while waiting rescue (roentgens)[4]	Total radiation dose to populace (roentgens)[3]
1	2	3	4	5	6
100 r/hr line:					
25	5½	72	85	14	26
50	2½	40	65	8	33
100	1¼	10	60	2	52
300 r/hr line:					
25	16	320	332	64	76
50	8½	260	285	52	77
100	5	205	260	41	91
500 r/hr line:					
25	25	600	612	120	112
50	14	500	525	100	125
100	7¼	400	450	80	130

[1] Based on a 2½-hour mission to rescue crew.
[2] Assuming ½ of out-of-doors exposure.
[3] Assuming populace receives ½ of exposure to rescue crew.
[4] Assuming ⅒ of out-of-doors exposure.

TABLE 3.—*Approximate areas encompassed by the effective biological isodose lines shown in the map (top of p. 196)*

Isodose line (r):	*Approximate areas encompassed (square miles)*
50	25, 000
100	12, 500
400	5, 000

TABLE 4.—*Approximate fission product activities (microcuries per milliliter of gram × 10[2]) to produce 1 Rad dose to lower large intestine* [1]

Duration of ingestion (days)	Start of intake (days after detonation)							
	1 (1st hour)	2 (24th hour)	3	4	5	10	15	20
1	35	2. 5	1. 9	1. 7	1. 4	1. 1	1. 1	1. 0
2	24	1. 7	1. 1	0. 89	0. 81	0. 62	0. 57	0. 53
3	15	1. 3	0. 82	0. 65	0. 56	0. 41	0. 40	0. 37
4	13	1. 0	0. 65	0. 53	0. 46	0. 33	0. 30	0 29
5	12	0. 9	0. 57	0. 44	0. 39	0. 28	0. 25	0. 22
10	9. 2	0. 64	0. 40	0. 29	0. 25	0. 17	0. 14	0. 13
15	7. 8	0. 53	0. 33	0. 26	0. 21	0. 13	0. 11	0. 097
20	7. 5	0. 49	0. 29	0. 21	0. 18	0. 11	0. 089	0. 079

[1] (*a*) Activities computed at start of intake period. (*b*) Based on intake of 2,200 milliliters or grams of water and food per day for adults.

TABLE FIVE

SOME POSSIBLE BIOLOGICAL EFFECTS FROM RADIATION DOSES TO SPECIFIC ORGANS *

Dose (Rads)	Gastrointestinal Tract	Thyroid	Bones
10,000		Minor changes in structure	
	Permanent or serious damage — survival threatened		Tumor production
1,000	Tumor Production		
	Immediate effects such as nausea and vomiting	Potential carcinogenic dose to thyroids of few percent of children and adolescents	Minor changes in structure
100			

*Lesser short term effects would be expected from the same doses distributed in time.

TABLE 6.—*U. S. Public Health Service monitoring stations during operation Plumbbob (spring 1957)*

Albany, N. Y.
Anchorage, Alaska
Atlanta, Ga.
Austin, Tex.
Baltimore, Md.
Berkeley, Calif.
Boise, Idaho
Cheyenne, Wyo.
Cincinnati, Ohio
Denver, Colo.
El Paso, Tex.
Gastonia, N. C.
Harrisburg, Pa.
Hartford, Conn.
Honolulu, T. H.
Indianapolis, Ind.
Iowa City, Iowa
Jacksonville, Fla.
Jefferson City, Mo.
Juneau, Alaska
Klamath Falls, Oreg.
Lansing, Mich.
Lawrence, Mass.
Little Rock, Ark.
Los Angeles, Calif.
Minneapolis, Minn.
New Orleans, La.
Oklahoma City, Okla.
Phoenix, Ariz.
Pierre, S. Dak.
Portland, Oreg.
Richmond, Va.
Salt Lake City, Utah
Santa Fe. N. Mex.
Seattle, Wash.
Springfield, Ill.
Trenton, N. J.
Washington, D. C.

TABLE 7.—*AEC monitoring stations during operation Plumbbob (spring 1957)*

Berkeley, Calif: Radiation laboratory, University of California
Cincinnati, Ohio: General Electric Co., aircraft nuclear propulsion department
Idaho Falls, Idaho: Idaho Operations Office
Lemont, Ill.: Argonne National Laboratory
Los Alamos, N. Mex.: Los Alamos Scientific Laboratory
New York, N. Y.: New York Operations Office
Richland, Wash.: Hanford Operations Office
Oak Ridge, Tenn.: Oak Ridge National Laboratory
Rochester, N. Y.: The atomic energy project, University of Rochester
Salt Lake City, Utah: Radiobiology laboratory, University of Utah
West Los Angeles, Calif.: Atomic energy project, University of California, Los Angeles

TABLE 8.—*U. S. Weather Bureau fallout sampling stations in operation during Operation Plumbob (spring 1957)*

Abilene, Tex.
Albany, N. Y.
Albuquerque, N. Mex.
Alpena, Mich.
Amarillo, Tex.
Atlanta, Ga.
Bakersfield, Calif.
Baltimore, Md.
Billings, Mont.
Binghampton, N. Y.
Bishop, Calif.
Boise, Idaho
Boston, Mass.
Buffalo, N. Y.
Caribou, Me.
Casper, Wyo.
Charleston, S. C.
Cheyenne, Wyo.
Chicago, Ill.
Cleveland, Ohio
Colorado Springs, Colo.
Concord, N. H.
Corpus Christi, Tex.
Concordia, Kan.
Dallas, Tex.
Del Rio, Tex.
Denver, Colo.
Des Moines, Iowa
Detroit, Mich.
Elko, Nev.
Ely, Nev.
Eureka, Calif.
Fargo, N. Dak.
Flagstaff, Ariz.
Fort Smith, Ark.
Fresno, Calif.
Goodland, Kans.
Grand Junction, Colo.
Grand Rapids, Mich.
Green Bay, Wis.
Hatteras, N. C.
Helena, Mont.
Huron, S. Dak.
Jackson, Miss.
Jacksonville, Fla.
Kalispell, Mont.
Knoxville, Tenn.
Las Vegas, Nev.
Los Angeles, Calif.
Louisville, Ky.
Lynchburg, Va.
Marquette, Mich.
Medford, Oreg.
Memphis, Tenn.
Miami, Fla.
Milford, Utah
Milwaukee, Wis.
Minneapolis, Minn.
Mobile, Ala.
Montgomery, Ala.
New Haven, Conn.
New Orleans, La.
New York (LaGuardia), N. Y.
Philadelphia, Pa.
Phoenix, Ariz.
Pittsburgh, Pa.
Pocatello, Idaho
Port Arthur, Tex.
Portland, Oreg.
Prescott, Ariz.
Providence, R. I.
Pueblo, Colo.
Rapid City, S. Dak.
Reno, Nev.
Rochester, N. Y.
Roswell, N. Mex.
Sacramento, Calif.
Salt Lake City, Utah
San Diego, Calif.
San Francisco, Calif.
Scottsbluff, Nebr.
Seattle, Wash.
Spokane, Wash.
St. Louis, Mo.
Syracuse, N. Y.
Tonopah, Nev.
Tucson, Ariz.
Washington, D. C. (Silver Hill, Md.)
Wichita, Kans.
Williston, N. Dak.
Winnemucca, Nev.
Yuma, Ariz.

TABLE 9.—*Foreign monitoring stations during Operation Plumbbob (spring 1957)*

Addis Ababa, Ethiopia	Mexico City, Mexico
Anchorage, Alaska	Midway Island
Bangkok, Siam	Milan, Italy
Beirut, Lebanon	Misawa, Japan
Belem, Brazil	Moncton, New Brunswick, Canada
Bermuda	Monrovia, Liberia
Buenos Aires, Argentina	Montreal, Quebec, Canada
Canal Zone	Moosoonee, Ontario, Canada
Canton Island	Nagasaki, Japan
Churchill, Manitoba, Canada	Nairobi, Kenya, East Africa
Clarke AFB, Philippines	Nome, Alaska
Colombo, Ceylon	North Bay, Ontario, Canada
Dakar, French West Africa	Noumea, New Caledonia
Deep River, Ottawa, Ontario, Canada	Oslo, Norway
Dhahran, Saudi Arabia	Ponape
Durban Natal, South Africa	Prestwick, Scotland
Edmonton, Alberta, Canada	Pretoria, South Africa
Fairbanks, Alaska	Quito, Ecuador
French Frigate Shoals	Regina, Saskatchewan, Canada
Goose Bay, Labrador	Rhein Main, Germany
Guam	San Jose, Costa Rica
Hilo, Hawaii	San Juan, Puerto Rico
Hiroshima, Japan	São Paulo, Brazil
Honolulu, Hawaii	Seven Islands, Quebec, Canada
Iwo Jima	Sidi Slimane, French Morocco
Johnson Island	Singapore
Juneau, Alaska	Stephenville, Newfoundland
Keflavik, Iceland	Sydney, Australia
Koror	T'ai-pei, Formosa
Kwajalein	Thule, Greenland
La Paz, Bolivia	Tokyo Air Base, Japan
Lagens, Azores	Truk
Lagos, Nigeria	Wake Island
Leopoldville, Belgian Congo	Wellington, New Zealand
Lihue	Wheelus AFB, Tripoli
Lima, Peru	Winnipeg, Manitoba, Canada
Melbourne, Australia	Yap

Table 10.—Estimated radiation exposures for communities around the Nevada test site

NEVADA

	Roentgen		Roentgen
Acoma	3.0	Las Vegas	0.2
Alamo	1.8	Lincoln Mine	4.0
Ash Springs	0.6	Lockes Ranch	1.3
Baker	0.8	Logandale	0.4
Barclay	2.0	Lund	0.8
Buckhorn Ranch	0.9	Mesquite	1.8
Bunkerville	4.3	McGill	0.4
Caliente	0.7	Moapa	0.8
Carp	3.6	Nellis AF Base	0.05
Clarks Station	0.8	North Las Vegas	0.2
Crestline	0.7	Nyala	1.7
Crystal	4.0	Overton	0.35
Crystal Springs	1.0	Pahrump	0.2
Currant	0.5	Panaca	0.65
Dry Lake	1.0	Pioche	0.7
Duckwater	0.8	Preston	0.7
East Ely	0.6	Reed	4.0
Eden Creek Ranch	0.7	Rox	3.0
Elgin	3.5	Ruth	0.5
Ely	0.6	Sharp's (Adaven)	1.2
Eureka	0.2	Shoshone	0.7
Fallini Ranch	0.8	Sunnyside	1.2
Glendale	0.7	Ursine	0.6
Groom	2.0	Warm Springs	0.5
Hiko	1.0	Warm Spring Ranch	1.0
Kimberley	0.5		

UTAH

	Roentgen		Roentgen
Alton	0.8	Modena	0.5
Anderson Junction	1.2	Mount Carmel	0.85
Bear Valley Junction	0.4	New Castle	0.6
Beaver	0.25	New Harmony	1.2
Beryl	0.5	Orderville	1.5
Beryl Junction	1.0	Panguitch	0.2
Cedar City	0.4	Paragonah	0.4
Enterprise	0.7	Parowan	0.4
Garrison	0.7	Pintura	1.2
Glendale	1.2	Rockville	3.0
Gunlock	2.6	Saint George	3.0
Hamilton Fort	0.6	Santa Clara	3.5
Hurricane	4.2	Shivwits	2.8
Kanab	1.6	Springdale	2.6
Kanarraville	1.2	Toquerville	2.0
Leeds	3.0	Veyo	2.0
Long Valley	0.8	Virgin	1.5
Lune	0.5	Washington	3.0
Minersville	0.2	Zane	0.3

ARIZONA

	Roentgen		Roentgen
Beaver Dam	2.0	Short Creek	1.6
Littlefield	1.6	Wolf Hole	1.3

FIGURE 1

GENERALIZED CONCEPTS: DIMENSIONS OF CLOUD AND STEM DISTRIBUTION OF ACTIVITY

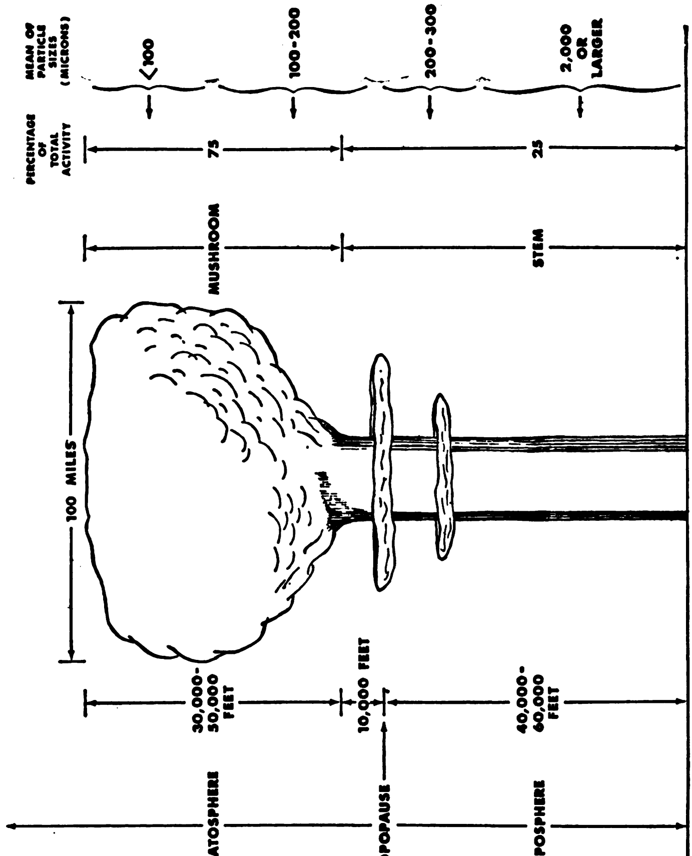
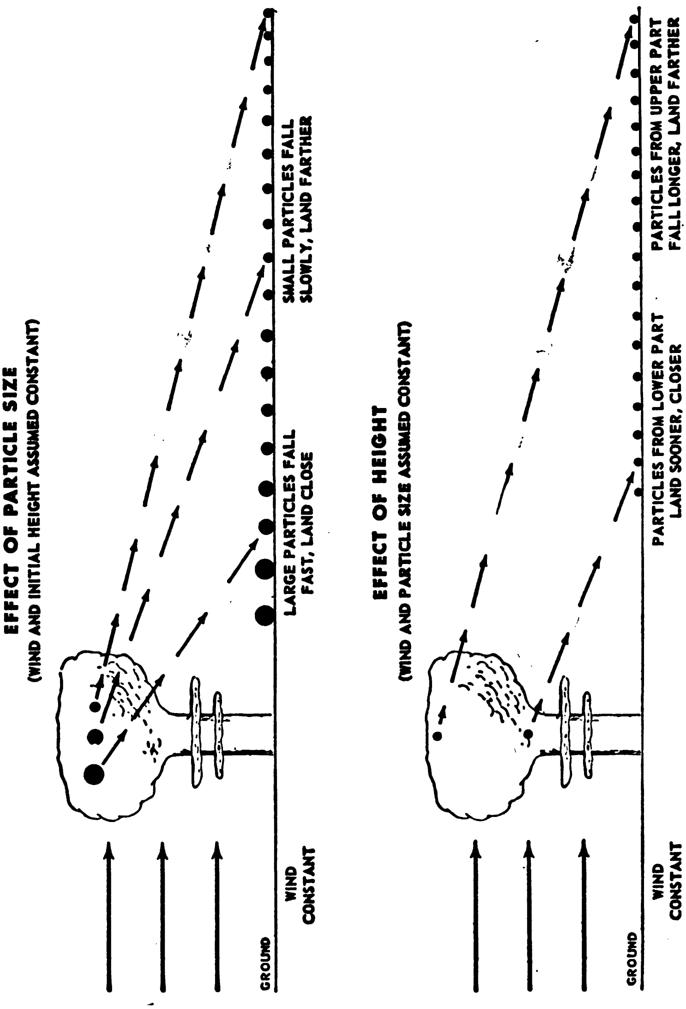


FIGURE 2a

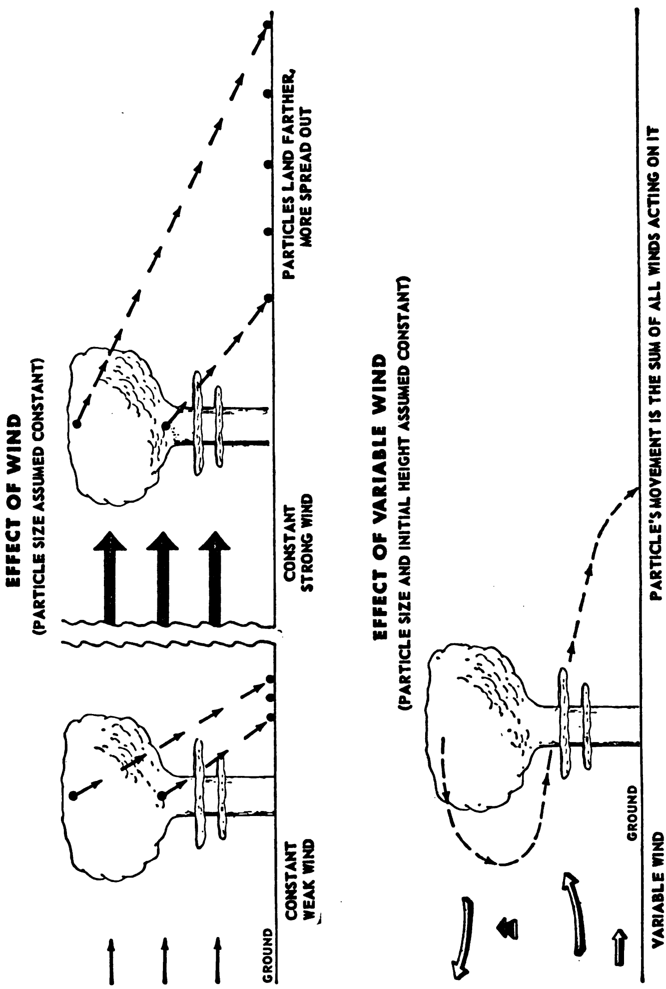
FACTORS AFFECTING DISTRIBUTION OF FALLOUT*



* As suggested in Civil Defense Technical Bulletin TB-11-21, Fallout and The Winds, October, 1955.

FIGURE 2b

FACTORS AFFECTING DISTRIBUTION OF FALLOUT *



* As suggested in Civil Defense Technical Bulletin TB-11-21, Fallout and The Winds, October 1955.

GPO 907068

FIGURE 3

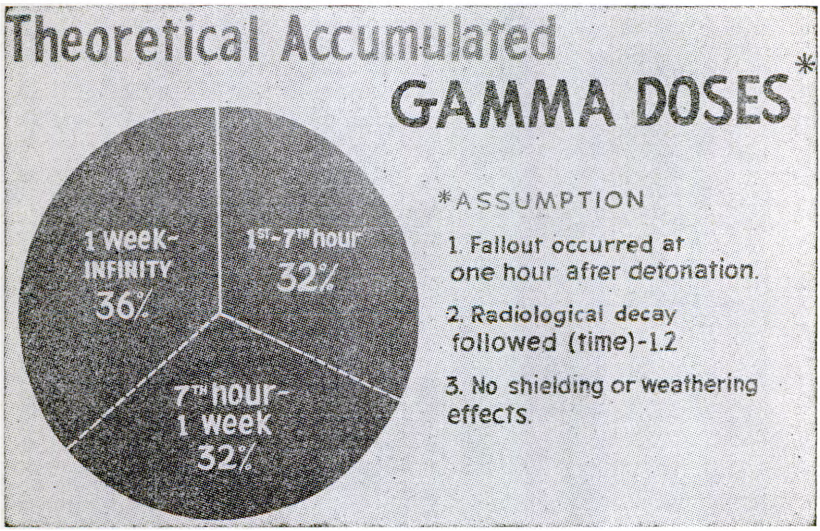

Theoretical Accumulated
GAMMA DOSES*
1 week-
INFINITY
36%
1st-7th hour
32%
7th hour-
1 week
32%
*ASSUMPTION
1. Fallout occurred at one hour after detonation.
2. Radiological decay followed (time)-1.2
3. No shielding or weathering effects.

FIGURE 4

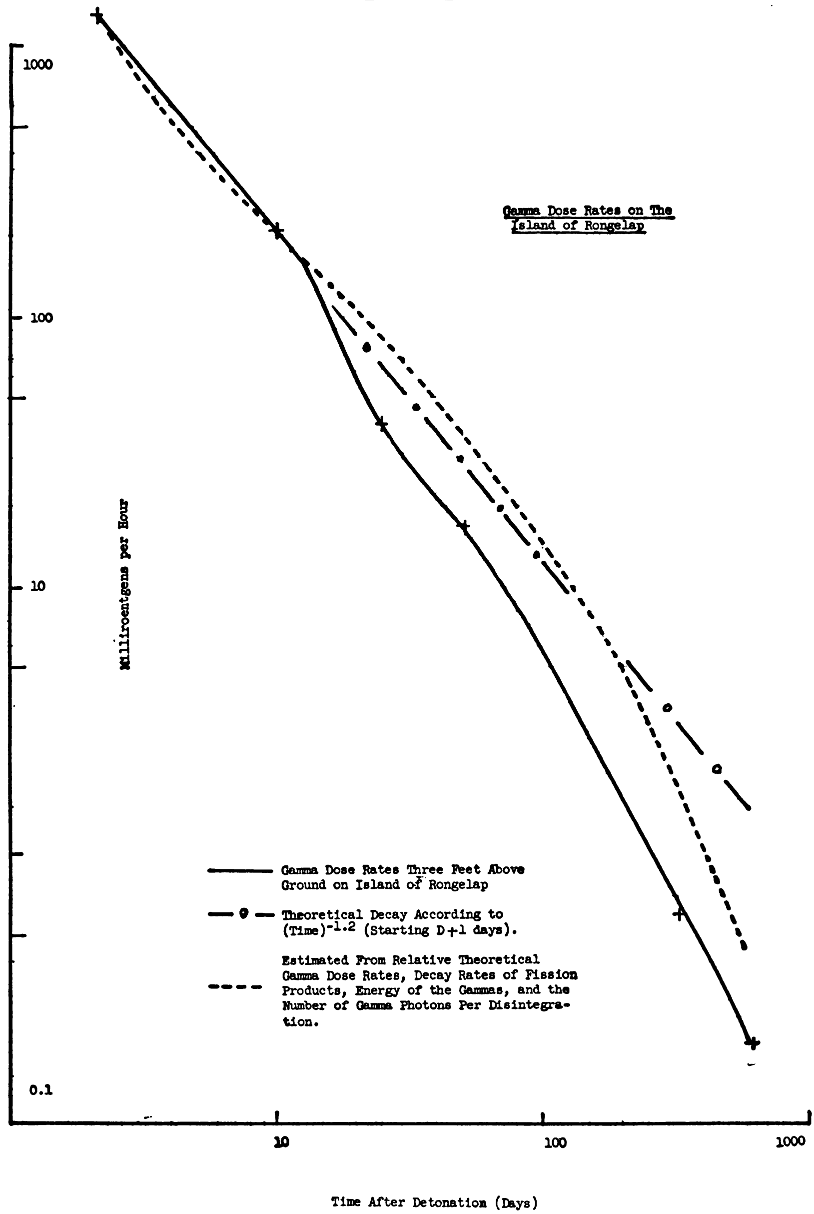

Gamma Dose Rates on The Island of Rongelap
1000
100
10
0.1
Milliroentgens per Hour
10
100
1000
Time After Detonation (Days)
Gamma Dose Rates Three Feet Above Ground on Island of Rongelap
Theoretical Decay According to (Time)$^{-1.2}$ (Starting D+1 days).
Estimated From Relative Theoretical Gamma Dose Rates, Decay Rates of Fission Products, Energy of the Gammas, and the Number of Gamma Photons Per Disintegration.

FIGURE 5

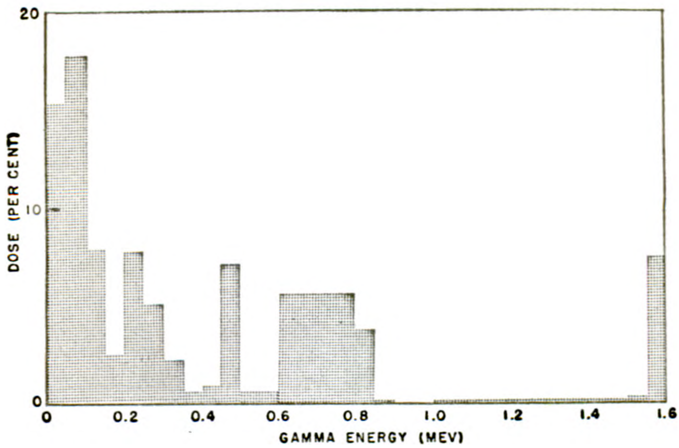
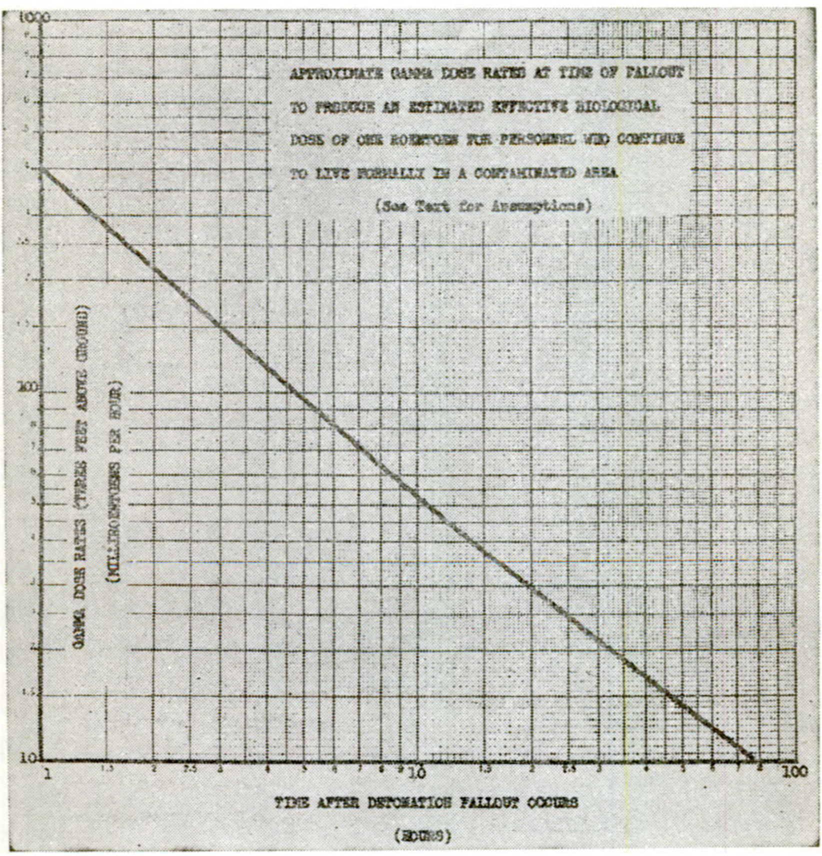


FIGURE 6



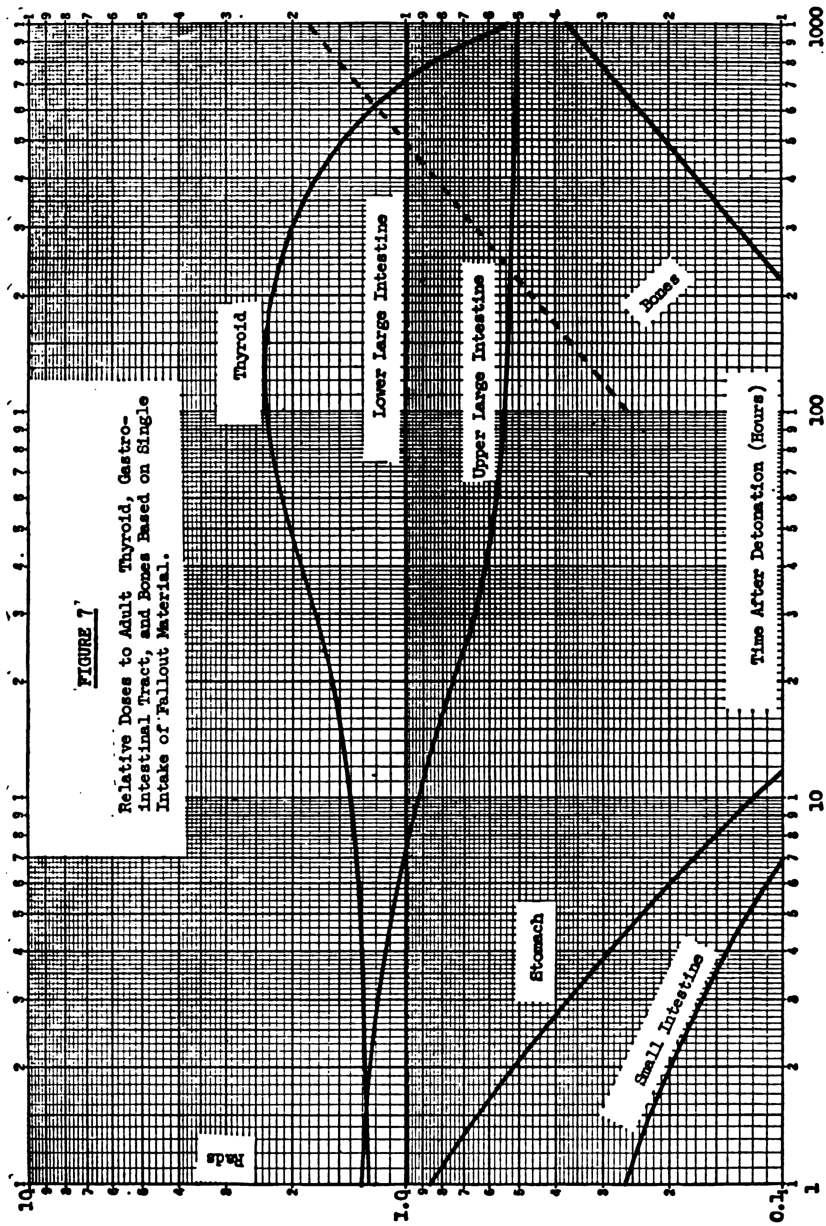


FIGURE 7

Relative Doses to Adult Thyroid, Gastro-intestinal Tract, and Bones Based on Single Intake of Fallout Material.

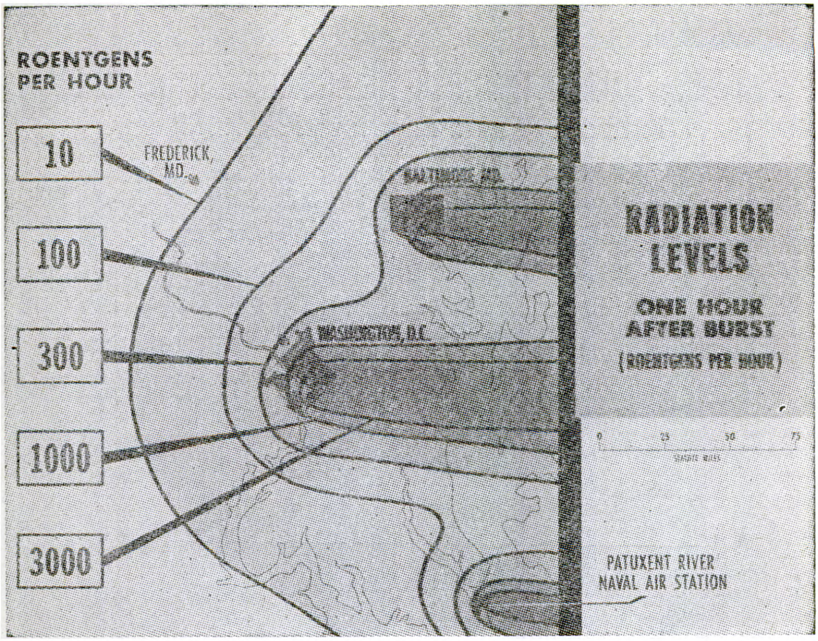


FIGURE 8

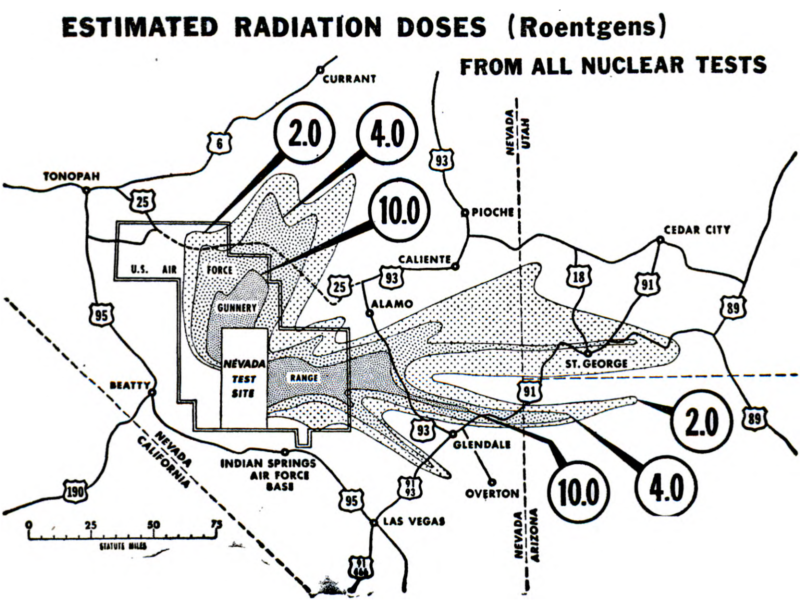


FIGURE 9

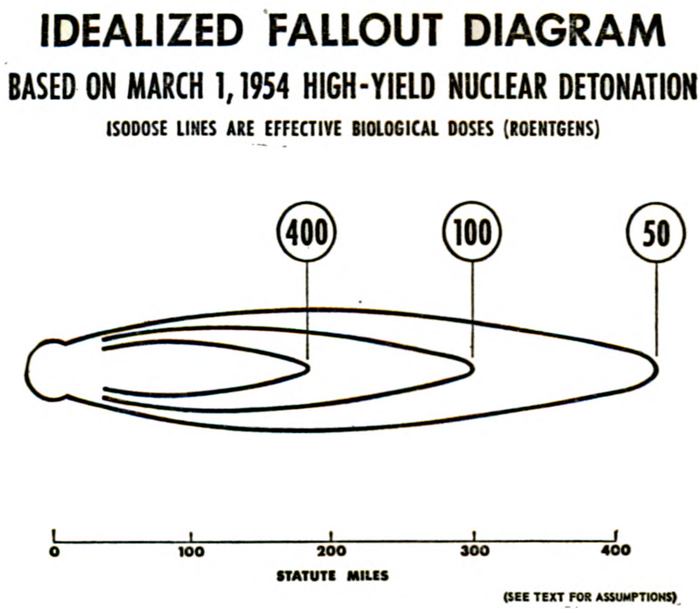


FIGURE 10. (See also table 3, p. 183.)

FIGURE 11.—Detonation during Operation Ivy; Fall 1952.

U. S. ATOMIC ENERGY COMMISSION,
Washington, D. C., March 20, 1957.

Hon. CHET HOLIFIELD,
Chairman, Military Operations Subcommittee,
Congress of the United States.

DEAR MR. HOLIFIELD: This is in reply to your letter of February 25, 1957, requesting "(*a*) the roentgen readings on Rongelap Atoll as of January 1, 1957; and (*b*) the roentgen readings as of the same date on the downwind island of Bikini Atoll where the fallout might be expected to simulate that of a suburban area of a big city." We do not have the data in the exact form which you requested, but are glad to give you the following information concerning radiation levels in these areas.

The last survey which we made of Rongelap was at the end of July 1956. This survey showed dose rates ranging from 0.2 to 0.5 milliroentgens per hour with an average of 0.4 milliroentgens per hour. Previous surveys had indicated that the dose rates in July 1956 would be aboout 0.1 milliroentgens per hour. The higher value found in July 1956 was undoubtedly due to the small additional fallout that occurred during Operation Redwing. If so, because of the relatively rapid decay of this fresh radioactive material we would expect that at the present time the radiation level is again in the neighborhood of 0.1 milliroentgen per hour or less. This is about one-half the currently recommended maximum permissible rate of exposure for general populations.

Gamma dose rates on the island of Rongelap observed in previous surveys are shown on the attached graph. The plotted points through which the solid line is drawn represent gamma dose rate readings at a point of 3 feet above the ground. The break in the curve between the 10th and 25th day was undoubtedly due to the first heavy rains that were known to have occurred after the detonation. Aside from this break you will note that the observed decrease of the gamma dose rate during the first 2 years follows rather closely values predicted from theoretical considerations.

The gamma dose rates on other islands in Rongelap Atoll have not been followed as closely but the data indicate similar *rates* of decay with the most heavily contaminated island being about 12 times higher activity than Rongelap Island. This was the uninhabited island of Naen on the northwestern rim of the atoll. The decay rates have not been similarly followed on the islands of Bikini because additional fallout occurred on these islands from subsequent detonations during Operation Castle and again during Operation Redwing. It would be expected, however, if the rates of decay for the March 1, 1954, fallout could have been followed, they would have been somewhat similar to those shown in the graph. (See p. 192.)

For any single fallout event, the degree of initial contamination in any area depends upon many variable factors. In general, the data suggest that after March 1954. Also plans are being developed for a continuing and long-range and the corresponding radiation dose rate in close-in areas (i. e., 10 to 20 miles) are not greatly higher than at 100 miles downwind, under wind conditions of some 15–20 miles per hour. However, it is important to realize that the radiation dose received by unprotected persons in the close-in areas is greater because they would receive a substantial portion of their total dose during the time required for the fallout to reach the more distant areas. You will recall that the fallout on the island of Rongelap started at about 5 hours following the detonation.

The Atomic Energy Commission is currently preparing a report summarizing the data from the surveys that have been made in the Marshall Islands since March 1954. Also plans are being developed for a continuing and long-range program of monitoring these areas.

Sincerely yours,

DAVID L. SHAW,
Assistant General Manager.

Dr. DUNNING. In describing and evaluating the effects of fallout, it is necessary to consider the characteristics of the radiations emitted from the material. These are of three types, as you learned yesterday: Gamma rays, beta particles, and alpha particles.

The gamma rays are the emissions of principal concern, because of their greater range, and we will speak primarily of them.

The gamma radiation dose that one may actually receive from fallout, and the biological effects are dependent upon five principal factors. Let us consider each of these 5 factors briefly, and then attempt to integrate them into 2 illustrative examples.

The first factor is radiological decay.

The decrease in radioactivity of fallout material roughly follows the relationship of time to the −1.2 power.

I have illustrated on the first chart the doses that might be accumulated if fallout were to occur 1 hour after detonation. If you were standing out of doors, fully exposed, from the first to the seventh hour after the detonation, one would accumulate 32 percent of the total possible exposure in that area. From the seventh hour to 1 week later, 32 percent more, and from 1 week to the full lifetime of the radioactive material, the other 36 percent. This is based on a 1-hour fallout. (See p. 191.)

If the fallout occurs at later times, then the exposures accumulate much less rapidly. In other words, it would take much more than the first 6 hours to accumulate the 32 percent of the total possible dose.

The second principal factor that determines doses and effects is weathering and shielding effects.

Obviously, these vary from time to time and place to place, so we cannot make any precise evaluation of them, but we can make some generalizations.

Based on data from the Pacific tests, especially the one of March 1954, we found that the dose rates on the islands were reduced by a factor of about two after the first heavy railfall; but after that the subsequent rainfalls did not seem to reduce these dose rates appreciably. However, there are good data lacking on the effects of rainfall on relatively heavy fallout patterns for large land masses having different soil characteristics, or on built-up areas.

The next chart summarizes some of the estimates of shielding that might be expected from different type structures. These are based principally upon theoretical calculations, since there are a paucity of field data. (See table 1, p. 182.)

In an ordinary 1-story frame house, such as many of us live in, on the first floor there would be about 50 percent as much exposure as there would be out of doors. In the basement, the center, about 10 percent as much as that out of doors; on the side of the basement less than 10 percent; in other words, better protective factors.

For a multistory reinforced concrete, on lower floors away from windows, a factor of 10; and for the basement we are again down to one-tenth of 1 percent of the out-of-doors exposure.

Likewise, with shelter equivalent to 3 feet of earth, we are down to one-tenth of 1 percent of the outdoor exposure.

Senator HICKENLOOPER. Mr. Chairman?

Representative HOLIFIELD. Senator Hickenlooper.

Senator HICKENLOOPER. May I ask about the building, the frame house. Does that contemplate all the windows closed, or does it contemplate free access of air? Or is it in the blast area? Or where are these houses located, for the record?

Dr. DUNNING. The assumption is made here that the windows are closed. If the windows and doors are open, this in itself makes little difference, but then, as you implied this will allow the radioactive material to drift into the house, and, of course, raise your level.

Senator HICKENLOOPER. How does the radioactive material get into the house if it is a closed structure?

Dr. DUNNING. These are the actual gamma rays coming from the material lying outside, and they will pass right through the walls, and in doing so will be partly absorbed, but nevertheless in this case we estimate about half of them will get through the walls.

Senator HICKENLOOPER. Gamma rays coming from particles in the atmosphere?

Dr. DUNNING. That is right, sir.

Representative HOLIFIELD. There is a significant difference on the point that Senator Hickenlooper brought up, and that is that your gamma rays are rays which penetrate building materials in the one instance, but in the case where the windows have been blown out or the doors are open, the fission fragments can drift in and settle and emit the same type of rays without any shielding at all. Is that not true?

Dr. DUNNING. That is correct, sir.

One example of the effect of winds—one example of winds occurred at the detonation at the Nevada test site in 1953, when strong winds blew almost at right angles across a narrow band of fallout field on the second and third day after the detonation. The gamma dose rates on the fourth day were found to be less than predicted without winds by factors ranging from 3 to 6. In other words, the winds simply blew this material away, or it worked itself into the ground. But the effect of winds would not be expected to be as great as this for large contaminated areas of nonsandy soils.

The third factor that affects doses and biological effects is that of energy of these rays, discussed at some length yesterday. So I merely mention that the energy does determine the depth to which these rays will penetrate an object as large as the human body, and also the energy of these rays will determine the amount of energy that is released to the tissues in passing through the body.

It is quite a complicated process to try to estimate these various gamma spectra of the rays and to estimate their biological effects, but this is what is attempted when you say people receive so much dosage.

The fourth factor is that of geometry. That is, it is found if you have radiation coming from only a point and passing through the body from only one direction, as it passes through the dose will drop off within the body. Simply some of the energy is absorbed.

In the case of fallout, however, we have rays coming at you in various directions, sometimes all directions, and so we find then that the dose delivered within the body is almost uniform, that is, you get almost as much in the center of the body as near the surface.

The last factor in determining dose and effect is the biological repair factor. It has been recognized, in general, that the longer the period over which a given radiation dose is delivered, the less is the resultant biological effect, except for such aspects as genetic effects. In situations of heavy fallout and relatively large potential radiation doses, the biological repair factor may be considered in estimating incapacitating and lethal doses.

Representative HOLIFIELD. At that point, will you give us an explanation of what you mean by lethal doses, what type of cells in the body can be repaired, or be replaced, and what type of cells or parts of cells are permanently mutated?

Dr. DUNNING. It is not so much the question of the cells as the effect on the cells. As you intimated in the last part of your statement there, it is believed that genetic effects or mutations, whether it be mutations of the gene cells or the somatic cells, the living cells within the person, these effects are linear with dose. That is, if you double the dose, you double the mutations.

What I spoke of here was more the gross effect. In fact, I tried to indicate that when we speak about incapacitating and lethal doses, you ask yourselves the question: Is that man going to become so sick he cannot work? Is this man going to die? It is that more gross evaluation I had in mind when I spoke about biological repair, but there is a tremendous amount more to be learned on this factor.

Representative HOLIFIELD. You were speaking of the ability of the spleen to replace white corpuscles, and that sort of thing.

Dr. DUNNING. It is tied up very intimately with the blood picture, the production of white and red corpuscles.

Senator PASTORE. On this very point, how far have our experiments in Hiroshima and Nagasaki gone in proving some of the things you have just developed here?

Dr. DUNNING. I think there is experience in the Japanese data. I think we do have some other limited experiences where others have been accidentally exposed to relatively high doses. In fact, Dr. Graves described briefly his experience yesterday. And we do have evidence of this biological repair factor, especially, as I say, in the blood picture.

There is still a considerable amount to be learned in this, and I will not dare to go much further on this point at this time.

Chairman DURHAM. You are speaking just of the normal repair system, Doctor, by the body, and not by the addition of any medicines. or any type of treatment?

Dr. DUNNING. That is correct.

Senator PASTORE. Would you say that, comparatively speaking, we have not learned a great deal concerning this on experiments on animals in Hiroshima?

Dr. DUNNING. I would not say we did not learn a great deal. Quite the opposite. I will add, too, that every time you learn 1 thing, there seems to be 2 more to be learned. It just opens up whole new avenues of thought and study.

As you well know, most of our work has been done with animals, and then one is faced with extrapolating to man, and this is always uncertain.

Chairman DOUGLAS. Did you participate in that, Doctor?

Dr. DUNNING. No, sir; I did not. Our present knowledge does not permit us to establish a precise overall relationship for the rate of recovery and degree of recovery, but the data seems to indicate that the rate of recovery in man is relatively slow compared with animals.

Therefore, any biological repair factor would have its greater influence where a total given dose is spread out over time rather than the doses that are given rather quickly in time, such as nearby fallout.

Let us pass on, then, to the first of two examples.

One of them is an exercise during the National Association of Civil Defense Directors meeting in Washington, D. C., April of this year, where it was assumed that four bombs were dropped simultaneously, as follows:

A 20 megaton on the Union Station, Washington, D. C.; 5 megaton on the National Airport; 20 megaton on Baltimore, Md.; and 10 megaton on the Patuxent River Naval Air Station. The next chart will give a view of the resultant fallout pattern. (See top of p. 195.)

The units are in roentgens per hour, but in these nearby areas essentially all of the activity is down within 1 hour; so that this is a fairly realistic picture.

These are the numbers, such as 10 roentgens per hour and so on, to 3,000 roentgens per hour.

I would like to call attention especially to the 300 roentgens per hour line, since we will use this for an example in just a moment.

Recalling that radioactive decay is rapid for this early fallout around ground zero, it becomes evident, if adequate protective areas are available, it would be wiser for people to remain indoors, rather than be exposed out of doors, to the out-of-doors dose during this period of highest activity.

Likewise if a delay in movement is possible, there will be more of an opportunity to evaluate the situation, and to then effect an orderly evacuation.

Since each situation is unique, no rigid criteria will be proposed here for permissible exposures or for mandatory evacuation, since there may be other factors present as potentially hazardous as radiation.

This chart was developed to illustrate the kind of thinking and planning possible for civil defense. (See table 2, p. 183.)

Looking now at the 300 roentgen per hour line, we may permit our rescue workers 25, 50, or 100 roentgens, let us say. If we allow our rescue workers only 25 roentgens, that means for them to move into this area and do their rescue work and move out again, they will have to wait until 16 hours after the detonation before they may do so. In that length of time, the populace that were in this area would have received 320 roentgens, and this is based on certain assumptions. This is based on one assumption of rather small protective factors [indicating].

Before the populace is fully evacuated, they would have received 332 roentgens. On the other hand, if we allowed our workers to receive 100 roentgens exposure during the rescue work, they could have moved in at 5 hours instead of 16 hours, after the detonation, during which time the populace would have received considerably less exposure, this [indicating] being again the total exposure, before fully evacuated.

Looking at the last two columns, and especially the last one, it is based on the same situation; only in this case the populace is well sheltered. It is a factor of 10. We assume they are in good protective shelters. I say "good"—a factor of 10. It can be much better than this with well-constructed sheltering.

Even here I think this illustrates the point [indicating]: While they are awaiting rescue, they receive significantly less doses than in the less protected shelter. It is the same context of meaning. If rescue workers are permitted 25 roentgens, the populace will receive 76, as you note here.

Interestingly, if the rescue workers move in early and take the populace out of their relatively well-protected areas into the open, they may receive more exposure here than if they had stayed in place for

the full 16 hours. On the other hand, moving in at 5 hours may be a large advantage for first aid and rescue work, and this sort of thing.

Representative HOLIFIELD. Of course, this makes almost a conclusive argument on behalf of adequate shelter for the people, because, as you note there, if you take them out of the basement where they are getting a factor of 10 percent of the outside radioactivity, they must pass through an outside radioactivity which actually gives them an accumulation of radioactivity more than if they had stayed for, say, a week, in a shelter.

Dr. DUNNING. This is quite possible, although this is not my area of work.

Representative HOLIFIELD. Of course, it does not even compare with the amount the workers would get going into a contaminated area and coming out in a shorter length of time. They would get even more.

Dr. DUNNING. That is right.

Representative HOLIFIELD. That is right. It shows it at the bottom.

Dr. DUNNING. As I say, this is not my area of work. But it just seems to me, in case of emergency such as this, there would be such a chaotic condition that one of the worst things people could do would be to lose their heads and run out of doors, and, in a sense, run around in circles. It would be far better, if they have any kind of protective shelter, to stay put until they know what the situation is like, and then move in an orderly fashion.

Then, in essence, this sort of thinking and planning, I feel, will be most valuable rather than striving to establish any rigid rules or criteria ahead of time.

The second example of the dosage one may receive and the resultant biological effect is for a more distant fallout pattern.

As has been described by Dr. Kellogg and others, there is a wide variability in possible patterns; therefore we can only generalize again.

The next chart is again an idealized pattern. By that, we mean a generalized pattern, because conditions will change from day to day and situation to situation, but if we take a generalized pattern based on the March 1, 1954, high-yield detonation, we see a pattern like this [indicating]. These are expressed in units of effective biological doses, because these incorporate all of the five factors I mentioned briefly at the beginning. These are the best estimates of exposure that people would get if they continue to live in these areas without any special measures of protection. (See top of p. 196.)

Representative HOLIFIELD. What is the time element on that, Doctor?

Dr. DUNNING. Sir?

Representative HOLIFIELD. What is the time element? Is that per hour of exposure?

Dr. DUNNING. This is if they continue to live there for the lifetime of the activity, that is, a period of years, and took no special protective measures, merely went about their daily business. As you can imagine, there are certain assumptions that go into this as to how much time people spend indoors, in what type of buildings, and so forth.

Representative HOLIFIELD. Please repeat the numbers on your chart, because your testimony does not interpret the chart.

Dr. DUNNING. The 50-roentgen effective biological dose line extends out slightly beyond the 400 miles, and this area here [indicating] encompasses about 25,000 square miles.

The 100-roentgen line encompasses about half of that, 12,500 square miles; and the 400 roentgen line about 5,000 square miles.

This is suggestive of a number, 400 roentgens, where the people receive such an exposure that perhaps half would die. The 100 roentgen is suggestive where a few percent might become ill. The 50 roentgen line has no unique significance, but does suggest a number where evacuation should at least be seriously thought of in the face of other hazards.

For those areas downwind, where fallout occurs several hours after detonation, these doses will not be accumulated rapidly. In fact, it will be a period of months, or if you want to go to the end of the curve of doses, it would take a period of years before you would get this full dose. So it would not require immediate emergency measures downwind this far [indicating].

The question is frequently asked as to the time one must spend within a shelter or remain outside of a contaminated area. Again, the answer depends upon what are the permissible levels of exposure, and what are the other hazards one faces if he does move out. But it does indicate that even in the areas of heaviest contamination that one may move in and do a short rescue work, and move out again, if you are willing to permit relatively high exposures to your rescue crew in the order of 100 roentgens.

One then asks the question, How long would I be denied this area? How long before I can go back in there to live?

We assume now they have been moved out. How soon may they move back?

Just taking 4 months later as a point of reference, at 4 months how would this picture look then?

Again, based on certain assumptions, this 50-roentgen line would have shrunk down to an area of about 2,500 square miles, meaning if people move back in here 4 months later and continue to live there indefinitely thereafter in a normal fashion, then they would accumulate about 50 roentgens, effective biological dose.

Then you may extrapolate still further and say, "How about 1 year later? What will this pattern look like then?" Again, it is quite uncertain. But based on certain assumptions again, the 50 roentgen line would have disappeared. In other words, there would be no area in here where people would accumulate 50 roentgens of exposure if they move back in 1 year after the fallout and continue to live there indefinitely.

This does incorporate the biological repair factor, but there are such effects as genetics, as I mentioned, that are linear. In this hottest area it is conceivable there could be several hundred roentgen doses delivered to people who move back in there 1 year later and continue to live there normally.

As I said before, the biological repair factor would bring this down below 50 roentgens, but in terms of genetics, and such aspects, the actual dosage might be several hundred roentgens.

Representative HOLIFIELD. Then this would mean that large areas of land might remain unoccupied for a considerable number of months?

Dr. DUNNING. Yes. But I was about to mention that this is based on the assumption that people do nothing to protect themselves, and nothing to decontaminate the area; simply let it lie there and decay.

Representative HOLIFIELD. It is a very difficult job to decontaminate the large areas of the earth.

Dr. DUNNING. That is quite true. Probably one of the best procedures, if one can afford to do this, is merely to wait and let the activity decay away. But in this area of highest activity which would encompass, perhaps, a few thousand square miles, perhaps measures could be taken on decontamination which would not have to be done in the larger 25,000 square mile area.

Senator BRICKER. What processes of decontamination are available?

Dr. DUNNING. This is a whole subject in itself, sir. I will just briefly mention that the United States Naval Radiological Defense Laboratory in San Francisco have made considerable studies on this subject, and have proposed certain measures of decontaminating buildings and land areas. How effective they are I think is yet to be shown. There has been some experimentation, but I feel a great deal more needs to be done.

Chairman DURHAM. You are speaking primarily to gamma rays now; are you not?

Dr. DUNNING. Yes; this has been completely on gamma rays.

There are other types of rays. The beta rays are of concern, it appears, according to our present data, only when the fallout material comes directly on the skin and remains there for a period of time.

In the case of the fallout of the Marshallese, it was very illuminating to note that even a single layer of cotton clothing was enough to prevent serious beta dose to the skin, and where the fallout material did land on the skin and did remain there, such as in the folds of the neck and in the elbow, there were these so-called beta burns, burns of the skin from these beta rays. Yet, where they had the light clothing on, there were no burns. Nor were there any on even the lower part of the leg, but there were on the feet where again the material had been scuffed up from the ground.

Representative HOLIFIELD. Can you refresh the committee's memory on how many days later this exposure occurred, and how far the place was from the point of detonation?

Dr. DUNNING. The inhabitants of Rongelap Island were about 110 statute miles from the point of detonation. Some were evacuated at 36 hours, and some at about 48 hours after detonation. Upon evacuation, they took baths. Some of them did beforehand, and some of them not. It would appear that those who did take baths in the ocean did not get beta burns. It is merely a physical picture of moving the material from the body.

Representative HOLIFIELD. I referred to that specifically, and I am glad you answered the way you did, because this gives you a chance to answer also in regard to the Japanese fishermen on the *Lucky Dragon* as to how many days later it was they were supposed to have received their exposure.

Dr. DUNNING. They were generally in the same distance, only somewhat closer than the Rongelapese. The fallout occurred on

them, and they did not wash in general. Most of the dose was delivered in the first few days, and so it is a question of getting it off and getting it off fast.

The next topic we will discuss is that of internal exposure which has been mentioned several times before this committee.

The principal hazards from intake of relatively large amounts of radioactive fallout for several weeks immediately following a nuclear detonation are doses one may get to the gastrointestinal tract, to the thyroid, and to the bones.

My written report to this committee considers in detail the amount of ingested fission product activity material required to produce certain radiation doses to these critical organs of the body, and the possible biological effects therefrom.

It is a somewhat long, complicated story, Mr. Chairman, and I would let it go at that, and quote one conclusion, and that is: If the degree of contamination of an area is such that the external gamma radiation may be accepted, for continuous occupancy, then probably the internal hazard would not deny this occupancy.

I think some folks get somewhat confused. They say: "Fine. I think I begin to understand the external doses, but should I drink this water? And should I eat this food?"

As I say, as an overall generalization, this conclusion would appear to be so, but like most of these it is tentative and awaits further information.

Representative HOLIFIELD. There is this one factor I think you will recognize: That in the ingestion of material from the secondary source of vegetables or milk, you would be ingesting the long-lifed element of strongtium 90, and not the comparatively short-lived gamma or beta rays.

Dr. DUNNING. That is correct, sir; and my conclusion took that into account.

Representative HOLIFIELD. Of course, while it might not be lethal, and would not be lethal in the quantities you speak of, it would be residual within the bones or the tissues of the body, and would be a permanent infestation, you might say.

Dr. DUNNING. That is correct. But then one must go from there to the next step, and say, "What is the actual dose delivered to the bones from this internally deposited material?" Because actually there is no significant difference between a roentgen of exposure, whether it comes from strontium 90 or from gamma rays, coming from material lying on the ground.

That is what I put into this conclusion when I said the amount of material that one gets into the body by eating food and drinking water in such an area would be acceptable in the sense that it would be far below lethal amounts. But I think we have to make a distinction in our mind here between peacetime tolerance levels and wartime. I do not have any specific number in my mind, but in these areas where we might permit occupancy in the case of warfare, they would probably accumulate internally deposited materials that would be in excess of our peacetime standards. I think we have to make this distinction.

Representative HOLIFIELD. Is there a distinction between an area that has a hundred roentgens of gamma radiation and its effect upon

the body and, say, the ingestion of 5 or 10 roentgens which remain permanently in the bone now?

Dr. DUNNING. There is very little difference between a roentgen delivered from internal or external sources.

I think, in general, one can make the flat statement that a hundred roentgens, whether it comes from material on the ground or in material you eat, and that goes to the bones, is about the same.

Representative HOLIFIELD. It is a hundred roentgens, but it is not a deposit of strontium 90, which has a persistence over a period of 28 years, while your outside exposure, you might say, to gamma rays or beta rays, would be something that would be temporary in nature and would be subject to repair, where a permanent deposit in the bone marrow would be permanent as far as the half-life is concerned; and, therefore, it would be something that you could not get away from, you might say.

Dr. DUNNING. Yes; I understand. I just repeat that, if we forget the time factor for a moment and simply say that so many roentgens of exposure to the bone, it makes no difference whether you get the hundred roentgens from the gamma rays or from the material in the body. It stays there. Sure, it persists there. What I was saying, as long as it persists, we have the doses year by year by year, and if it all adds up to a hundred roentgens, this is no different, in a sense, from a hundred roentgens of gamma rays, except possibly for the time factor.

Representative HOLIFIELD. This is getting in pretty deep water for me. My thought was that you have a permanent localized area of radiation in the ingestion of strontium 90, where you would not necessarily have a localized concentration of it in the case of allover bodily exposure of a hundred roentgens.

Senator BRICKER. I think there is a misunderstanding generally about the amount of strontium 90 that can be put in the bones from ingestion, because there is only a small percentage of fallout of strontium 90 that goes to plantlife, and only a limited percentage of strontium 90 that goes to animal life, and only a little percentage of that which goes into milk or meat. I think it is something like 6 percent.

Dr. DUNNING. If I may move on, that was my next point here.

Again now we are thinking in terms of warfare, and not in terms of testing.

We have the situation of this March 1 shot, where we have a relatively heavy fallout from a high yield weapon that appeared on the islands in the Pacific. Since then, we have had 10 radiological and biological surveys of these islands. I thought the committee would be interested in a summation of those data, that is, what was the actual contamination of environment in terms of food supply.

I would like to preface by saying that any conclusions are tentative because there are many uncertain factors here, but at least the data suggests in terms of strontium 90 the activity in plantlife in these islands built up over 1 year, that is, it takes time for material to get into the soil, plantlife, and edible parts.

By using rough extrapolations, the data suggests that if plantlife had been growing in the area of heaviest contamination it might have contained 10,000 to 30,000 Sunshine units, at 1 year's time. The corresponding values for the soils are several times higher. Based on

certain assumptions, these data suggest possible levels of strontium 90 in the bones of animals from continuous consumption of this food, of a few thousand to several thousand Sunshine units. Now the maximum permissible body burden for adult atomic-energy workers is equivalent to 1,000 Sunshine units.

There is some confirmatory evidence for this crude evaluation. A variety of native animals were left on the island of Rongelap after the fallout in March 1954. They were collected and sacrificed serially in time. Even after 2 years of continuous occupancy it was reported that there were no pathological changes that could be ascribed to radiation. Their bones contained from about 100 to a few hundred Sunshine units. Since the areas of highest contamination were about 12 to 14 times greater than Rongelap, an extrapolation would suggest values in the same range, that is, if animals had lived in the area of greatest contamination from this fallout, they might have accumulated from a few thousand to several thousand units, of strontium 90 in their bodies.

The Pacific island soils have higher calcium content than most soils in the United States, and, of course, there are differences in the type of plantlife and in the climate. However, theoretical calculations suggest that the same fallout in the United States might result in something like 100,000 Sunshine units in the soils of the United States with the highest contamination. Humans living exclusively off the foods grown in these soils might accumulate a body burden of strontium 90 of a few thousand to several thousand Sunshine units, keeping in mind that 1,000 is the maximum permissible body burden for atomic-energy workers.

Chairman DURHAM. Doctor, the effect of a hundred roentgens from the soil would be no more toxic than the 100 roentgens from the strontium; is that correct?

Dr. DUNNING. As far as the bones are concerned, it is correct. If you receive 100 roentgens from the gamma or strontium, it is essentially the same thing.

Chairman DURHAM. I was thinking of gamma rays.

Representative HOLIFIELD. Let me ask this question on that very point: If the 100 roentgens were ingested, would they not tend to go to certain organs of the body and have a concentrated effect, and, therefore, more of an effect upon, let us say, the liver or the spleen, or some other organ of the body that might be vital to the life of a man, than if the 100 roentgens were spread over the whole body?

Dr. DUNNING. If you could turn that around just a bit, Mr. Chairman. The way we compute it, we asked the question, How much material taken into the body will essentially result in 100 roentgens to the bones, to the liver, et cetera? We start the other way around from what you are saying. We simply ask how much material does one have to take in to end with a 100 roentgen dose. So we have reached our conclusions——

Representative HOLIFIELD. Again, are you not faced with the fact that you could not give a uniform dose of a hundred roentgens to every organ in the body, because some organs of the body—and I am speaking now in the case of ingestion of food or drink—some of the organs of the body would naturally process that, and it would be deposited in those organs rather than in the outside skin and toenails, and so forth.

Dr. DUNNING. That is correct. When we speak of external gamma, we mean essentially that each and every part of the body receives this 100 roentgens.

Representative HOLIFIELD. This I can understand, but I cannot understand how you can ingest contaminated foods or liquids and have it affect the body uniformly.

Dr. DUNNING. I did not mean to say that. If I did, it is incorrect.

Representative HOLIFIELD. You did not say it. I am saying it as a question or a statement for clarification.

Dr. DUNNING. You are quite correct.

Representative HOLIFIELD. Am I right in my supposition?

Dr. DUNNING. You are quite correct.

Chairman DURHAM. What we are saying, Doctor, whether it comes from Sunshine or whether it comes from strontium 90, that is, the gamma ray, it is no different as far as the effect of it, as if the same dose is taken.

Dr. DUNNING. That is correct, sir.

Lastly, then, I would like to mention briefly about the testing, and I do think we have to make a sharp demarcation in our minds that we have up to now been talking about more of a warfare situation. But intimately tied up with this is the testing.

Very extensive efforts are expended to protect the public in the planning of test nuclear detonations, and in the monitoring programs in operation during and between the test series. These are described in a detailed written report to the committee previously.

Since 1951, the United States has conducted 11 series of nuclear tests, 5 at the Nevada test site, and 6 at the Eniwetok Proving Ground for a total of more than 63 test detonations. A sixth series is currently underway at Nevada. So I understood by the report this morning.

The major effects near the testing sites of the fallout was on the inhabitants of some of the Marshall Islands in March 1954, which will be discussed by others, and fallout on the 23 Japanese fishermen. Worldwide effects will be discussed by others.

Since the committee manifested an interest yesterday in the fallout nearby, especially in Nevada, I do have a chart that may be of interest to you. This is our best estimate of exposures in areas around the Nevada test site. The units are roentgens of gamma exposure. They are based on certain assumptions, one of which is that the total dose is this [indicating] if one continues to live there indefinitely. (See bottom of p. 195.)

With those numbers before you, I would like to recall to your mind the recommendations of the National Committee on Radiation Protection and Measurement, and the recommendations of the National Academy of Sciences, which, in lay language, sort of lays the ground rules for our permissible exposures.

Both committees—expressed in somewhat different units, both committees said, in essence, that for individual exposures the maximum permissible amount should be 50 roentgens up to age 30.

Representative HOLIFIELD. At this point, it might be well for you to explain the term "Sunshine unit" in relation to roentgen. Is that not an occupational unit of measurement rather than a general population unit of measurement?

Dr. DUNNING. The Sunshine unit is a coined phrase which is used to express the amount of strontium 90 in relation to the amount of calcium, whether it be in the bones of man, or in the soil or anywhere else. Just like when one buys milk, you have to have some unit. It is merely a coined unit so that one in this business may know how much strontium 90 you are taking about when you say 1 Sunshine unit.

Representative HOLIFIELD. It is not to be confused with a roentgen?

Dr. DUNNING. That is correct. One could mathematically figure out the amount of roentgens 1 Sunshine unit would produce, but it is not the same.

These again are not Sunshine units; these are the more familiar roentgens that we have recently spoken about. So again we say that the maximum permissible exposures for individuals is 50 roentgens up to age 30.

Now, for general populations, which one may define as a million people or more, the maximum exposure from manmade sources, the maximum number recommended is 10 roentgens up to age 30.

So we have for individuals 50 roentgens, and for general population, 10 roentgens up to age 30.

Now, let us look at the records of exposure to populace in these areas.

The highest fallout exposure was in this motor court near Bunkerville, Nev., in 1953, where the people might have accumulated 7 to 8 roentgens of exposure. This might rightfully be compared to the 50 roentgens that I mentioned before as a ground rule.

In terms of general populace around the Nevada test site, I had a little problem finding a million people for a general population, but if one mentally makes larger and larger circles until he encompasses a million people, then the average exposure to the 1 million, is one-tenth of a roentgen for 6 years of testing, which is at the rate of one-half a roentgen per 30 years, which is one-twentieth of the maximum exposures recommended by the 2 committees.

Representative HOLIFIELD. This would be on one test?

Dr. DUNNING. These are all tests. I am sorry.

Representative HOLIFIELD. This is the accumulation of all tests?

Dr. DUNNING. This is the accumulation from all tests; not only Nevada, but all others. This is the sum total.

Lastly, on air and water concentrations, the internal exposure side of the record, I would say this: The highest concentration of activity in the air off the test site in the spring of 1953, the Nevada test site, amounting to 1.3 microcuries per cubic meter of air averaged over a 24-hour period. It was estimated that the radiation dose to the lungs from this activity was less than that delivered every month by naturally occurring radioactive isotopes in the air that we breathe every day.

Representative HOLIFIELD. That is based on an average, but not necessarily a hot spot locality?

Dr. DUNNING. This was the highest concentration of air found in any populated area. There are higher concentration spots on the gunnery range, the control area.

Representative HOLIFIELD. When you speak of normal does that mean sunshine?

Dr. DUNNING. When I said the doses to the lungs?

Representative HOLIFIELD. Yes.

Dr. Dunning. In this room, right in this air, there are naturally occurring radioactive materials. Every time you breathe in you get a certain radiation dose to your lungs.

What I was saying, then, was that by living normally over a period of a month, we have a certain dose delivered to our lungs from this naturally occurring material. Then I compared that with the people who were in this area where the fallout occurred, and said they breathed in the fallout, and then asked how much dose did they receive to the lungs from the fallout. And that is when I made the comparison that the dose from the fallout was less than they would have received each month from breathing naturally occurring substances.

Senator Bricker. That is background radiation principally?

Dr. Dunning. That is correct.

Senator Bricker. Have you anything to say about the effect upon the length of life? I ask that because of animal experimentation. Is there any indication that radiation does shorten life?

Dr. Dunning. This is again a topic in itself. I am not an authority on it, and would prefer to leave it to those who follow.

Senator Bricker. I am advised again there is a witness later.

Dr. Dunning. I think, again, one has to distinguish, though, between chronic and acute doses. There apparently is a significant difference in life shortening effect in large doses delivered in a short period of time versus low doses over a long period of time.

Senator Bricker. Even though there may be some effect from each?

Dr. Dunning. There may be some effect from each. I would leave the conclusion to others on that.

Lastly, how about the water contamination?

Once again, the record says that the highest concentration of activity in water off the controlled area was at Upper Pahranagat Lake, Nev., in the spring of 1955, amounting to 1.4 times 10 to the minus 4 microcuries per milliliter at 3 days after the detonation. This is one-thirty-sixth of the amount considered safe for continuous consumption.

Representative Holifield. Doctor, referring back to your statement on the Riverside motel cabin, what did you say their exposure was there?

Dr. Dunning. Estimated exposure to people, if continued to live there, was 7 to 8 roentgens.

Representative Holifield. We have in the committee record a document prepared in February 1955 by yourself, in which you say:

> In the case of Riverside Cabins, however, the radiological conditions were not ascertained until after the fallout had occured. The maximum infinity gamma dose in the later case was 12 to 15 roentgens.

Have you revised your opinion, or how do you reconcile your two statements?

Dr. Dunning. No, sir; I have not revised it. It is the difference in units. The infinity exposure is on the assumption that people live out of doors 24 hours a day, that there is no effect of weathering and shielding, that the material lies there, neither is lost by wind nor rains, nor does it sink into the ground.

We went back and made a study of that area and of the shielding effects of the homes and of the weathering, and were able to take a series of measurements of dose rate readings, with times, and by this we came up with this estimate of actual exposure of 7 to 8 roentgens. So the difference is in the units. Infinity does mean where they could

not have been any higher than this. In other words, we were giving outside limits, and a complete analysis of the situation led us to believe that the actual exposure would have been 7 to 8 roentgens.

Representative HOLIFIELD. Thank you very much.

Are there any further questions?

We have about 5 minutes. We have a few questions, Dr. Dunning.

If weather and terrain factors cannot be generalized, how reliably can one evaluate the situation at any particular locality? What information do we need to make such evaluation?

Dr. DUNNING. I did not hear the first part, Mr. Chairman.

Representative HOLIFIELD. If weather and terrain factors cannot be generalized, how reliably can one evaluate the situation at any particular locality?

(Discussion off the record.)

Representative HOLIFIELD. Dr. Dunning, will you please answer the question now?

Dr. DUNNING. From a precise scientific point of view this is certainly questionable. However, I think we are faced with the problem of either making our best estimates for planning purposes, or making none. It is on that basis that we have made our best estimates. We simply have said we know something about the effects of winds, we know something about the effects of rains; we will, therefore, try to generalize on what they might be under certain situations.

I do not think anyone is guaranteeing that they will be precisely this way in the case of an actual situation, and the same is true with terrain factors. Again we know some effects of terrain factors. We know the shielding effect of a hill, for example, and the unevenness of the ground as it affects the radiation exposure. Again we must generalize for planning purposes, or not generalize at all. I think that is our choice.

Representative HOLIFIELD. Dr. Dunning, due to the time, we are going to have to adjourn. I am going to hand you three questions here which I would like for you to prepare answers to, and then we will insert them at the conclusion of your testimony.

Dr. DUNNING. I would be delighted to, sir.

(The questions and answers referred to follow, together with a discussion of radiological safety criteria and procedures for public protection at the Nevada test site:)

Question. How was the 450 roentgens lethal dose figure established? What is the range of competent opinion on this number?

Answer. The lethal dose values for humans has been developed primarily from the Japanese data, plus extrapolations from animal experiments. The range of values for LD–50 values (half the people so exposed would die) are roughly from 375 to 650 roentgens. But this range is not as great as these figures might imply. As I have suggested a roentgen of dose as measured in air may deliver different doses within the body, depending upon the geometry of the source. That is, if the radiation is coming primarily from a point source such as the immediate radiation at time of burst, the radiation doses within the body will decrease as the rays pass through. On the other hand in the case of fallout the rays are entering the body from several directions and thus the doses will be more uniform within the body. Under this second set of conditions a lesser number of roentgens as measured in air could produce lethality. The 375 roentgens was estimated on the basis of fallout conditions while the 650 roentgens was for the immediate radiations from the burst.

Question. How constant is the relation between air dose and the biologically effective dose in view of the known gamma radiation energy changes with time?

Answer. It is correct that the energy spectra of gamma radiation dose changes with time and thus will affect the dose distribution within the body and the energy delivered to different parts of the body. Further, the energy spectra at any one time is quite complex, consisting of photons over a wide range of energies, except for long times after a detonation when only a relatively few isotopes remain, such as cesium 137. All of these do complicate the problem of estimating the biological effects. However, there are other variables, such as weathering and shielding and decay constants that have as great or probably greater influence in determining the effective biological dose accumulated.

Question. Compare the numbers derived from the $(\text{time})^{-1.2}$ law decay with that derived from the application of the known gamma emissions from the fission products.

Answer. The relation of $(\text{time})^{-1.2}$ was intended to apply to the actual disintegrations of the atom. We have accepted the rate of beta emissions as closely approximating the actual disintegrations of the atom. However, the ratio of gamma photons emissions to beta emissions varies with time (as does the gamma energies) so that the actual decay of *gamma dose rates* can deviate from the $(\text{time})^{-1.2}$. This deviation probably is not very great until several months after the detonation, when theoretical calculations indicate that the decay is significantly greater than $(\text{time})^{-1.2}$. This is shown in figure 4 of my written report. Of course, presence of any *induced* activity can also result in a departure from $(\text{time})^{-1.2}$.

Discussion of Radiological Safety Criteria and Procedures for Public Protection at the Nevada Test Site *

Gordon M. Dunning, United States Atomic Energy Commission, Division of Biology and Medicine, Washington, D. C., February 1955

INTRODUCTION

The criteria and procedures set forth in the following paragraphs were established after full consideration for protecting the health and welfare of the public, both in terms of radiological exposure as well as possible hazards, hardships, or inconveniences resulting from disruption of normal activities. Criteria are established as guides for the test organization in determining whether any special actions should be taken to protect the public.

With improved methods of predicting fallout and with the use of higher towers for detonating the nuclear devices, it is expected that fallout in populated areas from future tests at the Nevada test site will be less than the highest amounts which have occurred in the past.

Two basic assumptions are made in this report:

(*a*) It is the responsibility of the Division of Biology and Medicine to establish such criteria and procedures for the Atomic Energy Commission as deemed necessary to protect the health and welfare of the general populace from consequencies of weapons tests conducted at the Nevada test site.

(*b*) The operational procedures adopted for meeting these criteria and procedures shall be the responsibility of the test manager, as directed by the Division of Military Application, with the technical guidance of the Division of Biology and Medicine.

The following criteria do not apply to domestic or wild animals since levels of radiation which would be significant to them would have to be higher than those specified herein.

CRITERIA I. EVACUATION

Introduction

The decision to evacuate a community is critical for two principal reasons: One, presumably there might be a health hazard if the personnel were allowed

* This document was based on data and thinking of nearly 3 years ago. Since then the criteria have been revised and are reproduced on pp. 248 through 258. It is planned to revise further these criteria based on additional data and experience gained from operation PLUMBBOB (1957 test series at the Nevada test site).

to remain. Two, there is always an element of danger and/or hardship to personnel involved in such an emergency measure.

It is recognized that extenuating circumstances may accompany any situation where conditions indicate evacuation as a mode of action. The size of the community, areas, and accommodations available for the evacuees, means of transportation and routes of evacuation, disposition of ambulatory cases, protection of the property left behind, and many other factors may enter into the decision relative to evacuation. Further, it is recognized that, under certain conditions, the evacuation of a community might not only prove rather ineffectual but could result in more radiation exposure than if the population remained in place unless the situation be adequately evaluated. A blanket evaluation cannot be made in advance; each situation can be unique. The following criteria therefore are suggested as guides in assessing the possible radiological hazards; the final decision must be made on the basis of all relevant factors known at the time.

Criteria

Table I–a summarizes the radiological criteria to be used in evaluating the feasibility of evacuation.

TABLE I–A.—*Radiological criteria for evaluating feasibility of evacuation*

Effective biological dose [1] calculated to be delivered in a 1-year period following fallout	Minimum effective biological dose that must be saved by act of evacuation (otherwise evacuation will not be indicated)
Up to 30 roentgens	No evacuation indicated.
30 to 50 roentgens	15 roentgens.
50 roentgens and higher	Evacuation indicated without regard to quantity of dose that might be saved.

[1] The "effective biological dose" is an estimate of a biological "damage" dose, taking into account the length of time for delivery of a given dose, and the reduction of dose due to (*a*) shielding afforded by buildings and (*b*) the process of weathering.

The rationale for table I–a is as follows: The total effective biological dose that would be received if evacuation were not ordered is obviously a determining factor. Another consideration is the fact that such an action as evacuation could be dangerous to the individuals and could also possibly be detrimental to a very necessary national effort of weapons development. One must then ask, "Just how much will be gained (radiation dose saved) by evacuation?" Estimates of these two variables are indicated in table I–a. Thus, a populace may receive up to a calculated 30 roentgen effective biological dose in 1 year without indicating evacuation; from 30 to 50 roentgens, evacuation would be considered only if at least 15 roentgens could be saved by such action; and at 50 roentgens or higher evacuation would be indicated without regard to the possible savings in radiation dose.

In making a rough estimate of radiation doses, one may calculate a theoretical maximum infinity gamma dose and then arbitrarily divide by some number, such as 2, for an estimate of dose actually received. Whereas this may be satisfactory as a first approximation, a more accurate estimate should be attempted, especially when dealing with doses that might constitute a health hazard.

Owing to the necessity of making early measurements and decisions, it is to be expected that dose-rate readings, taken with survey meters, will be available evidence at the times of concern. Table I–b summarizes the parameters considered in estimating an effective biological dose based on dose-rate readings.

TABLE I–B.—*Predicting effective biological doses from dose-rate readings*

	A Theoretical maximum dose (based on best estimated rate of decay)	B Biological factor	C Attenuation and weathering factor	D Effective biological dose factor (column B×C)	E Effective biological dose (column A×D)
From time of fallout until time of evacuation	----------	1/1	1/2	1/2	----------
From time of evacuation to time of return [1]	----------	3/4	3/4	[2] 1/2	----------
From time of return to a time 15 days after initial fallout [3]	----------	3/4	3/4	[2] 1/2	----------
From 15 days until 1 year after initial fallout	----------	2/3	1/2	1/3	----------
Total	----------	----------	----------	----------	----------

[1] This estimate is based on the concept that if evacuation were not accomplished, then a certain radiation dose would be accumulated over the period of time selected. This time period also represents the radiation dose saved if evacuation were accomplished.
[2] The value of 9/16 has been rounded off to 1/2.
[3] This assumes that the time of return occurs before 15 days. A period of 15 days was selected to provide a dividing point between the time of initial exposure from fallout to a time 1 year later. The 15 days has not unique significance other than providing a basis on which to estimate the biological factor.

At a later time after fallout, better estimates of radiation doses received may be obtained from film-badge readings or dosimeters. If these film badges or dosimeters are worn on personnel and the evidence of their use supports the view that the readings are a reasonably accurate account of the radiation dose received, then the values recorded on the film badge or dosimeter may be accepted with a correction factor of 3/4 to account for the difference between the dose received by the film badge or dosimeter (including back scatter) and that received at the tissue depth of five centimeters. Table I–c may be used in estimating the effective biological dose from film badge or dosimeter readings.

TABLE I–C

	A Film badge reading	B Biological factor	C Film badge or dosimeter correction	D Effective biological dose factor (column B×C)	E Effective biological dose (column A×D)
From time of fallout until time of evacuation	----------	1/1	3/4	3/4	----------
From time of return to 15 days after initial fallout	----------	3/4	3/4	[1] 1/2	----------
From 15 days until 1 year after initial fallout	----------	2/3	3/4	1/2	----------
Total	----------	----------	----------	----------	----------

[1] The value of 9/16 has been rounded off to 1/2.

Discussion of the biological factor.—As longer periods of time are involved in the delivery of a given radiation dose, lesser biological effects may be expected. From the time of fallout until the time of evacuation probably will be a matter of hours, which has been considered essentially an instantaneous dose, that is, the biological dose factor is 1/1. From the time evacuation could be accomplished to time of return probably would be a matter of several days, so the biological factor has been estimated at 3/4. From 15 days after fallout until 1 year later is essentially a duration of 1 year, so the biological factor has been estimated at 2/3. It will be noted there is no calculation after 1 year, because it is expected under actual conditions of radiological decay and weathering that probably no significant dose will be delivered after a year's time in populated areas around the Nevada test site.

It is recognized that the precise quantities suggested for the biological factor cannot be supported by conclusive evidence. It is reasonable to expect that the delivery of a given radiation dose over a period of many days will have less

biological effectiveness than an instantaneous one (neglecting genetic effects) and that the extension of the period to essentially 1 year should yield a still lower biological factor. One piece of supportive evidence is the work of Strandgvist,[1] where X-ray doses to the skin were factionated into daily amounts, and the biological effects compared to a one-treatment dose. A log-log plot of total doses versus days after initial treatment yielded straight lines. For example, the curve for skin necrosis indicated a ratio of 3,000/6,700 roentgens for a 1-treatment versus 15 daily equally fractionated doses. Of course, daily radiation doses received from fallout are not equally fractionated, so that the ratio would be in the direction of unity. Day-by-day doses delivered from fallout from the 15th day to 1 year are more nearly equivalent than at early times (ignoring the weathering factor). Strandgvist data do not extend beyond 40 days and it is questionable to extrapolate his data in an attempt to derive a similar ratio as above based on 1 year, since other uncertainties are so great, that is, effects of weathering as affecting the rate of dose delivery, and so forth. The ratio would presumably be farther from unity than for a 15-day period. The skin is a relatively rapidly repaired organ and thus may tend to overemphasize the effects of fractionation when considering whole-body gamma doses.[2]

Cronkite reports:[3]

"In the dog, with cobalt gamma rays, the dose that will kill 50 percent of the dogs in a 30-day period when delivered in a single dose at roughly 15 roentgens per minute is approximately 275 roentgens. After this dose of radiation the animals become ill within a period of 7 to 10 days and deaths occur between the 8th and 25th day. Hemorrhage, infections, and profound anemia are prevalent. If the dose is decreased to 100 roentgens per day given over a 14-hour period, the lethal dose is increased to 600 to 800 roentgens. Under both conditions, the animals die in approximately the same period of time with identical manifestations. If the exposure is dropped to 25 roentgens per day given over a 14-hour period, the lethal dose is then increased to well over 1,200 roentgen, and the symptoms and findings are changed."

One problem in such experiments is the evaluation of possibility that the animals may be virtually dead while the exposures are continued. This might be illustrated in experiments using the burro where the daily doses of 400, 200, and 100 roentgens given to 3 separate groups required 3,600 to 4,000, 2,800 to 3,200, and 2,000 to 2,600 total roentgens, respectively, for 100 percent lethality.[4]

Experimental data reported by Boche[5] are summarized below.

Number of days	Dose per day (roentgens)	Dose per week (roentgens)	Survival time (weeks)	Total dose (roentgens)
20	10	60	24	1,440
10	6	36	83	2,988

NOTE.—Unfortunately normal survival times were not given nor were the ages of the animals (dogs).

Blair[6] has taken the two points from Boche's data, inserted these into his (Blair's) equation relating reparable and irreparable damage. The ratio of instantaneous dose to 15-day dose is 350/450 or 0.78, and for 4 months' dose about 350/525 or 0.67.

Blair suggests that "the points are too few to determine the constants (of the equation) with any accuracy but should at least be in the proper range." However, the constants of his equation have checked well with more extensive data on other animals. His equations indicate that the rate of recovery of reparable injury is fastest in the mouse (of the types of mammals selected), about one-half as fast in the rat, and about one-seventh as fast in the guinea

[1] Sievert, Rolf M. The Tolerance Dose and the Prevention of Injuries caused by Ionizing Radiations. British Journal of Radiology, vol. XX, No. 236, August 1947.

[2] See addendum.

[3] Medical Aspects of Radiological Defense. Cronkite, E. P. Lecture to Federal Civil Defense Administration, Regional Conference of Northeastern States of Radiological and Chemical Defense, New York City, October 22, 1953.

[4] UCLA-295. Response of the Burro to 100 Roentgens Fractional Whole-Body Gamma Ray Radiation. Haley, T. J., et al. June 10, 1954. Unclassified.

[5] MDDC-204. Observations on Populations of Animals Exposed to Chronic Roentgen Irradiation. Boche, R. D., 1947. Unclassified.

[6] UR-207. A Formulation of the Injury, Life Span, Dose Relations For Ionizing Radiations, II. Applications to the Guinea Pig, Rat, and Dog. Blair, H. A. July 3, 1952. Unclassified.

pig and dog, but as Blair pointed out, the reaction of the dog is more representative of the larger, longer lived animals.

Discussion of the attenuation and weathering factor.—From the time of fallout until the time of evacuation it is expected that personnel will be kept indoors. (See criteria II.) Major losses due to weathering cannot be relied upon during this period, so that the estimated factor is 1/2. From the time evacuation could have been accomplished until the time of estimated return it is assumed that personnel will be indoors aboout half of each 24 hours and that major losses due to weathering cannot be relied upon. The overall factor is thus 3/4.

The same reasoning applies to the third period of time, i. e., from assumed time of return to 15 days after fallout.

From 15 days after fallout until 1 year later it is estimated that the attenuation due to buildings and the effects of weathering will yield an overall factor of 1/2.

Dose-rate readings have been taken with survey meters outside and inside of houses around the Nevada test site after fallout occurred. The ratio of readings varied with the type of construction of the house and with the location within the building. Generally, the ratio of readings outside to inside a frame house was about 2/1 with a somewhat greater difference for masonry construction. A limited number of film badges were placed outside and inside of some houses during Tumbler-Snapper and also Upshot-Knothole. In the first case, the difference in total doses was again 2 to 1 or greater, but during Upshot-Knothole only about a 20 percent difference was noted. In fact, in one case during Upshot-Knothole the film badge inside read higher than outside. The differences between these experimental data will have to be investigated during future operations.

The very nature of the weathering factor makes this a difficult paremeter to evaluate. The probability of occurrence of precipitation and/or winds and to what degree has to be estimated, as well as their effects on radiation levels. Leaching effects were studied on soils about 130 miles from ground zero where fallout had occurred during Upshot-Knothole. Dose-rate readings were insignificantly lower than those predicted by radiological decay according to $t^{-1.2}$ after a period of more than 1 year. One example of the effects of winds was observed during Upshot-Knothole. The fallout from the March 17, 1953, detonation was in a long narrow pattern to the east of ground zero. The second day after a fallout a rather strong surface wind blew almost at right angles across the area, for about a period of a day. Dose-rate readings were taken on the first and fourth days at the same locations and then were compared. The fourth day dose rates were less, by factors of 3 to 6, than those to be expected from the first day's readings, based on rate of decay of $t^{-1.2}$. (Other fallout measurements indicated that the rate of decay of this fallout material was not significantly different from $t^{-1.2}$.) Because of the physical conditions described above, these reductions in contamination probably are near the upper limit to be expected from wind.

Operational feasibility of criteria

It is not the intent here to discuss operational procedures, but it should be indicated that the computing of radiation doses as recommended in criteria I is a not too difficult task. If one assumes a $t^{-1.2}$ rate of decay as a first approximation, then a single graph of dose rates versus times after detonation can be constructed that will represent a 30 roentgen effective biological dose for 1 year. An additional family of curves can be made that will provide the answers to the parameters of how much time would be available before evacuation and of how long a time personnel would have to remain out of the radiation area in order to provide for a savings of at least 15 roentgens.

The highest whole-body gamma dose recorded for any locality where personnel were present outside the Nevada test site was at Riverside Cabins, Nevada (about 15 people). following shot No. 7 of Upshot-Knothole. The maximum theoretical infinity gamma dose was estimated to be 12 to 15 roentgens.

CRITERIA II. PERSONNEL REMAINING INDOORS

When the gamma dose-rate reading as measured by a survey meter held 3 feet above the ground reaches the values given in graph II at the times indicated, it is recommended that personnel shall be requested to remain indoors with windows

and doors closed. Release from this restrictive action should be made on the basis of further evaluation of the radiological conditions.

In the event that there be convincing evidence that the radiation levels given in the graph will be reached, it is recommended that personnel be requested to remain indoors before fallout occurs or before the radiation levels equal those in graph II. Release from this restrictive action should be made on the basis of further evaluation of the radiological conditions.

It is recommended that people who had been out-of-doors during fallout of the above magnitude or greater be advised to change clothing and to bathe. The clothing may be cleaned by normal means. While bathing, special attention should be paid to the hair and any exposed parts of the body.

In the event that the monitoring takes place after the fallout has occurred, and extrapolation of the dose-rate readings equals or exceeds those in graph II at the estimated time of fallout, then it is recommended that the same advice be given as in the preceding paragraph.

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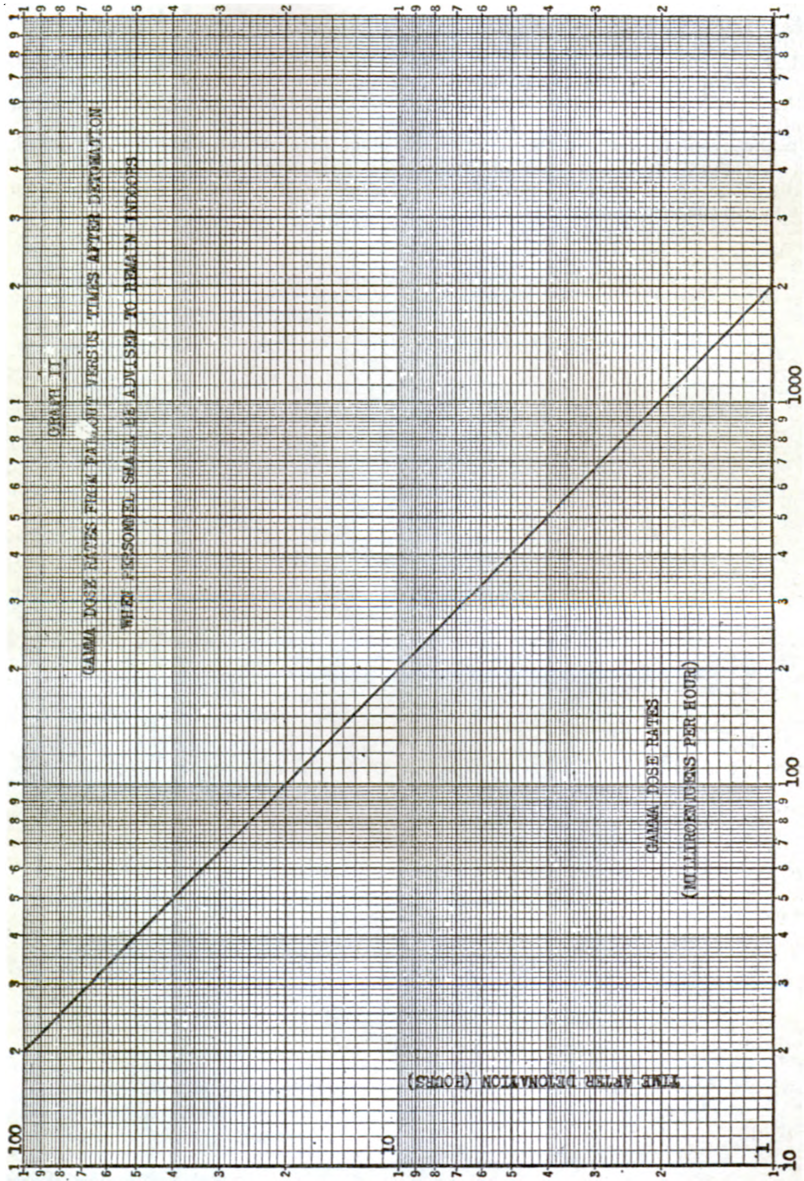


CHART II
GAMMA DOSE RATES FROM FALLOUT VERSUS TIMES AFTER DETONATION
WHEN PERSONNEL SHALL BE ADVISED TO REMAIN INDOORS
GAMMA DOSE RATES
(MILLIROENTGENS PER HOUR)
TIME AFTER DETONATION (HOURS)
10
100
1000
1
10
100

Discussion

The action of requesting personnel to remain indoors is predicated on the principle that the radiation levels are below those established for evacuation and that this action could reduce the amount of contamination of personnel and reduce somewhat the whole-body gamma dose. (See appendix A for estimates of reduction in whole-body gamma dose.) The actual "savings" healthwise have to be balanced against possible adverse public reaction.

The principal gain in requesting personnel to remain indoors is to prevent or reduce the amount of atomic debris that may actually fall on the body or clothing. Since the peak of fallout usually occurs shortly after the start of fallout, it is important that prompt decisions and actions be taken. Thus, by necessity, the most practical criteria upon which to base a decision are gamma dose rate readings, which are in turn related to the amount of fallout.

Beta dose to skin.—The most immediate solution might be to establish lower permitted dose rate levels at later times after detonation. However, if a series of dose rates are established for increasing times after detonation so that their relationship follows $t^{-1.2}$, then the doses delivered in X hours (before the material is washed off) will be greater for earlier times after detonation. If one were sure of the time that the fallout material was to remain in place, then a scale of dose rates versus time after detonation could be made to yield the same total dose over the X hours. Since there is obviously no set time period for duration of contact that would be valid for all cases, one might assume the worst case where the material remains in place until its activity has decayed to an insignificant level. Dose rates could then be approximated, to yield a given infinity dose, by:

$D=5At$ where: D=infinity dose; A=dose rate at time "t".

If the above discussion is accepted, then the remaining question is to set the infinity dose. Here, we must be clear that whereas the measurements taken by the monitors, and the data upon which action will be decided will be gamma dose-rate readings, the point of principal concern is the beta dose delivered to the basal layer of the epidermis (assumed as 7 milligrams per square centimeter). The ratio of emission of beta to gamma is a function of time after detonation and follows no simple relationship. Further, this ratio at any given time after detonation has not been firmly established. One report suggests the following data:

Time after detonation:	*Beta/gamma*
72 hours	157/1
168 hours	156/1

These data were obtained from a cloud sample rather than actual fallout material, and were a measure of surface dose on a plaque using a "dosimeter type beta-ray surface ionization chamber."

The method of collection suggests the possibility that the thickness of material on the plaques may be less than that to be expected from the amount of fallout that would be of concern when estimating probabilities of beta burns. This would result in a different angular distribution of the betas influencing the beta dose rate in the direction of a higher value for the plaques.

Another report indicates a beta to gamma ratio of 130 to 1 based on theoretical computations. A third report suggests a radically lower ratio; however, there may be some doubt as to its conclusions since the ionization chamber, used to measure gammas only, had a wall thickness of 1 mm. of bakelite which "* * * excluded a small part of the total gamma dose present, as well as a large, but unknown, fraction of the beta." (The range of 0.35 Mev. betas is about 100 mg./cm.^{2} or approximately 1 mm. of bakelite.) For our discussion here, we will assume a *surface* beta to gamma ratio of 150 to 1.

In estimating the beta dose to the basal layer of the epidermis, one may refer to the work of Henriques.[7] He exposed the skin of Chester White pigs to plaques containing different radioisotopes. Pertinent data are abstracted as follows:

Isotope	Energy	Surface dose required to produce recognizable trans-epidermal injury (roentgen-equivalent-beta)	Estimated amount of radiation that penetrated skin to a depth of 0.09 mm. (roentgen-equivalent-beta)
Yttrium 91	1.53	1,500	1,200
Strontium 90	.61	1,500	1,400
Ytrrium 90	2.20		

The average maximum energy of the beta particles from fallout material varies with time but will be assumed to be roughly comparable, in respect to depth dose, to yttrium 91 or Sr–90—Y–90. Since the gamma dose at a depth of 7 mg./cm.^{2} would not be significantly different from the surface gamma dose, the ratio of 130 to 1 for beta-gamma will be assumed at the basal layer of the epidermis.

(One experiment with sheep, using Sr–90—Y–90 plaques, showed that 2,500 reps at the plaques' surface produced ulceration in 1 but not another of 2 sheep.[8] On the other hand, 1,000 rads delivered to tissue depth of 7 mg./cm.^{2} from a P^{32} 1-inch diameter disk (type of animal not stated) produced tanning, prolonged erythema, and desquamation.)

It is to be remembered that the above discussion was first based on *surface* gamma dose rates whereas the monitors will be making their gamma measurements at a height of 3 feet. Past field experience has indicated that the gamma reading from ionization-type survey meters at ground level is about 50 percent higher than at 3 feet. Therefore, if it be assumed that a ground level gamma reading of a survey meter is equivalent to a surface dose rate, the ratio of beta dose rate at 7 mg./cm.^{2} to gamma dose rate at *3 feet* is about 200 to 1.

Another approach to estimating the ratio of beta dose rate at 7 mg./cm.^{2} to gamma dose rate at 3 feet is as follows: Assuming a uniform distribution of 1.0 megacurie per square mile of gamma activity, the dose rate reading from an infinite field is about 4.1 roentgens per hour.[9] Calculations given in appendix B indicate that a like concentration of fallout material will produce about 430 reps per hour at 7 mg./cm.^{2} This suggests a beta to gamma ratio of about 100 to 1 which is about a factor of 2 lower than the first approach. Added support to this latter method of estimating beta doses is found in appendix C.

Such considerations may be fraught with pitfalls. For example, the above discussion implies a uniform distribution of fallout material. Obviously this is not correct, but how far this deviates from the facts and to what extent this influences the results is difficult to assess. Calculations indicate that the production of recognizable beta burns from a single particle requires a high specific activity. (See criteria III for discussion.) It may well be, however, that the particles of fallout are close enough to have overlapping of radiation fields and thus require significantly lower specific activity of the particles to produce beta burns. This hypothesis has support in that even the most superficial beta burns of the natives exposed to fallout following the March 1, 1954, detonation showed a general area affected rather than small individual spots. On the other hand, the cattle and horses exposed near the Nevada test site showed burns over areas only about the size of a quarter. Even though these may not have been produced by single particles, they do represent less of an area effect than suggested for the natives. Also, radioautographs of the fallout in areas outside the Nevada test site suggest the occurrence of individual particles with nonoverlapping of radiation fields. However, in nearby areas where the fallout was relatively heavy, there was a definite overlapping of the fields.

[7] Effect of Beta Rays on the Skin as a Function of the Energy, Intensity, and Duration of Radiation. Henriques, F. W. Laboratory Investigation. Vol. 1, No. 2. Summer 1952.
[8] Comparative Study of Experimentally Produced Beta Lesions and Skin Lesions in Utah Range Sheep. Lushbaugh, C. E., Spalding, J. F., and Hale, D. B. LASL, November 30, 1953. (Unclassified.)
[9] Effects of Atomic Weapons. 1950.

With our present knowledge it should be stated that due to the particulate nature of fallout it would not be possible to establish reasonable and operationally workable criteria that at the same time would guarantee that there *never* would be an occurrence of a beta burn.

If one were to accept the assumed beta to gamma dose rates of about 100–200 to 1 (measured under the conditions given above), this might mean an infinity beta dose of 1,000 to 2,000 reps to the basal layer of the epidermis when the whole body infinity gamma dose was 10 roentgens. Of course, the fallout material may be removed before the infinity dose is delivered; yet, on the other hand, it is not improbable that it could remain in the hair for essentially this length of time. In the case of a 1-hour fallout, almost one-half of the dose would be delivered in the next 24 hours.

The efficiency of a surface for collecting and holding the fallout material is important. It is not surprising that the highest dose rate readings as well as biological effects were noted on the hair of the natives and also on parts of the exposed body where perspiration was present. Further, it was observed that even one layer of light cotton material was sufficient to protect against beta skin damage in most cases.[10] This was due probably not to the relatively small attenuation of the betas by the clothing but rather to the physical situation of holding the radioactive material at some distance from the skin, which effect would be relatively large.

An added consideration is the possibility of high beta doses delivered to personnel from the fallout material lying on the ground and other surfaces. If the highest degree of contamination considered under this policy is safe when in direct contact with the skin, then the beta dose from an equally contaminated ground will not be hazardous. (See criteria III for discussion on unequal contamination on personnel.) However, it is true that the contamination may exceed the amount to deliver dose rates given in graph II and yet not be great enough to consider evacuation. Some personnel may not go indoors, and those who did will eventually be released from this restrictive action and then may walk around in a relatively highly contaminated area. Because of the more limited range of the beta, the location of greatest concern is the lower legs.

One report estimates a beta to gamma dose rate ratio of about 75 to 1 at 10 centimeters above the ground.[11] Under criteria I it was recommended that consideration be given to evacuation when the gamma dose rate reading at 3 feet was, for example, about 6.2 roentgens per hour at H+3 hours. Roughly, this would correspond to about 575 reps per hour of beta at 10 centimeters. Of course, this activity decays, and also it is presumed that personnel would be sent indoors, at least for a few hours. On the other hand, it strongly suggests that biologically significant doses may be delivered to the feet if not protected. Skin lesions were frequent on the bare feet of the natives evacuated during Castle. This probably was a combination of beta dose from material on the ground and from that scuffed up over the bare feet and then clinging to the skin. (No lesions were observed on the bottom of the feet, undoubtedly due to the thick epidermis.) It would be expected that normal closed-type footwear (as compared to open sandals) would afford adequate protection to the feet from such high beta doses as discussed here. There is still no guaranty that beta radiation from material on the ground will not deliver significant biological doses to the ankles and perhaps lower legs, after personnel are released from staying indoors. For example, if the beta dose at 10 centimeters above the ground is 575 reps per hour at H+3 hours, it would be about 250 reps per hour 3 hours later and 160 reps per hour 6 hours later.

One further possibility is the accumulation of radioactive material around the ankles and lower legs resulting from normal walking about the area. This is discussed under criteria III.

Data on human exposures.—The work of Henriques[12] suggests that at the depth of 0.09 mm. in living porchine skin (maximum thickness of epidermis) that "1,400±300 roentgen-equivalent-beta" (delivered over short periods of time so that they may be assumed to be instantaneous) is required to produce recognizable transepidermal injury. The curve of biological damage rises rather

[10] ITR–923. Study of Response of Human Beings Accidentally Exposed to Significant Fallout Radiation, Cronkite, E. P., et al. May 1954.
[11] AD–95 (H). An Estimate of the Relative Hazard of Beta and Gamma Radiation from Fission Products. Condit, R. I., Dyson, J. P., and Lumb, W. A. S. NRDL 1949. (Unclassified.)
[12] Op. cit.

sharply so that at a dose of just under 2,000 reps (at 0.09 mm.), the epidermis may be expected to exfoliate and in the majority of cases go on to develop chronic radiation dermatitis persisting for months.

The preceding discussion suggests that, using the gamma dose rates listed in these criteria, which are based on an estimated 10 roentgen infinity gamma does, as high as 2,000 reps might be delivered to the basal layer of the epidermis over a period of time covered by the lifetime of the radioactive material.

There have been instances where the calculated infinity gamma dose in areas where personnel were present around the Nevada test site have reached 12 to 15 roentgens, but there have been no known cases of beta burns in these areas. The number of persons involved in these areas of highest contamination was relatively small, perhaps a few dozen, and with an observed duration of fallout of about 1 hour it is possible that they were not in a position to receive the full fallout. Likewise, minute areas of the skin may have been so affected yet not detected or reported. In other areas encompassing some 2,000 people the infinity gamma dose was about 8 roentgens and no instances of beta injury appeared.

The estimated whole-body gamma dose to natives evacuated from the island of Utirik following the March 1, 1954, detonation at the Pacific Proving Ground was about 15 roentgens for a period of about 3 days, but no beta burns appeared. It is fair to assume here that direct contamination took place due to their mode of living, including housing that was quite open to air currents. Gamma dose rate readings were taken over the bodies of the natives at about H+78 hours both on the beach and after boarding the ship. On the beach the personnel readings averaged about 20 mr. per hour gamma (but this probably included some contribution from the ground contamination), and after wading through the surf and boarding the ship the levels averaged 7 mr. per hour gamma.

The 18 natives on Sifo Island, Ailinginae Atoll, received an estimated whole-body gamma dose of 75 roentgens in about 2¼ days. Of these, 14 later experienced slight beta burns, 2, moderate burns, and none showed epilation.

In the case of the Rongelap natives, the estimated whole-body dose was about 170 roentgens in about 2 days. All 64 natives later experienced beta burns to some degree from slight to severe, and over half of the natives showed epilation from slight to severe.

The 16 natives from Rongelap evacuated directly by air to Kwajalein had personnel gamma dose-rate levels generally 80 to 100 mr. per hour although 1 was as high as 240 mr. per hour and 1 as low as 10 mr. per hour (at H+ about 55 hours). The remaining 48 natives evacuated by ship were reported to have personnel readings that "averaged" 60 mr. per hour before decontamination. The picture is further confused because some of the natives had bathed and some had not before the arrival of the evacuation team.

Most of the 28 United States service personnel stationed on Eniwetok Island, Rongerik Atoll, received about 40 to 50 roentgens, based on film badge readings. Three members of the group who were located for part of the time in another section of the island were estimated to have received somewhat higher doses. Seventeen of the twenty-eight personnel showed only slight, superficial lesions with one questionable case of epilation. It should be pointed out that the personnel were in metal buildings during some of the fallout time and for most of the time thereafter until evacuation. This reduced the direct contamination as well as the whole-body gamma dose. A film badge hanging on the center pole of a tent at one end of the island read 98 roentgens. Calculations based on dose-rate readings at another part of the island indicated somewhat lower doses, if personnel had remained in the open for the period of time from fallout (about H+7.5 hours) to evacuation (at about H+34 hours). Upon arrival at Kwajalein 1 personnel gamma dose rate reading was as high as 250 mr. per hour at about H+35 hours.

The above data do suggest that there may be possible a rough bracketing of gamma-beta doses versus beta burns. On the one hand, the natives from Utirik received an estimated whole-body gamma dose of 15 roentgens and showed no evidence of beta burns. On the other hand, the natives on Sifo Island, Ailinginae Atoll, received about an estimated whole-body gamma dose of 75 roentgens, with 14 personnel showing slight burns, 2, moderate burns, 2, no burns, 3 with moderate epilation, and 15 with no epilation. In addiiton, Rongelap natives received 170 roentgens whole-body gamma dose, and about 90 percent showed some degree of lesions and 56 percent some degree of epilation.

It is to be recalled that: (*a*) The natives probably were out of doors and received the full fallout; (*b*) the oily hair, seminaked, perspiring bodies, including bare feet, and lack of bathing for most, would tend to collect and hold the fallout material; (*c*) the time of delivery of essentially all of the doses was 2 to 3 days. Further, it may be speculated that the fallout on the more distant island of Utirik (about 300 statute miles) would consist of smaller particles and also perhaps lesser possibility of overlapping of radiation fields from these particles.

Some of the relevant data are summarized in table II. Due to the uncertainty of the degree of exposure of personnel on Rongerik to the direct fallout, this group is not included. It is to be immediately emphasized that any comparisons made or implied in the table are at the most only semiquantitative. Table II will be referred to in criteria III and IV but is included here as a summary of the data discussed above.

TABLE II

I	II	III	IV	V	VI			
Location	Estimated time of fallout (hours)	Best estimate of whole-body gamma dose (roentgens)	Skin effects	Personnel reading	Best estimate of average dose rates (mr./hr.) of the islands (taken at 3 feet above the ground) and of natives (personnel readings) after removal from radiation field, both at approximately same time			
					Island	Personnel	Ratio	Approximate time
Rongelap	5½	170	Lesions: 6 none. 19 slight. 22 moderate. 17 severe. Epilation: 28 none. 11 slight. 11 moderate. 14 severe.	(a) Majority: 80–100 mr./hr. at H+54 hours.[1] (b) Average: 60 mr./hr. at H+50 hours. Corrected average: 80 mr./hr.[3]	1300	80	16/1	H+50 hours.
Ailinginae	5½	75	Lesions: 2 none. 14 slight (very superficial). Epilation: 15 none. 3 moderate.	Average: 40 mr./hr. at H+52 hours. Corrected average: 53 mr./hr.[3]	410	53	8/1	H+52 hours.
Utirik	16–18	15	Lesions: None. Epilation: None.	Average: 20 mr./hr. Assumed: 15 mr./hr. at H+78.[4]	110	15	7/1	H+78 hours.

[1] 16 natives evacuated by air to Kwajalein and monitored upon arrival.
[2] 48 natives evacuated by U. S. S. *Philip* and monitored aboard the ship. Data suggest meter readings low by about 50 percent since natives from same island read 80 to 100 mr./hr. at Kwajalein some 4 hours later with calibrated meters.
[3] 40 mr./hr. corrected to 60 mr./hr. according to information in footnote 2. Report did not indicate range of values among individuals nor at different parts of body.
[4] Readings taken by monitors from the *Renshaw* on the Utirik beach where there may have been some contribution to dose rates from land. After wading to ship, average personnel readings were 7 mr./hr.

Data on animal exposures.—The data on animal exposures are less firm than those for humans. Unmistakable beta burns occurred on cattle at Alamogordo in July 1945, on cattle at the Nevada Proving Grounds in spring 1952, and on horses in spring 1953. (The skin damage observed on sheep in the spring 1953 was not established to be beta burns.) However, the exact positions of the animals in relation to known amounts of fallout are not clear.

Following the last detonation of the spring 1952 series at the Nevada Proving Grounds, about one-half of a herd of 150 head of cattle were found to have evidence of beta burns. They were thought to have been 15 to 20 miles from ground zero in Kawich Valley to the northeast and to have been exposed to fallout from the last detonation. Highest dose rate readings taken along a dirt road running lengthwise through this valley integrated to 75 to 100 infinity gamma doses.

During Upshot-Knothole, 16 horses showed skin lesions over the back, and eye damage was noted in a few. The best evidence indicated that the horses were some 10 to 12 miles to the east of ground zero on March 17, 1954, where the fallout occurred from the first detonation (about 15 KT on a 300-foot tower). Radiation levels in this area are not known with certainty, but the fallout occurred in a narrow band and was carried by relatively high velocity winds so that it probably fell on the horses at a time less than 1 hour. If so, probably more than one-half of the infinity dose was delivered during the next day.

Addendum

Since the original discussion above was written, further consideration has been given to the work of Strandgvist and others [13] on the effect of fractionation of doses delivered to the skin and the onset of the observed results. It will be recalled (p. 10) that X-ray doses to the skin were fractionated in equal daily amounts, and the biological effects compared to a one-treatment dose. A log-log plot of total doses versus days after initial treatment yields straight lines.

Basically, this means that as doses are being delivered to the skin a certain rate of repair is taking place. The overall effect might be that higher initial doses from fallout material might be allowed than if one were to integrate the dose over a period of time without consideration for the repair. Because of the difference in shapes of the total beta dose curves for varying times of initial fallout versus Strandgvist X-ray curves the difference between the two curves cannot be expressed as a simple relationship.

Strandgvist quotes a 1,000 roentgen dose in 1 treatment to produce erythema using X-rays (a somewhat smaller number than other data quoted above), 1,250 roentgens if divided into 2 equal daily doses, 1,450 roentgens if divided into 3 equal daily doses, etc. Of course, there are differences between these X-ray doses and beta doses from fallout material, such as differences in doses at increasing depth of tissue and the fact that the X-rays were delivered essentially as an instantaneous dose at intervals of a day while the beta dose rates are assumed to follow the $t^{-1.2}$. However, accepting the assumptions of biological equivalence of these roentgen and beta doses and $t^{-1.2}$, one may then ask the question, "What will the beta dose rates at varying times after detonation that the contamination occurs such that the integrated doses to the skin will at no time equal Strandgvist curve for erythema?"

For early fallout times the limiting factor will be to keep the first day's beta dose below 1,250 reps; for later times of initial fallout the first day dose may be less than 1,250 reps but subsequent accumulative doses may be greater than Strandgvist curve. A family of curves was prepared of beta dose rates versus time after contamination such that each would meet but not exceed Strandgvist curve for erythema for times out to 40 days, then, based on the discussion contained under Criteria I, a conversion factor of 125 was selected to convert beta dose rates at a depth of 7 mg./cm.^{2} of tissue to gamma dose rates at 3 feet above an infinite plane. These gamma dose rates are plotted in appendix C (a).

If one accepts all the assumptions that go into preparing this curve, then one does not have to estimate the variable of how long the fallout material was in contact with the skin, for the curve suggests that as long as the initial indicated gamma dose rates are not reached, then erythema might not be expected to appear. (However, this approach still does not give assurance that *single* hot particles will not produce erythema.)

Generally, the gamma dose rate readings in the curve (appendix C (a)) suggest theoretical maximum infinite gamma doses of about 20 roentgens for a 1-hour fallout, to about 55 roentgens for a 2-day fallout. For those early times after detonation when relatively heavier fallout might be anticipated, this in-

[13] Sievert, Rolf M. The Tolerance Dose and the Prevention of Injuries Caused by Ionizing Radiations. British Journal of Radiology. Vol. XX, No. 236, August 1947.

finity gamma dose is 2 to 3 times greater than the 10 roentgens which was used as a basis of developing criteria II. However, there are two further considerations: One, the interpretation of the data, and certainly the assumptions made in developing the curve in appendix C (a) are open to discussion. Two, if one accepts the interpretations and assumptions it means a safety factor of 2 to 3—not an unreasonable quantity.

Operational feasibility.—Under the criteria recommended in criteria II, there would have been two occasions in the past where personnel would have been requested to remain indoors. Once was at Lincoln mine following the second detonation of Upshot-Knothole where they were so requested to remain indoors for 2 hours and the other occasion would have been at Riverside Cabins (population about 15) following the ninth detonation of the same series. The dose rate reading at Lincoln mine was 580 mr. per hour at H+2. In the case of Riverside Cabins, however, the radiological conditions were not ascertained until after the fallout had occurred. The maximum infinity gamma dose in the latter case was 12 to 15 roentgens.

Personnel were requested to remain indoors (for about 2 hours) following the ninth detonation of Upshot-Knothole. The highest dose rate reading was 320 mr. per hour at H+4.5 hours. This is less than the current recommendations.

CRITERIA III. DECONTAMINATION OF PERSONNEL

Where it is not possible to monitor personnel outside of a general radiation field, it is recommended that an estimate be made of the degree of personnel contamination by determining the location of the individual at the time of fallout. In the event there is uncertainty as to the validity of such an estimate, the assumption will be made that the individual was out-of-doors. In those areas where the infinity gamma dose equals or exceeds 10 roentgens, it is recommended that the individual be advised to bathe and to change clothing.

For personnel being monitored outside the general radiation field where personnel contamination exists over relatively large areas of the exposed body (one-half square foot or more):

> When the reading of a survey instrument held with the center of the probe or center of the ionization chamber 4 inches from the center of the contaminated area equals or exceeds the values given in graph III, it is recommended that personnel shall be advised to bathe and to change clothing.

For personnel being monitored outside the general radiation field, where personnel contamination exists over relatively small areas of the exposed body (less than one-half a square foot):

> The recommended maximum values shall be one-half those given in graph III. Monitoring of the head, arms, hands, lower legs, and feet will be considered as coming under this category. Washing may be limited only to the contaminated parts, and also a change of clothing may not be indicated unless the radiation levels exceeds those stated below concerning monitoring of exterior surfaces of clothing.

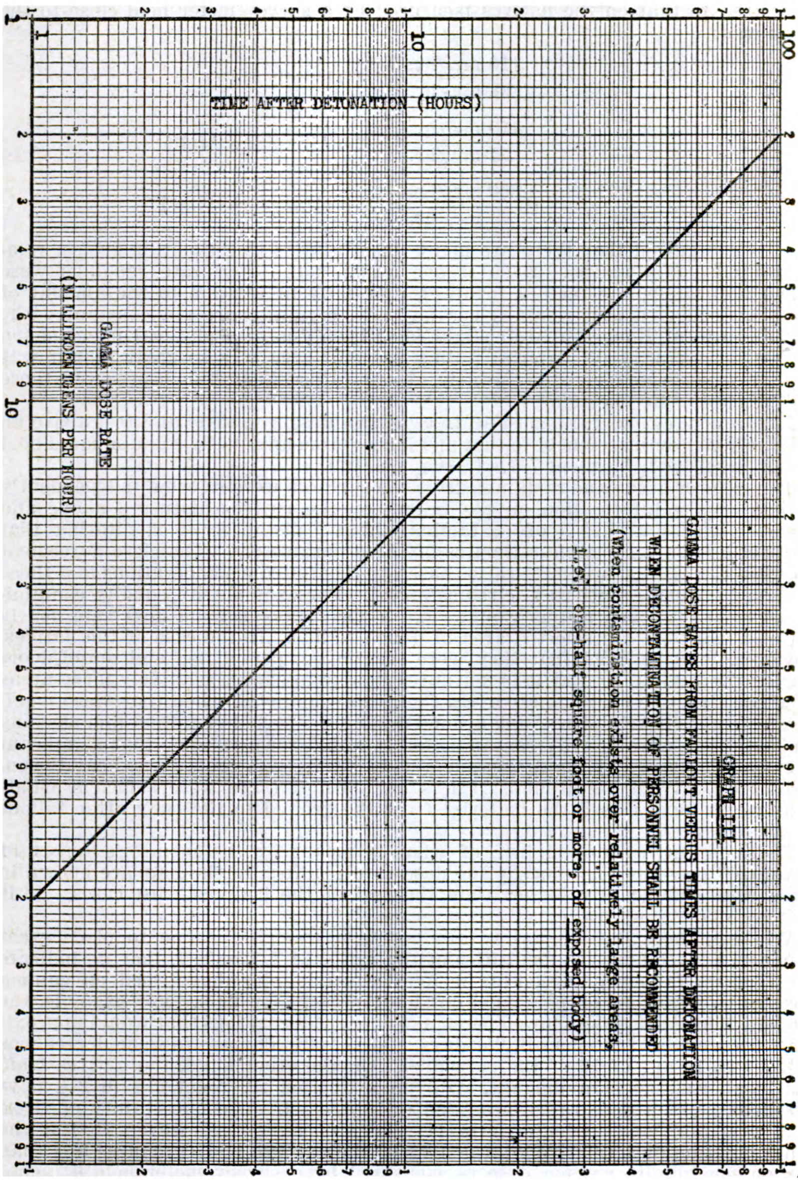
For personnel being monitored outside the general radiation field, and the contamination exists over only spots of exposed body (about the size of a half dollar or less):

> The recommended maximum values shall be one-fifth those given in graph III. Washing may be limited only to the contaminated parts, and also a change of clothing may not be indicated unless the radiation levels exceed those stated below concerning monitoring of exterior surfaces of clothing.

For personnel being monitored outside the general radiation field and the contamination exists over any size area on the exterior surface only of the clothing:

> The recommended values under these conditions will be twice those given in graph III. The first recommended action shall be to resort to such simple acts as brushing off the clothing. If this action does not reduce the radiation levels to twice those given in graph III or less, then personnel shall be advised to change clothing and to bathe.

When the general contamination of a community of the degree to produce an estimated maximum theoretical infinity gamma dose of 20 roentgens or greater, personnel who have been out-of-doors at any time during the first 2 days and generally moving around in the area (as opposed to such an act as walking only between a building and a vehicle) shall be advised to brush off the footwear (outdoors), to bathe, and to change clothing as soon as possible after the final return indoors each day. In addition, personnel who go out-of-doors for any length of time during the first 2 days after such a fallout shall be advised to wash their hands at least after the final return indoors each day, and more frequently if possible.


GRAPH III
GAMMA DOSE RATES FROM FALLOUT VERSUS TIMES AFTER DETONATION
WHEN DECONTAMINATION OF PERSONNEL SHALL BE RECOMMENDED
(When contamination exists over relatively large areas,
i.e., one-half square foot or more, of exposed body)
TIME AFTER DETONATION (HOURS)
GAMMA DOSE RATE
(MILLIROENTGENS PER HOUR)
1
10
100

Discussion

Data on humans.—In table II it was suggested that the relative average gamma dose rates from an infinity contaminated field at 3 feet above the ground compared to that on the natives measured by a survey meter held close to the body was:

$$\frac{110 \text{ mr./hr.}}{15 \text{ mr./hr.}} \cong 7/1 \text{ (Utirik Atoll)}$$

$$\frac{410 \text{ mr./hr.}}{53 \text{ mr./hr.}} \cong 8/1 \text{ (Ailinginae Atoll)}$$

$$\frac{1{,}300 \text{ mr./hr.}}{80 \text{ mr./hr.}} \cong 16/1 \text{ (Rongelap Atoll)}$$

It is recognized that there are many uncertainties in estimating such a relationship by this means. Even if one assumes the dose rate readings were taken accurately, the factors involved, especially in relation to the amount of material collected and retained on the body, certainly are not constant. The higher ratio at Rongelap Atoll might have been due to a physical phenomenon where the quantity of material falling per unit area was so great that it was not retained so completely on the body. Even if this explanation is accepted, there still remain many questions.

Theoretical considerations indicate a gamma dose rate ratio at 3 feet above an infinitely contaminated field to that at 4 inches from an equally contaminated field of 6-inch radius to be about 7/1. (See appendix D.)

The sizes of areas and distances from the surfaces were selected independently of any of the information on the fallout on the natives discussed above and were estimates of areas of contamination and distances of monitoring that appeared to be reasonable estimates of these parameters. The close agreement between the gamma dose rate ratios based on theoretical considerations and those observed with the natives is circumstantial. For example, an equally contaminated area of 3-inch radius would yield a theoretical gamma dose rate nearly 3 times less than the selected area of 6-inch radius. In the case of the natives, however, it is believed that they were seminaked, perspiring, and out-of-doors during the fallout, so that it is not unreasonable to expect relatively large areas of the body to be contaminated. In fact, this was noted when they were monitored. By their acts of walking around during the period of fallout and sleeping on mats that were heavily contaminated it would seem possible that significant areas of the bodies of the Ailinginae and Utirik natives could be as heavily contaminated as was the ground. (It is unknown if there were sufficient winds that might have raised the material from the ground to the body after fallout occurred.)

There is further uncertainty of what is meant by the monitor's report of "average" personnel readings. The dose rate readings in the hair are known to have been significantly higher than the rest of the body in most cases. It is unknown how these readings were "averaged."

Whereas these data certainly are not firm enough for one to place great confidence in the precise quantities of the ratios of 7/1 or 8/1, they do indicate the obvious fallacy of accepting a 10-roentgen infinity dose based on gamma dose rates measured on personnel outside the radiation field. For example, the natives from Ailinginae showed personnel dose rates readings that would approximate 9 roentgens (gamma) in 2¼ days, and yet skin damage to some degree was evident in 14 out of 16 of the personnel. On the other hand, the natives from Utirik showed no skin damage, with an estimated 2.2 roentgens in 2½ days based on gamma dose rates measured on personnel. The uncertainty of these data was discussed under criteria II. They do suggest, however, that if the contamination of a relatively large area of the exposed body produces less than 1 roentgen infinite gamma dose as measured by a survey meter held 4 inches from the surface there is a large probability that beta burns will not result. (See also discussion under criteria II.)

Doses from small sources.—When the same dose rate reading is produced at a given height above a surface from a smaller area, the amount of contamination per unit area is greater (other factors being equal). Therefore, it would seem desirable to reduce the recommended dose rate levels when relatively small areas are involved. It is recognized that radiation from another nearby spot may

contribute to the survey meter reading when monitoring a small area on personnel, but this has not been taken into account, first, because of the difficulty of establishing a prior appraisal of this variable factor and, second, whatever this contribution may be it will now become an added safety factor.

Of course, the problem is still complex, because when considering smaller and smaller areas the eventual end point is a single particle. An estimate of beta doses at the surface of an imaginary sphere surrounding a fallout particle is given in appendix E and an estimate of beta doses from a single particle required to produce recognizable erythema is presented in appendix F. Calculations indicate that the specific activity of some individual particles found in fallout would be great enough to produce recognizable erythema if held in contact with the skin for less than 1 day, yet the gamma dose rate reading at 4 inches may be relatively small. (See appendix G.)

Additional information on doses from individual particles has recently been reported.[14] The particles found in and around Hanford consisted principally of three radioisotopes, Ru-103, Ru-106, and its daughter Rh-106. The data and calculations in appendix H also strongly indicate that a single fallout particle could produce a recognizable erythema.

Contamination of clothing.—In the case of contamination of clothing, higher dose rates might be tolerated than those for exposed parts of the body. This was exemplified in the natives where no beta burns were observed under clothing of the most highly contaminated personnel. (This does not include such areas as under the waist line where material apparently collected and was held in place.) On the other hand, very large increases in contamination should not be tolerated since it is possible for the clothing to be rearranged so as to bring the contaminated surface in contact with the skin. Further, it is not unlikely that one may rub his hands over his clothing and then through the hair where the material could be held in place for relatively long periods of time.

Beta exposure to the hands.—A further consideration is the beta dose to the hands resulting from handling objects contaminated with fallout material. Although some data are available on beta burns from handling radioactive objects, the conditions are so different from those associated with fallout that comparisons probably would not be valid.[15]

If the above assumptions and calculations are correct concerning contamination of a general area from fallout, then the transfer of all the radioactive material to the hands from an object of equal area would not constitute a hazard. Thus, one might consider using as criteria for monitoring objects, the dose readings given above for monitoring personnel outside the general radiation field. However, the problem is more complex, since the hands may come into contact with contaminated surfaces many times larger in area than the hands, with an undetermined percentage of activity being transferred to the hands. Of course, an added uncertainty is the frequency of washing of the hands and/or the rubbing off of the material from the hands.

Further, one might speculate that a given surface could have significantly higher contamination than the general area and that the handling of such a surface could constitute a greater risk. This might be true because of the greater amount of activity transferred to the hands or because of the doses delivered during the time of actually handling the object. The uncertainty of the percentage of transfer of material has been mentioned. One uncertainty in the second case is the length of time the object would be handled.

Based on calculations in appendixes B and D, when an object is held in a hand, a rough estimate of the ratio of dose rates of beta to the basal layer of the epidermis to that of the gamma reading on a survey meter held 4 inches away from an object 2 inches in radious (outside a general radiation field) might be 5,000 to 1 (appendix I). Thus, if this object were contaminated with the same activity per unit area that would produce an infinity 10-roentgen whole-body gamma dose from general contamination of the area, it would produce about 50 mr. per hour gamma at 4 inches away at H+1 hours, and about 250 reps per hour at a depth of 7 mg./cm.^{2}. Since the palms of the hands have an approximate epidermal layer of about 40 mg./cm.^{2} the beta dose to the basal layer would be about 170

[14] HW-33068. A status report. September 15, 1954.
[15] Beta Ray Burns of Human Skin. Knowlton et al., The Journal of the American Medical Association, vol. 141, No. 4. September 24, 1949.

reps per hour. (The time of H+1 was selected to show about the highest magnitude of dose rates.) If one assumes that the decay is according to $t^{-1.2}$, then the total beta dose to the basal layer of the epidermis of the hand in the next 10 hours would be about 320 reps.

Whereas the above estimates do not indicate an alarming situation, a more serious problem may come when the contamination is just less than that where evacuation is indicated. For example, the contamination of the general area may be 5 or 6 times that used as an illustration in the preceding paragraph, without evacuation being recommended. Thus, beta dose rates from handling objects, especially in times soon after fallout, may be high enough to be a problem. A simple and expedient procedure to reduce this factor is frequent washing of the hands after handling objects that were in the fallout.

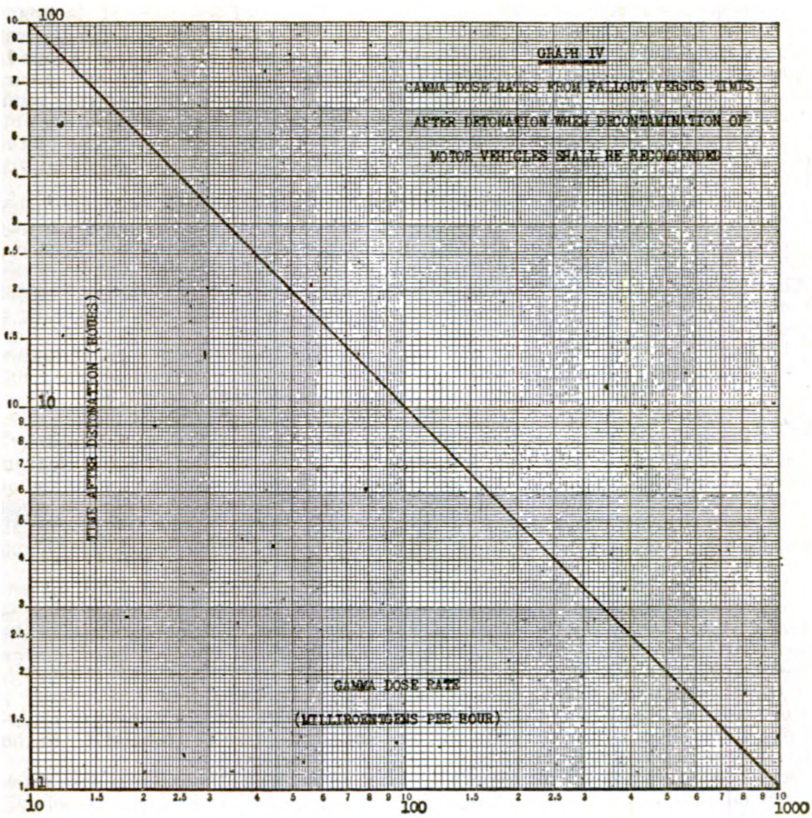
Beta exposure to the feet and lower legs.—It was suggested in criteria II that normal closed-type footwear (as compared to such as open sandals) would probably afford adequate protection against significant beta doses to the feet from fallout material on the ground. There is still the added problem if the material be scuffed up and cling to the ankles and lower legs. If there were no intervening clothing, or perhaps even with thin stockings or socks, this might result in significant biological beta doses being delivered to these parts. For example, if the gamma dose rate reading at H+3 hours were something less than 5 roentgens per hour, evacuation would not be indicated. However, for fallout material of the same concentration in contact with the skin the beta dose rate at 7 mg./cm.^{2} would be about 600 reps per hour. (See appendix B.) Presumably, personnel would be kept indoors for a few hours, but upon release the approximate beta dose rates at 7 mg./cm.^{2} would be 260 reps per hour 3 hours later, or 210 reps per hour 6 hours later. In addition, there is the variable factor of what concentration of fallout material may accumulate in the ankle region by walking around an area.

A concentration of fallout material on the ground that would result in about 20 roentgens maximum theoretical infinity gamma dose if in contact with the skin, would result in a beta dose rate to the basal layer of the skin of about 1/4 those indicated in the previous paragraph.

CRITERIA IV. MONITORING AND DECONTAMINATION OF MOTOR VEHICLES

It is recommended that when the predicted fallout across a main highway will be equivalent to a 10-roentgen infinity gamma dose or higher, vehicles be held until after the actual fallout has essentially ceased. They should be then warned to proceed with windows and air vents closed, and the cars should be monitored after passing through the contaminated area. When 5 to 10 roentgens are predicted across a main highway, vehicles should be warned to proceed with windows and air vents closed and should be monitored after passing through the contaminated area. Monitoring and warnings should be continued until there is reasonable belief that no or very few additional vehicles will exceed the values given in graph IV.

When the dose rate reading taken inside a vehicle, or taken over any exterior area that is readily accessible, equals or exceeds the values given in graph IV, the vehicle shall be cleaned inside and outside. Exterior areas to be monitored should include the wheels and under parts of the fenders but not the under carriage. The survey meter should be held approximately 4 inches from any surface.



Discussion

In the past, fallout has occurred across highways in significant quantities. Table IV–b below indicates some pertinent data during Upshot-Knothole.

TABLE IV–B

Shot No. (chronological)	Approximate yield (KT)	Tower (feet)	Time of fallout (hours)	Estimated dose rate reading of highway at time of fallout (mr./hr.)	Location	Approximate distance from ground zero (miles)
1		300	1¼	920	30 miles south of Alamo on Highway No. 93.	60
1		300	2¾	260	1 mile north of St. George, Utah.	130
6		300	5	325	Junction of U. S. Highway No. 91 and Nevada Highway No. 40.	80
7		300	4½	760	20 miles northwest Glendale, Nev., on Highway No. 93.	65
7		300	7	400	8 miles west of Mesquite, Nev., Highway No. 91.	105
9		300	2	1,000	36 miles north Glendale on Highway No. 93.	60
9		300	3¼	420	St. George, Utah, Highway No. 91.	130

Road blocks were established on Highways 93 and 91 following shots Nos. 7 and 9 of Upshot-Knothole. The highest reading on a private automobile was 100 mr./hr. (gamma) inside and 110 mr./hr. outside at H+3½ hours. About 75 cars were washed (roughly one-eighth of the total monitored). All of the cars that were washed, except the one mentioned above, had outside dose rate readings less than half of the highest. The ratio of dose rate readings on the outside of the car to inside varied from unity to about 4/1. Probably one of the important factors here is the difference between driving with windows and/or ventilators opened or closed.

One bus read 250 mr. per hour outside and average of 100 mr. per hour inside with a high inside reading over the rear seat of 140 mr. per hour at H+8¾ hours.

Considering the amount of time one normally spends in an automobile, these dose rates do not necessarily represent a health hazard in terms of gamma doses. What is probably a more limiting factor is the direct contamination one might acquire by rubbing against the outside of the car, especially when changing a tire.

It is assumed that monitoring will be accomplished outside a general radiation field. Theoretical calculations (appendix D) indicate that gamma dose rate readings taken at 4 inches from a surface will be 51 percent, 42 percent, and 27 percent of those by a meter at 3 feet above an equally contaminated infinite field when the radii of contamination are resjectively 3 feet, 2 feet, and 1 foot.

These data suggest that when the gamma dose rate reading at 4 inches from a generally contaminated car is about one-half that for an infinite plane taken at 3 feet, the degree of contamination per unit area will be about equal; and when the wheels are being monitored ½ to ¼ of a gamma dose rate reading will represent equivalent contamination (depending on the gamma contribution from the body of the contaminated vehicle).

Another factor to be considered is that the probability of collecting fallout material on the body from a generally contaminated area in which one lives is greater than from one's automobile. On the other hand, it has been noted in the past that significantly higher amounts of contamination have been found on the tires and under parts of fenders than on the remainder of the car. (Undoubtedly, this is a simple phenomenon of picking up the activity from the highway.) It one were to change a heavily contaminated tire, significant amounts of radioactive material might accumulate on the hands, and later be transferred to the hair or eyes by a simple rubbing of the hands over those parts.

A comparison might be made here between recommended maximum dose rates found on personnel and the establishing of levels of activity for automobiles. There is one obvious difference, howewver; in the first case the material is already on the person while in the second case one has to introduce the factor of probability of transfer of contamination (and to what degree) from the car to the body.

The dose rates (measured as stated) in graph IV would represent about equal contamination per unit area for a car as for an infinite plane if the car were rather uniformly contaminated. If the activity were confined, say, principally to the tires and under parts of the fenders, the dose rate readings might represent nearly twice the degree of contamination. One must weigh this condition with the probability that a tire will be changed before he activity has decreased significantly.

A given dose rate reading inside a vehicle may represent less contamination per unit area due to the contribution of gamma radiation from the exterior of the vehicle. On the other hand, contamination within a vehicle would more probably be picked up by personnel than if it were on the outside. Further, it is recognized that significantly high concentrations of radioactive fallout may accumulate in such parts as the air filters of an automobile. Again, this has to be weighted against the probability that they will be handled before the activity has decreased to low levels plus the fact that it is relatively difficult to monitor such parts on a mass basis. The uncertainties present in estimating possible hazards from vehicle contamination would not justify fine distinctions in monitoring the various parts. A thorough cleaning, inside and outside, would appear to be the best solution.

One of the obvious ways to avoid much of the problem discussed in criterion IV is to prevent vehicles entering an area during the time of fallout. This will not prevent the first vehicles passing through from picking up activity on the tires from the highway. It is believed, however, this will not constitute such a troublesome problem and past experience has indicated that the activity found

on the tires noticeably decreased after several cars had passed over the highway. Further, if vehicles are not present in the fallout it will help reduce contamination of the passengers and of the insides of the vehicles.

Operational feasibility.—In the past, the criteria used for washing cars has been 7 mr./per hour, and at a later time 20 mr./per hour (gamma), inside a vehicle. This resulted in washing about 75 cars (roughly one-eighth of the total monitored) following the seventh and ninth detonations of Upshot-Knothole. Under the recommendations given in criteria IV, the bus mentioned above, but probably none of the cars, would have been washed.

The data given in graph IV–b indicate that if these radiation levels given had been predicted before the fallout, Highways Nos. 91 and 93 would have been closed prior to the fallout from the seventh detonation and possibly Highway No. 93 for the ninth detonation.

CRITERIA V. CONTAMINATION OF WATER, AIR, AND FOODSTUFFS

In any area where the theoretical gamma infinity dose exceeds 10 roentgens, adequate sampling of the water, air, and foodstuffs should be made to ascertain the conditions of possible contamination. Based on past data, however, it is not expected that under those conditions of fallout, where the radiation levels are below those stipulated for possible evacuation, that the degree of contamination will be a health hazard. (Nor is it implied here that any level above this does constitute a serious contamination of water, air, or foodstuffs.) Therefore, it is recommended that no action be taken in regard to limiting intake except to advise the washing off of such exposed foods as leafy vegetables when that action seems desirable.

Discussion

Water.—Table VI–A lists the six locations having the highest concentrations of fission products in water sources during Upshot-Knothole, and for comparative purposes the estimated external theoretical maximum gamma infinity doses.

TABLE VI–A

Locality	Concentration (microcuries per milliliter extrapolated to 3 days after detonation)	External theoretical maximum wholebody gamma infinity dose (roentgens)
Virgin River irrigation canal, Nevada	8.7×10^{-5}	6.0
Irrigation ditch, 56 miles north of Pioche, Nev	4.5×10^{-5}	.15
Lower Pahranagat Lake, Nev	3.2×10^{-6}	2.0
Virgin River at Mesquite, Nev	2.6×10^{-6}	2.5
Bunkerville, Nev. (tap water)	1.2×10^{-6}	7.0
Crystal Springs, Nev. (tap water)	1.1×10^{-6}	.15

Due to weather and to attenuation of the gamma rays by buildings, the wholebody gamma dose estimated to have been actually delivered was probably closer to one-half of the values shown.

The maximum permissible concentration of fission products in drinking water is $5 \times 10^{-3} \mu c$/ml. extrapolated to 3 days after detonation. This is considered a safe concentration for continuous consumption.

Whereas, the monitoring of water sources is of value for documentary purposes it should be recognized that the concentrations found may vary widely within small geographical areas and even at the same location at different times (taking into account radioactive decay). Thus, confidence cannot be placed in precise values. Table VI–A suggests that even if one were to have stored up the water listed at Virgin River Irrigation Canal and subsisted entirely on this for a lifetime, the concentration would be about 58 times less than the maximum permissible amount. Normal factors of dilution by additional rainfall and/or by the influx of lesser contaminated ground water would be expected to reduce the level of activity.

Air.—Considerable effort has and is being made to evaluate hazards from airborne radioactive materials, including fission products. There are certainly many unanswered problems including the possible hazard from a single particle in

the lungs. Despite the uncertainties and as yet incomplete analysis of the inhalation hazard, the preponderance of evidence today is that the external gamma hazard from fallout is the more limiting factory of the two.[16] (However, see discussion on food contamination.)

During Upshot-Knothole quite complete data were collected of concentrations of airborne activity on about 150 occasions in some 40 different localities within 200 miles of the Nevada Proving Grounds. These included monitoring of all detonations. Histograms were made of air concentrations versus time after detonation for 30 occasions and estimates were made of doses to the lungs. These data for the five communities showing the highest air concentration are given in Table VI–B. The histogram for St. George (the highest 24-hour average concentration of fallout ever measured in a populated area) is reproduced in appendix J.

TABLE VI–B

Locality	24-hour average concentration (microcuries per cubic meter)	Dose to lungs (13 weeks) based on 20 percent deposition and 100 percent retention thereafter (mreps)[1]	Theoretical maximum whole-body gamma 13-week dose (roentgens)
St. George, Utah	1.29	130	3.5
Lincoln Mine, Nev	4.0×10^{-1}	12	1.5
Mesquite, Nev	1.7×10^{-1}	13	1.0
Groom Mine, Nev	3.4×10^{-2}	7	0.35
Pioche, Nev	2.0×10^{-2}	3	0.015

[1] The method used in estimating doses to the lungs is given in appendix K.

The criteria previously established by an Ad Hoc Jangle Feasibility Committee (Washington, D. C., July 13, 1951), for air concentrations were—

"At a point of human habitation, the activity of radioactive particles in the atmosphere, averaged over a period of 24 hours, shall be limited to 100 microcuries per cubic meter of air (corresponding approximately to a ground level gamma intensity of 30 mr. per hour).

"The 24-hour average radioactivity per cubic meter of air, due to suspended particles having diameters in the range 0 micron to 5.0 microns, shall not exceed one-hundredth of the above; nor is it desirable that any individual particle in this size range have an activity greater than 10^{-3} microcuries calculated 4 hours after the blast."

In the January 20, 1954, meeting of the ad hoc committee the basis for recommending the above air concentrations was discussed. Essentially, these criteria was selected by estimating the gamma dose that might be delivered by the passing of a radioactive cloud. Since there are better methods of estimating gamma doses and since there are uncertainties in evaluating the hazards of such transitory air concentrations as experienced from fallout, and since the preponderance of evidence from past nuclear test series indicates that the external gamma hazard is more limiting than the inhalation one, it was recommended in the January 20, 1954, meeting to strike from the record the past recommendations for maximum permissible air concentrations. It was recommended that an air monitoring program be continued for documentary purposes and for whatever value the data might have in the future when new analyses might be made in the light of additional knowledge.

A further discussion of the single particle problem may be made. In arriving at the recommendation" * * * nor is it desirable that any individual particle in this size range have activity greater than 10^{-3} microcuries calculated 4 hours after the blast" a computation was made that the average radiation dose from such a particle to a sphere one-half a millimeter in radius would be 385 reps.[17] However, the conclusions may be misleading. In the case of a single particle, relatively large doses are delivered near the particle and small doses at a greater distance. Appendix L suggests one possible estimate of this phenomenon. The

[16] Ad hoc committee meeting. Washington, D. C. Jan. 20, 1954.
[17] Minutes, Meeting of Committee to Consider the Feasibility and Conditions For A Preliminary Radiologic Safety Shot for Jangle. LASL. May 21–22, 1951.

parameters involved here are many and difficult to evaluate. For example, how long will a particle remain in one place in the lung and what dose will be delivered during that time?

It has been suggested that in the upper respiratory passage 20-micron diameter particles are the upper limit of size for deposition and that "Cilia sweep 4 to 6 cycles per second. The probability of a particle remaining within 1 millimeter zone for as much as one-half hour appears to be vanishing small. * * * Protection will also be provided by the mucus lining which is itself renewed several times an hour." Accepting the estimates above and the methods illustrated in appendixes E and F, it may be computed that about 8 reps would be delivered to the surface of an imaginary stationary sphere 1 millimeter in radius by a 20-micron particle (0.5 microcurie) in 30 minutes (appendix L). Larger doses will be delivered closer to the particle but with the relatively rapid movement of the particle, it does not appear that large doses will be delivered to a great number of cells. Multiple exposures might occur from additional particles but again this risk is difficult to evaluate.

Food.—Considerable effort is being directed toward the study of contamination of food from fallout. One element of major concern is Sr–90. It has been estimated that if one were to subsist entirely on food grown from soils containing about one-tenth to 1 microcurie per square foot of Sr–90 (1,000 pounds of calcium per acre to an average depth of 6 to 7 inches), that over a period of years there would accumulate in the human skeleton a body burden of 1 microcurie of Sr–90.[18] The highest Sr–90 activity found in soils from agricultural areas, about 100 miles from the Nevada test site, now shows a concentration of about 3.4×10^{-3} microcuries per square foot. This is a factor of 30–300 times less than the one-tenth to 1 microcurie of Sr–90 quoted above. The calcium content of soils around the Nevada test site is several times greater than the 1,000 pounds per acre used as a basis for calculations, which would materially reduce the strontium uptake.

(Although not of direct concern to the Nevada test site, it is of interest to note that soils were collected from the Marshall Islands following the fallout in early March 1954. Appendix M summarizes these data.)

A recent report strongly suggests that contamination of leaf surfaces followed by either direct consumption or intake by way of milk is a far more important pathway of intake than the soil-plant-animal cycle, at least for those times of year when plants may be in a state of growth to collect the fallout. Further analysis is being planned.

This same report raises a new problem. Based on stated assumptions, the data presented indicate relative doses of:

thyroid: tens of thousands of reps
Sr^{89-90}: 300 reps
external gamma: 40 roentgens

High radioiodine doses to the fetus and baby may be particularly important. Additional evaluation will be given this problem.

CRITERIA VI. ROUTINE RADIATION EXPOSURES

The whole-body gamma effective biological dose for off-site populations should not exceed 3.9 roentgens over a period of 1 year. This total dose may result from a single exposure or series of exposures.

If integrations of dose rate readings are used in estimating the effective biological doses, then table V may be used.

TABLE V

	Multiplication factor	Effective biological dose
Maximum theoretical radiation dose from time of fallout to 15 days later	¾	
Maximum theoretical radiation dose from 15th day to 1 year	½	
Total (best estimate of effective biological dose)		

[18] Private communication, L. A. Dean, U. S. Department of Agriculture, Beltsville, Md., April 28, 1954.

If film badges or dose meters are worn on personnel and the evidence of their use supports the view that the readings are a reasonably accurate account of the radiation dose received, then the values recorded on the film badge may be accepted with a correction factor of ¾ to account for the difference between the dose received by the film badges or dosimeters (including backscatter) and that received at the tissue depth of 5 centimeters.

CRITERIA VI. ROUTINE RADIATION EXPOSURES

Discussion

In 1953 the following recommendation was made in the report of Committee To Study Nevada Proving Ground:

"It is recommended, and found to be in conformity with the present principles of determining permissible exposure limits, that for test operation personnel the total body gamma exposure be limited to 3.9 r. in 13 weeks, and that the same figure be applied to the off-site communities with the further qualification in the latter case that this is the total figure for the year. In general, this implies a single test series in any given year."

On the basis of this recommendation and the reasoning discussed under criteria I, the criteria for estimating the whole-body gamma effective biological dose are summarized in table V. It will be noted that the biological factor included under criteria I is omitted in criteria V. In the first case we are dealing with relatively high doses that may require emergency measures with their attendant hazards. It is a situation where one wishes to estimate all pertinent factors in evaluating radiation doses even though they may not be known with preciseness, before recommending an emergency action that may produce greater problems. In the case of criteria V one is concerned with relatively lower doses during routine operations. It would be difficult to justify on the one hand the proposition that weekly doses for general populations may be integrated and taken in a single exposure without penalty and on the other hand, that a given dose received over a period of a year may be administratively reduced because of biological repair. Therefore, the biological factor is omitted.

The general effects of backscattering on measured radiation doses are fairly well established. Further, knowledge of depth (tissue)-dose curves has advanced to a quantitative state.[19] Thus, there seems to be little doubt that a film badge or dosimeter worn on the person will overestimate the gamma radiation dose delivered at a depth of 5 centimeters (assumed depth of blood-forming organs). A major factor in determining this difference is the quality of radiation under consideration. One report dealing explicitly with radiation in a fallout field suggests a factor of about ¾.

---

[19] Permissible Dose From External Sources of Ionizing Radiation. National Bureau of Standards Handbook 59. September 24, 1954.

APPENDIX A. SAMPLE ESTIMATION OF GAMMA DOSES SAVED BY REMAINING INDOORS

EXAMPLE I

Assume: Time of fallout = H + 3 hrs
Dose rate at H + 3 = 667 mr/hr

Then: Theoretical maximum dose from time of fallout to 3 hours later --- 1.30 r
Savings by remaining indoors for 3 hours --- 0.65 r
1 year effective biological dose if personnel did not remain indoors during the 3 hours (based on same assumptions contained in section on evacuation) --- ~5.5 r
Percent of 1 year effective biological dose saved by remaining indoors for the 3 hours --- ~12

EXAMPLE II

Assume: Time of fallout = H + 3 hrs
Dose rate at H + 3 = 667 mr/hr

Then: Theoretical maximum dose from time of fallout to 8 hours later --- 2.30 r
Savings by remaining indoors for 8 hours --- 1.15 r
1 year effective biological dose if personnel did not remain indoors during the 8 hours (based on same assumptions contained in section on evacuation) --- ~5.5 r
Percent of 1 year effective biological dose saved by remaining indoors for the 8 hours --- ~21

APPENDIX B. *Calculations of Beta Dose Rate at Depth of 7 Milligrams per Square Centimeter From a Thin Extended Source*

Assume: 1.5 Mev Beta (mean energy = 0.5 Mev)
$\mu = 10$ cm²/gm

(This assumes a single mass absorption coefficient.)

$$N = No\ e^{-\mu x}$$

where
No = number of betas at surface per cm² per sec.
N = number of betas at depth x
μ = mass absorption coefficient
x = distance (depth) under consideration

$$\frac{dN}{dx} = -\mu No\ e^{-\mu x}$$

$$R = \frac{\mu No\ e^{-\mu x} E}{2}$$

where
R = dose rate at depth x
E = mean energy of betas

$$R = \frac{(10) No\ e^{-(10)(0.007)} (0.5)}{2} = 2.33\ No \text{ Mev/gm-sec.}$$

$No = 3.7 \times 10^4 C$ where: C = activity in microcuries per cm²
$R = 8.65 \times 10^4 C$ Mev/gm-sec.
$R = (1.39 \times 10^{-1})(C)$ ergs/gm-sec.
$\cong 5.4\ C$ reps/hr
or $\cong 5.0\ C$ rads/hr

Example

Assume: $C = 80 \mu c/\text{cm}^2$ (beta)
$R = 5.4\ C$ where: R = dose rate at depth 7 mg/cm² in reps
C = activity/cm² in μc

$= (5.4)(80)$
$= 432$ reps/hr
or $= 400$ rads/hr

Comparison Beta Dose Rate (Reps/hr) at 7 Mg/cm² to Gamma Dose Rate Measured in Infinite Field at 3 Feet Above the Surface

Assume: 80 $\mu c/cm^2$ (beta), equivalent to 1 megacurie/mi² (gamma)

$$\frac{432}{4.1} \cong 105$$

APPENDIX C. *Experimental Data Versus Theoretical Calculations (Appendix B) in Estimating Beta Doses*

In one relevant experiment, a thin P^{32} source was prepared by soaking a filter paper in a solution of phosphates and allowing it to dry. The surface dose rates were then measured with a surface ionization chamber.[1] Pertinent data are abstracted as follows:

Thickness of source	9.6 mg/cm²
Activity of source	77.0 $\mu c/cm^2$
Surface dose rate	0.127 rep/sec; 457 reps/hr
Dosage rate at depth of x centimeters	$e^{-9.5x}$

A. Theoretically:

Using the equation from Appendix B

$$R = \frac{\mu No e^{-\mu x} E}{2} \text{ (for } P^{32}\text{)}$$

Substituting above data:

$$R = \frac{9.5 No e^{-(9.5)(0.007)} .69}{2}$$

$$= 7.0\ C \text{ reps/hr}$$

$$\text{Let } C = 77\ \mu c/cm^2$$

$$\text{Then } R = 7.0 \times 77$$

$$= 539 \text{ reps/hr at 7 mg/cm}^2\ (P^{32})$$

B. Experimentally:

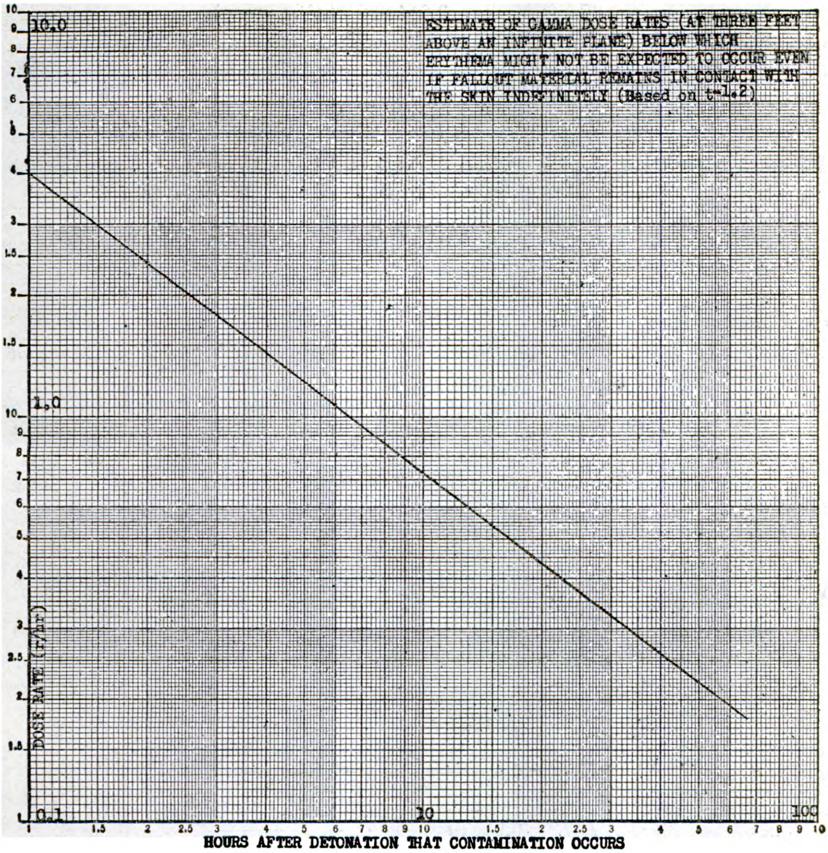
$$R = 457\ e^{-(9.5)(0.007)}$$

$$= 427 \text{ reps/hr at 7 mg/cm}^2\ (P^{32})$$

The two above approaches are within 26 percent of each other. If one extrapolates the experimental data from a source of 9.6 mg/cm² to a thin source (for comparative purposes) the two methods are within 20 percent.

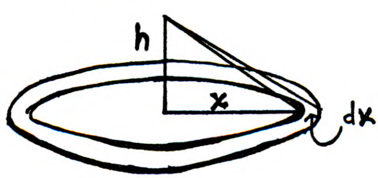
[1] *Effects of External Beta Radiation.* Zirkle, Raymond E. McGraw-Hill Book Co. 1951.

APPENDIX C (a)



APPENDIX D. *Calculations Gamma Dose Rate From a Field 6 Inches in Radius and Center of Chamber 4 Inches Above Surface*

Dose rate of gamma from a point source:



$r \cong 6CE$ where: r = r/hr
C = activity in curies per square foot
E = average energy of gammas (Mev)

$$D = 6CE\, 2\pi \int_0^x \frac{x\,dx}{h^2+x^2}, \text{ where } D = \text{dose rate in r/hr}$$

$$D = 18.8\, CE \ln \left[\frac{h^2+x^2}{h^2}\right]$$

Example:

Let: $x=1/2$ foot
$C=40\mu c/cm^2$ or 3.6×10^{-2} c/ft² (gamma)
$E=0.7$ Mev
$h=1/3$ foot

$$D=(18.8)(3.6\times 10^{-2})(0.7)\ln\left[\frac{(1/3)^2+(1/2)^2}{(1/3)^2}\right]$$
$$=0.56 \text{ r/hr}$$

Comparison Gamma Dose Rates From Infinite Plane at a Height of 3 Feet Above the Ground to Area of 6-Inch Radius and Height of 4 Inches

Assume: 1 megacurie/mile²
(3.6×10^{-2} c/ft²)

$$\frac{4.1 \text{ r/hr}}{0.56 \text{ r/hr}}=7.3$$

APPENDIX E. *Estimate of Dose Delivered by a Single Particle of Fallout Material*

Assume: a. Point source
b. 0.5 Mev average beta energy
c. $\mu=10$ cm²/gm
d. Rate of decay follows $t^{-1.2}$

The dose delivered at the surface of an imaginary sphere at distance R from a point source.[1]

(1) $$K(R)=\frac{CE\mu}{4\pi R^2}e^{-\mu R}\ \frac{\text{Mev}}{\text{gram}}$$

where: $K(R)$ = dose delivered at the surface of an imaginary sphere at distance R
E = average energy of beta particles
C = total number of disintegrations
μ = mass absorption coefficient

Substituting:

$\mu=10$ cm²/gm
$E=0.5$ Mev

Then: (2) $$K(R)=0.4\,\frac{e^{-10R}}{R^2}\ \frac{\text{Mev}}{\text{gm-disintegration}}$$

or (3.a.) $$K(R)=6.9\times 10^{-6}\,\frac{e^{-10R}}{R^2}\ \frac{\text{millireps}}{\text{disintegration}}$$

or (3.b.) $$K(R)=6.4\times 10^{-6}\,\frac{e^{-10R}}{R^2}\ \frac{\text{millirads}}{\text{disintegration}}$$

NOTE.—Equation (3.a.) is plotted on the attached graph.
For fission products:

(4) $$A_a=A_1 t_a^{-1.2}$$

where: A_a = disintegrations per unit time at time "a" after detonation
A_1 = disintegrations per unit time at one unit of time after detonation

Integrating equation (2),

(5.a.) $$C=5A_1(t_a^{-0.2}-t_b^{-0.2})$$
and (5.b.) $$C=5A_a t_a^{1.2}(t_a^{-0.2}-t_b^{-0.2})$$

where: C = total number of disintegrations from time "a" to "b"
t_a = time after detonation
t_b = later time after detonation.

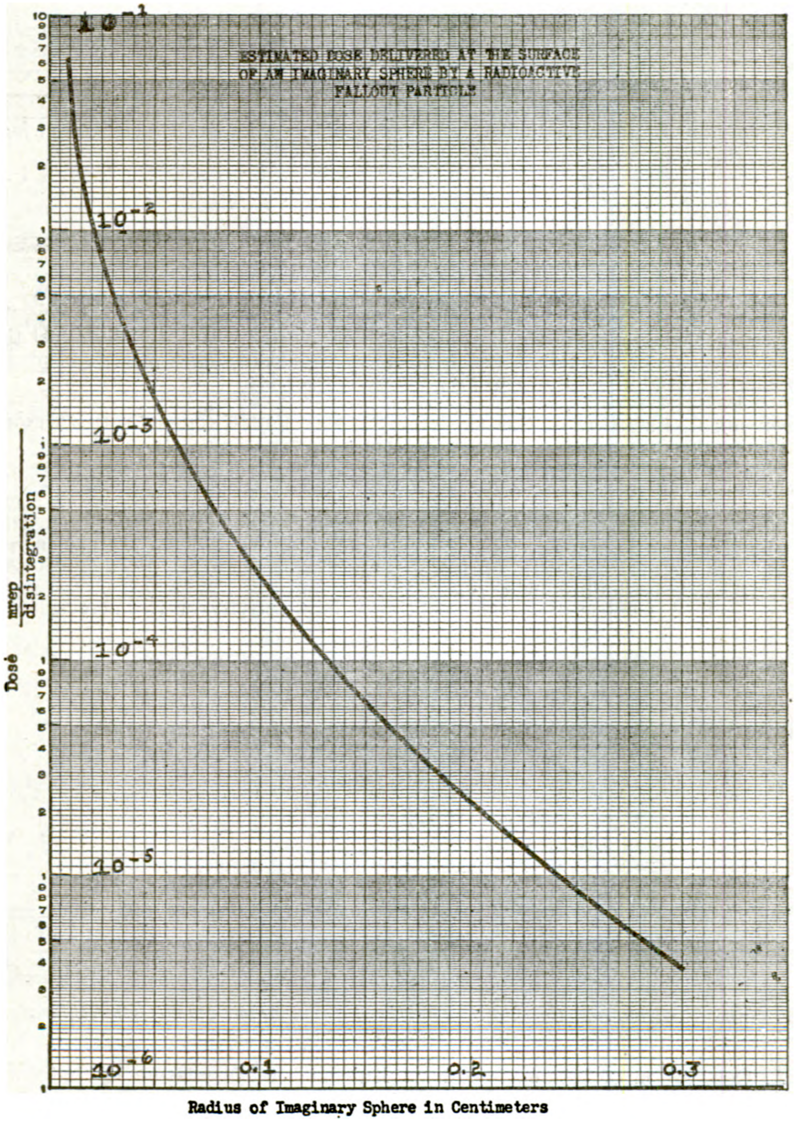
When t_b is infinite,

(6) $$C_\infty=5A_t t.$$

By the use of equations (3.a.) or (3.b.) and (5.b.) one may compute an estimated dose at the surface of an imaginary sphere.

Of course, the problem is the determination of "t_a" and "t_b", i. e., how long after detonation will a radioactive particle be deposited and how long will the particle remain in place. The first time (t_a) is much easier to estimate than the later (t_b).

[1] Rossi, H. H. and Ellis, R. H. "Distributed Beta Sources in Uniformly Absorbing Media.". *Nucleonics.* July 1950. V. 7 No. 1.



ESTIMATED DOSE DELIVERED AT THE SURFACE OF AN IMAGINARY SPHERE BY A RADIOACTIVE FALLOUT PARTICLE
Dosé mrep/disintegration
10-1
10-2
10-3
10-4
10-5
10-6
0.1
0.2
0.3
Radius of Imaginary Sphere in Centimeters

APPENDIX F. ESTIMATE OF BETA DOSES FROM A SINGLE PARTICLE ON THE SKIN (POSSIBLE PRODUCTION OF RECOGNIZABLE ERYTHEMA)

Let: $t_a=3$ hours (time particle is deposited on skin)
$t_b=27$ hours (time particle is removed)

Assume: 1500 reps=total dose required in one day to produce recognizable erythema
0.1 cm=radius of imaginary sphere within which cells must receive 2000 reps or larger.

According to appendix E, 2.5×10^{-7} reps/disintegration is delivered to surface of imaginary sphere 0.1 centimeter in radius.

$$\frac{1.5\times10^{3}}{2.5\times10^{-7}}=6\times10^{9} \text{ disintegrations required}$$

$$C=5A_a t_a^{1.2}[t_a^{-1.2}-t_b^{-0.2}]$$

$$6\times10^{9}=5A_a 3^{1.2}[3^{-0.2}-27^{-0.2}]$$

$$A_a=1.14\times10^{9} \text{ d/hr}$$

or about 8.6 μc at $H+3$ hours.

Of course, the radius of the imaginary sphere selected will materially affect the calculations. For example, a radius of 0.2 cm would require a particle of about 96 microcuries at $H+3$ hours to give the same dose.

APPENDIX G. ESTIMATE OF GAMMA DOSE RATE AT FOUR INCHES FROM A SINGLE PARTICLE OF FALLOUT MATERIAL

Assume: a. The average gamma energy of fission products may be compared with radium; that the average energy of fission products is 0.7 Mev; that the average energy from radium daughters is 0.8 Mev with 2.3 photon emissions per disintegration or that the average energy per disintegration is 2.6 times greater than per disintegration of fission products.
b. A particle of 150 microcuries of beta activity or 75 microcuries of gamma activity. (See appendix H.)

$$I=\frac{8.4 \text{ mg (mc)}}{d^2} \text{ for radium through 0.5 mm of platinum.}$$

where: I=gamma dose rate (r/hr)
d=centimeters

Let: $mc=7.5\times10^{-2}$
$d=10$ cm

$$I=\frac{(8.4)(7.5\times10^{-2})}{10^2}$$

=6.3 mr/hr gamma dose rate at 4 inches (for radium)

$$\frac{6.3}{2.6}\cong 2.4 \text{ mr/hr for fission products}$$

APPENDIX H. *Data and Calculations on Doses From Single Particles of Ruthenium and of Fallout Material*

A. Comparison of beta energies from Ru^{103} and Ru^{106} mixture to that from fission products.

Ru^{103} 0.3 Mev beta (T=42d.)
Ru^{106} ~0.03 Mev beta (T=1.0y.)
Rh^{106} 3.55 Mev beta (T=30s.)

Assume: Ru^{103}/Ru^{106} ratio of 0.75 [1]

[1] All of the basic data contained herein on ruthenium is contained in HW-33068. A status report. Sept. 15, 1954.

To estimate a mean average energy of betas from mixture:

Parts	Isotopes	Maximum energy beta	Weighted maximum energy betas
1.0	Ru^{103}	0.35	0.35
1.33	Ru^{106}	0.04	0.05
1.33	Ru^{106}	[1] 3.35	4.45
Total			4.85

[1] Average.

$$\frac{4.85}{3.66} \cong 1.3$$

Average energy ~0.43 or roughly equivalent to that assumed for fission products.

(Of course, the average energy of the betas is not the sole consideration. The spectral distribution of the betas from Rh^{106} probably is quite different from that of fission products, thus affecting the depth dose curve.)

B. Data on doses and effects from single particles of Ru^{103} and Ru^{106}:

	a	b
1. Size of particle	40 μ	120 μ.
Activity of particle	1.1 μc	11 μc.
Dose rate to 7 mg/cm²	6,600 rads/hr	27,500 rads/hr.
Time dose delivered	~6 days	~6 days.

2. Survey dose rate (mrads/hr)[1]	Total skin dose (rads)[2]	Effects
400	~500,000	None visible.
750	~900,000	Reddening.
2,500	~2,000,000	Desquamation.
11,000	~6,000,000	Tissue destruction.
21,000	~7,000,000	Tissue destruction—2 cm across. 8 mm deep.

[1] 90 mrads/hr≅1 μc.
[2] "Total dose refers to the hot spot directly below the particle, and is valid only as to order of magnitude."

C. $\frac{750}{90} \cong 8.3$ μc estimated activity of particle producing reddening effect in about 144 hours. The estimated size is 100 microns.

D. (8.3) (144) = 1200 μc total activity accounted for in the 144 hours that the dose was delivered. (Assuming constant activity during the 144 hours.)

E. What specific activity of a particle of fallout would be required to deliver the same dose in the same length of time?

The answer to this question depends upon the time after detonation that the particle comes in contact with the skin. Assuming this time to be H+3 hours, the specific activity would have to be about 150 μc for the same size particle.

Since the particle may be washed off before 6 days have expired, one may consider the problem another way. What must be the specific activity of a particle at H+3 hours to deliver this dose in the next 24 hours?

According to Strandgvist (p. 6), only about 70 percent of a 6-day dose need be delivered in one day to produce the same effect (erythema). Accepting this, then a particle with about the same activity (160 μc) at H+3 hours would be sufficient to deliver an erythema dose in 1 day.

F. The following data are reported for single particles collected during Upshot-Knothole and Tumbler-Snapper.

Size of particle (μ)	Activity extrapolated to H+3 hours (μc)	Distance from ground zero (miles)
[1]	1,000	45
[1]	200	130
1,626 x 924	900	10
919	480	11
723	350	14.7
714	400	10
555	140	14.7
387	250	14.7
234	47	14.7
115	5.2	95
81	3.0	14.7
20	.5	

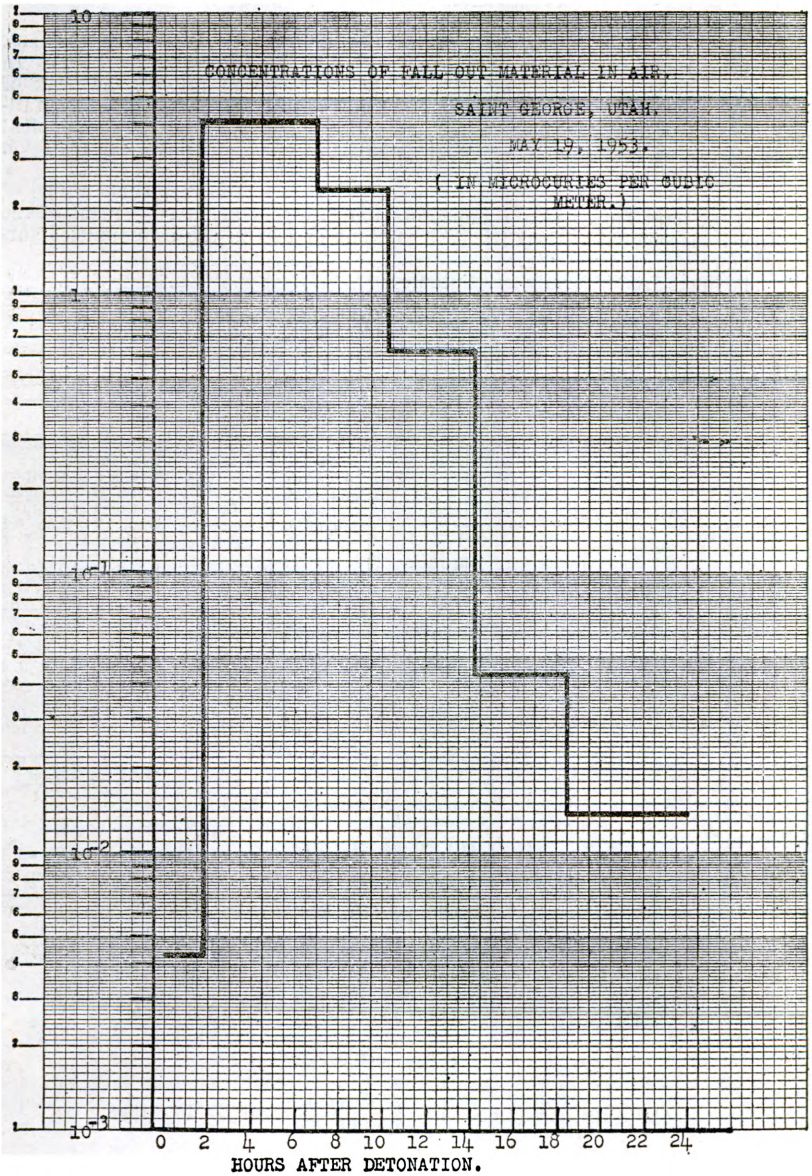
[1] Data from estimations based on radioautograph methods.

It is not intended here to imply these are the maximum specific activities per particle that existed or could exist. The data at 14.7 miles are reported to show the wide range of specific activity that may occur at one locality.

Appendix I. Estimation of Ratio of Surface Beta Dose Rate to Gamma Dose Rate at 4 Inches from an Object 2 Inches in Radius

One may assume a ratio of beta dose rate (at 7 mg/cm^2 depth of skin) to gamma dose rate (3 feet above the ground) of 125/1. If a contaminated object of say 2-inch radius were removed (or shielded) from a general radiation field the gamma dose rate at 4 inches from the surface might be some 40 times less than from an infinite plane with the same degree of contamination (appendix D), while the beta dose rate might remain almost the same value if the object is in contact with the skin. Thus, the beta-to-gamma dose rates measured under these conditions might be 5,000–1. For other than a plane surface, the gamma dose rates might be higher, thus reducing this ratio.

APPENDIX "J"


CONCENTRATIONS OF FALL-OUT MATERIAL IN AIR.
SAINT GEORGE, UTAH.
MAY 19, 1953.
(IN MICROCURIES PER CUBIC METER.)
10
1
10^{-1}
10^{-2}
10^{-3}
0 2 4 6 8 10 12 14 16 18 20 22 24
HOURS AFTER DETONATION.

APPENDIX K. METHOD USED IN ESTIMATING DOSES TO THE LUNGS FROM INHALATION OF FALLOUT MATERIAL

Assumptions

The following assumptions are made in estimating radiation doses to the lungs.

A. Twenty percent of the inhaled activity is deposited.

B. There will be no elimination of particles during their radioactive lifetimes. There is uncertainty as to the biological half life of particles in the lungs. In those communities showing the highest concentrations of fallout, the peak of airborne material (which accounted for the greatest percentage of total fallout) occurred only a few hours after detonation. If one assumes a radiological decay according to $t^{-1.2}$ and a biological half life of say 30 days, the omission of biological half life would not affect seriously the computed total dose.

C. All of the activity is associated with particles in the respirable range of sizes. Past data from cascade impactors indicate that about 90 percent of the activity is associated with particles 5 microns or less in the communities surrounding the Nevada test site.

D. The lungs are uniformly irradiated.

E. The weight of the lungs is 900 grams.

F. An individual inhales 20 cubic meters per 24 hours.

G. The average beta energy is 0.5 Mev.

H. The gamma dose is negligible compared to the beta dose.

Data at St. George, Utah

(Short time) 0505 (I)	Duration (II)	Approximate midpoint after detonation III	$^{1}\mu c/M$ (IV)	μc Inhaled (col. II times col. IV times 0.834) (V)	μc Retained (col. V times 0.2) (VI)
	Hours	*Hours*			
0610 to 1130	4.8	3	4.17	15.9	3.0
1130 to 1445	3.2	8	2.38	6.3	1.26
1445 to 1845	4.0	11.5	6.3×10^{-1}	2.1	0.42
1845 to 2300	4.2	15.6	4.4×10^{-2}	0.15	0.03
2300 to 0635	7.5	21.5	1.4×10^{-2}	0.09	0.02
[1] 0635 to 1835	12.0	31.5	1.4×10^{-2}	0.14	0.03

[1] Assumed.

Sample calculations

$$D=5At_a^{1.2}[t_a^{-0.2}-t_b^{-0.2}]$$

Let: $t_a=3$ hours
$t_b=2184$ hours (13 weeks)
$A=3\ \mu c$

$$D=(5)(3\times2.22\times10^{6}\times60)(3)^{1.2}[3^{-0.2}-2184^{-0.2}]$$

$=4.4\times10^{9}$ disintegrations from 3d hour to 13th week.

Assume: $E_{avg.}=0.5$ Mev

$$(4.4\times10^{9})(0.5)(1.6\times10^{-6})\left(\frac{1}{900}\right)\left(\frac{1}{39}\right)=4.2\times10^{-2}\text{ reps}$$

$$=42\text{ mreps}$$

Total lung dose for 13 weeks: ~130 mreps.

APPENDIX L. ESTIMATE OF DOSE AT SURFACE OF IMAGINARY SPHERE 1 MILLIMETER IN RADIUS

Assume: Average activity for 30 minutes is 0.5 μc at H+3 to H+3½ hours. (See reference appendix H.)

Then: $0.5\times 2.2\times 10^6\times 30=3.3\times 10^7$ disintegrations/30 minutes.

At surface of imaginary sphere 1.0 mm. in radius the dose rate from a point source is

$$2.52\times 10^{-4}\frac{\text{mreps}}{\text{disintegration}} \text{ (See appendix E.)}$$

$$(3.3\times 10^7(\ (2.52\times 10^{-4})=8.3\times 10^3 \text{ mreps/30 min.}$$
$$\cong 8 \text{ reps/30 min.}$$

For particles of higher specific activity, the dose would be correspondingly higher, of course.

APPENDIX M

Estimate of Sr^{90} in soils of Pacific islands

Location	Total activity ($\mu c/ft^2$) (measured) I	Sr^{89}–Sr^{90} ($\mu c/ft^2$) (measured) II	Rough estimate external infinity gamma dose (roentgens) III
Likiep [1]	1.2×10^{-1}	8.7×10^{-3}	4
Jemo	3.0×10^{-1}	1.2×10^{-2}	4
Ailuk	1.0	3.8×10^{-2}	12
Mejuit	1.1	2.8×10^{-2}	8
Ormed	3.2×10^{-1}	1.1×10^{-2}	4
Kaven	1.6×10^{-1}	4.8×10^{-3}	2
Wotho	7.8×10^{-2}	1.3×10^{-3}	0.5
Rongelap:			
(Northern)	62.0	1.08	500
(Central)	40.0	5.5×10^{-1}	500
(1 mi. N. Village)	5.0	5.3×10^{-1}	500
(So. Cistern)	4.5	9.2×10^{-1}	500
Eriirippu [1]	230.0	12.5	4.500
Eniwetok	50.0	1.2	1,500
Kabelle	200.0	4.9	3,300
Utirik	53.0	9.8×10^{-2}	60
Bikar	3.3	4.4×10^{-1}	250
Eniwetok	8.0	6.6×10^{-1}	400
Sifo	6.1×10^{-1}	9.6×10^{-2}	170

[1] All data as of May 5, 1954, except island of Eriirippu where date is May 20, 1954.

UNITED STATES ATOMIC ENERGY COMMISSION,
Washington D. C., August 2, 1957.

Hon. CHET HOLIFIELD,
Chairman, Special Subcommittee on Radiation,
Joint Committee on Atomic Energy,
Capitol Building, Senate Post Office, Washington, D. C.

DEAR MR. HOLIFIELD: As a part of the written record of The Nature of Radioactive Fallout and Its Effects on Man, there is being reproduced a document Discussion of Radiological Safety Criteria and Procedures for Public Protection at the Nevada Test Site written by me some time ago. I would greatly appreciate it if a footnote (attached) were added to this document.

Also enclosed is a copy of the revised radiological safety criteria (April 1957) that are currently being used. I would like to suggest respectfully that these revised criteria also be printed so that the reader may have the benefit of our latest thinking on these matters.

Sincerely yours,

GORDON M. DUNNING,
Health Physicist, Division of Biology and Medicine.

Radiological Safety Criteria During Nuclear Weapons Testing at the Nevada Test Site

(April 1957)

INTRODUCTION

The criteria and procedures set forth in the following paragraphs were established after full consideration for protecting the health and welfare of the public, both in terms of radiological exposure as well as possible hazards, hardships or inconveniences resulting from disruption of normal activities. Criteria are established as guides for the Test Organization in determining whether any special actions should be taken to protect the public.

These criteria are not established with the expectation that the coming tests at the Nevada Test Site actually will result in radiation levels which will be greater than heretofore. Rather, they formalize past criteria to give even clearer guides for protecting the public. With improved methods of predicting fallout and with the use of balloons and higher towers for detonating the nuclear devices, it is expected that fallout in populated areas from future tests at the Nevada Test Site will be less than the highest amounts which have occurred in the past.

Two basic assumptions are made in this report:

(*a*) It is the responsibility of the Division of Biology and Medicine to establish such criteria for the Atomic Energy Commission as deemed necessary to protect the health and welfare of the general populace from consequences of weapons tests conducted at the Nevada Test Site.

(*b*) The operational procedures adopted for meeting these criteria shall be the responsibility of the Test Manager, as directed by the Division of Military Application, with the technical guidance of the Division of Biology and Medicine.

The following criteria do not apply to domestic or wild animals since levels of radiation which would be significant to them would have to be higher than those specified herein.

Section I. Evacuation

BACKGROUND

The decision to evacuate a community is critical for two principal reasons. One, presumably there might be a health hazard if the personnel were allowed to remain. Two, there is always an element of danger and/or hardship to personnel involved in such an emergency measure.

It is recognized that extenuating circumstances may accompany any situation where conditions indicate evacuation as a mode of action. The size of the community, areas and accommodations available for the evacuees, weather conditions, means of transportation and routes of evacuation, disposition of ambulance cases, protection of the property left behind, and many other factors may enter into the decision relative to evacuation. Further, it is recognized that under certain conditions, the evacuation of a community might prove not only rather ineffectual but could result in more radiation exposure than if the population remained in place unless the situation be adequately evaluated. A blanket evaluation cannot be made in advance; each situation can be unique. The following criteria therefore are suggested as guides in assessing the possible radiological hazards; the final decision must be made on the basis of all relevant factors known at the time. They are intended to apply principally to relatively large populations since small groups may be evacuated without equivalent potential hazards.

Owing to the necessity of making early measurements and decisions, it is to be expected that dose-rate readings, taken with survey meters, will be the available evidence at the times of concern. This necessitates making rough approximations in advance of the effects of weathering and of shielding from normal housing, in reducing the radiation exposure. The variable nature of these two parameters makes impossible the establishment of a precise rule covering all situations. Therefore, the following may be used in making conservative estimates of these effects:

(*a*) For weathering—the measured gamma dose rates at three feet above the ground be assumed to decay according to $(t)^{-1.2}$ for the first week after a detonation, $(t)^{-1.3}$ for the second week, and $(t)^{-1.4}$ thereafter.[1]

(*b*) For shielding—the accumulated dose per day be 25% less than the out-of-doors dose.[2]

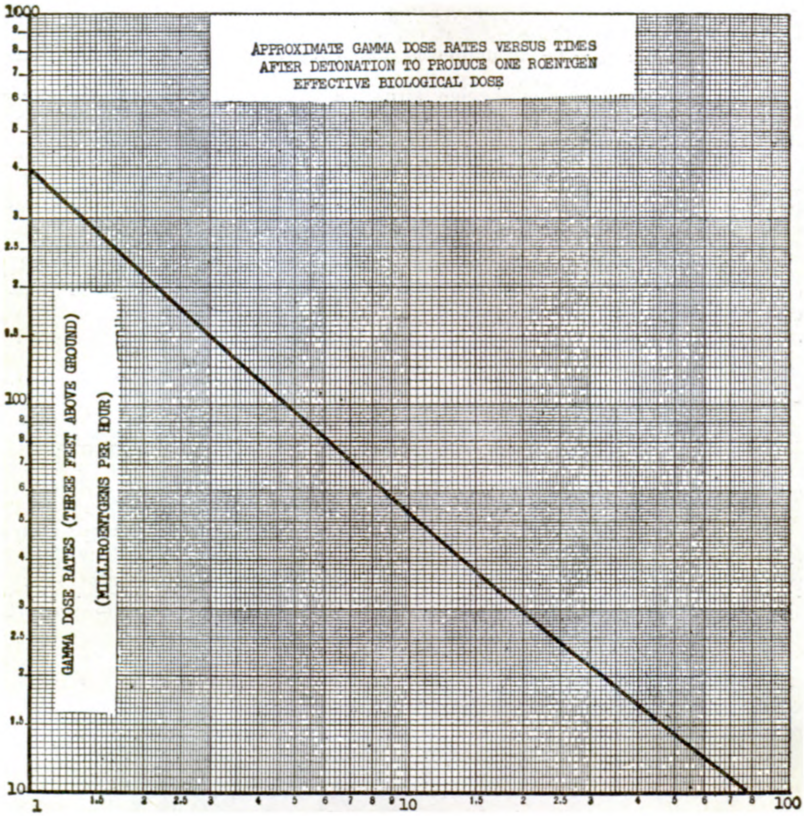
In the case of a truly emergency situation where potential hazards may exist either from the fallout or from mass evacuation of large populations, it would seem proper than due consideration be given to the biological repair process that takes place with radiation doses distributed in time (recognizing that such effects from radiation as genetic changes and life shortening may not be time dependent). The estimates for biological repair for man are quite uncertain so a conservative value is used here of a half-time of repair of about four weeks.

Graph I incorporates the above factors of weathering, shielding, and biological repair into a single curve. This graph may be linearly extrapolated to other dose rate readings. For example, if fallout occurs three hours after detonation and the dose rate is 10 r per hour, then about 67 r (effective biological dose) may be accumulated, i. e., $\frac{10}{0.15}\times 1.0=67$

[1] This concept was suggested after analyzing data from both the Nevada Test Site and the Eniwetok Proving Ground and is intended to give generalized estimates to cover a wide variety of situations. It is recognized that with the smaller fallout patterns and with the sandy soils around the Nevada Test Site, the effective decay constants may be greater than these. An expanded monitoring program will be in operation during Operation Flumbbob (1957 Series) for the collection of pertinent data to allow better estimates of effective rates and of the efforts of shielding provided by buildings.

[2] This is based on an average 12 hours per day stay in a frame house having an attenuation factor of two. It is recognized that some individuals will be in buildings having higher attenuation factors, and for longer periods of time. On the other hand, this is generally an area where people may live an appreciable amount of time out of doors and where windows and doors are left open, so the fallout material may enter the buildings. Possible revision of these estimates will await results from the expanded monitoring program during Operation Plumbbob.

GRAPH I



CRITERIA I

Effective Biological Doses may be calculated according to Graph I.

Table I may be used in evaluating the feasibility of evacuating relatively large populations.

TABLE I.—*Radiological criteria for evaluating feasibility of evacuation*

Effective biological dose:	Minimum effective biological dose that must be saved by act of evacuation (otherwise evacuation will not be indicated):
Up to 30 roentgens	(No evacuation indicated.)
30 to 50 roentgens	15 roentgens.
50 roentgens and higher	(Evacuation indicated without regard to quality of dose that might be saved, providing adequate shelters are not available and the estimated hazards concomitant with evacuation are acceptable.)

Section II. Personnel Remaining Indoors

BACKGROUND

By remaining indoors (*a*) the gamma exposure will be reduced, and (*b*) there is less possibility that the fallout material will come into contact with the skin. (Beta burns have occurred in the past only when the fallout material has remained in direct contact with the skin.) To prevent or greatly reduce this latter effect, it is highly desirable to make decisions before or very shortly after the start of the fallout. Likewise, partial shielding at these early times will be of optimum benefit due to the relatively high gamma dose rates. Thus, the decisions must be based on predicted fallout in an area, or on dose-rate readings from field monitors' reports.

These predictions are of course subject to varying degrees of uncertainly so that personnel may be asked to remain indoors unnecessarily. On the other hand decisions and action must be taken relatively quickly if optimum benefits are to be derived and remaining indoors until the radiological information is more accurately evaluated probably represents one of the easiest and effective ways of meeting an emergency situation.

Due to uncertainties in our knowledge, and recognizing the usual unequal distribution of fallout, it has not been possible to establish precisely the amount of fallout in an area that could produce beta burns. The Marshallese experience showed such effects for those people exposed to 175 r and 69 r whole body gamma radiation, but none for those individuals on the Island of Utirik (370 miles from ground Zero) receiving 14 roentgens. Whether these results would hold true for other situations is not known, i. e., different particle size distribution, different type skin, etc. At one location, Riverside Cabins, Nevada, about 15 people were in an area receiving fallout in an amount equivalent to infinity dose of 15 roentgens, with no known cases of beta burns, although it is not known if anyone was out-of-doors during the time of fallout. Until more is learned of this phenomenon, it would appear advisable to remain out of the direct fallout when the amount would be such as to produce about 10 roentgens gamma infinity dose as measured at three feet above the ground. In the event personnel are out of doors during the time of this amount of fallout, the possibility of beta burns could be greatly reduced by the simple expedient of changing clothing and of bathing.

If people were not asked to remain indoors during the period of highest dose rates in an area where the infinity dose was 10 roentgens or more, their actual exposure might be in excess of 3.9 roentgens of wholebody gamma. This would not necessarily be hazardous but would exceed the established criteria for Plumbbob (Criteria VI).

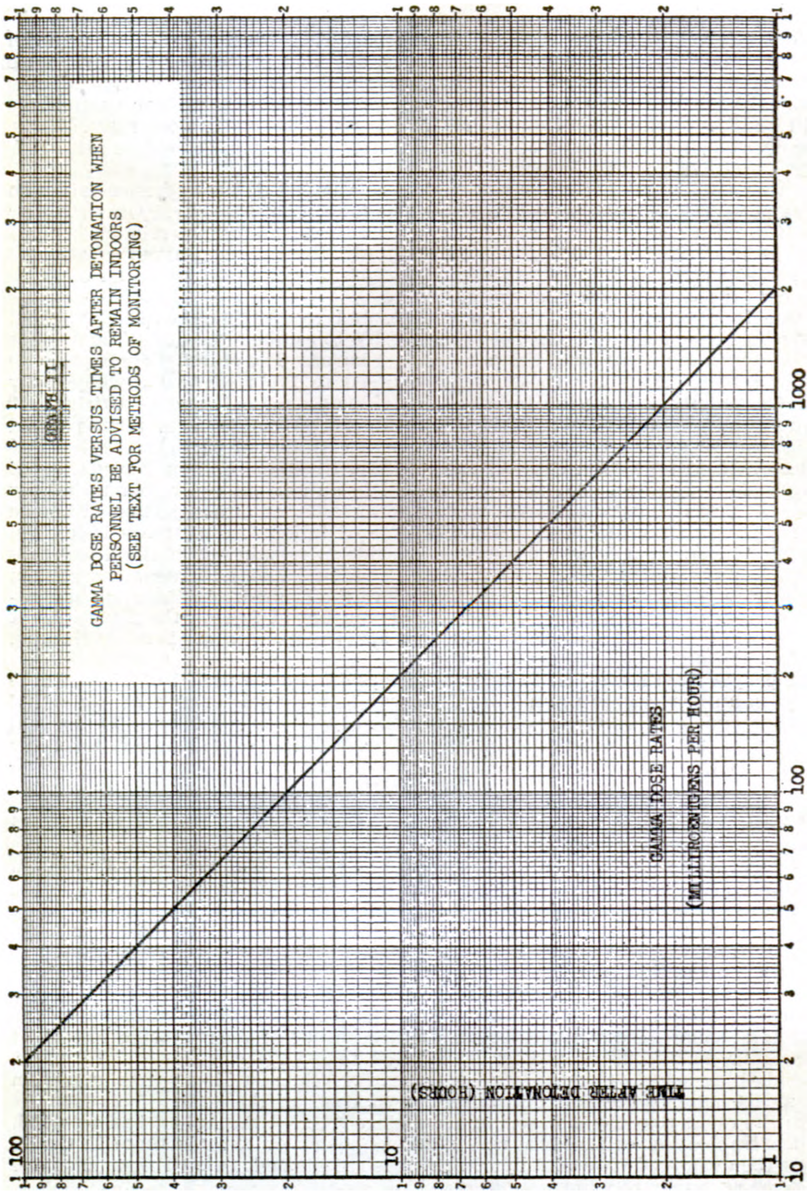
CRITERIA II

When the gamma dose rate reading as measured by a survey meter held three feet above the ground reaches the values given in Graph II at the times indicated, it is recommended that personnel be requested to remain indoors with windows and doors closed. Release from this restrictive action should be made on the basis of further evaluation of the radiological conditions.

In the event that there be convincing evidence that the radiation levels given in the graph will be reached, it is recommended that personnel be requested to remain indoors BEFORE fallout occurs or before the radiation levels equal those in Graph II. Release from this restrictive action should be made on the basis of further evaluation of the radiological conditions.

It is recommended that people who had been out of doors during fallout of the above magnitude or greater be advised to change clothing and to bathe. The clothing may be cleaned by normal means. While bathing, special attention should be paid to the hair and any exposed parts of the body.

In the event that the monitoring takes place AFTER the fallout has occurred, and extrapolation of the dose rate readings equals or exceeds those in Graph II at the estimated time of fallout, then it is recommended that the same advice be given as in the preceding paragraph.


GRAPH II
GAMMA DOSE RATES VERSUS TIMES AFTER DETONATION WHEN PERSONNEL BE ADVISED TO REMAIN INDOORS
(SEE TEXT FOR METHODS OF MONITORING)
GAMMA DOSE RATES
(MILLIROENTGENS PER HOUR)
TIME AFTER DETONATION (HOURS)
100
1000
10
1
10
100

Section III. Decontamination of Personnel

BACKGROUND

The principal purposes for decontaminating personnel are to reduce the potential beta doses to the skin, and to a lesser degree reduce the external gamma exposure. The discussion on beta doses in Section II is applicable here. In addition, there is much unknown about monitoring methods for personnel contamination. The following criteria were previously developed on the basis of measuring the gamma radiations (and then extrapolating to the accompanying beta radiations) with existing instruments. Recently new field instruments have been developed for direct beta measurement, but there remains considerably more work necessary to calibrate them in terms of beta dose rates to the body. Until this is accomplished, the past criteria may be used.

CRITERIA III

Where it is not possible to monitor personnel outside of a general radiation field, it is recommended that an estimate be made of the degree of personnel contamination by determining the location of the individual at the time of fallout. In the event there is uncertainty as to the validity of such an estimate, the assumption will be made that the individual was out-of-doors during the time of fallout. In those areas where the infinity gamma dose equals or exceeds 10 roentgens, it is recommended that the individual be advised to bathe and to change clothing.

For personnel being monitored outside the general radiation field where personnel contamination exists over relatively large areas of the EXPOSED body (one-half square foot or more):

When the reading of a survey instrument held with the center of the probe or center of the ionization chamber four inches from the center of the contaminated area, equals or exceeds the values given in Graph III it is recommended that personnel be advised to bathe and to change clothing.

For personnel being monitored outside the general radiation field, where personal contamination exists over relatively small areas of the EXPOSED body (less than one-half a square foot):

The recommended maximum values are one-half those given in Graph III. Monitoring of the head, arms, hands, lower legs, and feet will be considered as coming under this category. Washing may be limited only to the contaminated parts, and also a change of clothing may not be indicated, unless the radiation levels exceeds those stated below concerning monitoring of exterior surfaces of clothing.

For personnel being monitored outside the general radiation field, and the contamination exists over only spots of EXPOSED body (about the size of a half-dollar or less):

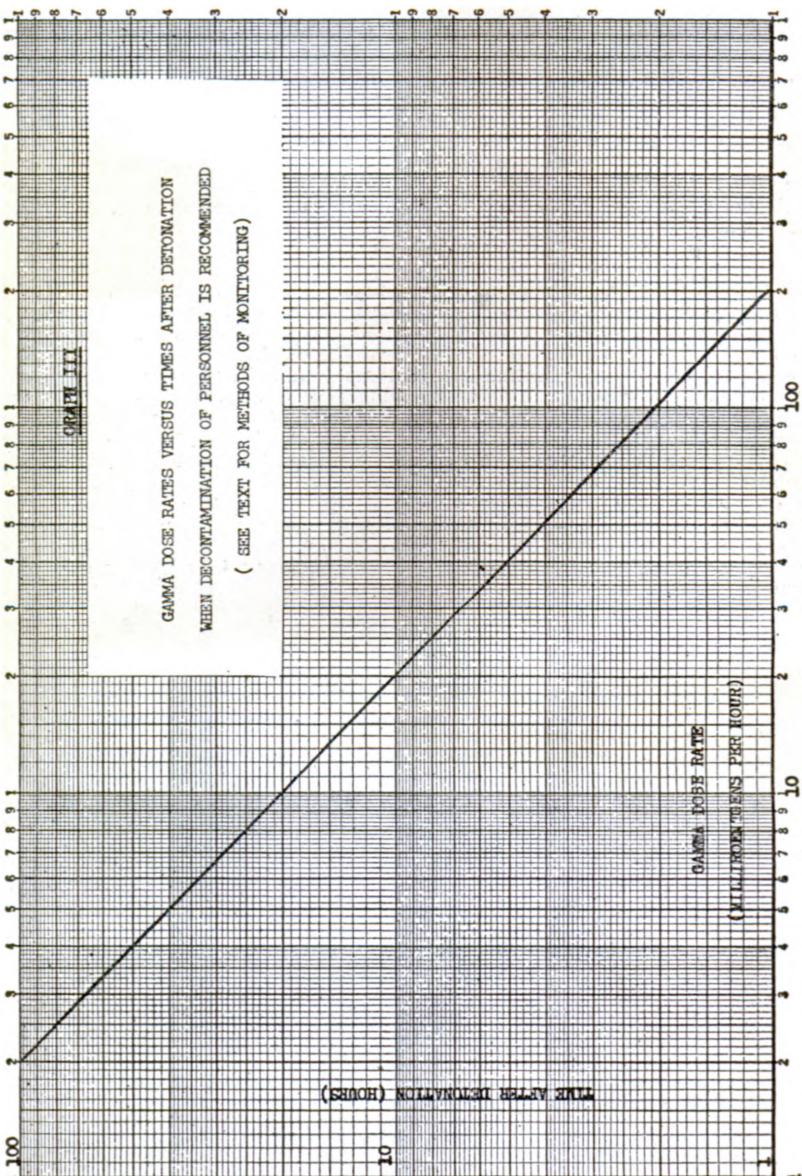
The recommended maximum values are one-fifth those given in Graph III.

Washing may be limited only to the contaminated parts, and also a change of clothing may not be indicated unless the radiation levels exceed those stated below concerning monitoring of exterior surfaces of clothing.

For personnel being monitored outside the general radiation field and the contamination exists over any size area on the exterior surface only of the clothing:

The recommended values under these conditions are twice those given in Graph III. The first recommended action shall be to resort to such simple acts as brushing off the clothing. If this action does not reduce the radiation levels to twice those given in Graph III or less, then personnel should be advised to change clothing and to bathe.

When the general contamination of a community is of the degree to produce an estimated maximum theoretical infinity gamma dose of 20 roentgens or greater, personnel who have been out-of-doors at any time during the first two days and generally moving around in the area (as apposed to such an act as walking only between a building and a vehicle) should be advised to brush off the footwear (outdoors), to bathe and to change clothing as soon as possible after the final return indoors each day. In addition personnel who go out-of-doors for any length of time during the first two days after such a fallout should be advised to wash their hands at least after the final return indoors each day, and more frequently, if possible.


GRAPH III
GAMMA DOSE RATES VERSUS TIMES AFTER DETONATION
WHEN DECONTAMINATION OF PERSONNEL IS RECOMMENDED
(SEE TEXT FOR METHODS OF MONITORING)
GAMMA DOSE RATE
(MILLIROENTGENS PER HOUR)
TIME AFTER DETONATION (HOURS)
1
10
100
1
10
100

SECTION IV. DECONTAMINATION OF MOTOR VEHICLES

BACKGROUND

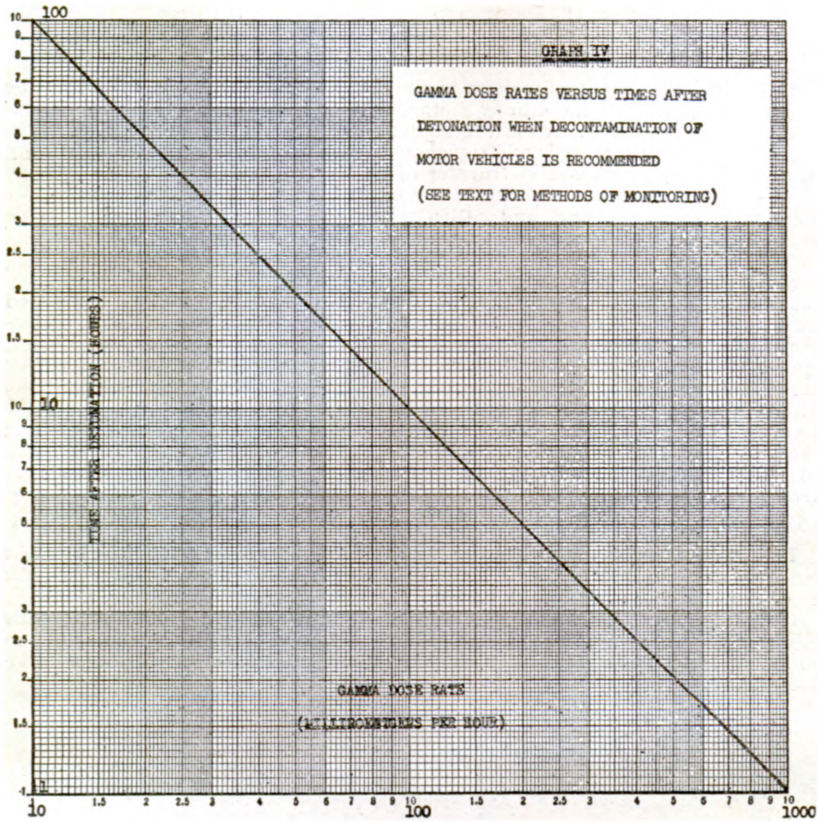
The principal purposes for decontaminating motor vehicles are to reduce the potential beta doses to the skin by contact with the vehicle, and to reduce the external gamma exposure. All of the uncertainties inherent in personnel monitoring are applicable here plus additional ones, such as estimates of the probability of contact and the amount of transfer of radioactive material from the vehicle to the skin. The following criteria for monitoring motor vehicles (Graph IV) were previously developed, and until the new beta measuring instruments (see Section III) are calibrated, will continue to be recommended.

One method of avoiding or significantly reducing vehicle contamination is to prevent their being in an area during the time of actual fallout. It is possible that fallout across a highway may be higher than that permitted for populated areas. When such a condition is predicted, it would be advisable to hold vehicular traffic until after the fallout had essentially ceased. Past experience has shown that very significantly less vehicle contamination occurs when it passes through an area afterwards compared to being present during the fallout time, although appreciable amounts can still be picked up on the tires and under the fenders. Obviously, there is not a precise value that may be given, but it is recommended that if the amount of fallout across a main highway is predicted to be in an amount equivalent to 10 roentgens or greater infinity dose, that traffic be temporarily halted until the fallout has essentially ceased.

CRITERIA IV

It is recommended that when the predicted fallout across a main highway be equivalent to 10 roentgens or greater infinity gamma dose, vehicles be held until the fallout has essentially ceased

Graph IV may be used in determining the advisability of decontaminating motor vehicles. The survey instrument should be held with the center of the probe or center of the ionization chamber four inches from any readily accessible surface.



SECTION V. CONTAMINATION OF WATER, AIR AND FOODSTUFFS

BACKGROUND

In any area where the theoretical gamma infinity dose exceeds 10 roentgens, adequate sampling of the water, air, and foodstuffs should be made to ascertain the conditions of possible contamination, if for no other reasons than as precautionary and documentary measures. Based on past data, however, *it is not* expected that under those conditions of fallout where the radiation levels are below those stipulated for possible evacuation, that the degree of contamination would be a health hazard. Nor is it implied here that any level above this does constitute a serious contamination of water, air, or foodstuffs. One good point of reference is the Marshallese experience where the whole-body gamma exposure was 175 roentgens yet the internal deposition from ingestion and inhalation was relatively small. In the event of a relatively heavy fallout, but less than one calling for evacuation, a common sense rule would be to wash exposed foods, such as leafy vegetables, since this is the most probable mode of intake of activity.

CRITERIA V

Monitoring of air, fod and water should be made as soon as possible in areas where the infinity dose equals or exceeds 10 roentgens. There need be no restrictive action imposed on food and water intake in areas where the fallout is less than that calling for evacuation. Washing off of such exposed foods as leafy vegetables may be advised when such action seems desirable.

SECTION VI. ROUTINE RADIATION EXPOSURES

BACKGROUND

The Atomic Energy Commission has adopted, as an operational guide, 3.9 roentgens whole body external gamma radiation for off-site exposure resulting from Operation Plumbbob.

The discussion in Section I on effects of weathering and shielding on determining the actual radiation exposure is applicable here. However, the factor of biological repair is not considered for routine exposures. This factor bears on somatic effects and may justifiably be considered in emergency situations when it is necessary to weigh the relative hazards from radiation versus mass evacuation. However, for routine exposures, the actual (estimated) roentgen dose should be used. To distinguish from the Effective Biological Dose and the Infinity Dose, this exposure will be expressed as the Estimated Dose.

Graph V incorporates the assumed effects of weathering and of shielding according to the discussion in Section I. The graph may be linearly extrapolated to other dose-rate readings. For example, if fallout occurs three hours after detonation and the dose rate is 360 milli-roentgens per hour, then about three roentgens (estimated dose) may be accumulated, i. e., $\frac{360}{120}\times 1=3$.

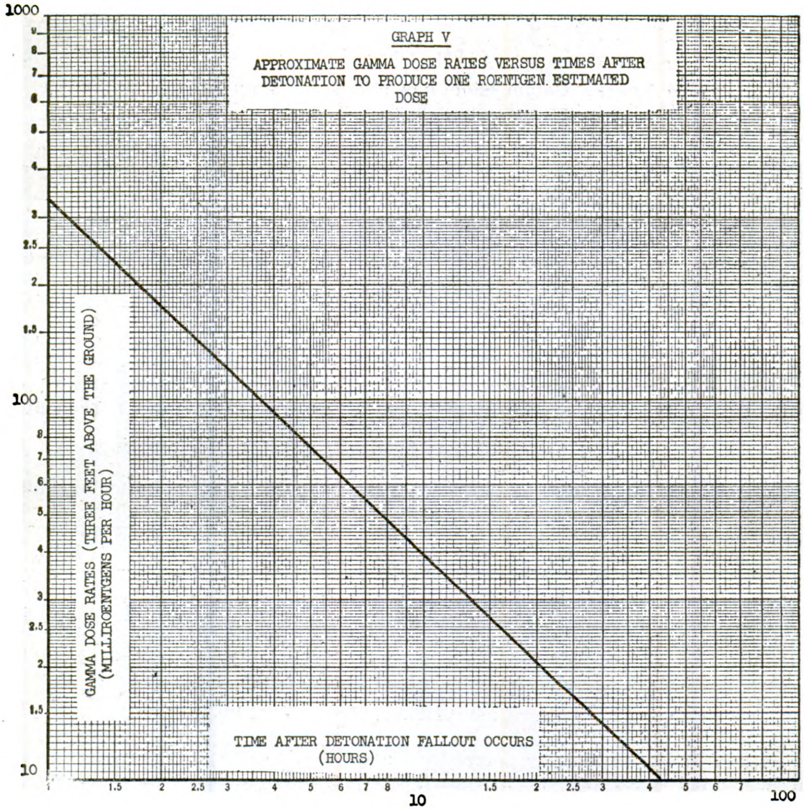
As discussed in Section I, the estimates of the effects of weathering and of shielding may be conservative for areas around the Nevada Test Site. A range of radiation doses is to be expected for these people since they will not all be living under identical conditions. The radation doses estimated by the present method is expected to fall within and toward the upper end of such a range. The information obtained from the expanded radiological monitoring program for Operation Plumbbob, should yield refinements in the method of estimating the radiation exposures.

In those cases where film badges are worn properly by personnel, the values recorded may be accepted as the Estimated Dose.

CRITERIA VI

Estimated Doses may be determined according to Graph V. In those cases where film badges are worn properly by personnel, the values recorded may be accepted as the Estimated Dose.

The whole-body gamma Estimated Dose for off-site populations should not exceed 3.9 roentgens resulting from Operation Plumbbob. This total dose may result from a single exposure or series of exposures.



Representative HOLIFIELD. This afternoon we will have Dr. Forrest Western, Division of Biology and Medicine, Atomic Energy Commission; Dr. Lyle Alexander, Department of Agriculture; and Dr. Roger Revelle, Scripps Institute of Oceanography, as witnesses.

We will meet in the Senate caucus room, room 318, at 2 p. m.

Before we recess, I have several statements to insert in the record at this point. The first is a statement of the United States Naval Radiological Defense Laboratory concerning the prediction, measurement and analysis of fallout and radiological countermeasures. Next a statement by LeRoy H. Clem, of Headquarters, Air Weather Service, United States Air Force. The third is a statement by Col. B. G. Holzman, and Col. Norair M. Lulejian, of the Air Force Research and Development Command, fourth is a statement by Dr. Donald M. Swingle, of the Army Signal Corps, Evans, South Carolina Laboratory, and finally a presentation submitted by James G. Terrill, Jr., Chief, Radiological Health Program, Public Health Service.

STATEMENT OF UNITED STATES NAVAL RADIOLOGICAL DEFENSE LABORATORY

PREDICTION OF FALLOUT

It was realized after the early weapons test operations that there existed a requirement for predicting the then little understood phenomenon of fallout. NRDL made the first studies on this subject by employing scaling techniques (1, 2, 3) similar to the approach used in the determination of blast and thermal

effects for weapons over a wide range of yields. Such scaling of radiological phenomena resulted in satisfactory results when compared to the meager experimentally determined field data (4). As more effects data became available from subsequent weapons test operations (5, 6, 7, 8) the limitations of a straightforward scaling technique were observed and the increasing dependence of the fallout on the dynamical parameters involved, such as the meteorological variables, became apparent. This led to the development of a physical model that would hopefully explain the mechanism of fallout such that given the required input parameters a knowledge of the fallout phenomenology for any type of nuclear detonation could be predicted (2, 9, 10). This model development was initiated by concentrating the effort on surface land detonations. Very little factual data were available for construction of such a model. However, it was realized that this approach offered the most positive chance of success and consequently theoretical assumptions regarding the model input parameters would have to be made. This model then defined the cloud source and associated parameters such as particle size distribution and relation of activity to particle size. A mechanism theory based on the particle settling rates and the effect of the winds aloft in determining the trajectories of these particles was established. A mathematical technique of summing the deposited activity on the earth's surface was developed such that the fallout pattern would then be established.

Because of the many initial assumptions made a great deal of effort was taken in subsequent nuclear weapons test operations to obtain refinements of these parameters by measurement (2). This work included detailed physical, chemical and radiochemical analyses of fallout particles, time dependent studies on the fallout such as time of arrival as a function of distance, rate of arrival, and time to peak activity. Activity levels as a function of distance were made (5, 6, 7). Rockets were employed to establish the radioactivity profiles within the mushroom cloud (11). Such experimental data were employed in the refinement of the physical model as well as were detailed studies of the effect of time and space variation of the winds aloft on the trajectories of the fallout particles. This data greatly improved the ability of the model to predict the fallout and continuing refinements are being made. The use of a physical model for understanding and predicting fallout appears justified (12).

A fallout forecasting technique has been developed to satisfy the immediate needs of the military. This technique employs many of the model parameters established. However it was designed for operational use and predicts only the perimeter of the fallout pattern and the radiological axis of the area or "hot line" (13, 14). It is a rapid system that was tested at Operation Redwing and proved very satisfactory for both surface land and surface water detonations. The details of this technique are described in the enclosed NRDL Technical Reports TR-127 and TR-139.

There has not been developed a satisfactory physical model for underwater or underground detonations to date. For these cases and environmental conditions other than surface or near surface burst the use of scaling techniques holds the most promise. However it is not inconceivable that the mechanism of such detonations will be understood and subsequent models developed.

The accuracy of prediction of fallout is very dependent on the quality of the meteorological data available. With precise meteorological data the area of fallout and direction of the axis of the pattern can be excellently forecast. The quantitative prediction of radiation levels at any point within the fallout area is much more difficult to predict.

It is considered essential in order to insure the application of fallout prediction technique and radiological hazard assessment to a wide variety of detonation conditions that the basic mechanisms responsible for formation of fallout, movement of fallout material in atomic clouds, its dispersal by meteorological forces and return to the earth's surface be thoroughly understood. Only a beginning to develop such an organized set of scientific data has been made.

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Measurement of Fallout

It has been the overall objective of the fallout measurements made by NRDL at the Nevada test site (3, 9, 12) and the Eniwetok Proving Grounds (1, 5, 6), to obtain those data which would allow prediction techniques to be tested and assessment methods developed for the radiological situations resulting from a wide range of nuclear detonation conditions (8).

Since fallout predictions result in the construction of gamma intensity contours, one group of measurements has featured the collection of experimental data for such contours. Direct measurement of the gamma ionization rate at a large number of points in the fallout area with a hand survey meter is the simplest and in many ways the most satisfactory method of obtaining this type of information (2, 4). When the fallout has been deposited on a solid surface, as in Nevada, surveys of this type have generally been used and further supplemented with measurements on instruments calibrated in terms of ionization of the activities of samples collected at certain locations for the primary purpose of physical, chemical, and radiochemical studies. When the fallout has been deposited on a water surface, as in the Pacific, certain other measurements are required for the interpretation of survey results. Because of the way in which the fallout material settles and disperses in the water, it has been necessary to measure its distribution to the total depth of mixing at each point of measurement before the total fallout deposited at that point could be computed. This has been accomplished in part by the use of a radiation sensitive probe which could be lowered to various depths, and in part by measuring the activities of samples collected at various depths. Both procedures have required critical instrument calibrations and theoretical work involving a number of assumptions, however, and it is probable that the results are much less accurate than those for the land surface case. In general, the measurements of this kind made by NRDL have shown that areas of the order of tens of square miles are subjected at early times to ionization intensities greater than 5 r/hr. by events in the low KT range and areas of the order of thousands of square miles to ionization intensities greater than 5 r/hr. by events in the MT range. Levels of several thousand

r/hr. at early times for both yield ranges have been measured or inferred, although less than 10 percent of the total affected area was estimated to have experienced these levels. While the probable error for contours from survey ionization rate measurements has been estimated ±20 percent for Nevada KT events, corresponding land equivalent contours for MT events in the Pacific cannot be estimated closer than within a factor of 2 or 3 at the present time.

Another group of measurements has ben directed toward obtaining time dependent data, such as the variation of the gamma field intensity and gamma energy spectrum with time and the distribution of particle sizes deposited with time at a number of locations in the fallout area (10, 12). Such information is needed both to check model theory which yields similar results and to provide a complete description of fallout phenomena. The changing gama radiation field has usually been measured by means of an instrument which recorded increments of ionization dose received at its location from all sources within unit time intervals, while gamma energy spectra have been measured on fallout samples from a known fallout area with an instrument utilizing a crystal detector, a photomultiplier and a pulse height discriminator (7, 12). NRDL results have shown that the gamma radiation field due to fallout outside the area of severe blast damage tends to build up to a maximum in approximately twice the time required for the fallout to arrive, varying from a few minutes near ground zero to 24 hours or more at distances of over 100 miles. The radioactive decay of fission products may be approximately by a straight line of slope—1.2 on a log log plot; however the more general case in which several induced activities are present, and the fission products are fractionated, leads to a complex decay curve. Spectral measurements show the average energy of the fallout gamma radiations to vary from about 0.6 Mev. at 10 hr. to 0.3 Mev. at 360 hr.

The determination of particle size distributions with time has required the development and application of specialized collectors capable of sampling automatically over consecutive time intervals from a few minutes to an hour or more, as well as special methods and instruments for sizing and counting the collected particles. It has been found that particles with diameters between 100 and 300 microns predominate in most collections with larger sizes (2,000–3,000 microns) increasing nearer ground zero and smaller sizes (20–100 microns) increasing farther away from ground zero. In general, data of this kind, being more direct, are more reliable for computing fraction of the bomb in the total fallout than survey results—although several sources of error such as sample bias 11 and radionuclide fractionation, do exist. On the scale utilized above, standard error in fraction calculations might be estimated at about ±25 percent for the gamma energy and emission rate method, as opposed to possibly several hundred percent by the survey method for water surfaces and less than 100 percent for land surfaces.

Extensive physical, chemical, and radiochemical analyses have been performed on the particulate produced by detonations occurring on the sandy Nevada soil and on coral atolls and the ocean surface in the Pacific. The mass of such material as well as the fraction of the bomb deposited per unit area at a number of locations has also been determined by weighing collected samples and performing radiochemical analyses. Since fallout ingestion constitutes a separate hazard from exposure to external fallout radiation, and since countermeasures and recovery procedures depend heavily on knowledge of the various properties of the contaminant, information of this kind is essential for assessment purposes.

NRDL has consistently emphasized measurements of local fallout and characterization of the phenomena associated with it. It has been possible, nevertheless, to estimate the fraction available for worldwide fallout by subtraction of the local fallout from the total produced, and this has been found to be something of the order of 50 percent for both land surface and water surface events. No closer estimate can be given because of the many uncertainties and sources of possible error in the measurements and calculations.

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Environmental Aerosol Analysis

The United States Naval Radiological Defense Laboratory (USNRDL) collects and analyzes daily aerosol samples for airborne activity of air passing over the laboratory. Twenty-four-hour air samples have been collected and analyzed daily since January 1950, and is a continuing program at the laboratory. The attached graphs present a summary of the long-lived activity and half-life for these daily aerosol samples. Additional analysis on representative aerosol samples indicated this activity is due to airborne beta-gamma fission products. The appropriate dates of the various United States nuclear weapon tests are indicated.

It is observed from the graphs that in 1950 there was essentially no fission product activity in excess of 10^{-14} $\mu c/cc$. In 1951, the aerosol activity rose during the Ranger and Greenhouse operations but then dropped back to an average of $3x10^{-14}$ $\mu c/cc$. Successive rises and falls of the aerosol activity are noted for the succeeding years. The rises in aerosol activity, fall of 1951, 1953, 1954, 1955, and 1956 were not produced by the United States or United Kingdom nuclear weapon tests. Since December 1955, the aerosol activity has varied between 10^{-13} and 10^{-12} $\mu c/cc$. In other words, the fission product activity background was less than 10^{-14} $\mu c/cc$ in 1950, but is now around $5x10^{-13}$ $\mu c/cc$. The present concentration of airborne fission products is at most one-tenth of the natural radiactive aerosol (radon and thoron) concentration and is one-fifty thousandth of the industrial maximum permissible concentration for continuous exposure to undetermined mixtures of beta-gamma emitters.

NATURAL RADIOACTIVE AEROSOLS

The earth's surface atmosphere normally contains radioactive aerosols produced by the radioactive decay products of uranium and thorium minerals in the earth's crust. The amount of these natural radioactive aerosols varies from 10^{-14} μc/cc. to 10^{-11} μc/cc. as determined by the earth's mineral composition in a particular locale. The unit μc/cc. (microcuries per cubic centimeter) is a measure of the amount of radioactive material per unit volume suspended in the air. 10^{-9} μc/cc. is the present industrial maximum permissible concentration for continuous exposure to undetermined mixtures of beta-gamma emitters.

PROCEDURES

Every air sample contains natural radioactive decay products (mainly radon from the uranium series and thoron from the thorium decay series), and any other radioactive material contamination such as fission products, suspended in the air. In the analysis of the aerosol samples collected at USNRDL, the natural radioactive aerosol products, radon and thoron, are subtracted from the total aerosol activity. The daily aerosol samples are collected at USNRDL by passing 600 cubic meters of air through a 2-inch diameter high efficiency filter paper. The filter paper is then automatically counted for beta-gamma activity with a geiger tube and scaler. The minimum limit of detection on the USNRDL equipment is 10^{-14} μc/cc.

RADIOLOGICAL COUNTERMEASURES

INTRODUCTION

Most of the USNRDL concepts relative to countermeasures against nuclear attack have been previously presented in the hearings before the Subcommittee of the Committee on Government Operations. One part covering our general concepts appears in part 6 of the hearings on Civil Defense for National Survival held at San Francisco and Los Angeles on May 24, 25, 28, 29, and 31, 1956. These concepts were expanded in those portions involving the emergency periods at hearings held on H. R. 2125 at Washington, D. C., on February 5 and 6, 1957.

GENERAL CONCEPTS

The overall objective of radiological defense is to minimize the effects of nuclear attack on operations. Three time phases of defense actions are apparent: emergency phase; operational recovery phase; final recovery phase. The objectives in each phase are respectively: survival; early recovery of essential functions; ultimate recovery of normal functions. In everyday language, these objectives on a national scale are survive, stay in the war, and win the peace. There is a definite interaction between these objectives: actions which can be taken in any one phase will depend on those taken in other phases.

The general concepts on which the time phases and the respective countermeasures which apply to each phase are discussed in references 1, 2, 6, 8, 9, and 13. Our basic conclusions (reference 9) are that shelter is the central countermeasure in the emergency phase and reclamation is the central countermeasure in the operational recovery phase. The central countermeasure for the final recovery phase will probably be some form of exposure control actions; the precise nature of the actions that will be required have not as yet been taken under investigation by NRDL. In addition, reference 9 discusses other countermeasures that can be used to supplement the central countermeasures; these are called peripheral countermeasures. They include such actions as dispersal, evacuation, operational adjustments to regulate exposure to radiation.

These concepts are applicable to the case of large-scale attack with high yield thermonuclear weapons wherein all persons will be subject to effects of the attack and significantly larger areas will be subject to fallout than any other combination of effects. The concepts also apply to attack with small yield weapons in localized areas where a more limited number of people are subjected to the effects of fallout alone. People in these areas must be prepared to achieve the objectives of the defense system.

COUNTERMEASURE APPLICATIONS

Radiological countermeasures are actions that are designed to reduce, eliminate, or control exposure of personnel to radiation from radioactive material. In the emergency and operational recovery phases, the principal radiological

hazard to personnel is that from external gamma radiation. In the final recovery phase (starting several years after attack), the principal hazard is an internal one from continuous ingestion and/or inhalation of long-lived radioactive materials. Because of the long periods of possible exposure of personnel living in a radioactively contaminated environment, countermeasures must be planned as a phased, long-term, continuous, and coordinated system. Concepts and guides for planning such a system are given in references 1, 2, 8, 12, and 13. The overall system planned for a given installation or locality must be based on an analysis of the vulnerability of the target area to attack and must be independent of the condition of attack.

COUNTERMEASURE COMPONENTS

1. Shelters

The optimum requirement for the shielding afforded by a radiological shelter is a reduction in the radiation intensity of the order of 1,000 to 5,000; the use and requirements for shelters are discussed in references 1, 8, 9, 10, 13, 14, and p21. Reference 14 describes a national shelter system consisting of (*a*) simple radiological shelters, (*b*) radiological shelters with fire-storm protection in areas where fire storms could occur, and (*c*) high-performance shelters in densely populated areas to protect against high blast pressures and fire storms as well as against nuclear radiations. Many existing buildings can serve as adequate shelters; others can serve with some modifications.

2. Reclamation

The basic reclamation techniques presently available and tested are (1) manual decontamination of paved areas, building surfaces, ships, and aircraft; (2) earth-moving procedures on open land areas; and (3) automatic washdown on ships. Most of the data on decontamination are from laboratory experiments on sea-water type fallout, of which only a few of the more recent reports are referenced here. The reports that deal with the general chemistry and physics of decontamination are references 15, 16, 17, 18, 19, p4, p11, p12, and p13. Field-test and engineering-scale data are given in references 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, and p19. The application of these data to countermeasure systems planning is described in references 1 and 8.

In addition to cost of equipment, operating rates, manpower required, and radiation dose incurred, the efficiency of many of the reclamation methods depends on the physical and chemical nature of the fallout. For similar levels of radiation, fallout from detonations at sea consists mainly of sea water residues, and is most difficult to remove from surfaces; fallout from harbor detonations consists of sea water and harbor bottom soils and is more easily removed; and fallout from land detonations consists of contaminated soil particles and is easiest to remove. However, even for the latter case, decontamination of large areas is difficult and time consuming. The relative amounts removed by a method such as firehosing an asphalt road might be in the order of 30, 75, and 95 percent, for sea water harbor and land detonation fallout respectively. The amount of fallout, condition of the surface, and time of removal also influence the results. For this reason, the effectiveness of a washdown system on a ship (at sea) cannot be used as a meaure of effectiveness of a similar system on buildings (on land) without experimental verification.

Peripheral countermeasures in the operational recovery phase include (1) evacuation of nonessential persons to safe areas, (2) readjustment of operation schedules to reduce exposure times, (3) applied shielding such as sandbags around living and working areas. Whereas the presently employed concept in controlling radiation exposure is to exclude personnel from contaminated areas, the only feasible concept in radiological defense is just the reverse: the exclusion of radioactive material from clean areas.

SUMMARY OF STATE OF INFORMATION

The conceptual philosophy of an adequate radiological defense system is, at present, in a more advanced stage of development than are the supporting experimental data required for successful implementation of the system.

The main areas for which experimental data are needed are (1) shelter design and testing (habitability tests, contamination ingress, effects under firestorm conditions, operations in shelter related to outside conditions and future operations, communications problems, and control problems); (2) operational data necessary for testing many of the planning procedures such as those given

in reference 1; (3) reclamation procedures (improve reliability of data on presently available methods, develop and test new, dry, decontamination methods to replace presently recommended methods which require large amounts of water, develop and test of automatic or low-exposure decontamination methods, improve rates and techniques of application of present methods—especially on larger areas, obtain correlations of laboratory and engineering-scale data with data from nuclear tests—most all nuclear tests are not applicable since they are intentionally detonated not to produce fallout of the kind required); (4) definition of the radiological situation and development of countermeasures therefor during the final recovery phase from data on the transport of the radioactive elements in the region of heavy fallout from the radioactive particles into plants and animals used as foodstuffs; such data are required for the planning of necessary countermeasures for a resumption of normal living conditions; and (5) a proof test of a complete proposed countermeasure system under realistic attack conditions (a test in which the complete countermeasure system with all its tested components are put together and in which the countermeasure actions are continued through all the phases); such a test cannot be made under the biased influence of a weapons-development test, since it would take 5 to 10 years to complete, not counting the preparation time.

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A Fallout Plotting Device

RESEARCH AND DEVELOPMENT TECHNICAL REPORT USNRDL–TR–127, NS 081–001, AND UNITED STATES ARMY, NOVEMBER 30, 1956

By E. A. Schuert, Physics, Techncal Objective AW–7; Radiological Capabilities Branch, L. B. Werner, Head; Chemical Technology Division, E. R. Tompkins, Head; Scientific Director, P. C. Tompkins; Commanding Officer and Director, Capt. Richard S. Mandelkorn, USN, United States Naval Radiological Defense Laboratory, San Francisco, Calif.

ABSTRACT

A fallout plotting device was developed. The method requires no drafting equipment and is ideally suited for field use. At Operation Redwing it was found that untrained personnel could quickly become proficient in its employment.

SUMMARY

The problem

A plotting device is needed to speed up forecasting where fallout will fall in the field. Such a device should require no drafting equipment but still accurately plot the required data in a manner compatible with the latest fallout model theories. It should be so constructed that untrained personnel can quickly become proficient with it.

Findings

Such a device was developed and tested at Operation Redwing. It proved to be satisfactory, and suitable for field operations.

ADMINISTRATIVE INFORMATION

This work was done under Bureau of Ships project No. NS 081–001, subtask 1, technical objective AW–7. The work is described in United States Naval Radiological Defense Laboratory annual progress report to the Bureau of Ships, DD form 613, of July 1956 (enclosure (1) to commanding officer and director, USNRDL Secret letter 3–905–471 EHC: dlc serial 0014921 of August 31, 1956). The plotter was tested at Operation Redwing, project 2.6.3, as described in subtask 4B of NS 088–001 of February 1956.

The work also is part of the technical program for the Department of the Army established between Department of the Army, Office, Chief of Research and Development and Bureau of Ships (joint agreement, Nov. 23, 1955).

INTRODUCTION

This paper describes a rapid technique for plotting "particle-size"[1] and "height" lines in mapping fallout from a nuclear detonation. Since this method,

[1] Particle-size lines are often referred to as hodographs or weighted hodographs.

one of hand computation, uses a fallout plotting device that requires no drafting equipment, it is ideally suited for field use. It was employed successfully at Operation Redwing where it was found that untrained personnel could quickly become proficient in its employment.[3]

The use of particle-size and height lines in mapping fallout is a standard technique employed in most analytical methods now in use. It simply describes a grid (fig. 1) on the earth's surface indicating where certain sizes of fallout particles, originating along a line source through the axis of symmetry of the cloud, will arrive and from what altitude they will come. These parameters are the basic data for describing the fallout pattern.

There are three requirements for determining this grid: the initial distribution of material in the atmosphere; the falling or settling rate of the material from its initial elevation; and the wind field through which the material is falling and by which it is being displaced.

The fallout plotting device computes the points of arrival on the earth's surface of a given particle size that originates at various altitudes within the mushroom cloud and its stem. Particles originating at elevations of every 5,000 feet, from the surface to 120,000 feet, are considered. In the construction of the device, account is taken of the variable speed of the settling particles due to changes in the vertical distribution of the atmosphere's density and viscosity. Aerodynamic falling equations were employed in its design. However, selection of particle falling speeds and altitude increments is arbitrary and not a fixed factor in the basic design of the plotter.

If the average wind speed and direction within a given altitude increment and the time required for a particle to fall through it are known, then the horizontal displacement of the particle can be computed for that altitude layer. Knowledge of the particle's point of arrival on the surface may be deduced from tracing a settling particle as it is displaced by each wind in each altitude increment. Plotting trajectories for each particle size at every starting elevation is the first step in determining the resultant fallout pattern; however, the drafting involved is tedious and time consuming. This effort can be reduced greatly by plotting from the ground up, as is done in the construction of a hodograph. Such a plot is made by starting at ground zero and working up through the altitude increments to the desired elevation. Although this technique does not plot the trajectory of the particle, it does define the arrival points on the surface of the earth of particles starting at each altitude increment (fig. 2).

DESCRIPTION AND USE OF DEVICE

To plot these size-lines one must make the preliminary computations of particle falling times through each altitude increment to obtain the displacement for various wind velocities. The plotter was designed with these computations built in, thereby speeding up the plotting process significantly.

The plotter consists of two parts, a base for direction or azimuth orientation and a wheel for distance or displacement. Since both of its parts are constructed of clear plastic, the plotter does not obscure the map over which it is placed. The base consists of a wind-rose having a radial line at each 10° interval on the compass. The base (fig. 3) has a narrow slot along the 180° line. If a given wind direction (in degrees from which the wind is blowing) is selected and its radial line oriented to north on the map (parallel to the north-south grids), the 180° slot becomes oriented in that direction in which a falling particle will be displaced. Thus by orienting the base of the plotter as described for any measured wind direction, the vector azimuth for the particle can be drawn through the slot of the plotter base.

The wheel (fig. 4) is pivoted at the center of the base. It has 24 equispaced radial slots. Each slot represents an altitude increment of 5,000 feet. Concentric circles intersect the radial slots to form a scale of wind speed in knots. Since the particle falling speed is a function of the atmosphere's density and viscosity and since these factors vary with altitude, the wind speed scales are so weighted

[3] A USNRDL report which will describe the detailed techniques of forecasting used at Operation Redwing and how the employment of the plotter was adapted to consider time variation of the winds is in preparation.

and the indicated length of the scale actually represents the horizontal displacement of the particle through the altitude layer of interest.

To obtain the distance the particle is displaced along its azimuth, the wheel is rotated until the proper altitude layer is alined with the 180° slot in the base and a line is plotted on the map.

It should be remembered that the weighted scales of wind speed fix the map scale, which in this case was 1 : 970,000 or 1 inch = 13.2 nautical miles. Different wind speed wheels have been constructed for several particle sizes; at present four wheels have been made.[3]

In plotting a size-line with the fallout plotter (fig. 5) one uses the same technique as one does when employing a drafting machine. However, all computations of horizontal displaced distance the particle experiences when falling through a given altitude layer are eliminated.

To plot a size-line or a trajectory, the following steps are necessary:

1. Rotate the wheel until the desired altitude increment coincides with the 180° slot in the base.
2. Place the plotter with the zero value of the wind speed scale over the given point and orient the base so that the radial line, showing the direction from which the wind blows, parallels the north-south grids of the map.
3. Draw a wind speed vector through the coincident slots.
4. Continue the process using the tip of the vector just drawn as the next point.

In constructing the prototype plotters certain specialized parameters were used in making the computations; for example, atmospheric density and viscosity were computed for a Marshall Island atmosphere, particle parameters were typical of coral fallout and special aerodynamic falling speed equations were used. Any of these variables as well as altitude increments may be so selected that a similar plotter for specialized or more general input data becomes possible. Also if one wished to assume a constant falling rate for a given size particle the wheel could be eliminated and the single wind speed scale laid out along the 180-degree slot on the base.

Figures 6A, 6B, 6C, and 7A, 7B, 7C are reproductions of the component parts of the four plotters that have been constructed. These figures can be used to construct a set of plotting devices. A reference scale has been added on each figure to relate the reduced drawings to their original size wherein the scale relationship was 1 : 970,000.

Approved by:

EUGENE P. COOPER,
Associate Scientific Director.

[3] These wheels are for irregular-shaped particles of density 2.36 g/cc and having diameters of 75, 100, 200, and 350μ. A plotter may be adapted for more than one particle size by adding parallel scales to each radial slot on the wheel.

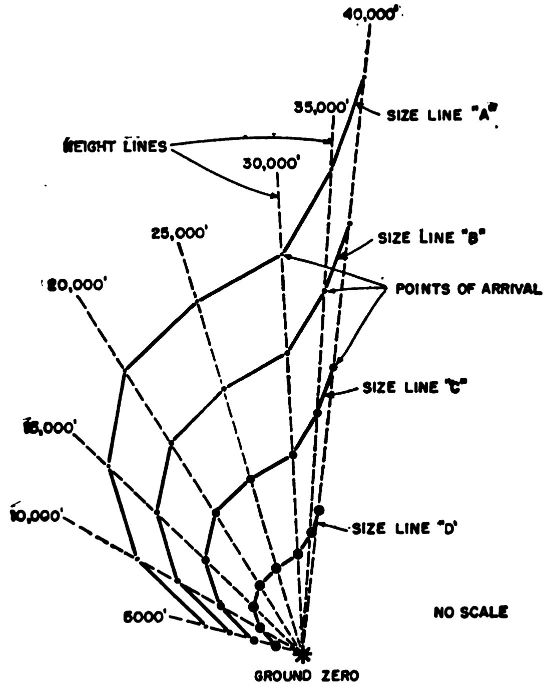

FIGURE 1.—Basic fallout plot showing grid of size lines and height lines.

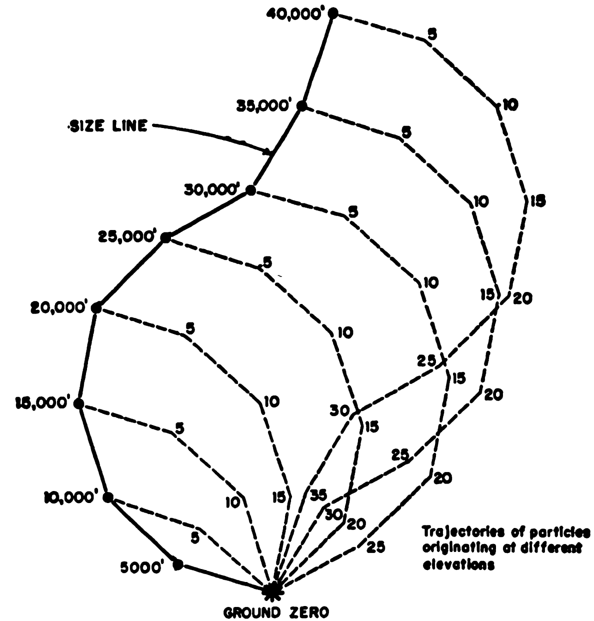

FIGURE 2.—Comparison of plotting techniques either by use of trajectories or by use of a size line.

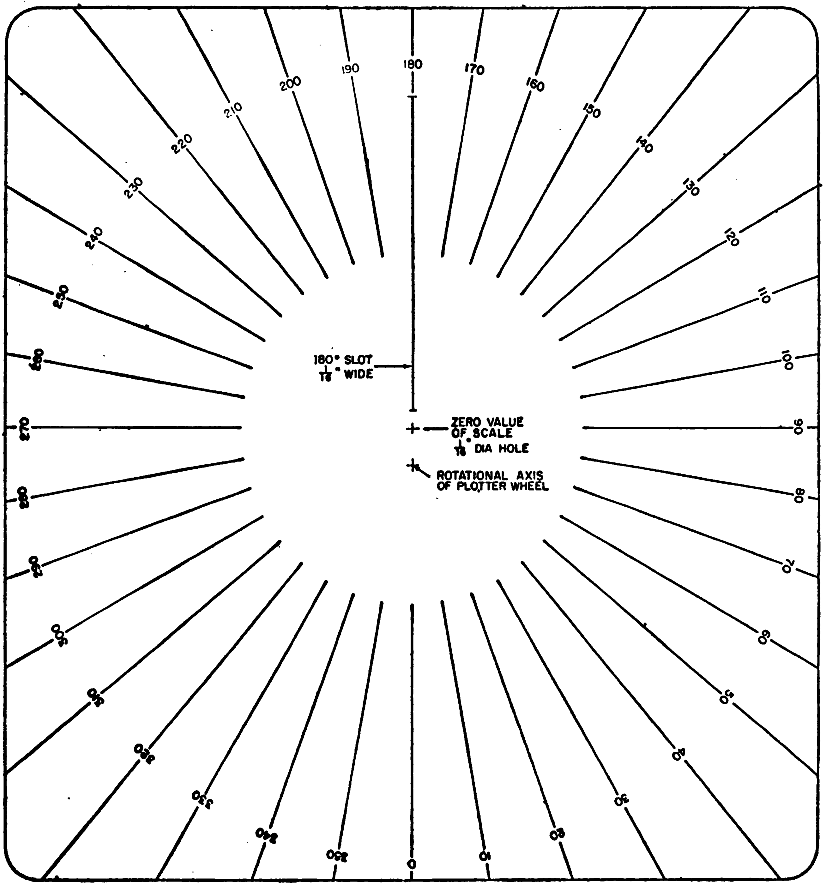

FIGURE 8.—Plotter base, for determining direction.

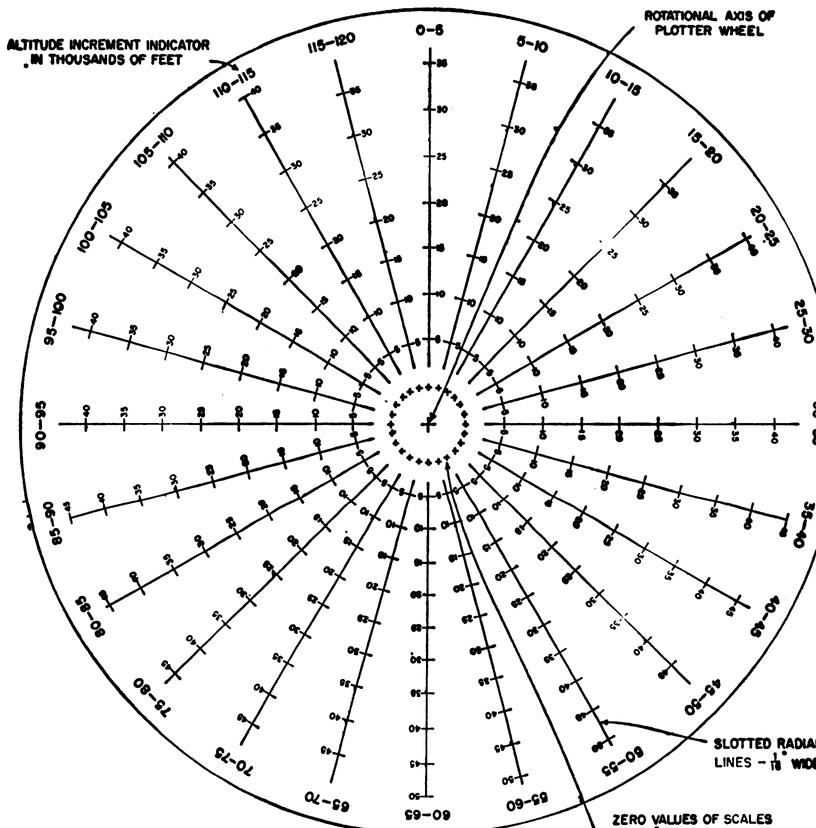

FIGURE 4.—Plotter wheel for determining displacement of 75-μ particles.

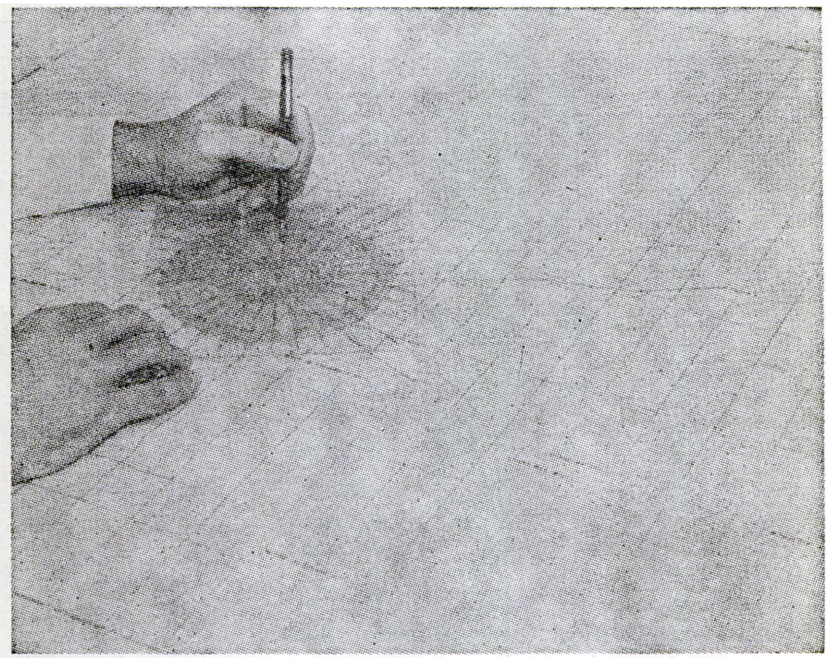

FIGURE 5.—Plotting device being used.

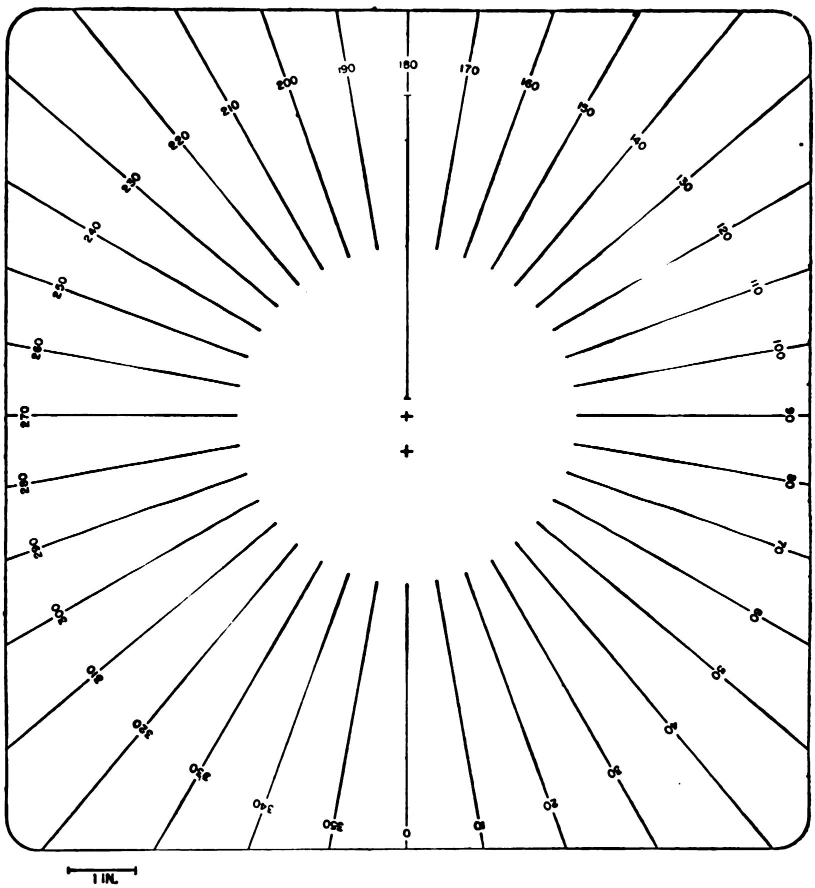

FIGURE 6A.—Plotter base for 75- and 100-μ particles.

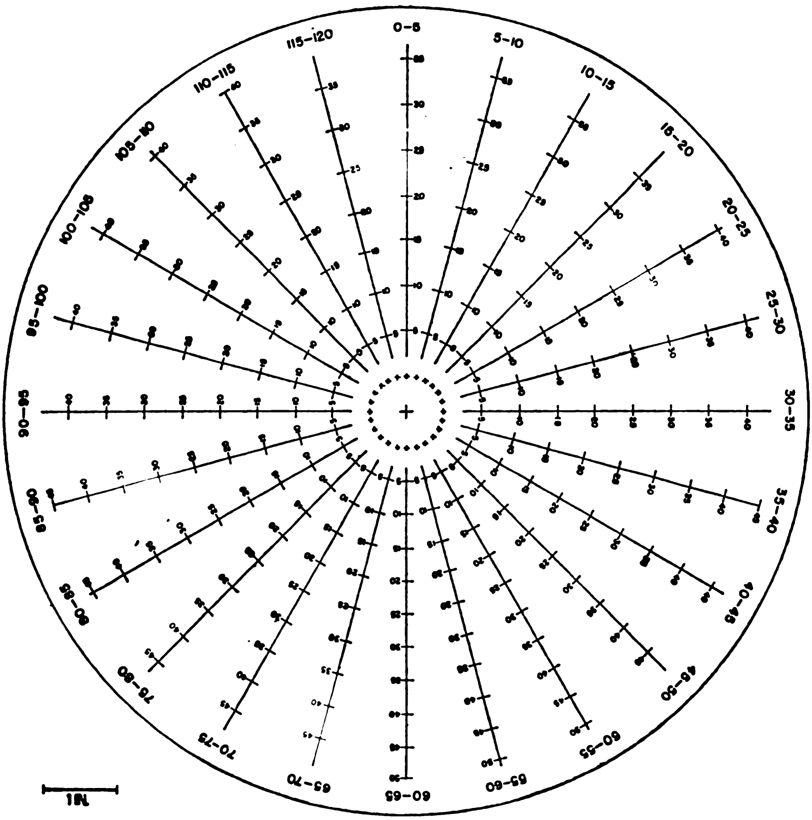

FIGURE 6B.—Plotter wheel for 75-μ particle.

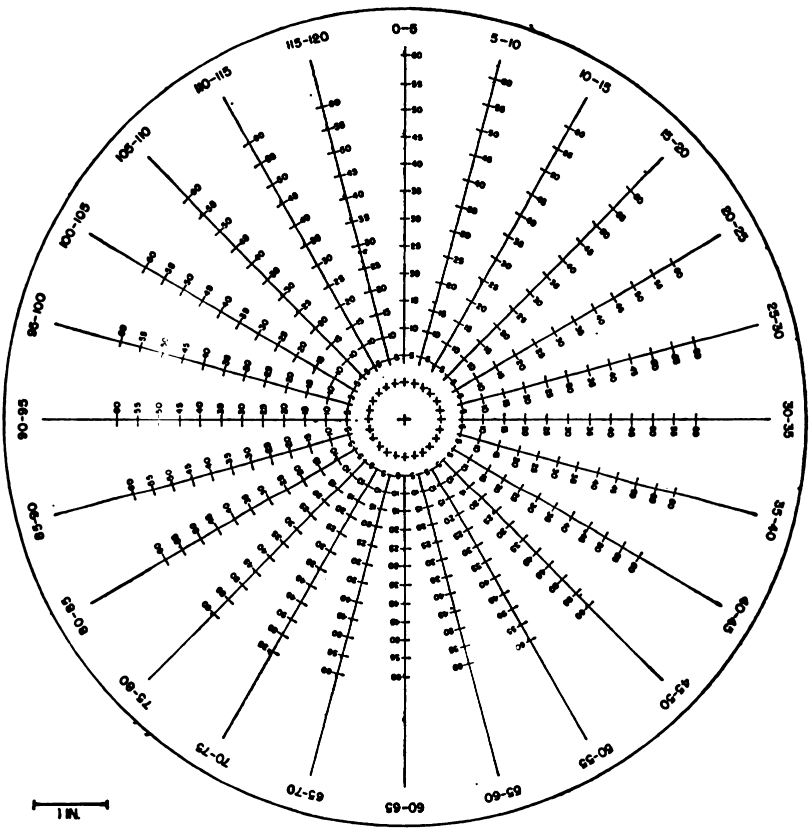

FIGURE 6C.—Plotter wheel for 100-μ particle.

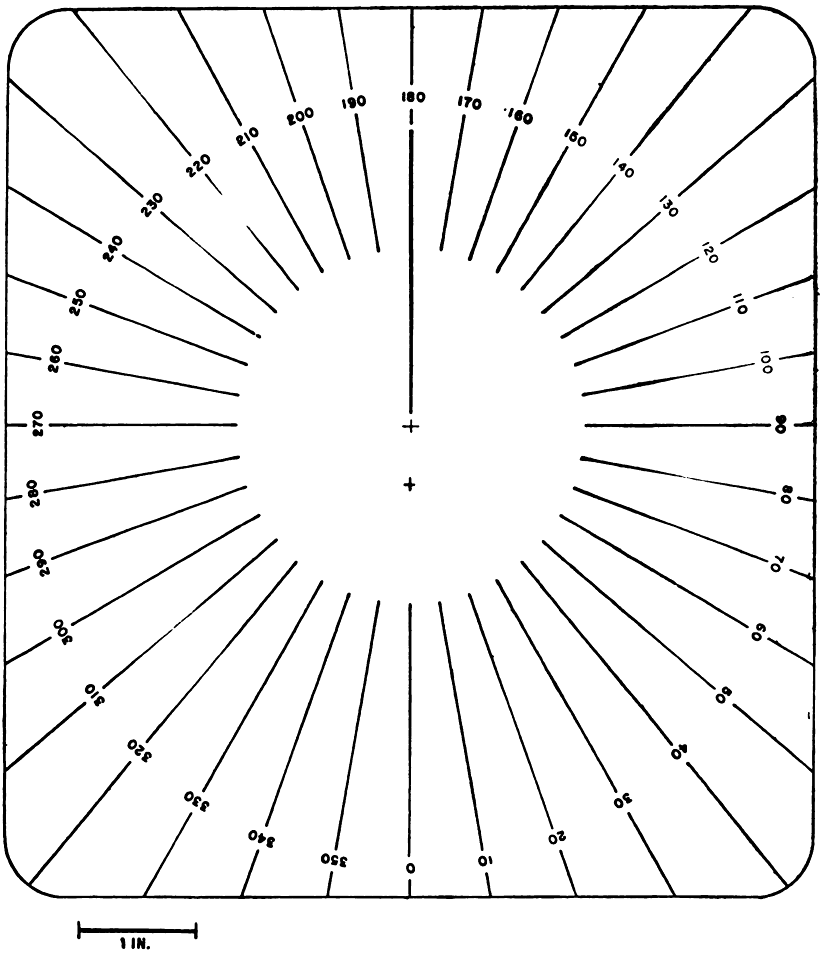

Figure 7A.—Plotter base for 200- and 350-μ particles.

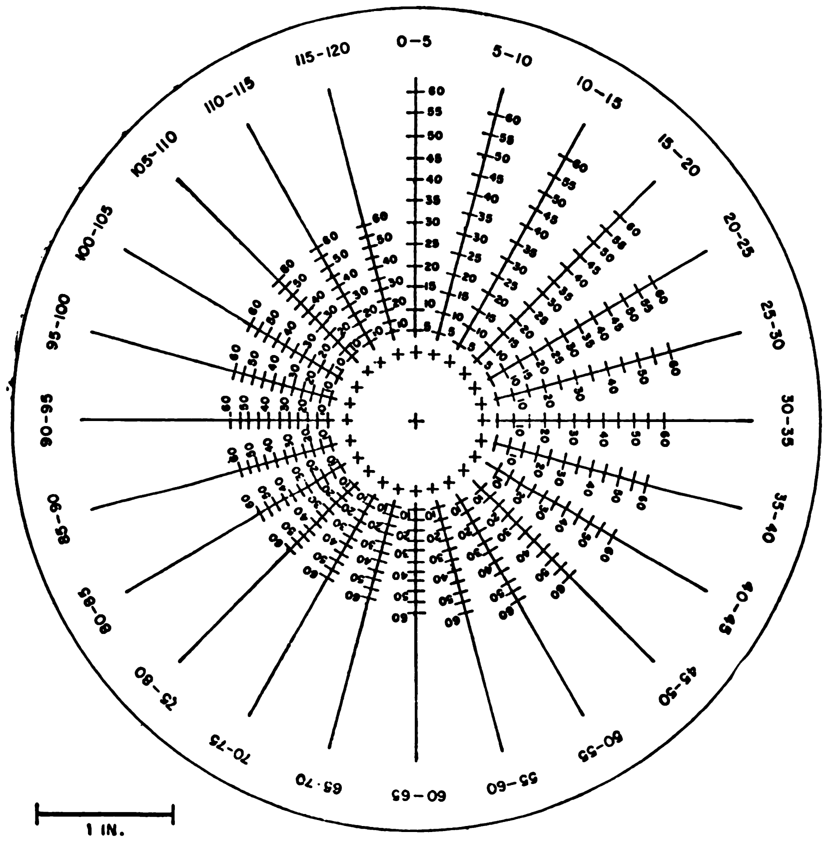

FIGURE 7B.—Plotter wheel for 200-μ particle.

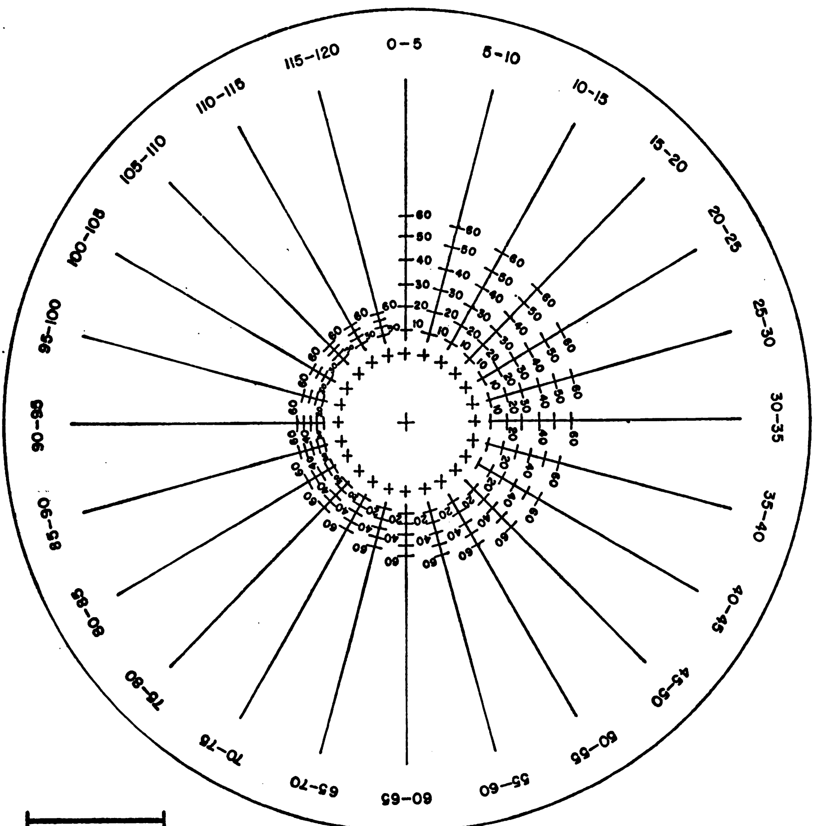

Figure 7C.—Plotter wheel for 350-μ particle.

Naval Radiological Defense Laboratory. USNRDL-TR-127. A FALLOUT PLOTTING DEVICE, by E.A. Schuert. 30 Nov. 1956. 19 p. illus. UNCLASSIFIED A fallout plotting device was developed. The method requires no drafting equipment and is ideally suited for field use. At Operation REDWING it was found that untrained personnel could quickly become proficient in its employment.	1. Fallout - Course mapping 2. Plotters I. Schuert, E.A. II. Title. III. NS 081-001. UNCLASSIFIED

A Fallout Forecasting Technique With Results Obtained at the Eniwetok Proving Ground [Draft]

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ADMINISTRATIVE INFORMATION

The work described herein is a part of the research sponsored by BuShips and the United States Army and locally designated as program 2, problem 3, phase 3. Its technical objective is AW-7 and it is described on RDB card NS 081-001.

SUMMARY

The problem: A fallout forecasting technique is needed to qualitatively describe the fallout hazard resulting from nuclear detonations. This technique should have such flexibility that its employment is valid for field use.

Findings: A summary of the latest experimental and theoretical considerations has resulted in the development of a technique whose complexity is dependent on the required accuracy of the results desired. This technique has been satisfactorily tested at the Eniwetok Proving Grounds for land surface and water surface bursts.

ABSTRACT

A generalized fallout forecasting technique is presented with detailed computations of input parameters for use in the Marshal Islands.

Results obtained at a recent weapons test are briefly discussed by comparison of forecast fallout with preliminary measured data.

1. INTRODUCTION

Fallout research continues to seek a theoretical working model that will describe in detail the mechanism of fallout. Aside from this long-range problem, consideration must be given to making available a working tool that will meet the needs of the military for solving fallout problems in the field. Such consideration requires a simplified rapid system capable of producing qualitative if not quantitative results.

Within a program studying fallout at a recent weapons test operation there was a fallout forecasting assignment that had many aspects of the practical

field problem yet, at the same time required quantitative results for use in reducing other data. This program needed positioning data such that three ships could be located properly in the fallout to obtain data on its parameters. Also, aerial and oceanographic survey projects required knowledge of the fallout to instigate their navigational procedures properly.

To meet these requirements a technique for rapid fallout forecasting was developed which not only satisfied the needs of the fallout program but also was accurate enough to allow comparison between meteorological aspects of model work and results obtained from surface measurements. This technique was restricted to describing quantitatively the perimeter of the fallout, the axis of the "hot line," and to determining the time of arrival of fallout throughout the pattern. No attempt was made to quantitate the expected levels of gamma activity or to develop radiation contour lines.

The task force employed a fallout prediction unit at this operation for determining the safe time to detonate the test devices. Many of their techniques for forecasting were similar to those described in this report, however, their problem was of a different nature than that of the fallout program. Several of their methods were unique in that portable analog computers were tested as field instruments. These computers permitted consideration of many complex parameters. One, in particular, obtained essentially an instantaneous solution to the problem once the meteorology was available.

The fallout program and the task force prediction unit functioned independently. It was not feasible for the two to employ the same technique because the postshot variability of the winds aloft were especially critical in ship-location problems in the fallout program. This problem will be discussed in detail later.

1.1 *Objective*

This report describes a technique of forecasting fallout employed at a recent weapons-test operation. The results obtained in the field are discussed as examples of the reliability of the techniques. Although the technique was designed for analysis of land surface detonations where the fallout is particulate, its application to water surface detonations is considered.

2. FORECASTING TECHNIQUE

The forecasting technique uses many ideas from fallout model work. Several simplifications as well as a plotting device have been developed to the end that the time involved has been reduced greatly without sacrificing accuracy. In general, an initial source of activity is defined describing the "stabilized" nuclear cloud by appropriate spatial and size distributions of radioactive particles. These particles are tracked to the earth's surface by considering their falling speeds and effects of the winds existing aloft.

2.1 *Basic considerations*

In some cases the input parameters for the forecasting technique were obtained from weapon-test measurements. In others where data were lacking, the parameters were derived from theory.

2.1.1 *Source model*

The optical or visible dimensions of the initial cloud from a nuclear detonation have been documented in past weapons tests. Available data describe such parameters as height to base of mushroom, height to top of mushroom, and mushroom diameter all as functions of time. Vertical rise stabilizes in approximately 6 min post detonation. This time is independent of yield, however, the expansion of the mushroom diameter particularly for the megaton devices continues for perhaps 30 min. Available diameter measurements have not been made in excess of H+10 min, however, fairly reliable data are known for the optical cloud dimensions as functions of yield to H+10 min. The ultimate cloud diameter can be extrapolated from low-yield curves and some qualitative data. Figures 1 and 2 present values of the cloud dimensions from past tests. The source model was assumed cylindrical having, for a given yield, these dimensions. Its stem diameter was taken as 10 percent of mushroom diameter.

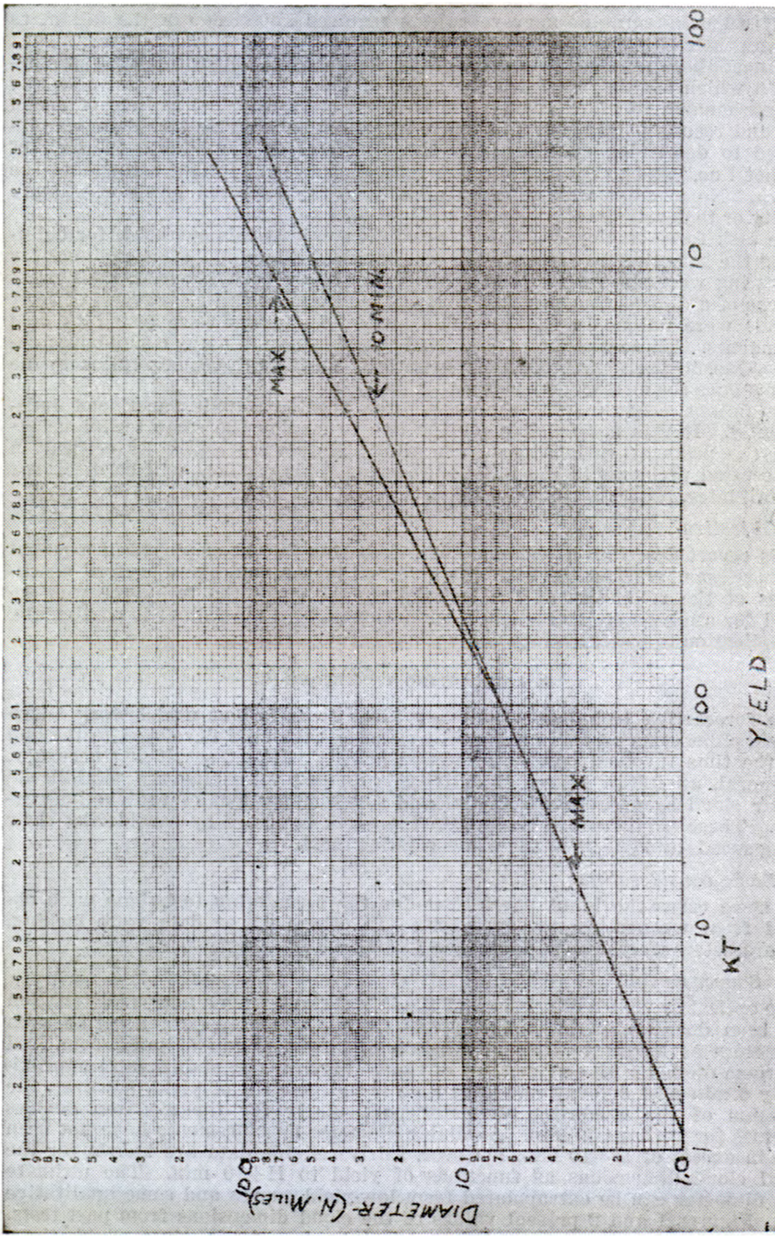

Figure 1.—Mushroom diameter as a function of yield.

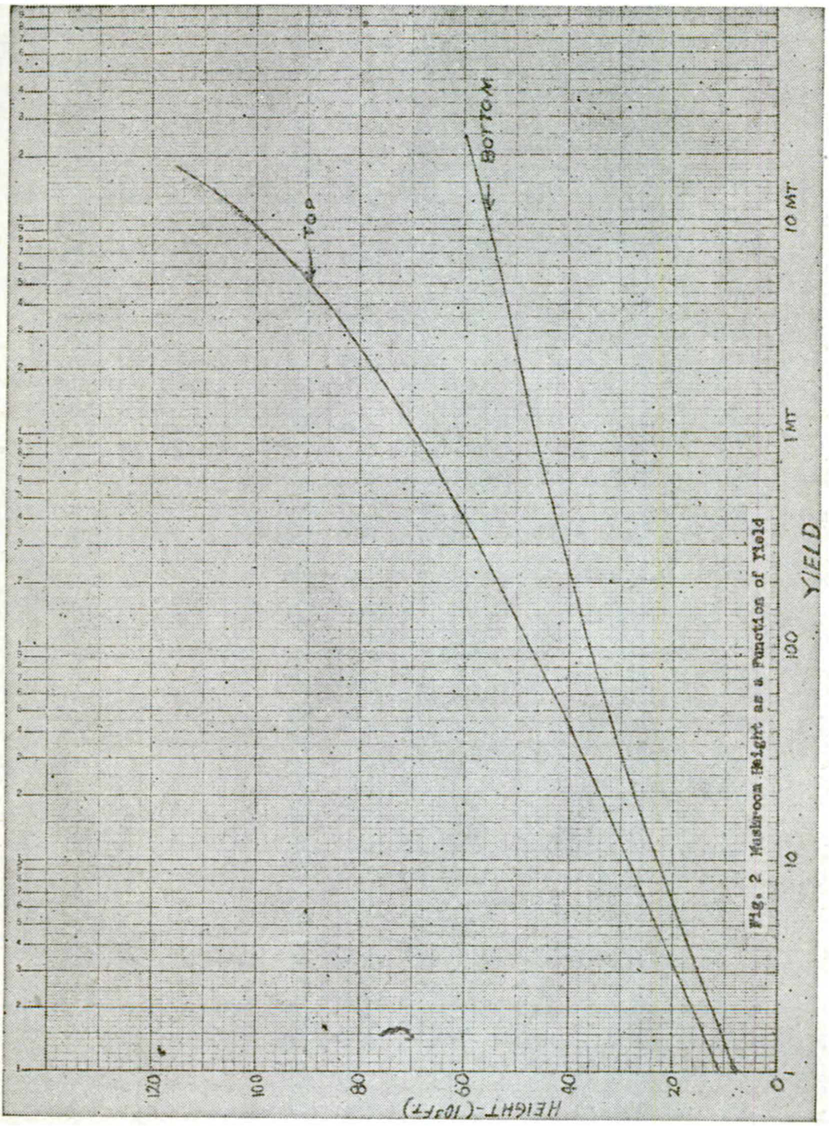

Fig. 2 Mushroom Height as a Function of Yield

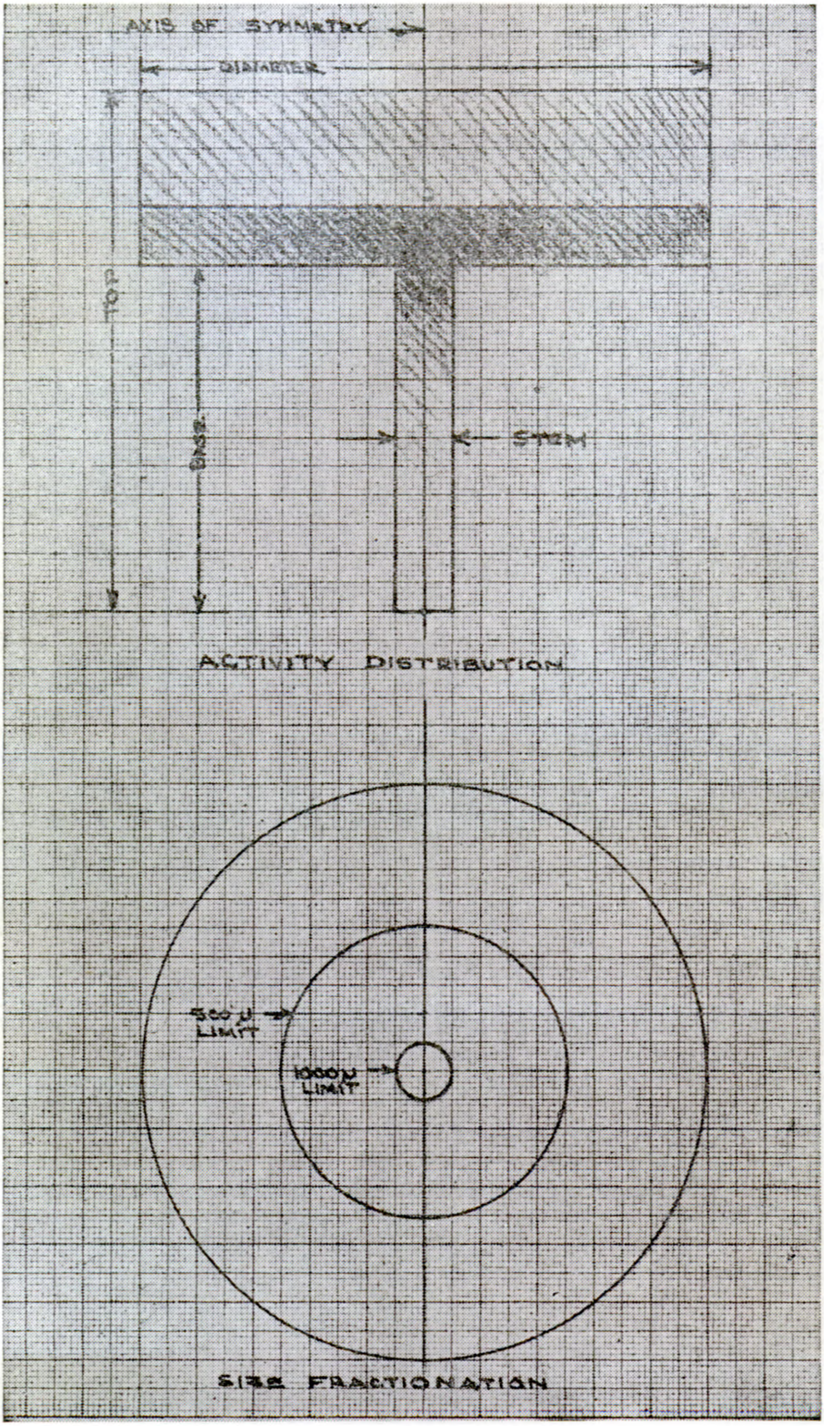

FIGURE 3.—Source model.

2.1.2 *Activity distribution in source model*

The great part of the activity was assumed to be concentrated in the lower third of the mushroom. The lower two-thirds of the stem was ignored; the remainder of the stem and upper two-thirds of the cloud were weighted lightly. This description (fig. 3) of the activity distribution within the cloud appeared most reasonable in the light of available data and logical theoretical considerations. The activity was concentrated nearer the axis of symmetry of the cloud than at its outer edges.

2.1.3 *Particle size distribution in source model*

All particle sizes were assumed at all elevations within the cloud except the lower two-thirds of the stem. However, to obtain agreement with past fallout measurements and with the optical diameter of the mushroom, it was necessary to fractionate the particle size distribution radially within the cloud. Otherwise, the computed fallout area about ground zero would be too large. The fractionation was specified as follows: particles of 1,000 microns in diameter and larger were restricted to the inner 10 percent of the mushroom radius or approximately the stem radius; those from 500 to 1,000 microns in diameter were limited to the inner 50 percent of the cloud radius. Since the relation of activity to particle size is some function of the particle diameter this fractionation tends to concentrate the activity about the axis of symmetry of the cloud.

2.1.4 *Particle falling speeds or settling rates*

Computations of the terminal velocities of the particles were based on aerodynamic considerations for a still atmosphere having temperature and density distributions typical of the Marshall Islands atmosphere in the spring months.

Experimental data from past tests at Eniwetok Atoll indicated that the particles were irregular in shape and had a mean density of 2.36 g/cu cm.

It can be shown that particles falling at their terminal speed experience three types of flow in a fluid: streamline or laminar flow where viscous forces predominate, ($10^{-4} \leq R_e \leq 2.0$); intermediate flow where inertia forces predominate, ($2 \leq R_e \leq 500$); turbulent flow where inertia forces predominate, ($500 \leq R_e \leq 10^5$). Below a Reynolds number of 10^{-4} certain corrections must be applied to the equations because the particle diameter approaches the mean free path of the fluid medium; the region above a Reynolds number of 10^5 is important only in ballistics. These limiting cases will not be discussed here.

The parameters actively affecting a particle's falling speed are: its weight, its drag coefficient, its density, as well as the fluid density and fluid viscosity.

Most empirical equations developed in past experimental work have been for spheres dropped in various liquids. Some work has been done on irregular shaped particles and some done in wind tunnels. The equations[1] used to determine the falling rates for particles in a fluid medium follow.

For Streamline motion, $10^{-4} \leq R_e \leq 2.0$

$$V_s = K_s \left(\frac{\rho - \rho_o}{\rho_o}\right) (d^2) \left(\frac{\mu}{\rho_o}\right)^{-1} \qquad (1)^2$$

where V_s = terminal velocity in cm/sec
ρ = particle density in gms/cm^3
ρ_o = fluid density in gms/cm^3
d = particle diameter in cm
μ = absolute viscosity of fluid in poises
K_s = constant incorporating gravity
= 54.5 for spheres
= 36.0 for irregular shaped particles.

The limiting diameter to which Eq. 1 holds is

$$d' = \left(\frac{36\mu^2}{g\rho_o(\rho - \rho_o)}\right)^{1/3}$$

for spheres and

$$d' = \left(\frac{54.4\mu^2}{g\rho_o(\rho - \rho_o)}\right)^{1/3}$$

for irregular shaped particles.

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[1] J. M. Dallavalle, Mircomeritics, Pittman Publishing Corp., 1948.

For Intermediate motion, $2.0 \leqq R_e \leqq 500$

$$V_I = K_I \left(\frac{\rho - \rho_o}{\rho_o}\right)^{2/3} \left(\frac{\mu}{\rho_o}\right)^{-1/3} d_o \qquad (2)^{2}$$

where $d_o = d - \xi d'$
$\xi = 0.4$ for spheres
$\xi = 0.279$ for irregular shapes
d' = limiting diameter to which streamline motion applies
$K_I = 30.0$ for spheres
$= 19.0$ for irregular shapes.

The limiting diameter to which the Eq. 2 holds is

$$d'' = 43.5 \left(\frac{\mu^2}{g\rho_o(\rho - \rho_o)}\right)^{1/3}$$

for spheres

$$d'' = 51 \left(\frac{\mu^2}{g\rho_o(\rho - \rho_o)}\right)^{1/3}$$

for irregular shapes.

For Turbulent motion, $500 \leqq R_e \leqq 10^5$

$$V_T = K_T \left[\left(\frac{\rho - \rho_o}{\rho_o}\right) d\right]^{1/2} \qquad (3)^{2}$$

$K_T = 54.6$ for spheres
$= 50.0$ for irregular particles.

The question of particle diameter becomes puzzling when the equations are applied to irregular shaped particles. Although the equations for irregular shaped particles cannot be applied to an individual particle, they are assumed valid in establishing the average falling rates of many irregular particles clustered about this defined size.

2.1.5 Marshall Islands atmosphere

Marshall Islands atmospheric conditions determined the values for the density and viscosity parameters used in computing particle falling rates. Available data on the temperature, pressure, density, and viscosity as functions of altitude for the atmosphere common to the Marshall Island area in the spring months follow.

It was not possible to use a "standard atmosphere" in this problem because such use introduced a large error in the particle falling rate at high altitudes. This error originates primarily because of the assumed isothermal layer above the tropopause.

2.1.5.1 Temperature distribution

From the weather data published by Task Force Weather Central at Operation Castle, four published radiosonde runs obtained temperature measurements to high altitudes:

March 1, 1954, 0600 M Bikini
March 27, 1954, 0600 M Bikini
April 7, 1954, 0620 M Bikini
April 26, 1954, 0610 M Bikini

No data were available above 67,000 feet. Fortunately two of these runs penetrated the tropopause which was located at approximately 55,000 feet. To extend the measured data beyond 67,000 feet climatological averages [3] for latitude 12° North were employed. Agreement with measured data was satisfactory except for the range from 50,000 to 65,000 feet where the climatological data indicated a well-defined isothermal layer. The most significant finding from the measured data was the complete lack of an isothermal layer above the tropopause. Instead, a distinct and rapid inversion was observed which when extrapolated as a straight line agreed with the climatological data above 70,000 feet. Since the atmosphere was to be defined to 120,000 feet further extrapolation was necessary. The only temperature data available at these higher altitudes were taken by rockets [4] over White Sands, N. Mex. A plot of 3 points from the rocket data justifies to some extent a continued extrapolation of the curve to 120,000 feet.

[2] These equations were taken from reference 1; however, certain constants have been reevaluated.
[3] Brunt, David, Physical and Dynamical Meteorology, the University Press, 1941.
[4] Chief of Naval Operations, A Study of the Atmosphere Between 30,000 and 100,000 Feet (preliminary report), September 1948.

Therefore the profile of the vertical temperature gradient (fig. 4) was based on measured data to 67,000 feet and extrapolated to 120,000 feet on the basis of supporting climatogical data and temperature measurements made at high altitudes with rockets.

2.1.5.2 *Pressure distribution*

Published high altitude measurements of the pressure distribution were obtained on two occasions at Operation Castle. These measurements [5] were made at Bikini on April 7 and 26, 1954, and were not taken above 65,000 feet. Above this altitude the pressure was extrapolated as a straight line on semilog paper to 120,000 feet. Agreement with published rocket data from White Sands, N. Mex., was good to 90,000 feet (fig. 5).

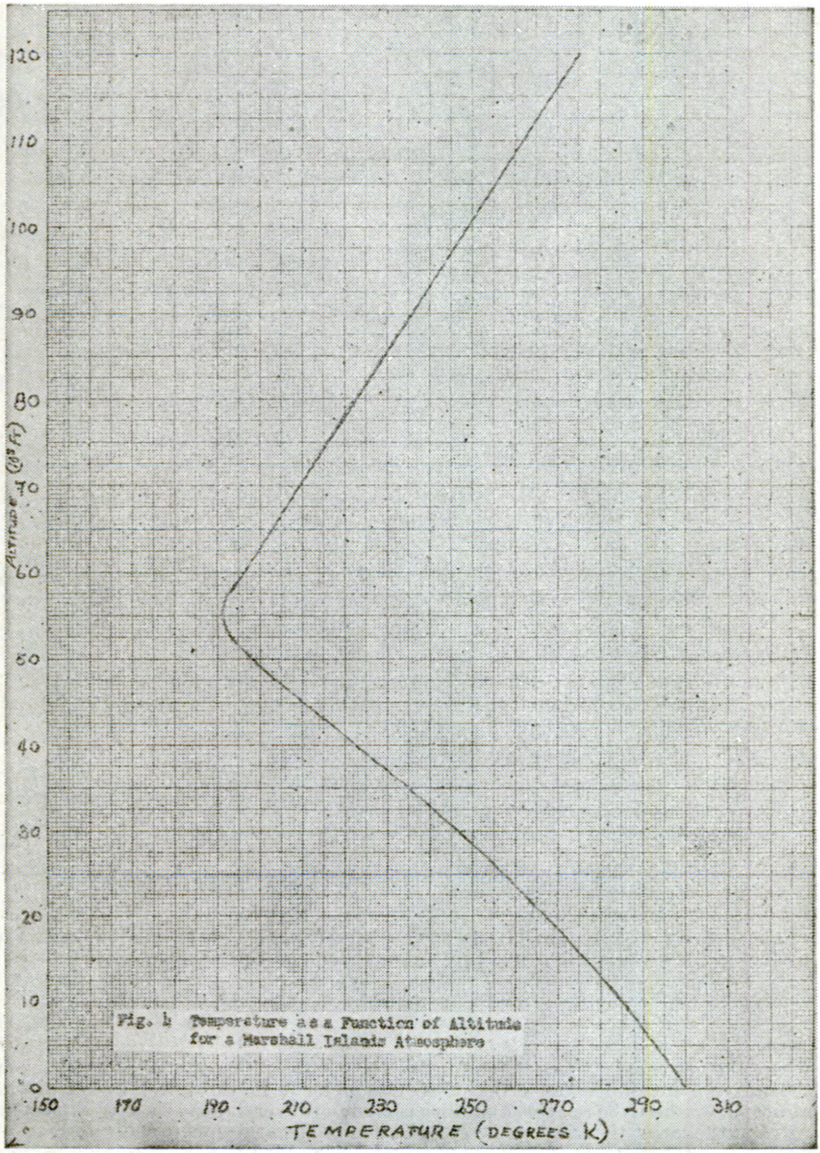


Fig. 4 Temperature as a Function of Altitude for a Marshall Islands Atmosphere

[5] Hq. T. U.–13 operation memo No. 14, April 30, 1954.

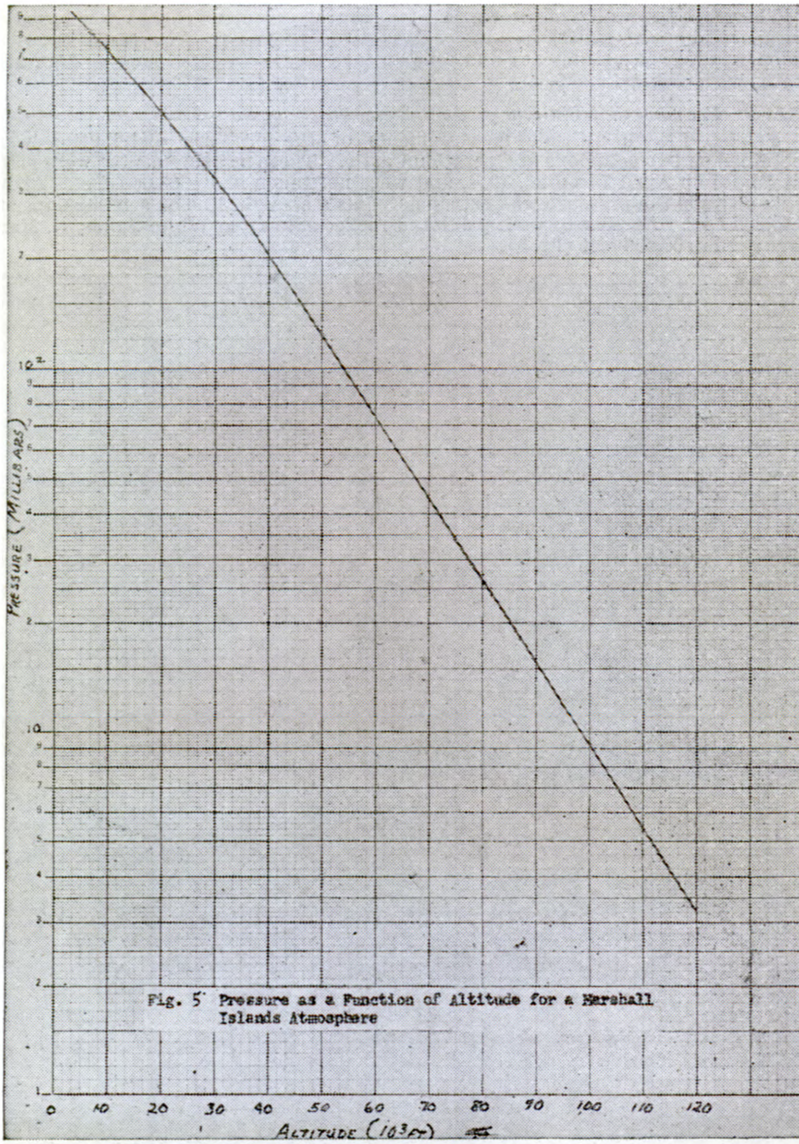


Fig. 5 Pressure as a Function of Altitude for a Marshall Islands Atmosphere

2.1.5.3 Density distribution

The density distribution of the atmosphere (fig. 6) was calculated from the perfect gas law using the above pressure and temperature distributions,

$$\rho = \frac{P}{RT}$$

where the gas constant was taken for dry air. This assumption of no moisture in the mixture introduces an error of several percent in the lower layers of the atmosphere where the relative humidity is high; however, it can be safely neglected. As well, the latest theories on the composition of the atmosphere indicate it to be constant to altitudes above 150,000 feet which justified the assumption of a non-varying gas constant.

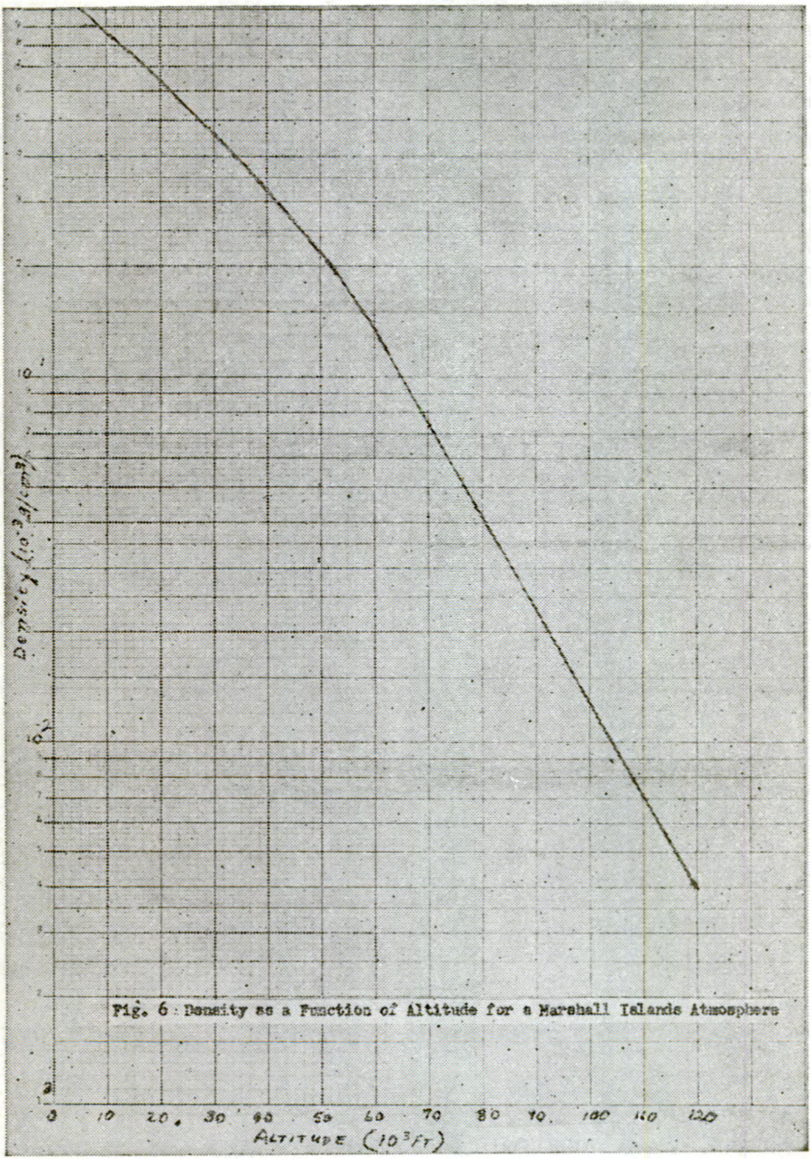


Fig. 6 Density as a Function of Altitude for a Marshall Islands Atmosphere

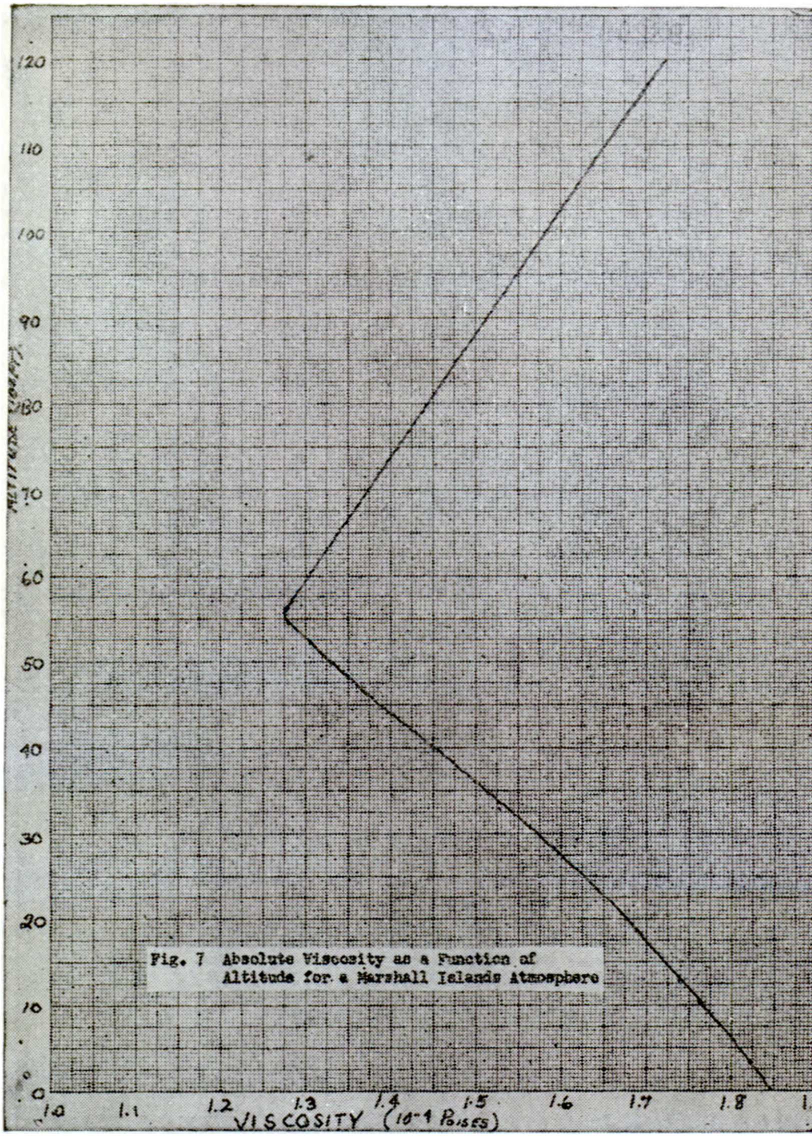


Fig. 7 Absolute Viscosity as a Function of Altitude for a Marshall Islands Atmosphere

2.1.5.4 *Viscosity distribution*

The variation of absolute viscosity with altitude was computed from the observed temperature distribution using Sutherland's formula,[6]

$$\mu = u_o \left(\frac{T_0+114}{T+114}\right)\left(\frac{T}{T_o}\right)^{3/2}$$

$$u = 0.01709 \left(\frac{387.17}{t_i+114}\right)\left(\frac{t_i}{273.17}\right)^{3/2}$$

where t_i equals temperature in degrees Kelvin and μ is viscosity in centipoises. These data are plotted in figure 7.

The data on pressure, temperature, density, and viscosity in 1,000-foot intervals to 120,000 feet are summarized in table 1.[7]

TABLE 1.—*Table of temperature, pressure, density, and viscoscity of the atmosphere over the Marshall Islands during the spring*

Altitude (feet)	Temperature °K	Pressure (Mb)	Density ($g/cm^3 \cdot 10^3$)	Viscosity ($poises \cdot 10^4$)
SFC	300	1,006	1.17	1.84
1,000	299	980	1.13	1.83
2,000	297	950	1.10	1.825
3,000	296	930	1.06	1.815
4,000	295	909	1.03	1.810
5,000	293	870	1.0	1.805
6,000	292	850	.97	1.795
7,000	290	820	.94	1.785
8,000	289	800	.91	1.780
9,000	288	770	.88	1.770
10,000	285	740	.86	1.765
11,000	284	720	.83	1.775
12,000	282	690	.80	1.745
13,000	280	660	.78	1.740
14,000	278	640	.76	1.730
15,000	276	620	.73	1.720
16,000	274	590	.71	1.715
17,000	273	570	.69	1.705
18,000	271	550	.67	1.695
19,000	269	530	.65	1.685
20,000	267	500	.63	1.675
21,000	265	480	.61	1.665
22,000	263	460	.59	1.655
23,000	261	440	.57	1.645
24,000	259	420	.55	1.635
25,000	257	410	.53	1.625
26,000	255	390	.52	1.615
27,000	252	370	.50	1.600
28,000	250	355	.49	1.590
29,000	248	340	.47	1.580
30,000	246	320	.45	1.570
31,000	243	310	.43	1.560
32,000	241	300	.42	1.545
33,000	239	280	.41	1.535
34,000	236	270	.39	1.525
35,000	234	260	.38	1.510
36,000	232	245	.37	1.500
37,000	230	235	.36	1.490
38,000	227	225	.35	1.475
39,000	225	215	.33	1.465
40,000	223	205	.32	1.450
41,000	220	195	.31	1.440
42,000	218	185	.30	1.430
43,000	215	175	.29	1.420
44,000	213	165	.28	1.405
45,000	211	160	.27	1.395
46,000	209	150	.26	1.380
47,000	206	145	.25	1.370
48,000	204	135	.24	1.355
49,000	201	130	.23	1.345
50,000	199	125	.22	1.335
51,000	196	115	.21	1.320
52,000	194	110	.20	1.310
53,000	193	105	.19	1.295
54,000	192	100	.18	1.285

[6] Smithsonian Physical Tables, 1954.

[7] A great deal of excellent upper air data for the Marshall Islands was obtained at Operation Redwing in 1956. Reduction of these data will result in a much better description of the Marshall Islands atmosphere than has been previously available.

TABLE 1.—*Table of temperature, pressure, density, and viscosity of the atmosphere over the Marshall Islands during the spring*—Continued

Altitude (feet)	Temperature °K	Pressure (Mb)	Density ($g/cm^3 \cdot 10^3$)	Viscosity (poises·10^4)
55,000	191	95	.17	1.275
56,000	191	90	.16	1.275
57,000	192	85	.155	1.280
58,000	193	80	.145	1.290
59,000	194	77	.14	1.295
60,000	195	73	.135	1.300
61,000	197	70	.125	1.310
62,000	198	66	.115	1.320
63,000	199	63	.110	1.325
64,000	201	60	.105	1.330
65,000	202	56	.10	1.340
66,000	203	53	.094	1.345
67,000	205	50	.088	1.350
68,000	206	48	.083	1.360
69,000	207	46	.078	1.365
70,000	208	43	.073	1.370
71,000	210	41	.070	1.380
72,000	211	39	.066	1.385
73,000	213	37	.062	1.395
74,000	214	35	.058	1.400
75,000	215	33	.054	1.405
76,000	217	32	.052	1.415
77,000	218	30	.049	1.420
78,000	219	28	.046	1.430
79,000	221	27	.044	1.435
80,000	222	26	.042	1.440
81,000	223	24	.039	1.450
82,000	225	23	.037	1.455
83,000	226	22	.034	1.465
84,000	227	21	.032	1.470
85,000	229	20	.030	1.480
86,000	230	19	.029	1.485
87,000	231	18	.027	1.490
88,000	233	17	.026	1.500
89,000	234	16	.024	1.505
90,000	235	15	.023	1.510
91,000	237	14.5	.0215	1.520
92,000	238	14	.0205	1.525
93,000	239	13	.019	1.535
94,000	241	12.5	.018	1.540
95,000	242	12	.017	1.550
96,000	243	11	.016	1.555
97,000	245	10.5	.015	1.565
98,000	246	10	.014	1.570
99,000	247	9.5	.0135	1.575
100,000	249	9	.0130	1.585
101,000	250	8.5	.0102	1.590
102,000	251	8	.01015	1.600
103,000	253	7.6	.0105	1.605
104,000	254	7.4	.010	1.610
105,000	255	7.0	.0095	1.620
106,000	257	6.6	.0090	1.625
107,000	258	6.2	.0085	1.635
108,000	259	6.0	.0080	1.640
109,000	261	5.6	.0075	1.650
110,000	262	5.4	.0070	1.655
111,000	263	5.1	.0068	1.660
112,000	265	4.9	.0064	1.670
113,000	266	4.6	.0060	1.675
114,000	267	4.4	.0056	1.685
115,000	269	4.2	.0054	1.690
116,000	270	3.9	.0050	1.700
117,000	271	3.7	.0048	1.705
118,000	273	3.6	.0044	1.710
119,000	274	3.4	.0042	1.720
120,000	275	3.2	.0040	1.725

2.1.5.5 Terminal velocity computations

The average falling speed through 5,000-foot layers was computed for 4 particle sizes over an altitude range from 0 to 120,000 feet. In these computations all in-flight transition of the particles from streamline to intermediate flow had to be considered through use of the plot shown in figure 8.

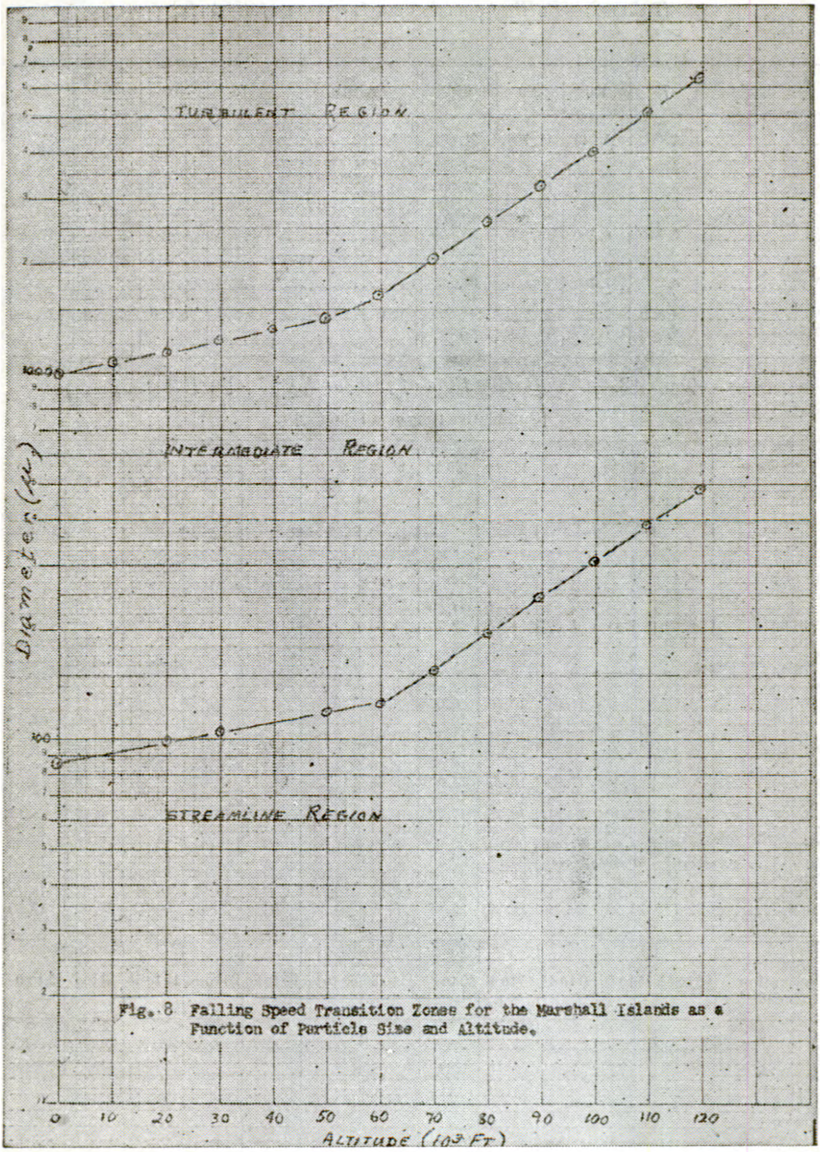


Fig. 8 Falling Speed Transition Zones for the Marshall Islands as a Function of Particle Size and Altitude.

Four particle sizes (75 μ, 100 μ, 200 μ, and 350 μ diameter) were employed since there was evidence from past tests that the 75 μ particle defined the limiting distance of fallout of interest and the larger sizes best described the pattern wihin this limit. Table 2 presents the falling speeds computed for the 4 sizes. Tables 3, 4, 5, and 6 display the cumulative time of fall from a given altitude for these particle diameters.

TABLE 2.—*Falling speeds as a function of altitude*

[Falling speeds (foot-hour)]

Altitude	75	100	200	350	Altitude	75	100	200	350
0	3,060	5,040	11,700	21,600	65	4,190	7,480	26,100	51,100
5	3,120	5,240	12,300	22,900	70	4,110	7,320	27,600	55,200
10	3,200	5,480	12,900	24,100	75	4,010	7,150	28,100	59,700
15	3,270	5,750	13,700	25,500	80	3,910	6,960	27,800	61,900
20	3,360	5,980	14,400	27,100	85	3,800	6,770	27,100	67,800
25	3,470	6,160	15,300	28,800	90	3,720	6,640	26,500	71,300
30	3,570	6,380	16,300	30,800	95	3,620	6,470	25,800	77,300
35	3,720	6,640	17,500	33,000	100	3,550	6,340	25,300	80,200
40	3,870	6,910	18,600	35,300	105	3,470	6,180	24,800	75,800
45	4,040	7,200	19,800	37,800	110	3,400	6,050	24,000	74,200
50	4,210	7,520	21,400	40,600	115	3,330	5,930	23,700	72,600
55	4,420	7,860	23,200	44,600	120	3,260	5,800	23,400	71,100
60	4,200	7,700	24,400	47,200					

TABLE 3.—*Cumulative time of fall for the 75-μ particles*

[Cumulative time of fall (hours)]

Starting elevation feet 10^{-3}	120 to 115	115 to 110	110 to 105	105 to 100	100 to 95	95 to 90	90 to 85	85 to 80	80 to 75	75 to 70	70 to 65	65 to 60
120 to 115	1.52											
115 to 110	3.01	1.49										
110 to 105	4.46	2.94	1.45									
105 to 100	5.88	4.36	2.87	1.42								
100 to 95	7.27	5.75	4.26	2.81	1.39							
95 to 90	8.63	7.11	5.62	4.17	2.75	1.36						
90 to 85	9.96	8.44	6.95	5.50	4.08	2.69	1.33					
85 to 80	11.26	9.74	8.25	6.80	5.38	3.99	2.63	1.30				
80 to 75	12.52	11.00	9.51	8.06	6.64	5.25	3.89	2.56	1.26			
75 to 70	13.75	12.23	10.74	9.29	7.87	6.48	5.12	3.79	2.49	1.23		
70 to 65	14.95	13.43	11.94	10.49	9.07	7.68	6.32	4.99	3.69	2.43	1.20	
65 to 60	16.14	14.62	13.13	11.68	10.26	8.87	7.51	6.18	4.88	3.62	2.39	1.19
60 to 55	17.30	15.78	14.29	12.84	11.42	10.03	8.67	7.34	6.04	4.78	3.55	2.35
55 to 50	18.46	16.94	15.45	14.00	12.58	11.19	9.83	8.50	7.20	5.94	4.71	3.51
50 to 45	19.67	18.15	16.66	15.21	13.79	12.40	11.04	9.71	8.41	7.15	5.92	4.72
45 to 40	20.93	19.41	17.92	16.47	15.05	13.66	12.30	10.97	9.67	8.41	7.18	5.98
40 to 35	22.25	20.73	19.24	17.79	16.37	14.98	13.62	12.29	10.99	9.73	8.50	7.30
35 to 30	23.62	22.10	20.61	19.16	17.74	16.35	14.99	13.66	12.36	11.10	9.87	8.67
30 to 25	25.04	23.52	22.03	20.58	19.16	17.77	16.41	15.08	13.78	12.52	11.29	10.09
25 to 20	26.50	24.98	23.49	22.04	20.62	19.23	17.87	16.54	15.24	13.98	12.75	11.55
20 to 15	28.01	26.49	25.00	23.55	22.13	20.74	19.38	18.05	16.75	15.49	14.26	13.06
15 to 10	29.55	28.03	26.54	25.09	23.67	22.28	20.92	19.59	18.29	17.03	15.80	14.60
10 to 5	31.13	29.61	28.12	26.67	25.25	23.86	22.50	21.17	19.87	18.61	17.38	16.18
5 to 0	32.75	31.23	29.74	28.29	26.87	25.48	24.12	22.79	21.49	20.23	19.00	17.80

Starting elevation feet 10^{-3}	60 to 55	55 to 50	50 to 45	45 to 40	40 to 35	35 to 30	30 to 25	25 to 20	20 to 15	15 to 10	10 to 5	5 to 0
120 to 115												
115 to 110												
110 to 105												
105 to 100												
100 to 95												
95 to 90												
90 to 85												
85 to 80												
80 to 75												
75 to 70												
70 to 65												
65 to 60												
60 to 55	1.16											
55 to 50	2.32	1.16										
50 to 45	3.53	2.37	1.21									
45 to 40	4.79	3.63	2.47	1.26								
40 to 35	6.11	4.95	3.79	2.58	1.32							
35 to 30	7.48	6.32	5.16	3.95	2.69	1.37						
30 to 25	8.90	7.74	6.58	5.37	4.11	2.79	1.42					
25 to 20	10.36	9.20	8.04	6.83	5.57	4.25	2.88	1.46				
20 to 15	11.87	10.71	9.55	8.34	7.08	5.76	4.39	2.97	1.51			
15 to 10	13.41	12.25	11.09	9.88	8.62	7.30	5.93	4.51	3.05	1.54		
10 to 5	14.99	13.83	12.67	11.46	10.20	8.88	7.51	6.09	4.63	3.12	1.58	
5 to 0	16.61	15.45	14.29	13.08	11.82	10.52	9.13	7.71	6.25	4.74	3.20	1.62

Table 4.—*Cumulative time of fall for the 100-μ particles*

[Cumulative time of fall (hour)]

Starting elevation feet 10^{-3}	120 to 115	115 to 110	110 to 105	105 to 100	100 to 95	95 to 90	90 to 85	85 to 80	80 to 75	75 to 70	70 to 65	65 to 60
120 to 115	0.85											
115 to 110	1.68	0.83										
110 to 105	2.50	1.65	0.82									
105 to 100	3.30	2.45	1.62	0.80								
100 to 95	4.08	3.23	2.40	1.58	0.78							
95 to 90	4.84	3.99	3.16	2.34	1.54	0.76						
90 to 85	5.58	4.73	3.90	3.08	2.28	1.50	0.74					
85 to 80	6.30	5.46	4.63	3.81	3.01	2.23	1.47	0.73				
80 to 75	7.02	6.17	5.34	4.52	3.72	2.94	2.18	1.44	0.71			
75 to 70	7.71	6.86	6.03	5.21	4.41	3.63	2.87	2.13	1.40	0.69		
70 to 65	8.38	7.53	6.70	5.88	5.08	4.30	3.54	2.80	2.07	1.36	0.67	
65 to 60	9.04	8.19	7.36	6.54	5.74	4.96	4.20	3.46	2.73	2.02	1.33	0.66
60 to 55	9.68	8.83	8.00	7.18	6.38	5.60	4.84	4.10	3.37	2.66	1.97	1.30
55 to 50	10.33	9.48	8.65	7.83	7.03	6.25	5.49	4.75	4.02	3.31	2.62	1.95
50 to 45	11.01	10.16	9.33	8.51	7.71	6.93	6.17	5.43	4.70	3.99	3.30	2.63
45 to 40	11.72	10.87	10.04	9.22	8.42	7.64	6.88	6.14	5.41	4.70	4.01	3.34
40 to 35	12.46	11.61	10.78	9.96	9.16	8.38	7.62	6.88	6.15	5.44	4.75	4.08
35 to 30	13.24	12.39	11.56	10.74	9.94	9.16	8.40	7.66	6.93	6.22	5.53	4.86
30 to 25	14.03	13.18	12.35	11.53	10.73	9.95	9.19	8.45	7.72	7.01	6.32	5.65
25 to 20	14.85	14.00	13.17	12.35	11.55	10.77	10.01	9.27	8.54	7.83	7.14	6.47
20 to 15	15.70	14.85	14.02	13.20	12.40	11.62	10.86	10.12	9.39	8.68	7.99	7.32
15 to 10	16.59	15.74	14.91	14.09	13.29	12.51	11.75	11.01	10.28	9.57	8.88	8.21
10 to 5	17.52	16.67	15.84	15.02	14.22	13.44	12.68	11.94	11.21	10.50	9.81	9.14
5 to 0	18.49	17.64	16.81	15.99	15.19	14.41	13.65	12.91	12.18	11.47	10.78	10.11

Starting elevation feet 10^{-3}	60 to 55	55 to 50	50 to 45	45 to 40	40 to 35	35 to 30	30 to 25	25 to 20	20 to 15	15 to 10	10 to 5	5 to 0
120 to 115												
115 to 110												
110 to 105												
105 to 100												
100 to 95												
95 to 90												
90 to 85												
85 to 80												
80 to 75												
75 to 70												
70 to 65												
65 to 60												
60 to 55	0.64											
55 to 50	1.29	0.65										
50 to 45	1.97	1.33	0.68									
45 to 40	2.68	2.04	1.39	0.71								
40 to 35	3.42	2.78	2.13	1.45	0.74							
35 to 30	4.20	3.56	2.91	2.23	1.52	0.78						
30 to 25	4.99	4.35	3.70	3.02	2.31	1.57	0.79					
25 to 20	5.81	5.17	4.52	3.84	3.13	2.39	1.61	0.82				
20 to 15	6.66	6.02	5.37	4.69	3.98	3.14	2.46	1.67	0.85			
15 to 10	7.55	6.91	6.26	5.58	4.87	4.13	3.35	2.56	1.74	0.89		
10 to 5	8.48	7.84	7.19	6.51	5.80	5.06	4.28	3.49	2.67	1.82	0.93	
5 to 0	9.45	8.81	8.16	7.48	6.77	6.03	5.25	4.46	3.64	2.79	1.90	0.97

TABLE 5.—*Cumulative time of fall for 200-μ particles*

[Cumulative time of fall (hour)]

Starting elevation feet 10^{-3}	120 to 115	115 to 110	110 to 105	105 to 100	100 to 95	95 to 90	90 to 85	85 to 80	80 to 75	75 to 70	70 to 65	65 to 60
120 to 115	0.21											
115 to 110	.42	0.21										
110 to 105	.62	.41	0.20									
105 to 100	.82	.61	.40	0.20								
100 to 95	1.02	.81	.60	.40	.020							
95 to 90	1.21	1.00	.79	.59	.39	0.19						
90 to 85	1.40	1.19	.98	.78	.58	.38	0.19					
85 to 80	1.58	1.37	1.16	.96	.76	.56	.37	0.18				
80 to 75	1.76	1.55	1.34	1.14	.94	.74	.55	.36	0.18			
75 to 70	1.94	1.73	1.52	1.32	1.12	.92	.73	.54	.36	0.18		
70 to 65	2.13	1.92	1.71	1.51	1.31	1.11	.92	.73	.55	.37	0.19	
65 to 60	2.33	2.12	1.91	1.71	1.51	1.31	1.12	.93	.75	.57	.39	0.20
60 to 55	2.54	2.33	2.12	1.92	1.72	1.52	1.33	1.14	.96	.78	.60	.41
55 to 50	2.76	2.55	2.34	2.14	1.94	1.74	1.55	1.36	1.18	1.00	.82	.63
50 to 45	3.00	2.79	2.58	2.38	2.18	1.98	1.79	1.60	1.42	1.24	1.06	.87
45 to 40	3.26	3.05	2.84	2.64	2.44	2.24	2.05	1.86	1.68	1.50	1.32	1.13
40 to 35	3.54	3.33	3.12	2.92	2.72	2.52	2.33	2.14	1.96	1.78	1.60	1.41
35 to 30	3.84	3.63	3.42	3.22	3.02	2.82	2.63	2.44	2.26	2.08	1.90	1.71
30 to 25	4.16	3.95	3.74	3.54	3.34	3.14	2.95	2.76	2.58	2.40	2.22	2.03
25 to 20	4.50	4.29	4.08	3.88	3.68	3.48	3.29	3.10	2.92	2.74	2.56	2.37
20 to 15	4.86	4.65	4.44	4.24	4.04	3.84	3.65	3.46	3.28	3.10	2.92	2.73
15 to 10	5.24	5.03	4.82	4.62	4.42	4.22	4.03	3.84	3.66	3.48	3.30	3.11
10 to 5	5.64	5.43	5.22	5.02	4.82	4.62	4.43	4.24	4.06	3.88	3.70	3.51
5 to 0	6.06	5.85	5.64	5.44	5.24	5.04	4.85	4.66	4.48	4.30	4.12	3.93

Starting elevation feet 10^{-3}	60 to 55	55 to 50	50 to 45	45 to 40	40 to 35	35 to 30	30 to 25	25 to 20	20 to 15	15 to 10	10 to 5	5 to 0
120 to 115												
115 to 110												
110 to 105												
105 to 100												
100 to 95												
95 to 90												
90 to 85												
85 to 80												
80 to 75												
75 to 70												
70 to 65												
65 to 60												
60 to 55	0.21											
55 to 50	.43	0.22										
50 to 45	.67	.46	0.24									
45 to 40	.93	.72	.50	0.26								
40 to 35	1.21	1.00	.78	.54	0.28							
35 to 30	1.51	1.30	1.08	.84	.58	0.30						
30 to 25	1.83	1.62	1.40	1.16	.90	.62	0.32					
25 to 20	2.17	1.96	1.74	1.50	1.24	.96	.66	0.34				
20 to 15	2.53	2.32	2.10	1.86	1.60	1.32	1.02	.70	0.36			
15 to 10	2.91	2.70	2.48	2.24	1.98	1.70	1.40	1.08	.74	0.38		
10 to 5	3.31	3.10	2.88	2.64	2.38	2.10	1.80	1.48	1.14	.78	0.40	
5 to 0	3.73	3.52	3.30	3.06	2.80	2.52	2.22	1.90	1.56	1.20	.82	0.42

TABLE 6.—*Cumulative time of fall for 350-μ particles*

[Cumulative time of fall (hours)]

Starting elevation feet 10^{-3}	120 to 115	115 to 110	110 to 105	105 to 100	100 to 95	95 to 90	90 to 85	85 to 80	80 to 75	75 to 70	70 to 65	65 to 60
120 to 115	0.07											
115 to 110	.14	0.07										
110 to 105	.21	.14	0.07									
105 to 100	.27	.20	.13	0.06								
100 to 95	.33	.26	.19	.12	0.06							
95 to 90	.40	.33	.26	.19	.13	.07						
90 to 85	.47	.40	.33	.26	.20	.14	0.07					
85 to 80	.55	.48	.41	.34	.28	.22	.15	0.08				
80 to 75	.63	.56	.49	.42	.36	.30	.23	.16	0.08			
75 to 70	.72	.65	.58	.51	.45	.39	.32	.25	.17	0.09		
70 to 65	.81	.74	.67	.60	.54	.48	.41	.34	.26	.18	0.09	
65 to 60	.91	.84	.77	.70	.64	.58	.51	.44	.36	.28	.19	0.10
60 to 55	1.02	.95	.88	.81	.75	.69	.62	.55	.47	.39	.30	.21
55 to 50	1.14	1.07	1.00	.93	.87	.81	.74	.67	.59	.51	.42	.33
50 to 45	1.27	1.20	1.13	1.06	1.00	.94	.87	.80	.72	.64	.55	.46
45 to 40	1.41	1.34	1.27	1.20	1.14	1.08	1.01	.94	.86	.78	.69	.60
40 to 35	1.56	1.49	1.42	1.35	1.29	1.23	1.16	1.09	1.01	.93	.44	.75
35 to 30	1.72	1.65	1.58	1.51	1.45	1.39	1.32	1.25	1.17	1.09	1.00	.91
30 to 25	1.89	1.82	1.75	1.68	1.62	1.56	1.49	1.42	1.34	1.26	1.17	1.08
25 to 20	2.07	2.00	1.93	1.86	1.80	1.74	1.67	1.60	1.52	1.44	1.35	1.26
20 to 15	2.26	2.19	2.12	2.05	1.99	1.93	1.86	1.79	1.71	1.63	1.54	1.45
15 to 10	2.46	2.39	2.32	2.25	2.19	2.13	2.06	1.99	1.91	1.83	1.74	1.65
10 to 5	2.67	2.60	2.53	2.46	2.40	2.34	2.27	2.20	2.12	2.04	1.95	1.86
5 to 0	2.89	2.82	2.75	2.68	2.62	2.56	2.49	2.42	2.34	2.26	2.17	2.08

Starting elevation feet 10^{-3}	60 to 55	55 to 50	50 to 45	45 to 40	40 to 35	35 to 30	30 to 25	25 to 20	20 to 15	15 to 10	10 to 5	5 to 0
120 to 115												
115 to 110												
110 to 105												
105 to 100												
100 to 95												
95 to 90												
90 to 85												
85 to 80												
80 to 75												
75 to 70												
70 to 65												
65 to 60												
60 to 55	0.11											
55 to 50	.23	0.12										
50 to 45	.36	.25	0.13									
45 to 40	.50	.39	.27	0.14								
40 to 35	.65	.54	.42	.29	0.15							
35 to 30	.81	.70	.58	.45	.31	0.16						
30 to 25	.98	.87	.75	.62	.48	.33	0.17					
25 to 20	1.16	1.05	.93	.80	.66	.51	.35	0.18				
20 to 15	1.35	1.24	1.12	.99	.85	.70	.54	.37	0.19			
15 to 10	1.55	1.44	1.32	1.19	1.05	.90	.74	.57	.39	0.20		
10 to 5	1.76	1.65	1.53	1.40	1.26	1.11	.95	.78	.60	.41	0.21	
5 to 0	1.98	1.87	1.75	1.62	1.48	1.33	1.17	1.00	.82	.63	.43	0.22

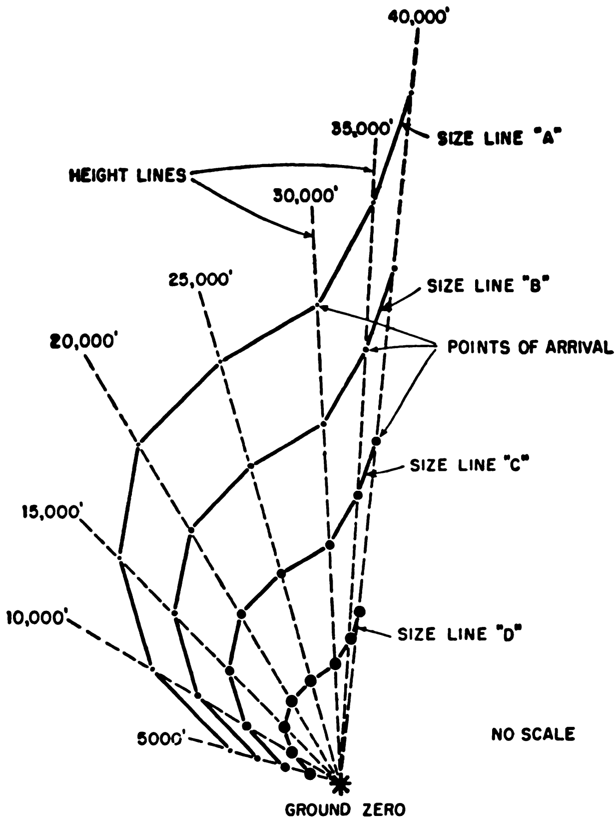
2.1.5.6 Meteorological procedures

It is necessary to have available the best possible description of the winds aloft to determine the arrival points of particles of various sizes originating at various altitudes. Such data are usually available from the normal upper air soundings routinely taken by Weather Bureau and military meteorological stations. Although wind velocity as a function of height varies continuously, it can be described by an average speed and direction in discrete layers. Such averaging can best be obtained from the WBAN–20 form where the original data are recorded. The technique employed in this report was to divide the atmosphere into layers 5,000 feet thick and determine an average speed and direction for each layer. When the average falling speed of particles through these 5,000-foot layers and the speed and direction of the wind are known, horizontal displacement can be computed. Thus for each particle size a vector may be drawn for the average particle displacement in a particular 5,000-foot layer. Addition of such vectors from all layers describes the trajectory projection of a particle of given size. Similar plotting for all particle sizes originating at all elevations within the cloud source will map the fallout on the earth's surface.

This technique is valid for any atmosphere that has negligible vertical motion and is in a steady state condition with respect to the horizontal winds during the time needed for the slowest particle to fall from the highest altitude to the ground. Such an assumption is not realistic for situations arising from many of the megaton devices because 15 to 20 hours are necessary to establish the fallout area. Consequently, when computing particle trajectories, an attempt should be made to consider how the wind varies with time, how it varies with distance from ground zero, what effect vertical motions have on particle falling speeds, and how they vary with space and time. Such considerations complicate computation of trajectories extremely. In most cases valid input data describing these variables are not available. This phase of the problem is discussed below.

2.2 Plotting technique

The use of "particle size" and "height" lines in mapping fallout is a standard technique employed by most analytical methods. This technique simply describes a grid (fig. 9) on the earth's surface indicating where fallout particles of certain sizes will arrive and from what altitude they came. These parameters are the basic data for describing the fallout pattern.



NOTE: Four particle sizes originating at altitudes up to 40,000 feet in a hypothetical wind field

FIGURE 9.—Basic Fallout Plot Showing Grid of Size Lines and Height Lines.

Assuming steady state meteorological conditions without vertical motion or space variation of the winds, it is very easy to construct a grid describing arrival points on the earth's surface for particles of various sizes originating at different altitudes. This grid is constructed by ignoring the horizontal distribution of particles in the cloud model and by plotting those trajectories that originate along the line source describing the vertical axis of the cloud.

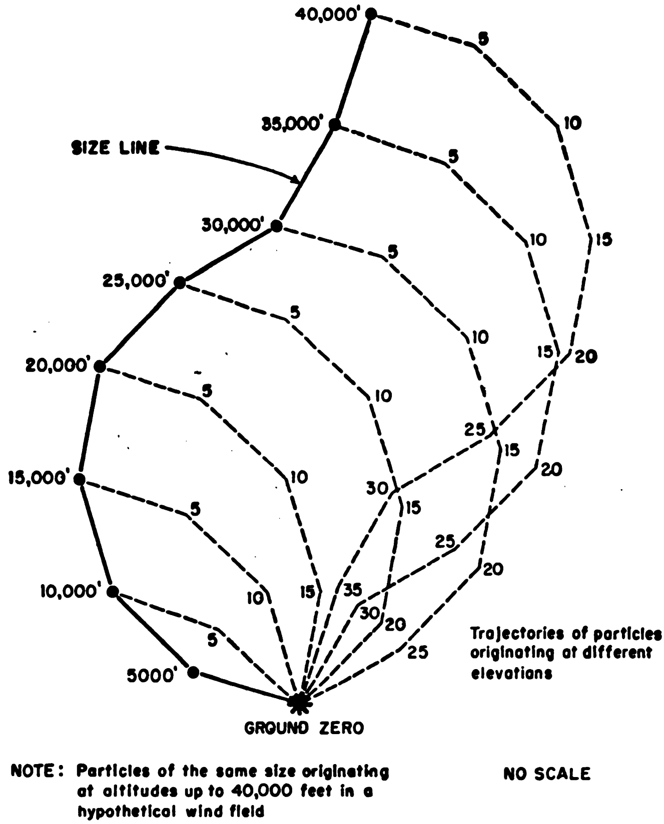


FIGURE 10.—Comparison of plotting techniques either by use of trajectories or by use of a size line.

Plotting trajectories for each particle size at every starting elevation is the first step in determining the resultant fallout pattern; however, the drafting involved is tedious and time consuming. This effort can be reduced greatly by plotting from the ground up, as is done in the construction of a wind hodograph. Such a plot is made by starting at ground zero and working up through the altitude increments to the desired elevation. Although this technique does not plot the trajectory of the particle, it does define the arrival points on the surface of the earth of particles starting at each altitude increment (Fig. 10). To plot these size lines one must make the preliminary computations of particle falling times through each altitude increment to obtain the displacement for various wind velocities as described earlier in section 2.1.5.5.

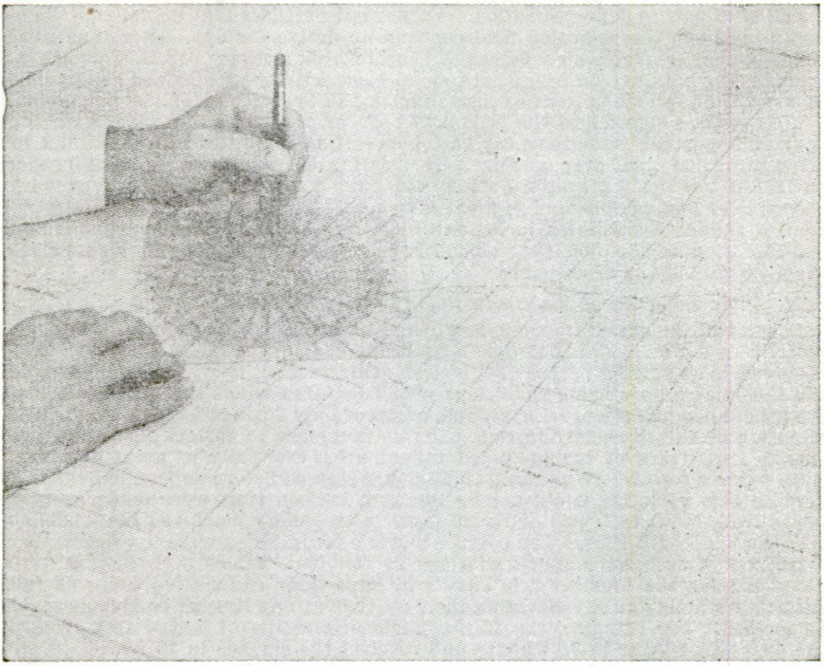


FIGURE 11.—Fallout plotting device.

A plotting device (fig. 11) has been described elsewhere[8] which facilitates the computations required for the size lines of the fallout pattern. Such devices were constructed for 4 particle sizes: 75, 100, 200, and 350 μ in diameter. With these plotters trajectories or size lines can be plotted from any elevation to 120,000 feet for the 4 particle sizes. The plotters automatically account for the variable particle falling speed. They also eliminate the need for drafting equipment. After establishing the particle arrival points by either the use of size lines or trajectories, height lines can be constructed. These lines joining surface zero with the arrival points of all particles from the same elevation are most descriptive for they define the path along which all particle sizes will deposit from that originating altitude.

The height lines describing the fallout from the lower portion of the mushroom immediately establish the "hot line." The "hot line" is best defined as that portion of the fallout area wherein the highest levels of activity are found relative to the adjacent areas. Under most meteorological conditions this area is described by a line from surface zero that coincides with the height lines from the altitude layers that include the base of the mushroom; for the source model was so defined to concentrate the activity in this volume.

Since the plotted grid of size lines and height lines was based on a line source of activity each particle point must be expanded to the appropriate cloud or stem diameter from which it originated. This expansion, after taking into consideration the radial particle size fractionation in the source model, defines the perimeter of the area. One then has a map indicating the fallout area and the path of expected highest activity.

Curves of time of arrival of fallout through the pattern are established by simply assigning the appropriate value of falling time to each expanded circle about the arrival points and by constructing from this network of values isotime contours that indicate the earliest time at which fallout will arrive at a given distance from the shot point. The determination of the time of cessation of fallout at any location may be plotted similarly, however, one is faced with the

[8] E. A. Schuert, A Fallout Plotting Device, USNRDL Technical Rept. 127, February 1957.

question of how to define cessation. Very small particles that do not contribute significantly to the radiation field continue to arrive for days after time zero. Consequently, a plot which describes time to peak activity seems more meaningful. During the field operation time to peak activity was defined as the time of arrival of fallout particles originating in the lower third of the mushroom.

This method determines the fallout plot under conditions that do not involve several important meteorological variables. It is most valid for a fallout of short duration and over a relatively small area, for example, a 1-kiloton surface detonation. Megaton devices and large kiloton yields deposit primary fallout over long periods and to great distances. To map such extensive deposition of fallout necessitates inclusion of complex meteorological variables and consideration of the fact that clouds from these large detonations extend to great heights in the atmosphere.

2.2.1 Time variation of the winds aloft

In most of the observations made at the Eniwetok Proving Ground, the winds aloft were not in a steady state. Significant changes in the winds aloft were observed in as short a period as 3 hours. This variability was probably due to the fact that proper firing conditions which required winds that would deposit the fallout north of the proving ground, occurred only during an unstable synoptic situation of rather short duration. It was necessary to correct for this variation to keep track of the predicted fallout area, especially at great distances from surface zero where as much as 20 hours elapsed before deposition.

Since this variation could not be forecast, balloon runs were made every 3 hours from H+O to H+24 and each particle trajectory employed the winds as they changed with time. The correct particle trajectories were approached by a method of successive approximations as follows: Tables 3 through 6 were computed for the four particle sizes and gave their cumulative times of fall such that starting at any elevation their altitude at any time after H-hour could be located. For example, the 75-μ particle originating at 70,000 feet entered the 40,000-foot layer in 7.18 hours and reached the surface in 19 hours. Since new upper air observations were obtained every 3 hours it was assumed that the balloon released at H+O represented the winds aloft until H+3 hours and the balloon released at H+3 hours represented the winds until H+6 hours and so on. Therefore, as the particle settled to earth the appropriate winds aloft were applied to it.

The first step was to plot size lines for the particles based on the H+O hour winds. This established a fallout plot that assumed the winds would not change with time. When the H+3 winds became available a similar plot was made based on them. With the aid of tables 3 through 6 the particles starting at various elevations were located in altitude at H+3. These H+3 hour points are marked at the proper altitude on each size line. The two size lines, H+O and H+3 are then overlayed such that the H+3 hour points are conicident and the combined size lines determined with the aid of a light table. This is done by taking the upper portion of the H+O hour size line and the lower portion of the H+3 size line. This first approximation then assumes that the H+3 hour winds will remain steady for the remainder of the particles flight. The process is repeated using the combined size line and the new size line for the next set of wind data until the particle reaches the surface. Therefore for each new wind observation a closer approximation of the corrected time variable plot is made until ultimately the plot is quantitative.

2.2.2 Space variation

The preceding computations assumed that the winds aloft as measured at the point of detonation at a given time are the same throughout the area for that time. Since the fallout can deposit hundreds of miles from surface zero, ideally, one would like winds-aloft measurements throughout the volume traversed by the particles. Correction for space variation of the winds is then necessary, however, in most cases not as significant as is time variation. Most weather networks are not refined enough to allow quantitative correction for these errors.

2.2.3 Vertical motions

In applying particle falling speeds to the forecasting technique, it is assumed that the atmosphere has no vertical velocity. Computations made at the Eniwetok Proving Ground[*] to 50,000 feet indicated that large cellular vertical

[*] Under the direction of Comdr. Daniel F. Rex, Joint Task Force Seven Meteorological Center, Pearl Harbor, T. H.

motions in the atmosphere sometimes attained speeds equal to and greater than the settling speed of a 75-μ particle. A time-space correction should be made to the falling speeds of the particles to compensate for this parameter. However, in the work at the test site it was not possible to include this effect in the fallout forecasts. Certain anomalies discussed below may be due to such an effect and postshot analysis is being conducted to see whether they are resolved when the vertical motions have been taken into account.

3. DISCUSSION OF FIELD TEST RESULTS

The forecasting technique described was employed by the fallout program at the Eniwetok Proving Ground to satisfy certain project requirements. One project had three ships equipped to collect fallout and their positions had to be determined for most efficient collection; another sampled the ocean for fallout; while another made an aerial survey of the contaminated area. The navigational schedules for these latter projects were based on the forecast fallout pattern. Operations were controlled through the program control center aboard the task force command ship where the forecasts were prepared.

The meteorological data was received from the weather ship at Bikini Atoll as well as from weather stations at Rongerik Atoll and Eniwetok Atoll. Furthermore all forecasts made by the task force weather central at Eniwetok Atoll were usually available aboard the command ship by facsimile through the ships weather station.

Upper air measurements were made at Bikini, Rongerik, and Eniwetok Atolls every 3 hours starting at H—24 hour and continuing until H+24 hour for any given detonation. The frequency of observations was usually increased during the period from H—6 to H—2 hours. The altitudes reached on the wind runs were remarkably high and gave perhaps the best set of winds-aloft measurements to date. The average termination altitude was approximately 90,000 feet with many runs over 100,000 feet. Such excellent coverage of the winds aloft was a major help in the fallout forecasting.

Fallout forecasts were made every 3 hours starting at H—24 hour using the *measured* winds available at the time. This process was continued up to shot time and from then on the technique of correcting for time variation was employed every 3 hours until the fallout event was completed. It was not feasible to correct for space variation and vertical motions during this period because of lack of time and data.

3.1 Fallout plots

The fallout forecasts determined at the weapons-test operation were based entirely on measured data and quantitatively considered time variation of the wind. No space variation corrections or computed values of vertical motions were employed in their construction.

The area of measured fallout from shot A is compared with the forecast fallout plot in figure 12. Figures 13, 14, and 15 are similar comparisons for shots B, C, and D. Although C and D were water-surface shots, it is evident that the forecasting technique succeeded in representing the measured fallout area as well as it did for the land-surface detonations, A and B.

The comparison is excellent for all shots except B and as yet the discrepancy betwen the forecast fallout area and that which was measured is unknown. There is some indication that consideration of vertical motions will have to be made for shot B during the time of fallout since computed vertical motions were significant in magnitude. Such analysis including space variation is being carried out at this time for all four detonations and the refined data will be published later.

4. SUMMARY

The fallout forecasting technique described in this report was successfully employed for both land surface and water surface detonations at the Eniwetok Proving Ground. With known meterological data such a technique will successfully qualify the area of fallout and indicate qualitatively the relative intensity of radiation.

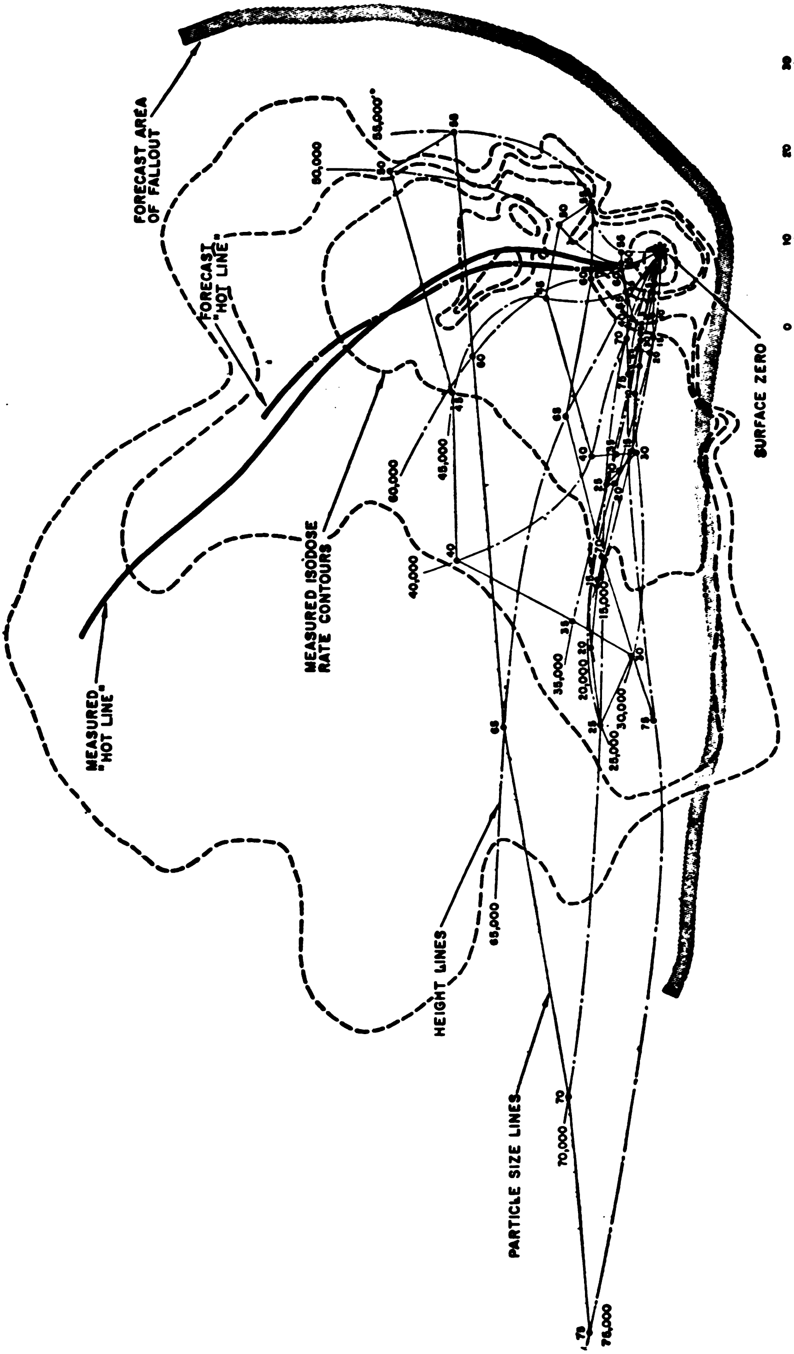


FIGURE 12.—*Comparison of fallout forecast with test results—shot A*

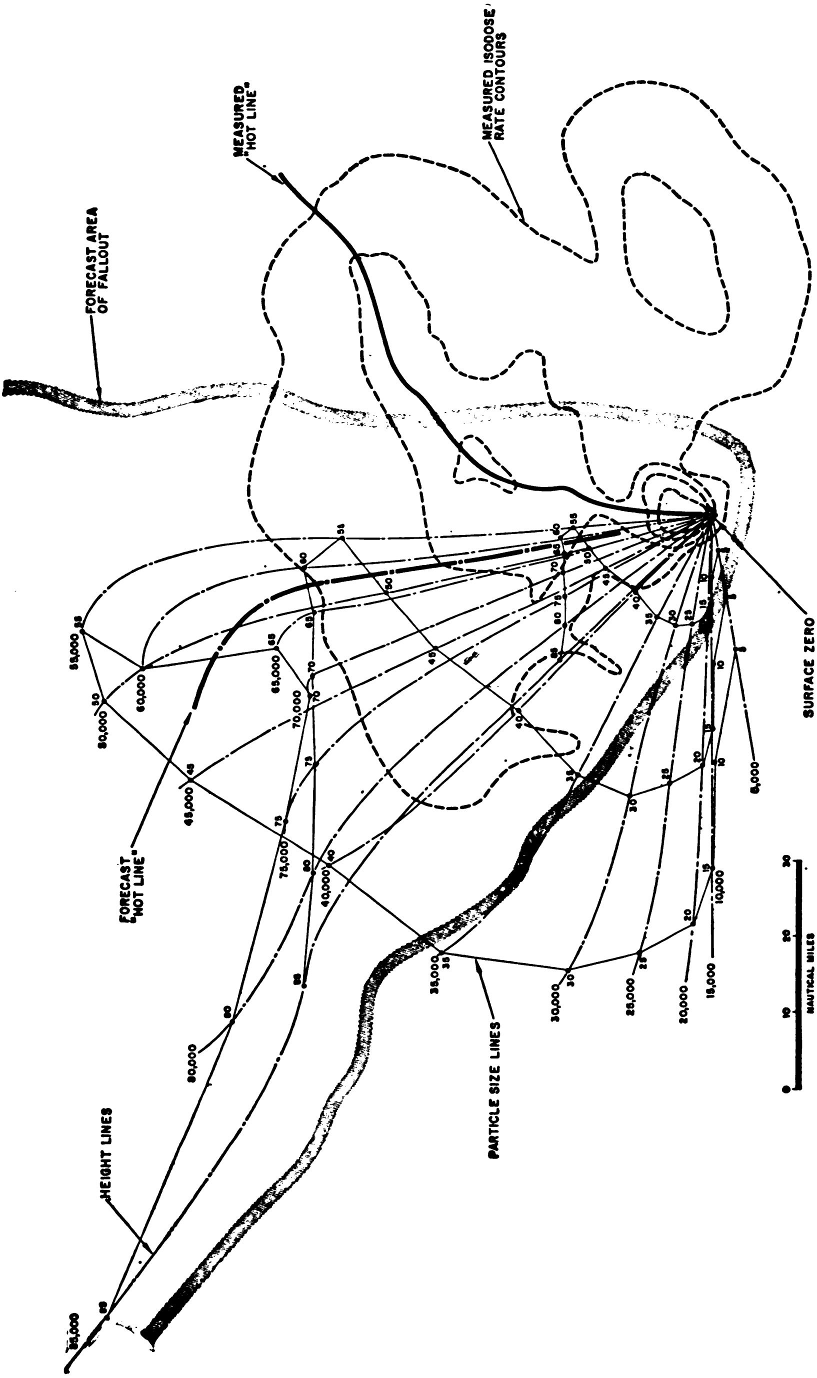


FIGURE 13.—*Comparison of fallout forecast with test results—shot B.*

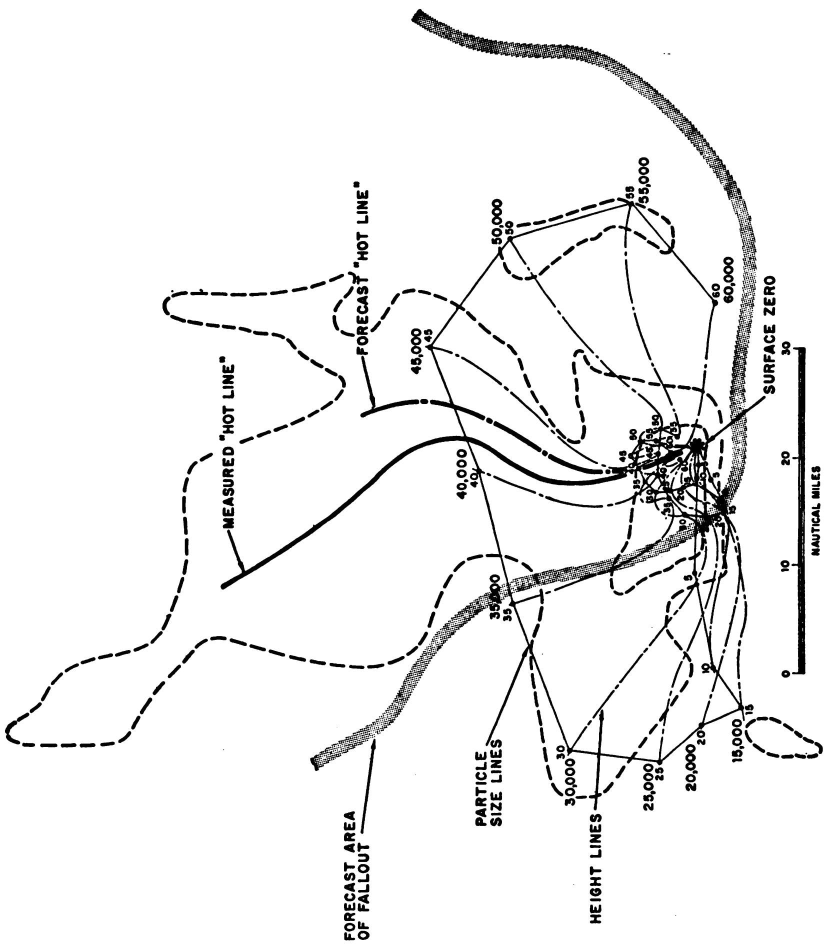


FIGURE 14.—*Comparison of fallout forecast with test results—shot C.*

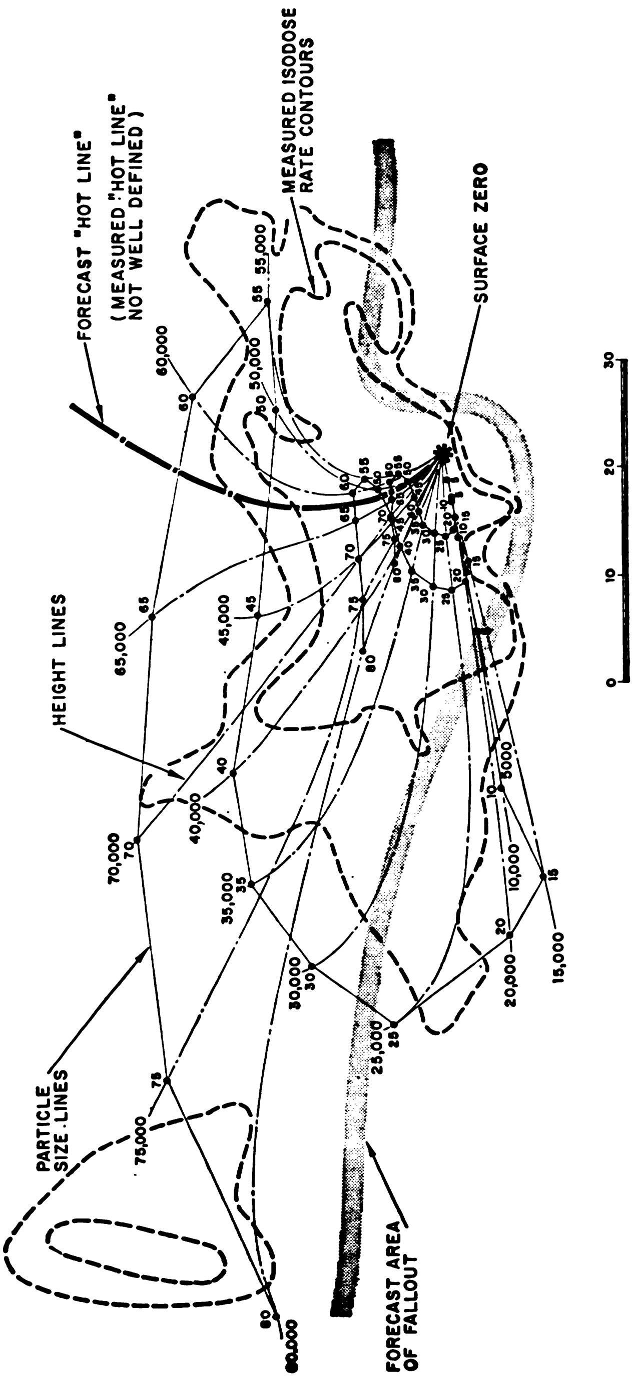


Figure 15.—*Comparison of fallout forecast with test results—shot D.*

Precise determination of the fallout area requires consideration of many complex meteorological parameters. However from the above analysis a practical field tool can be developed that in most cases will satisfactorily define the area of interest.

Mass Activity Relationships Derived From Fallout Measurements

Since fallout consists mainly of nonradioactive debris from nuclear detonations, the essential physical and chemical behavior of fallout is determined by the nature of the inert debris. For example, fallout from detonations on the surface of the ocean will consist of sea water, construction materials of the weapon, and radioactive materials, while fallout from a land-surface burst is produced from soil and target materials, weapon-construction materials, and radioactive elements. In terms of mass, or weight, the relative abundance of the radioactive elements is much less than 1 part in a million. Therefore, except for their radioactive properties, these elements would be extremely difficult to detect.

Accordingly, all major physical and chemical processes which deal with fallout require information on the relation of activity with the other bulk material. The simplest of these relationships is the ratio of activity to the gross mass or its inverse. A more complicated one would be the relation of activity to particle size.

In the fallout area, activity is commonly measured in terms of r/hr, and since r/hr is proportional to activity per unit area the equivalent mass quantity is, therefore, weight of fallout per unit area. For convenience in other computations, the relation of interest is defined as a ratio of mass per unit area to r/hr at a given time after burst; thus it is called the mass contour ratio and is given the symbol $M_r(t)$, since its value depends on the time after burst (due to decay). The generalized scaling equation for the mass contour ratio is

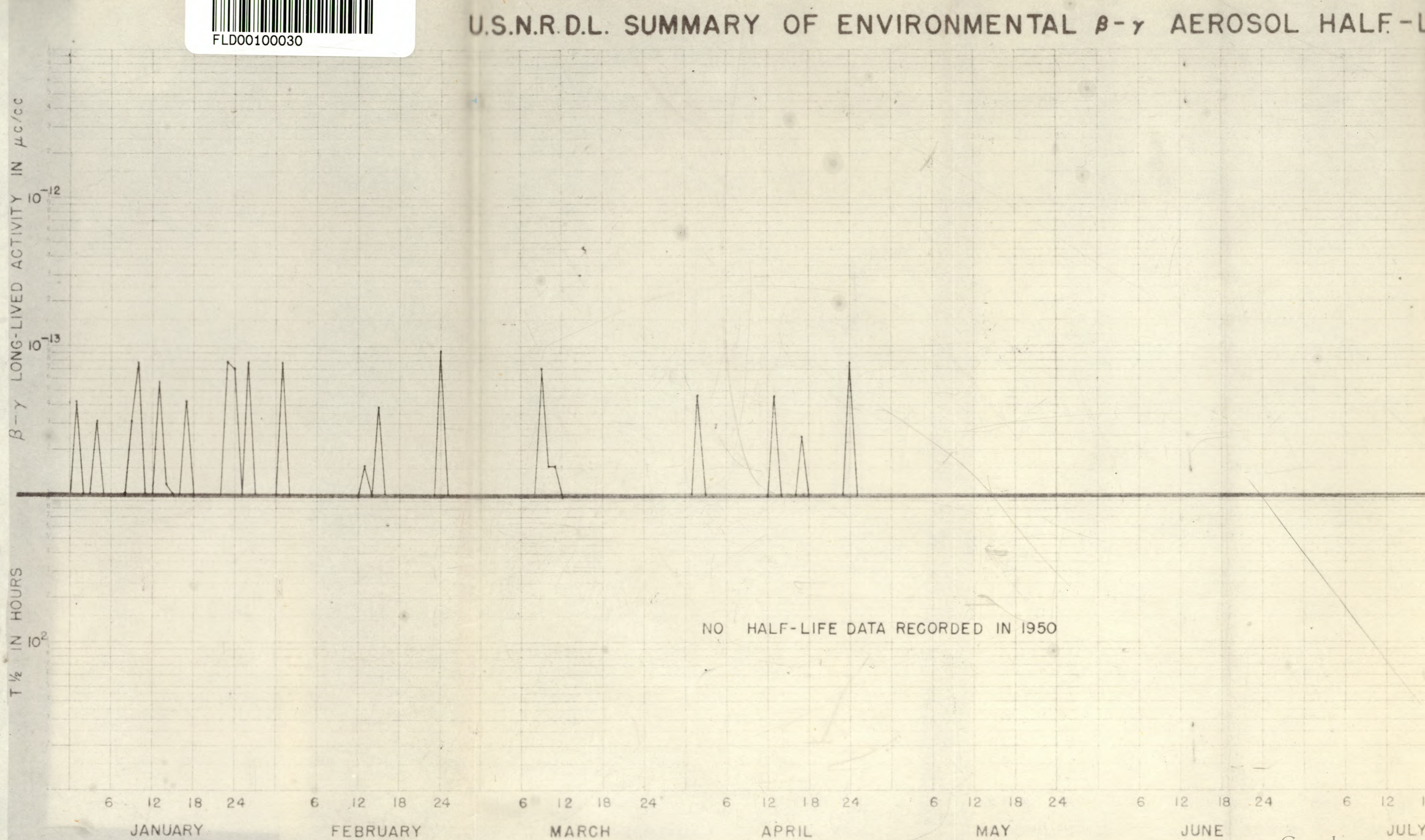
$$M_r^{\lambda}(t)=\frac{f\cdot KA_{\lambda}W^{(n-1)}}{bR(t)[a_{FP}(t)+a_I(t)]} \tag{1}$$

in which

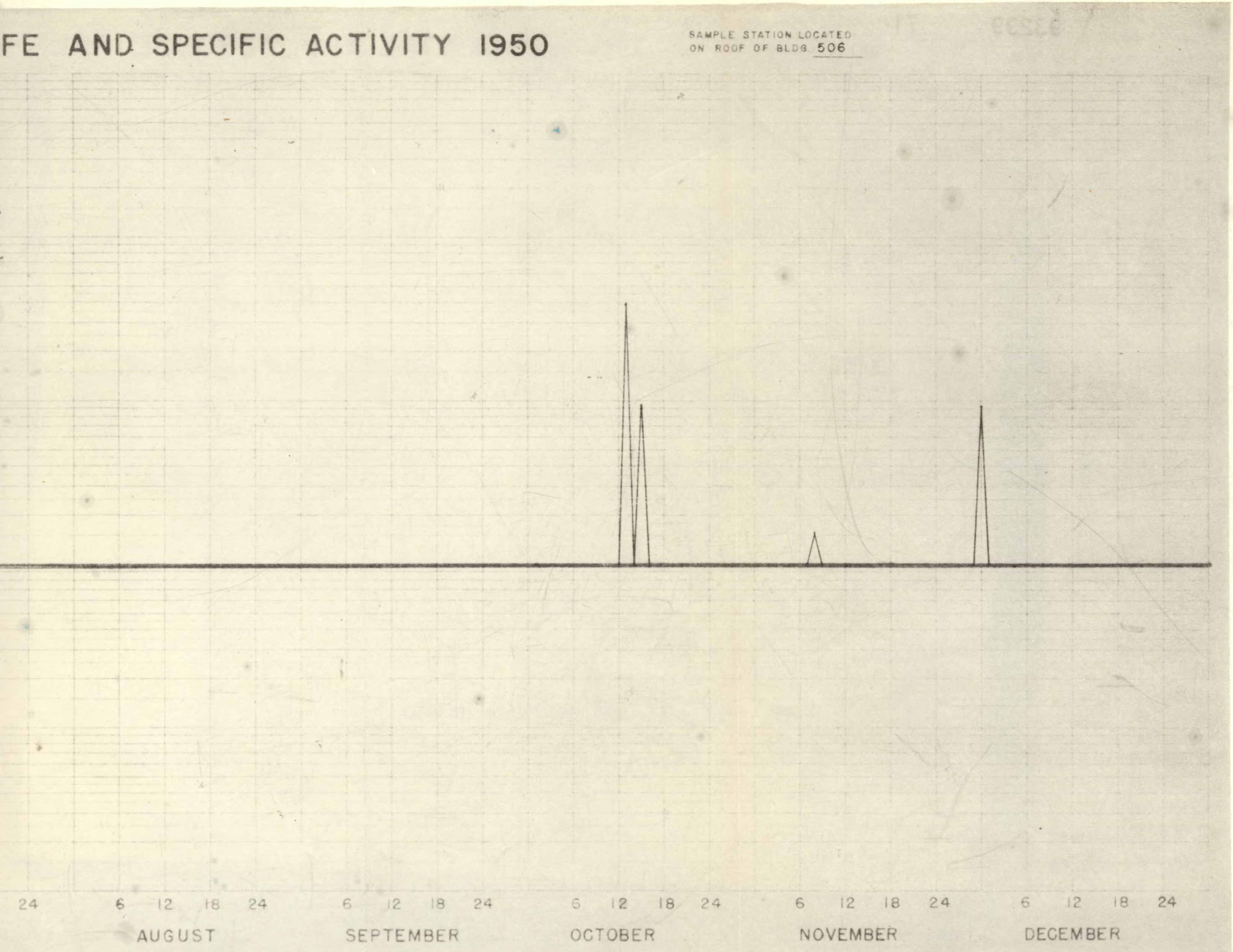
1. W is the total yield of the weapon.
2. $a_{FP}(t)$ is the activity of the fission products in d/m per unit area for 10^6 fissions at time, t.
3. $a_I(t)$ is the sum of the induced activities in d/m per unit area for 10^6 fissions at time, t.
4. R(t) is the ratio of r/hr to d/m per unit area at time, t. If the ratio is computed for an ideal infinite plane, then $R(t)=uR_o(t)$ in which $R_o(t)$ is the computed ratio and u is a factor which accounts for the effect of the terrain on the radiation intensity; knowledge of the photon spectra of the FP + induced activities at time, t, are required to make a reliable calculation of $R_o(t)$.
5. b is the ratio of fission to total yield.
6. bW is then the fission yield (W^{-1} is in the numerator).
7. K is a conversion factor relating the choice of units for W (lbs. of TNT, KT, MT, etc.) to the number of fissions per unit of fission yield, and the units of area and mass desired.
8. $A\lambda W^n$ is the scaling relation for the mass of material removed from the crater. The constant $A\lambda$ depends on the height or depth of burst; it is usually given as a function of the scaled depth, λ, and λ is defined as the depth or height of the burst in feet divided by the cube root of the yield in equivalent lbs. of TNT.
9. f is the fraction of the total mass of material which is thrown out of the crater and gets mixed with the radioactive elements.

The equation constants have been evaluated from available data. The dependence of $A\lambda$ on λ is usually given in graphical form; the results of its use with available data is satisfactory within the limits of error of the data. The mass contour ratio, defined by Eq. 1, is a major parameter in evaluating decontamination results for fallout from detonations near land and harbors; it is also used to convert results from one condition of burst to another.

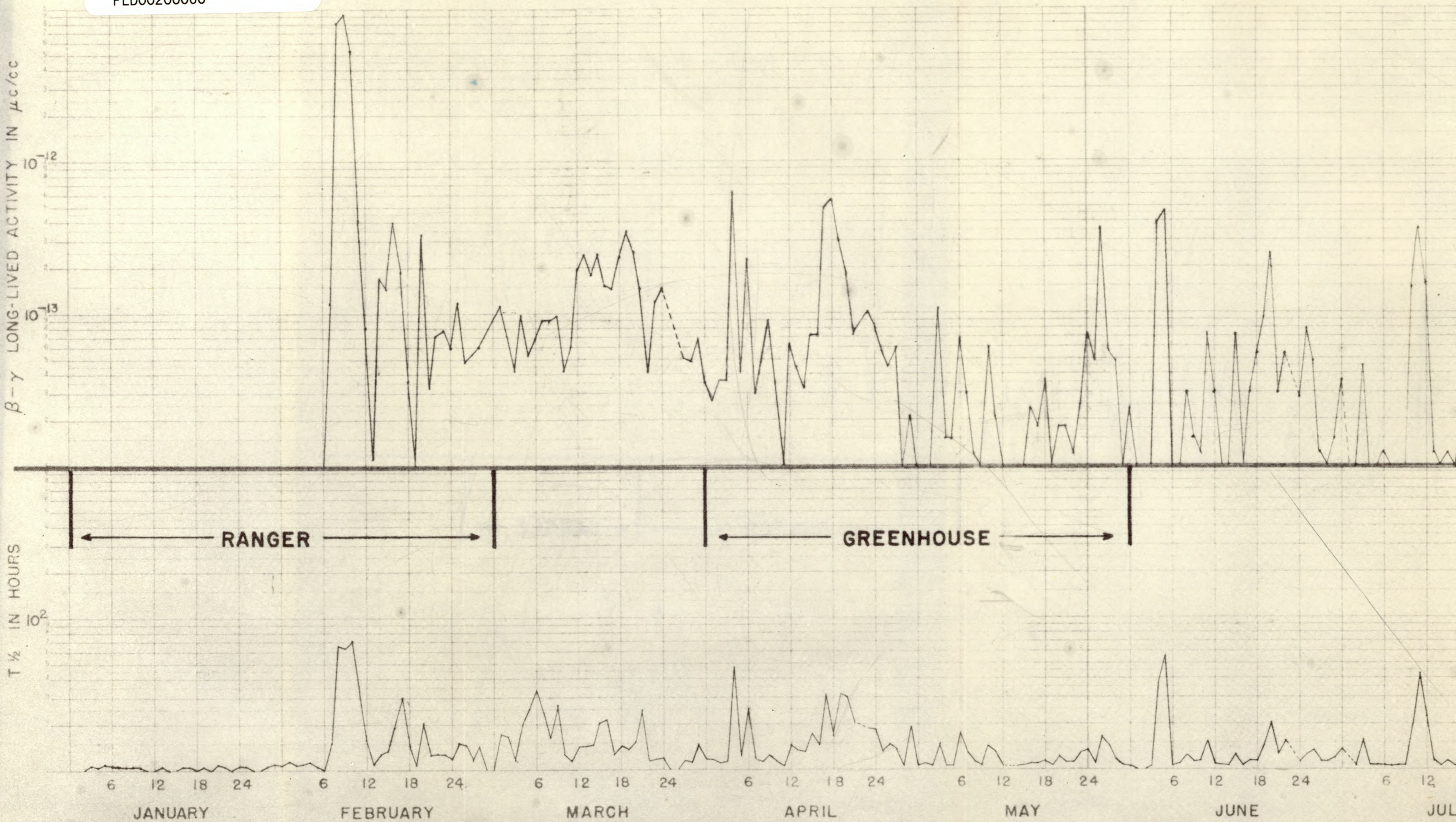
For high yield detonations, the relation holds, within a given error, up to within the heavy blast damage region; for smaller yields, and under ground bursts where large crater lips are formed and much inactive soil and debris are thrown randomly about, no given relation holds.



93299 O - 57 - pt. 1 (Face p. 308) No. 1


FE AND SPECIFIC ACTIVITY 1950
SAMPLE STATION LOCATED
ON ROOF OF BLDG. 506
24
6 12 18 24
AUGUST
6 12 18 24
SEPTEMBER
6 12 18 24
OCTOBER
6 12 18 24
NOVEMBER
6 12 18 24
DECEMBER

U.S.N.R.D.L. SUMMARY OF ENVIRONMENTAL $\beta-\gamma$ AEROSOL HALF-



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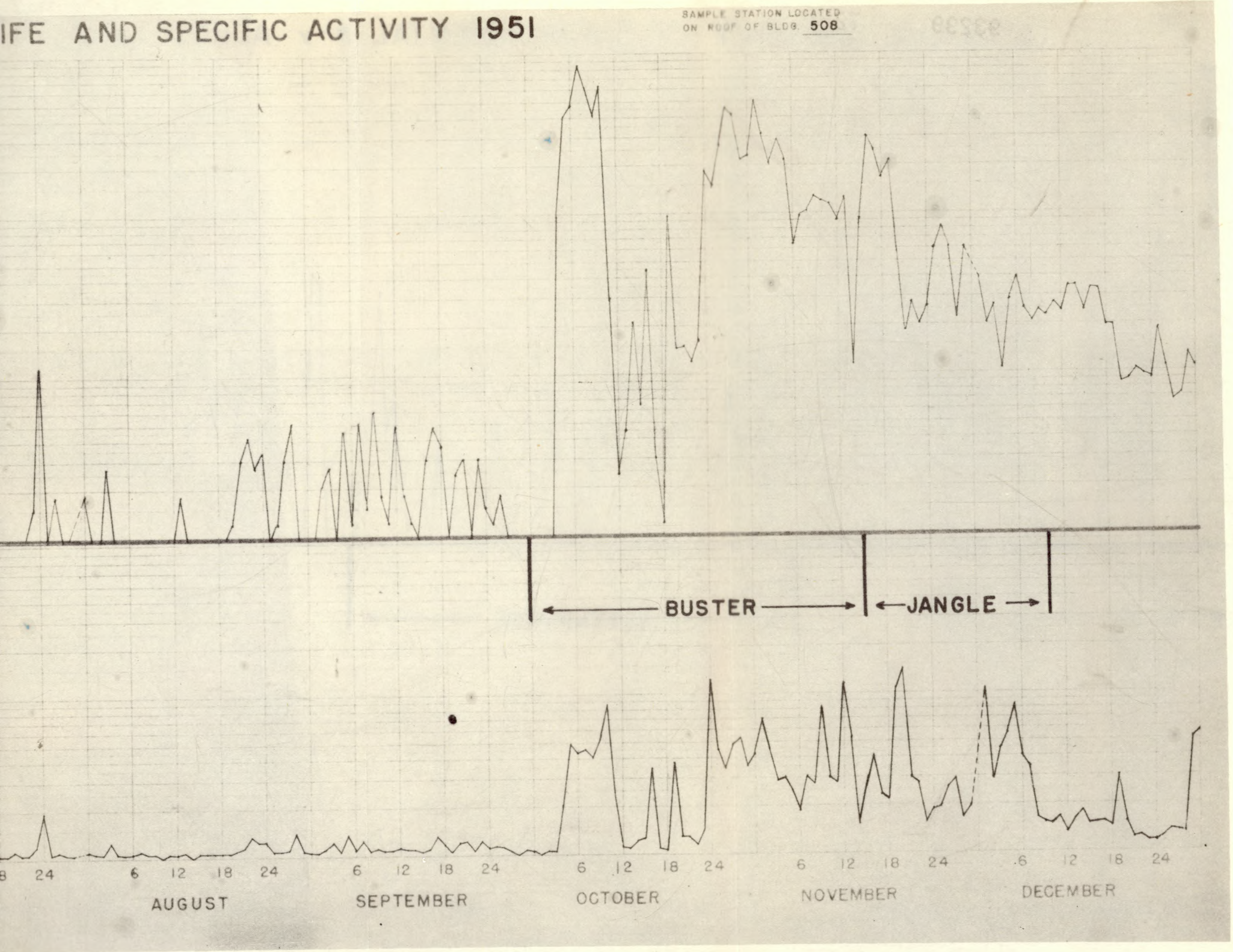
IFE AND SPECIFIC ACTIVITY 1951
SAMPLE STATION LOCATED ON ROOF OF BLDG. 508
93239
BUSTER
JANGLE
8
24
6
12
18
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12
18
24
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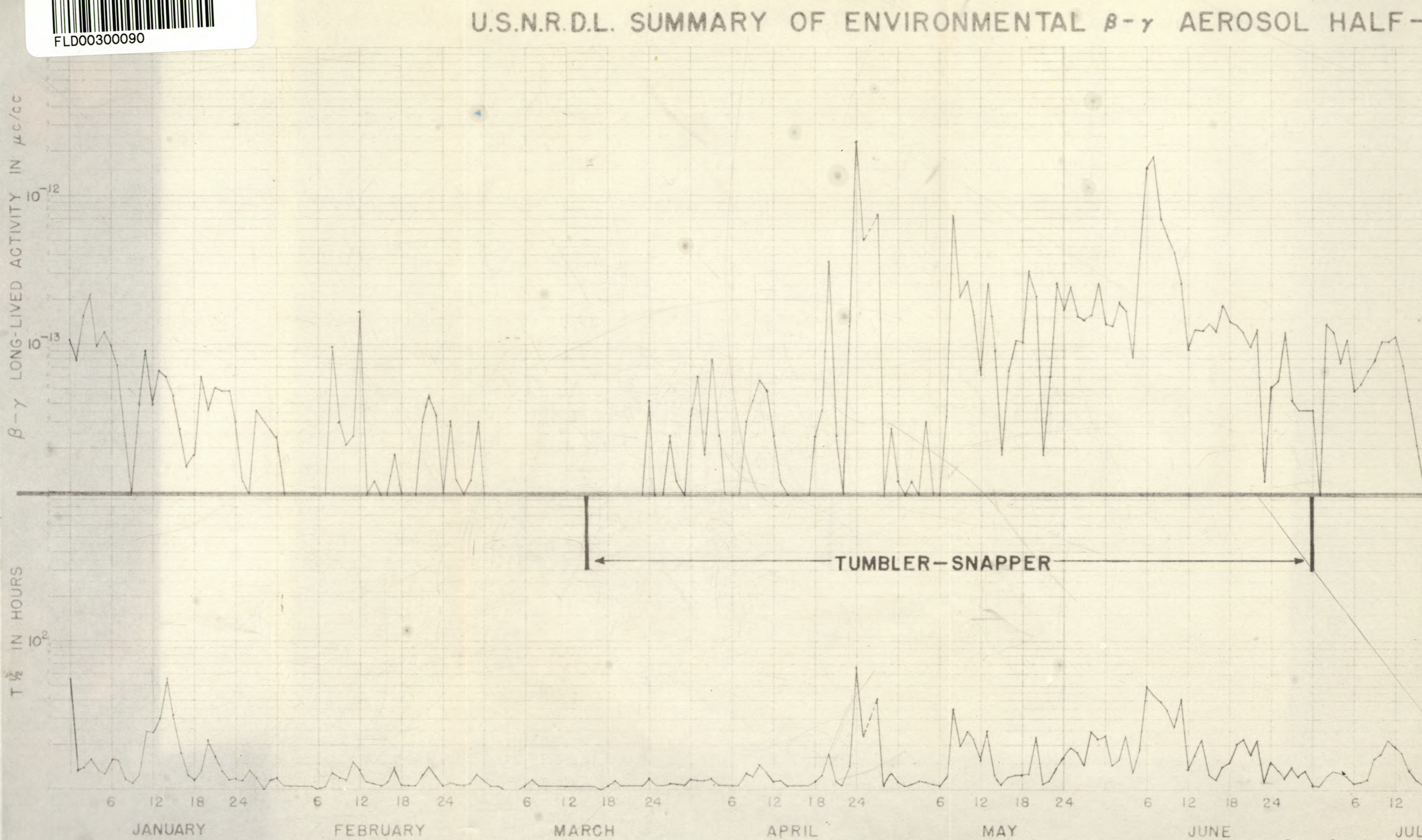
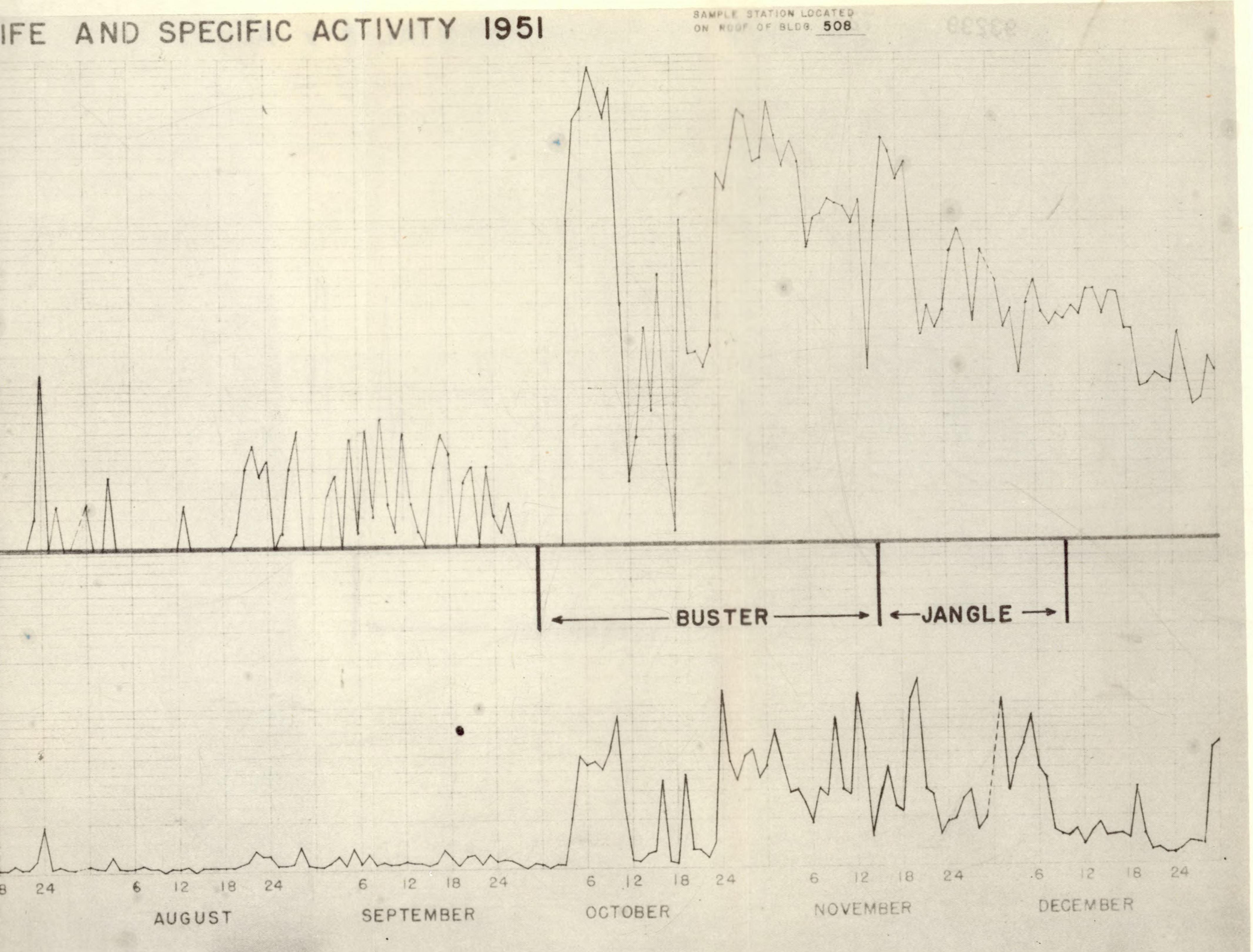
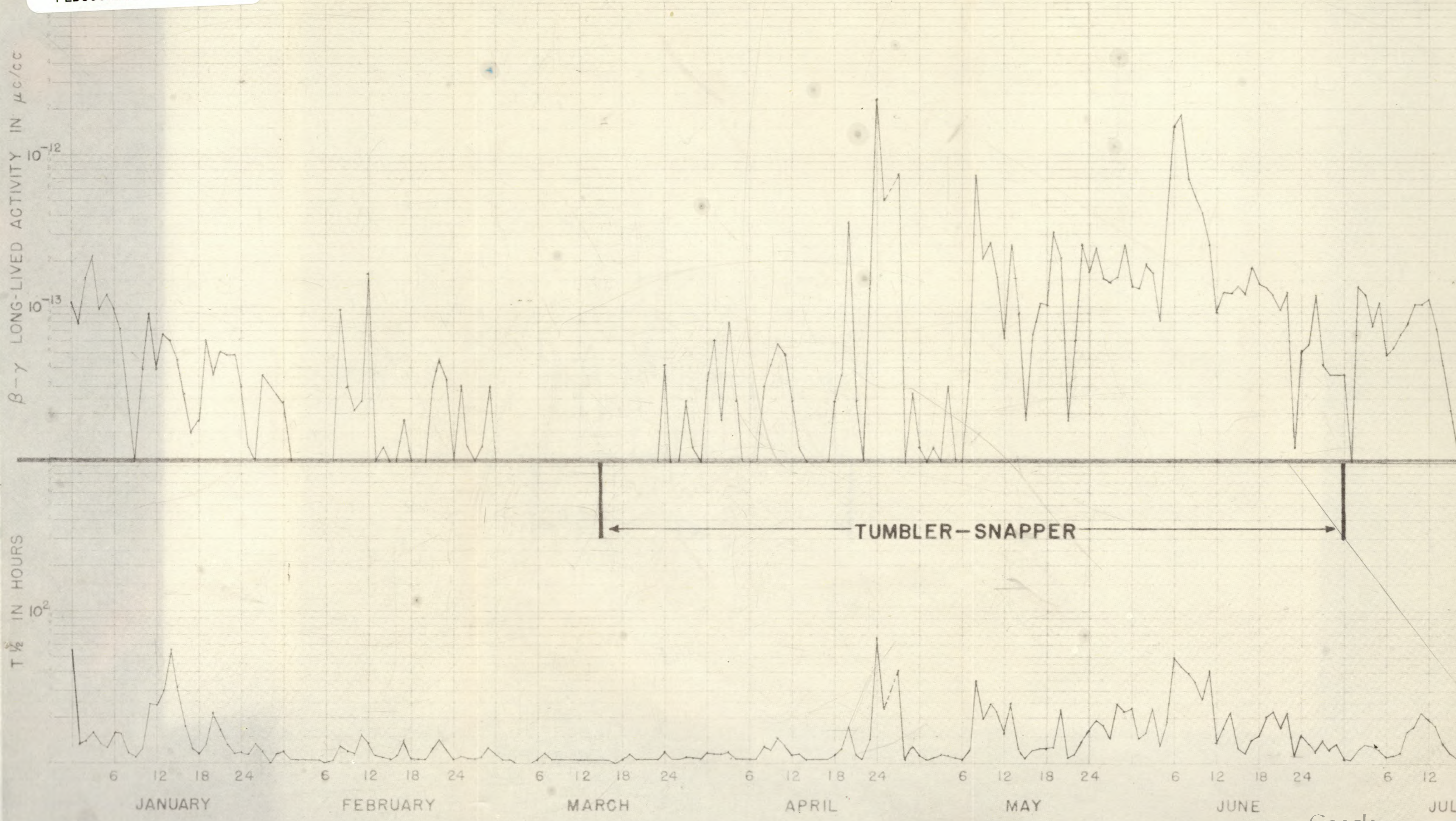
SEPTEMBER
OCTOBER
NOVEMBER
DECEMBER



U.S.N.R.D.L. SUMMARY OF ENVIRONMENTAL β-γ AEROSOL HALF-
β-γ LONG-LIVED ACTIVITY IN μc/cc
10^{-12}
10^{-13}
TUMBLER-SNAPPER
$T_{1/2}$ IN HOURS
10^{2}
6
12
18
24
JANUARY
FEBRUARY
MARCH
APRIL
MAY
JUNE
JUL


IFE AND SPECIFIC ACTIVITY 1951
SAMPLE STATION LOCATED
ON ROOF OF BLDG. 508
BUSTER
JANGLE
8 24 6 12 18 24 6 12 18 24 6 12 18 24 6 12 18 24 6 12 18 24
AUGUST
SEPTEMBER
OCTOBER
NOVEMBER
DECEMBER

U.S.N.R.D.L. SUMMARY OF ENVIRONMENTAL β-γ AEROSOL HALF-



93299 O - 57 - pt. 1 (Face p. 308) No. 3

IFE AND SPECIFIC ACTIVITY 1952

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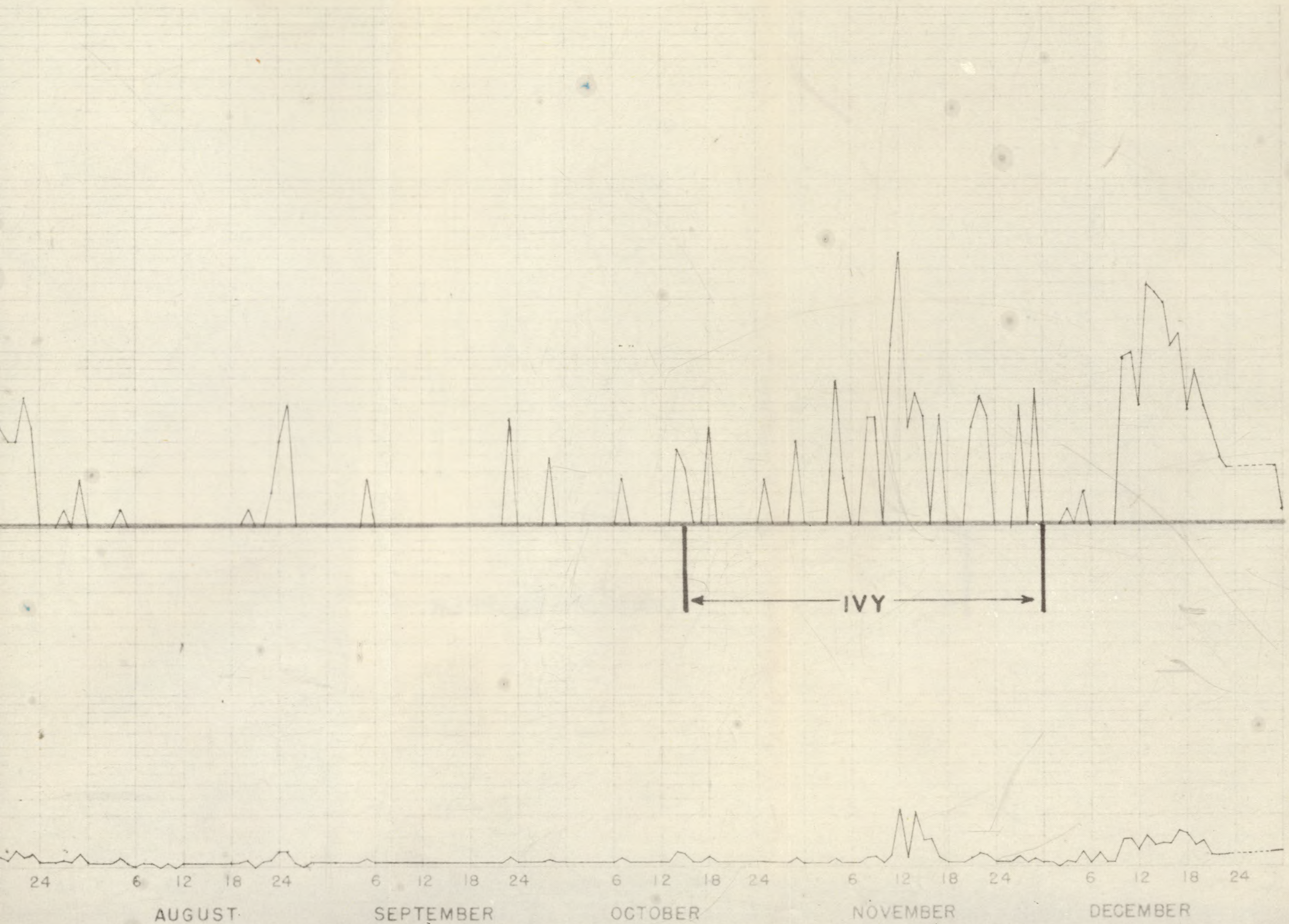

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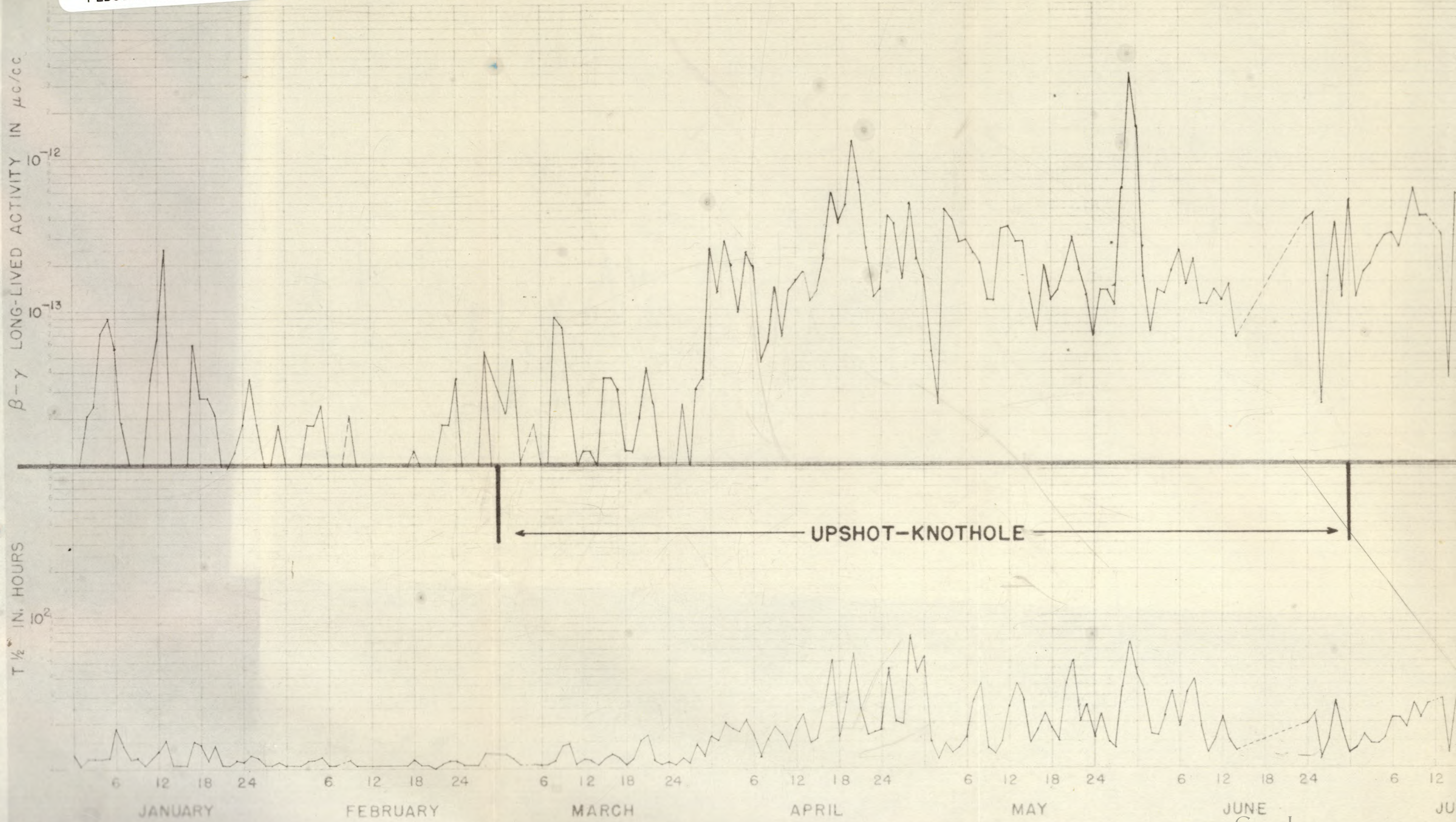

LIFE AND SPECIFIC ACTIVITY 1953

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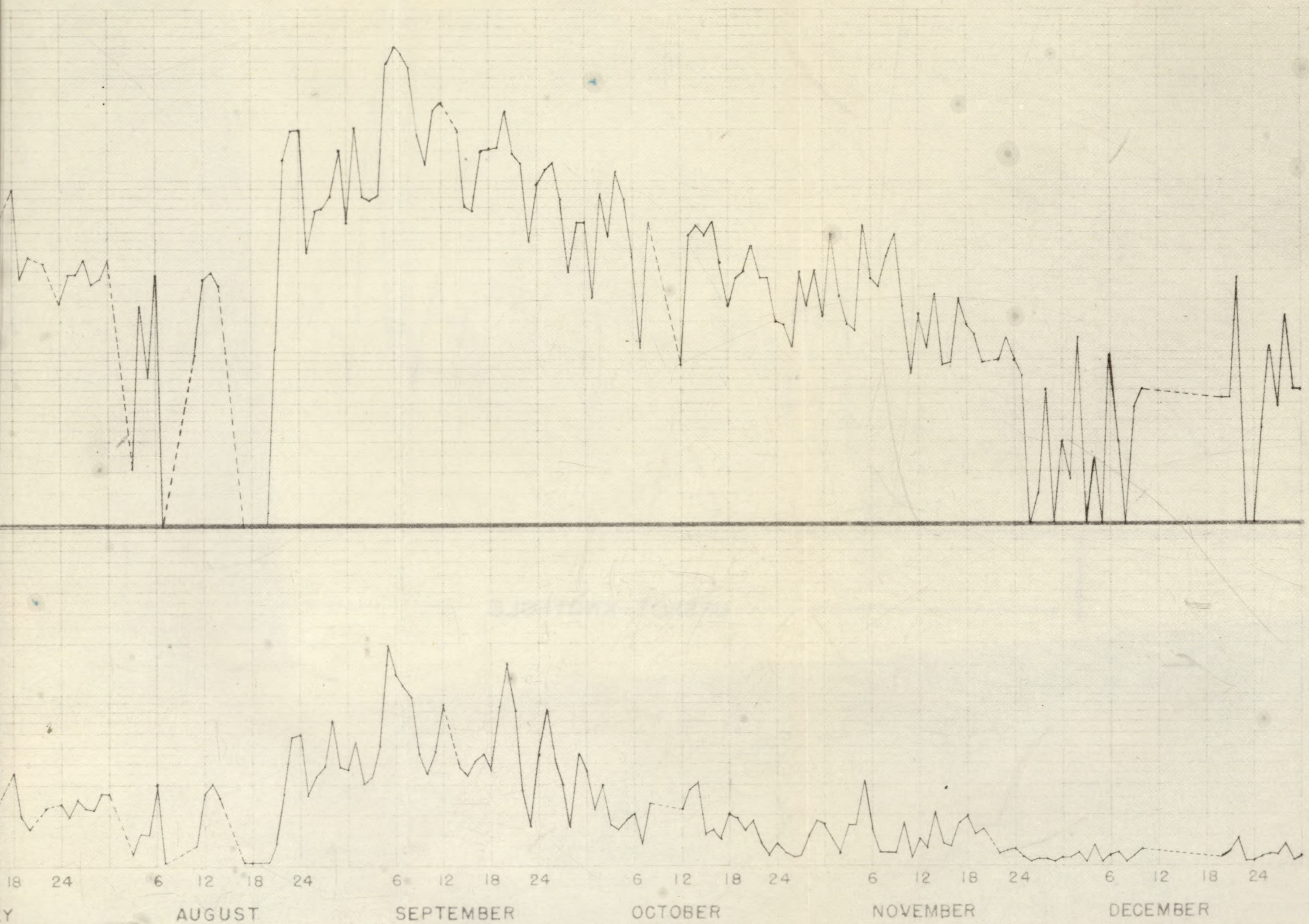

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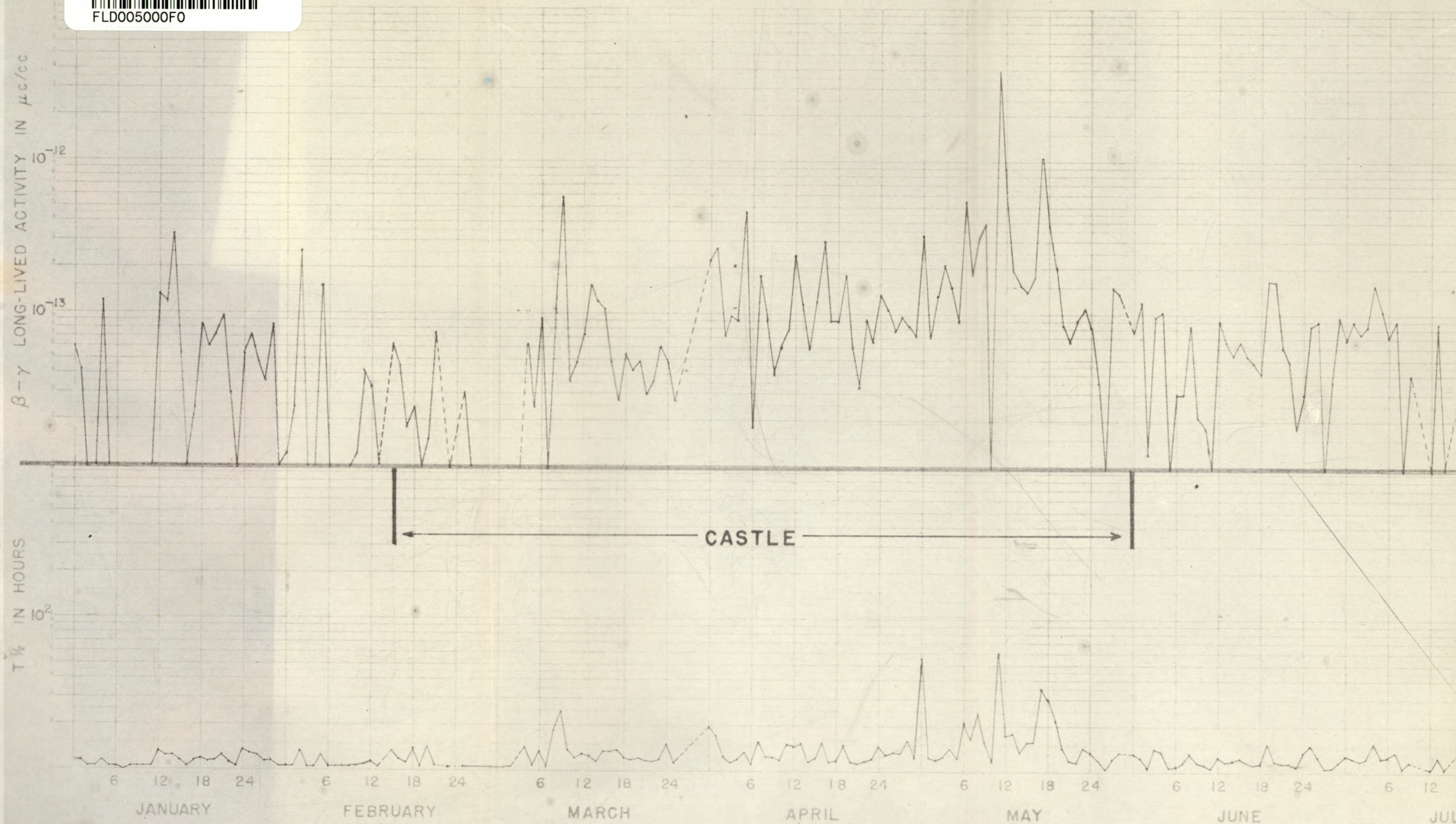
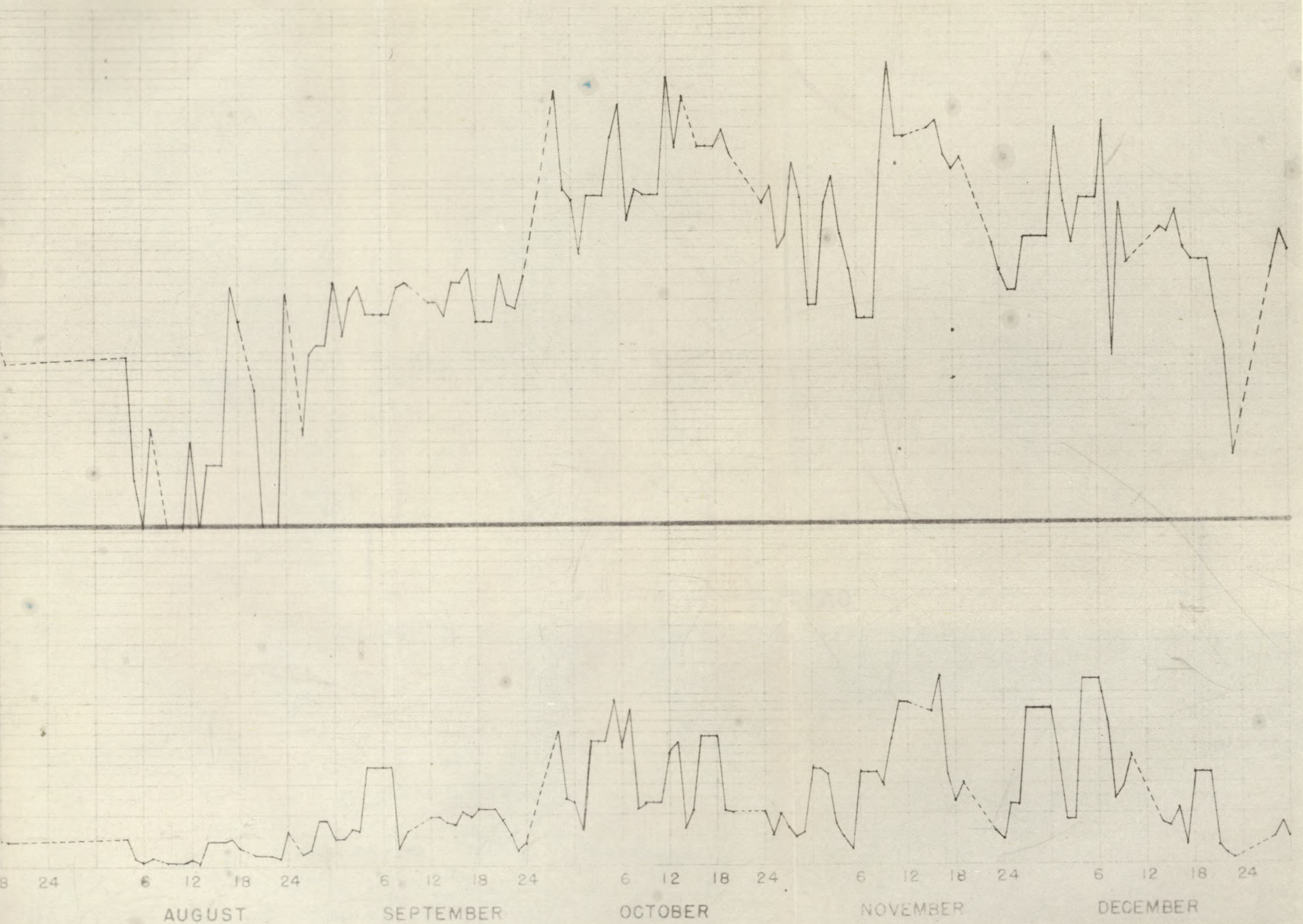


LIFE AND SPECIFIC ACTIVITY 1954



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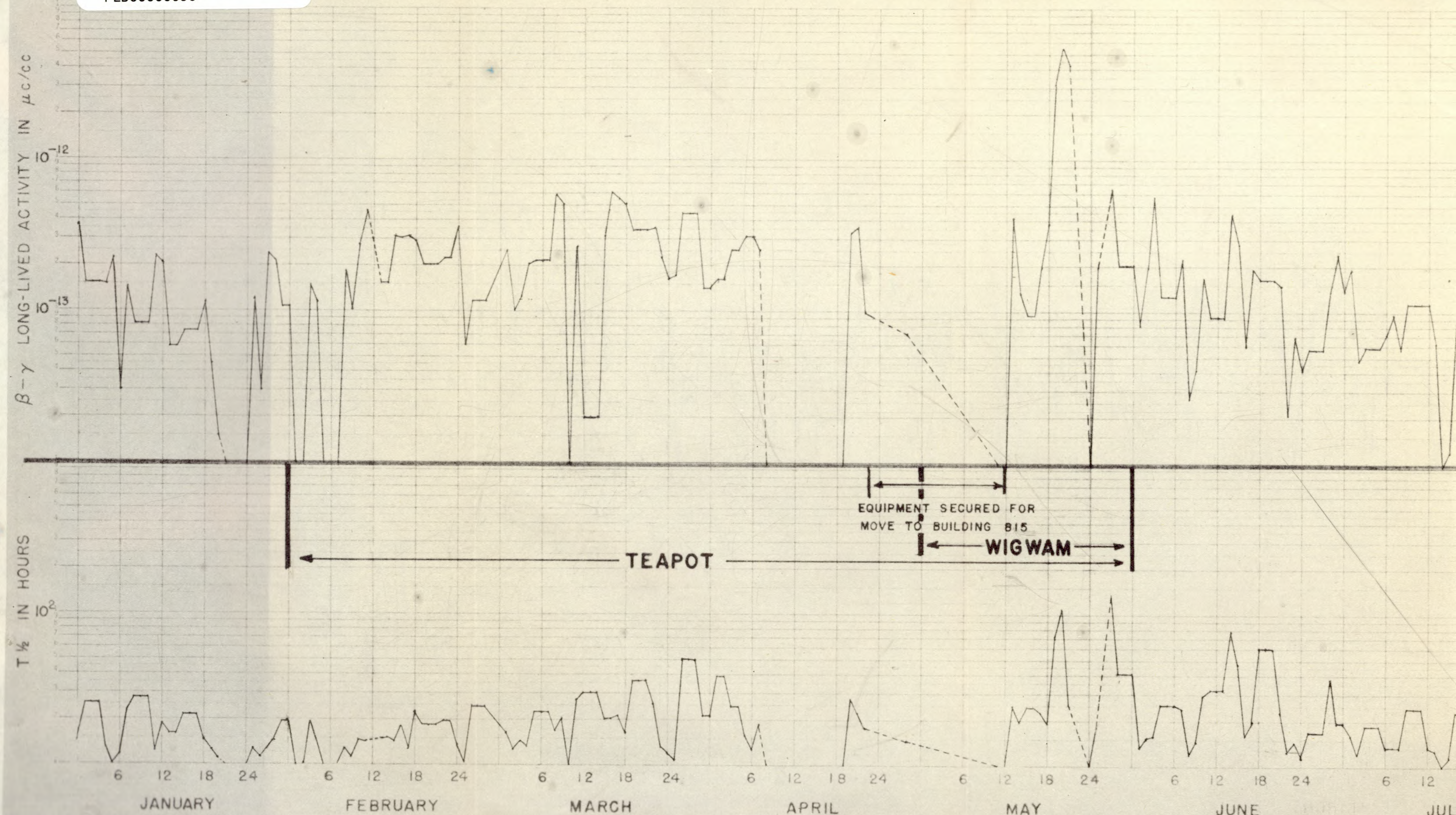

45294 O - 57 - pt. 1 (Face p. 308) No. 6

LIFE AND SPECIFIC ACTIVITY 1955

SAMPLE STATION LOCATED
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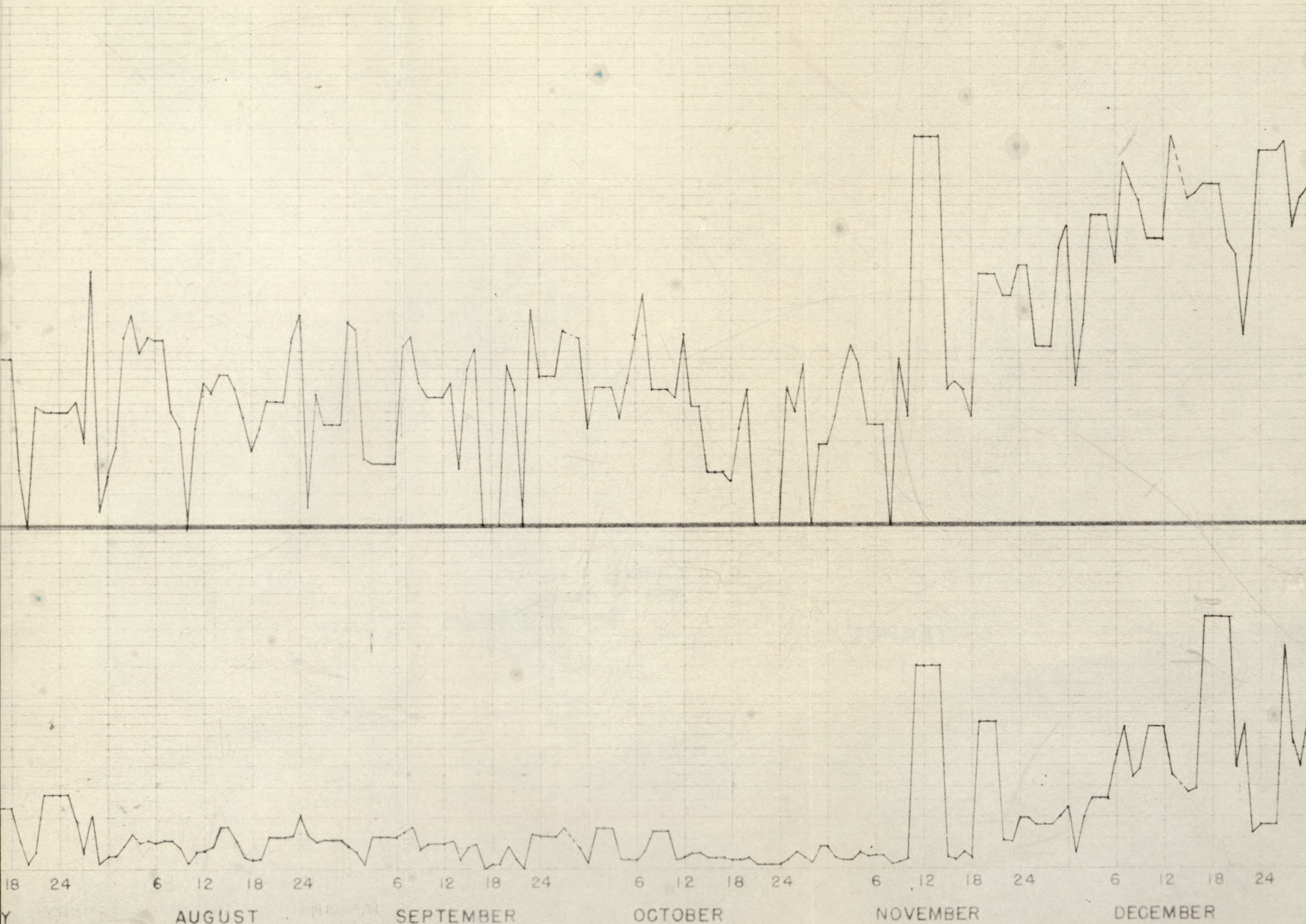
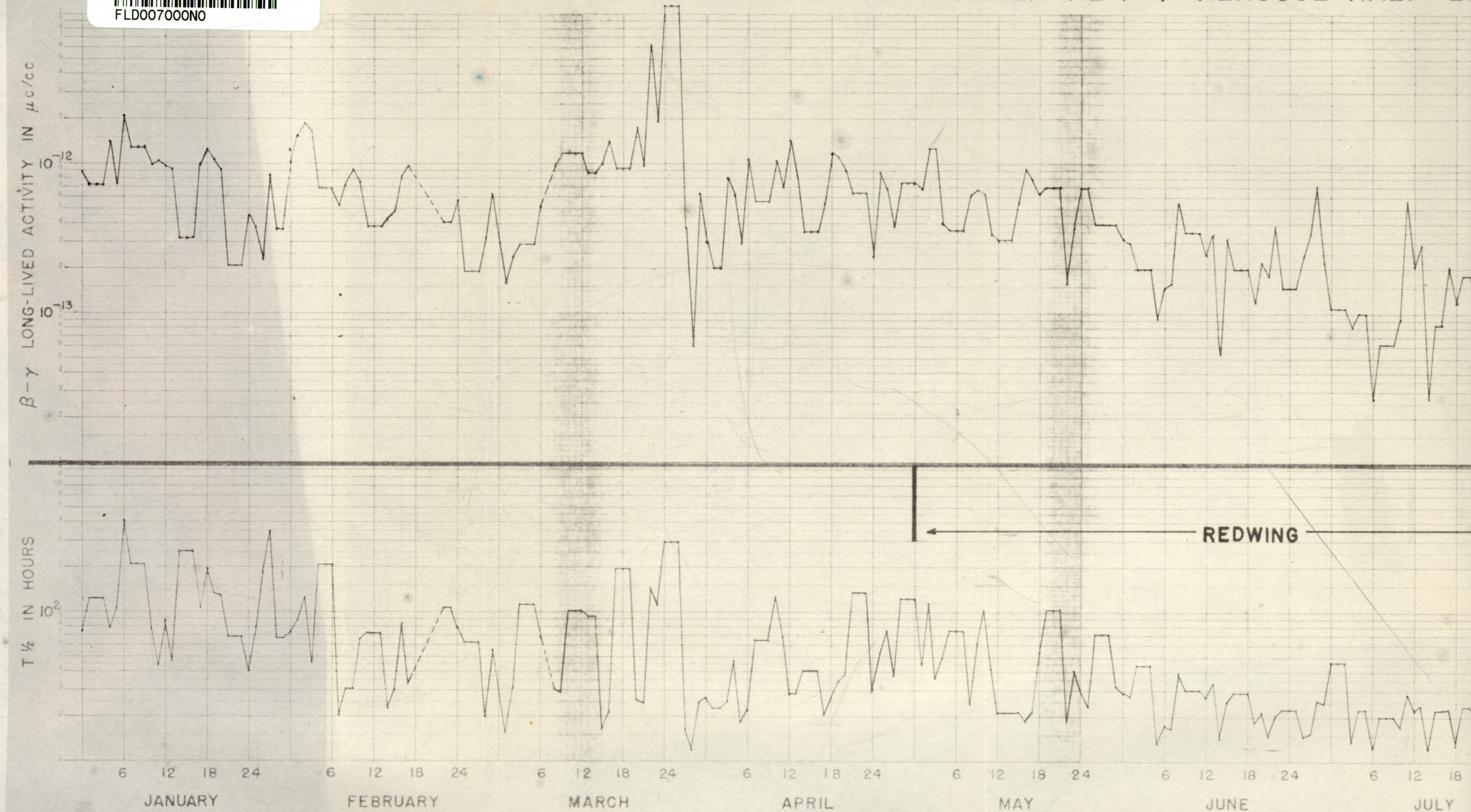


U.S.N.R.D.L. SUMMARY OF ENVIRONMENTAL $\beta-\gamma$ AEROSOL HALF-LI



93299 O - 57 - pt. 1 (Face p. 308) No. 7

FE AND SPECIFIC ACTIVITY 1956

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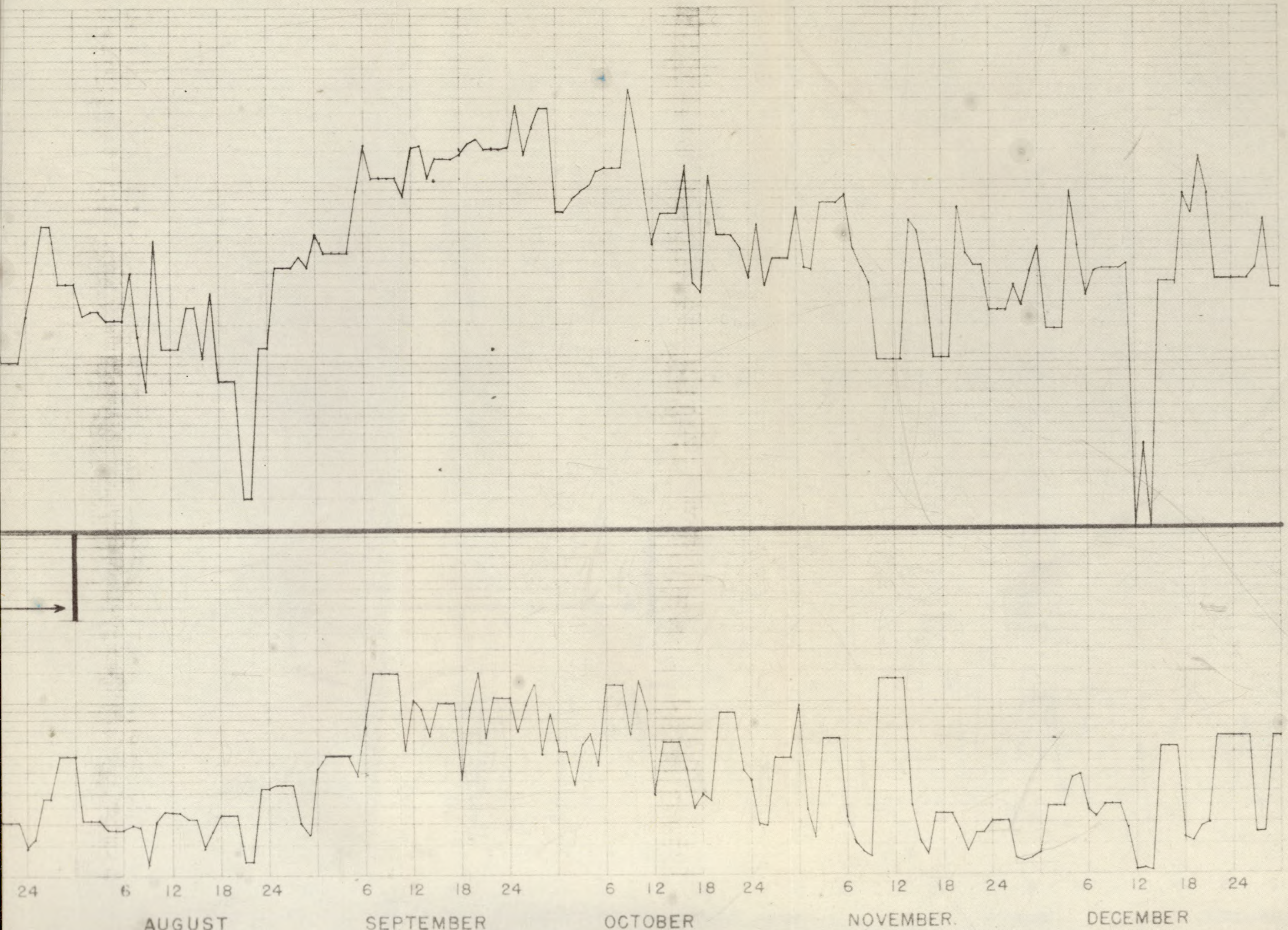

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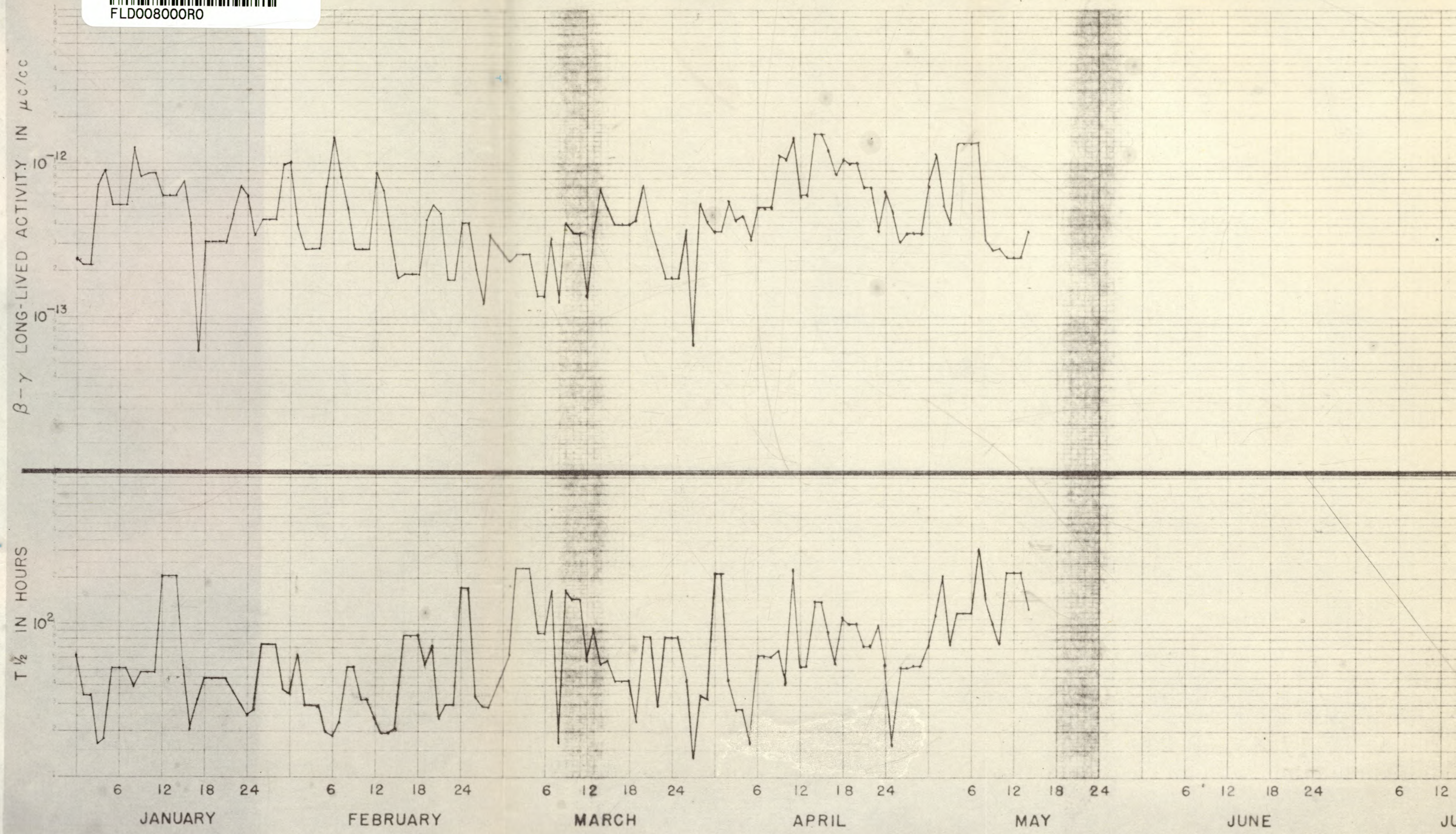

LIFE AND SPECIFIC ACTIVITY 1957

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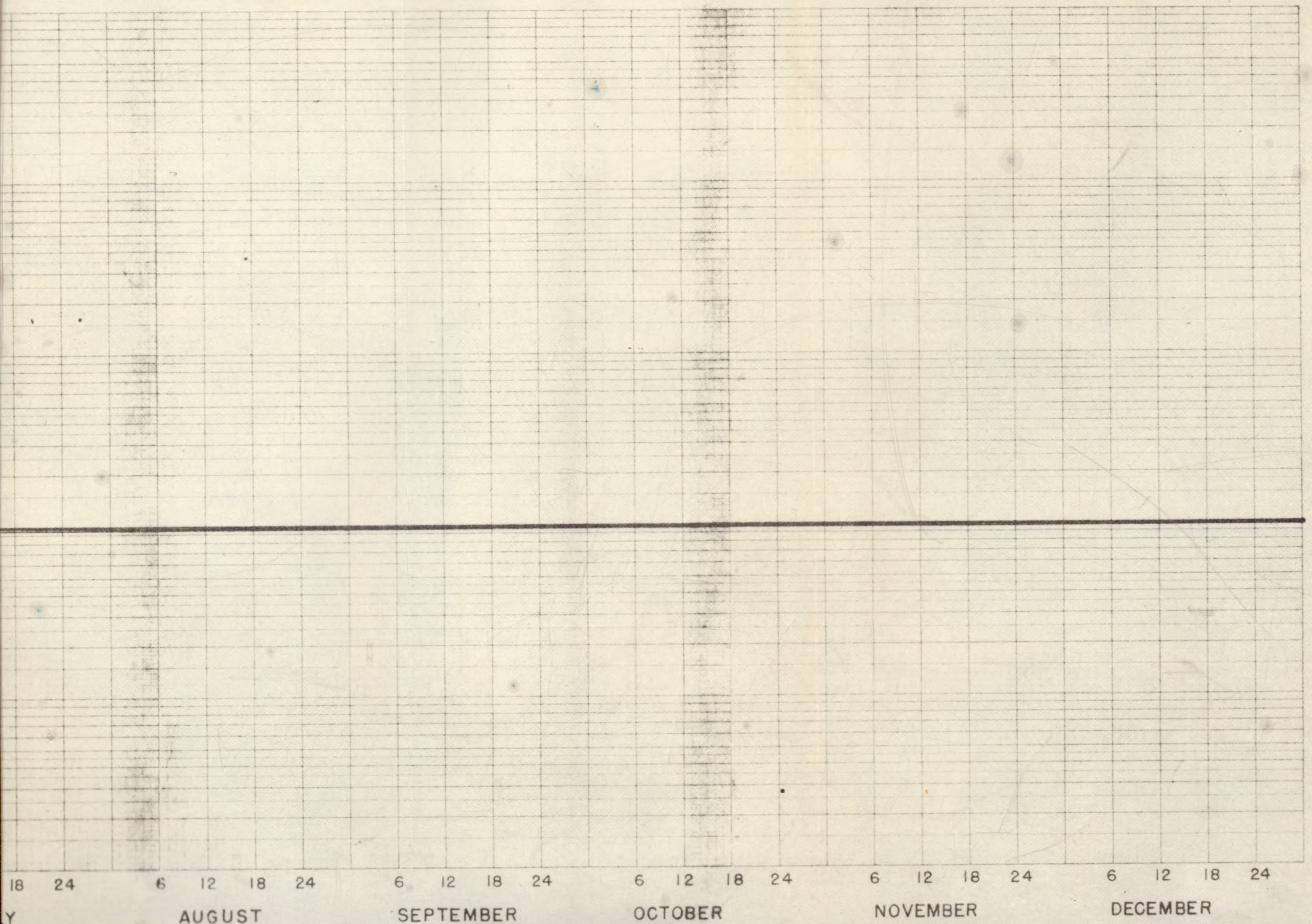

Relation of Radioactive Decay to Countermeasures

The fundamental concepts of a time-phased countermeasure system are based on the fact that the radiation intensity decreases with time. The time-history of the radiation intensity at some location in the fallout area starts when fallout arrives; the intensity increases or "builds up" to a peak sometime during fallout and then decreases when the decay rate is more rapid than the rate of arrival of fallout. After fallout ceases, the radiation intensity decreases with a rate determined by the half-lives and relative abundances of the radionuclides present in the fallout.

Knowledge of the shape of the radiation intensity time-history curve is required to determine what countermeasures can be used at different times after attack and what their requirements are. Mathematically, the areas under the time-history curve for various time periods give estimates of the radiation dosages that could be received during those periods without countermeasures.

In the early period, when the radiation intensity is rapidly increasing toward the peak, large dosages can be received in a very short time; therefore, in areas where the peak is higher than some given value, all personnel exposed to the radiation (for a given time) will become casualties. If these personnel are protected from radiation during this period, they will be able to perform their functions at a later time.

After the intensity decreases (due to decay), certain short-time controlled tasks can be performed in the open without risk of casualties.

The major short-time task is the reclamation of vital operations by decontamination or other actions which will reduce the dose to personnel over a subsequent period to an acceptable amount.

After a longer period of time only the very long-lived radionuclides remain. In fission products alone, the majority of these nuclides are beta emitters. Thus when the gamma emitters have decayed beyond a certain level as to be of no hazard to personnel, the only countermeasures required will be those that deal with ingestion of long-lived beta emitting nuclides.

Again, the guide for determining the appropriate time(s) of application of a given countermeasure(s), what the performance specifications should be, and how the operational application should be made all depend upon the degree of knowledge of the radiation intensity time-history curve—i .e. range of absolute magnitude of the intensities, areas involved for different yields, etc.

The dosage, or area under the intensity-time curve, is sensitive to the shape of the curve. One commonly used decay curve for estimating dosage is that described by the $t^{-1.2}$ function. Since this function is known to be only an approproximation, it is of interest to know to what degree it represents—or misrepresents—the true nature of the intensity-time curve; and, specifically, to what degree the dose estimates from its use differs from other theoretical curves which have given intensity-time curves similar to those observed experimentally. Integrated doses for various periods of possible exposure (stay times) at a series of entry times for a standard intensity of 1,000 r/hr (at $H+1$ hr) are given in table 1 for both the $t^{-1.2}$ function and a normal decay (for radioactivity from a hypothetical weapon in which the decay curve is a normalized average from a number of experimental decay curves) titled "Theoretical Decay" in the table. It may be noted that the intensity for the theoretical decay is higher than for the $t^{-1.2}$ until between 1 and 2 months after burst; after this time, the theoretical decay curve is lower.

The differences in dose between the two for different stay times is given in table 2; a generalizing of the tabulation is that the $t^{-1.2}$ function underestimates the dose for early entry and short stay time and overestimates the dose for late entry and long stay times. The magnitude and order in which the differences occur are considered to be operationally significant for planning purposes; therefore the $t^{-1.2}$ function is no longer used by the Military Evaluations Group of NRDL in its countermeasure evaluations investigations for military sponsors.

TABLE 1.—*Accumulated doses (r) for various exposure periods in a field of 1,000 r/hr at 1 hr after burst*

Entry time	Intensity at entry time		Exposure period							
	$t^{-1.2}$ law (r/hr)	Theoretical decay (r/hr)	1 day (r)	1 week (r)	1 month (r)	6 months (r)	1 year (r)	3 years (r)	5 years (r)	10 years (r)
1 day	22	29.2	342	901	1,320	1,720	1,840	1,990	2,060	2,140
				1,490	2,000	2,355	2,414	2,437	2,439	2,441
2 days	9.6	16.2	180	599	983	1,375	1,495	1,650	1,715	1,795
				1,046	1,504	1,849	1,908	1,931	1,933	1,935
1 week	2.13	3.76	47.5	232	509	865	985	1,141	1,204	1,275
				325	633	943	999	1,022	1,024	1,026
2 weeks	.95	1.13	21.5	121.5	322	642	755	912	972	1,052
				137	354	621	675	697	699	701
1 month	.371	.404	9	55	175	435	540	690	753	830
				59	171	384	427	448	450	452
2 months	.162	.150	5	26.9	91	290	378	525	595	671
				27	82	225	259	278	280	282
6 months	.044	.032	2	7.4	29.8	124	192.5	312	368	455
				4	19	60	73	84	86	88
1 year	.019	.00575	1	3.2	13.5	64.7	108.5	203	256	315
				1	4	13	20	24	26	28
1½ year	.0114	.00206		1.9	8	42	73.5	149	189	252
				.4	1.5	7	9	11	13	15
2 years	.008	.000985		1.3	5.7	30.8	54.4	117	156	215
				.2	.7	3.4	4.9	7.5	9	11
4 years	.0035			.6	2.5	14.4	27.5	67.1	94.5	140
	.0015			.25	1.2	6.5	12.8	34	50.8	82.2

TABLE 2

Error Chart for Difference in Dose Between Use of $t^{-1.2}$ and A Standard Decay Curve
(values in percent difference in dose)

Entry Time		Stay Time: 1 wk	2 wk	3 wk	1 mo	2 mo	3 mo	6 mo	9 mo	1 yr	2 yr	3 yr	5 yr	10 yr	
1 day		+65	+59	+55	+51	+47	+42	+37	+34	+31	+25	+22	+18	+14	+10%
2 days		+75	+63	+57	+53	+59	+41	+34	+28	+27	+21	+17	+13	+8	
3 days		+74	+59	+53	+47	+40	+35	+29	+29	+21	+14	+10	+6	+1	
4 days	>50	+68	+53	+47	+42	+34	+29	+23	+19	+16	+8	+4	–	-6	-10%
5 days		+62	+45	+39	+36	+28	+23	+18	+13	+10	+2	-2	-7	-13	
6 days	+50	+53	+40	+34	+31	+24	+21	+13	+10	+6	-2	-6	-11	-18	-20%
7 days		+40	+30	+28	+24	+18	+14	+9	+4	+2	-7	-12	-17	-24	
8 days		+34	+26	+24	+21	+16	+11	+6	+2	-1	-10	-15	-21	-30	
9 days	+20	+26	+21	+19	+17	+12	+8	+3	-1	-4	-13	-19	-26	-35	
10 days		+18	+18	+16	+14	+9	+5	+1	-3	-7	-17	-25	-30	-40	
11 days	+10	+17	+15	+24	+13	+8	+4	–	-4	-8	-19	-25	-32	-42	
12 days	+10	+10	+8	+9	+8	+5	+1	-3	-7	-11	-22	-29	-37	-47	
13 days		+13	+13	+12	+11	+3	+2	-3	-7	-11	-23	-29	-37	-48	
2 weeks		+12	+14	+12	+10	+6	+2	-3	-8	-12	-23	-31	-39	-50	-50%
3 weeks	+10	+16	+21	+11	+7	+2	-2	-8	-13	-18	-32	-41	-52	-65	
4 weeks		+5	+3	+3	–	-4	-6	-12	-19	-25	-41	-52	-65	-80	
1 month	+10	+7	+5	+4	-2	-4	-8	-13	-20	-26	-43	-54	-67	-84	
5 weeks		+4	+1	-2	-5	-11	-13	-17	-26	-32	-53	-64	-77	-93	-100%
6 weeks		–	-7	-7	-8	-18	-6	-23	-34	-37	-64	-74	-91	-112	
2 months		–	+2	-8	-11	-12	-17	-29	-39	-46	-71	-89	-106	-138	
9 weeks	-10	-18	-15	-13	-22	-21	-26	-34	-44	-53	-79	-93	-118	-142	
3 months	-20	-8	-15	-18	-17	-21	-24	-37	-52	-66	-99	-118	-150	-180	-200%
4 months	-50	-20	-25	-43	-35	-31	-39	-62	-79	-97	-140	-166	-198	-240	
6 months	-100	-75	-100	-90	-58	-71	-74	-103	-140	-163	-230	-275	-323	-435	
9 months	-200	-125													
1 year		-200													

PHYSICAL CHEMISTRY OF FALLOUT AS IT RELATES TO DECONTAMINATION COUNTERMEASURES

GENERAL BACKGROUND

In countermeasures, the basic physical chemistry of fallout is concerned with the interactions of fallout with surfaces. These interactions are then related, in a practical sense, to an appropriate choice of reclamation procedures. Further, an understanding of the chemistry leads to a precise concept of the nature of fallout and to the significant properties of both the weapon and the target that combine to produce fallout of a given chemical nature as well as the implication of these properties on the countermeasure performance.

In radiological defense designed for nuclear attack there are three basic kinds of fallout of interest: (1) From detonations at sea, (2) from detonations on land, and (3) from detonations on harbor targets. The general description of the interactions of these kinds of fallout will aid in explaining some of the technical problems; some of the experimental data and techniques are also applicable to reclamation problems which might range from a laboratory spill of a small amount of activity to a reactor accident although none of these would ever approach the scope of the reclamation task envisioned in event of fallout from nuclear attack.

SEAWATER FALLOUT

Seawater fallout will consist of seawater, bomb structural and target materials, and radioactive products. The bulk of the material thrown up in a detonation at sea will be seawater of which about 3 percent of the weight is salt (mainly sodium chloride); the radioactive elements will be present at concentrations less than 1 part in a million. A high yield explosion will throw this material to such altitudes that much of the water can evaporate in falling back to earth; with lower yield explosions less will evporate. Depending on the humidity, in one extreme the fallout might arrive as wet, saturated salt particles and in the other as water droplets much like rain.

When these droplets or pseudo crystalline salt particles strike a surface (for simplicity, assume an impervious surface such as painted metal or wood) they will tend to stick where they land, and since fine mistlike particles travel almost horizontally more of them can strike and stick on vertical surfaces. Larger water droplets, however, when deposited in large numbers will fall more vertically and run off vertical surfaces.

Since the bulk material (salt) is water soluble and never completely dries out (or stays dried out) in the presence of water vapor in the atmosphere, the radioactive as well as the salt atoms (ions and colloids) can move about in water and diffuse toward the surface. Within several hours after deposition, the droplets will all evaporate to the same degree under the same conditions and reach the same equilibrium state with respect to the surface.

In this environment each radioactive atom has some freedom of movement and each kind (element) will interact with the surface in its own characteristic manner. The major interaction with the surface in this case is adsorption of individual elements (especially the metallic elements). The alkali elements (like sodium or cesium) do not adsorb on surfaces very strongly, the alkaline earth elements adsorb in larger extent, and the rare earth elements to a greater extent than the alkaline earths. The degree of adsorption is in order of the charge on the ions from +1, +2, to +3. The equilibrium amount of each adsorbed depends on the amount of each present initially in the drop. The amount directly adsorbed by the surface cannot be removed without either removing some of the surface, or without imparting a great deal of energy to the surface layer either by physical or chemical means. Water washing, for example, simply washes away the equilibrium amount left in the salt layers above the surface.

The Freundlich adsorption isotherm can be adapted to describe the adsorption process and subsequent washing of the surface. For a given element, it is—

$$F_j = R_j/I = a_j I^{n_j - 1} \quad (1)$$

in which I is the initial level (later defined in total r/hr of which element j contributes a stated fraction at the time, t, after detonation; R_j is the amount left after washing (i. e., the amount adsorbed); and a_j and n_j are the impirical adsorption constants. The fraction remaining is—

$$R_j = a_j I^{n_j} \quad (2)$$

equations (1) and (2) apply either to a single drop or to the whole surface if at least a single layer of drops has been deposited. The intermediate situation is for rather low contamination levels and requires rather complicated equations; therefore, the following treatment will assume a serious contamination of the surface as the lower initial level of interest. For chemisorption which obeys equation 2, n_f is less than 1 so that the fraction remaining decreases with decreasing initial level.

In this type of fallout, another interaction can take place within the water drop either during its fall to earth or after it lands. Likely bomb and target structural materials will include fairly large amounts of iron, aluminum, etc. These materials will be oxidized and will form hydrous precipitates in the water drops. Many of the radioactive atoms will adsorb or mix with these precipitates. When these are present, a three-way interaction occurs on the surface rather than just the one previously described. Simple washing methods will dissolve only a small amount of the precipitates once they are dried on the surface, and during the process an equivalent amount of the radioactive elements will be released.

A rigorous mathematical solution of all the physical chemistry equations and material balance equation for the three-way process cannot be made; however, suitable approximations can be made for simple water washing of the surface. The fraction remaining for this case is—

$$F_i = a_i I^r j + \frac{I}{k_i + I} \tag{3}$$

in which, for a given element,

$$K_i = dK_c V / q_r(t) \tag{4}$$

in which (1) d is the density of the hydrous oxides.

(2) V is the equivalent initial volume of the fallout (before evaporation begins), deposited per unit area of surface.

(3) K_c is the equilibrium constant for the distribution of the element between the liquid and solid phase,

and (4) $q_r(t)$ is the ratio of the weight of the bomb (and target, as Al, Fe, etc.) per unit area to the amount of radioactive elements present; $q_r(t)$ therefore depends on the yield and is given by

$$q_r(t) = \frac{kM_B Y(t)}{bW[A_{FP}(t) + a_I(t)]} \tag{5}$$

in which (1) M_B is the total mass of bomb (and target material thrown up),

(2) $Y(t)$ is the ratio of r/hr to d/m per unit area at time, t, after detonation and depends on the photon energy spectrum,

(3) W is the total yield of the weapon,

(4) b is the ratio of fission to total yield,

(5) $a_{FP}(t)$ is the activity of the FP from 10^6 fissions in d/m at time, t.

(6) $A_I(t)$ is the sum of the induced activities for 10^6 fisisons in d/m at time t.

and (7) K is a constant relating the fission yield and the number of fissions in appropriate units.

The various quantities illustrate the kind of weapon or detonation parameters which are related to the chemical interaction at a surface many miles away from the attack as well as the information required to properly interpret decontamination test experimental results.

LAND FALLOUT

Land fallout will consist of soil material, bomb structural and target materials, and radioactive products. The bulk of the fallout material from a surface detonation will be soil particles in which the radioactive elements are fixed. Therefore a decontamination procedure which moves the particles from a surface also moves the radioactive material.

In this case, the fallout particles maintain their size all through the process and since their density is high, the majority fall more vertically than the sea-water fallout (in same wind speed), and, where the initial deposits are high enough to be of concern the horizontal surfaces will be more highly contaminated than vertical ones.

For surfaces which are contaminated with more than a single layer of particles, only the bottom layer actually "contaminates" the surface: Most decontamination methods are capable of removing all the superficial layers of particles. If a method removes all but the surface layer (or, all layers and all particles greater than a given size in contact with the surface), then the amount left after decontamination is a constant, independent of initial deposit. For this process, the fraction of the mass of fallout remaining is

$$F_m = \frac{R_M}{y} \tag{6}$$

in which R_M is a constant dependent on the particle size and method of decontamination and y is the initial deposit in mass of (solid) fallout per unit area. The mass representation is used here to emphasize the fact the soil particles and not radioactive atoms are being acted upon during the contamination and decontamination process.

For surfaces which are contaminated with less than a single layer of particle, the above mechanism of removal is postulated for the fraction of the area which is covered with particles. Since the landing of a given particle on a given spot on a surface is a statistical process with the probability of the next particle landing on a "clean" spot being proportional to the clean area, the fraction of area covered at any time (say, at the end of fallout) will be

$$f = l - e^{-\alpha y} \tag{7}$$

in which α is a constant called the spreading coefficient; its value depends on the average particle size and the roughness of the surface. Thus the general equation for levels of initial deposit is

$$F_m = \frac{R_M(1-e^{-\alpha y})}{y} \tag{8}$$

The fraction, f or $(l-e\lambda^y)$, becomes one for a single layer of particles at which level equation 8 reduces to equation 6.

Equation 8 is converted to radiation intensities by means of a quantity called the mass contour ratio $M\lambda rt$), in units of the ratio mass of fallout per unit area to r/hr. Briefly, the mass contour ratio is given by

$$M_r\lambda t) = \frac{f'k'A\lambda W^{(n-1)}}{bY(t)[a_{FP}(t) + a_I t]} \tag{9}$$

in which (see equation 5)

(1) $A\lambda W^n$ is the scaling relation for the mass of material thrown out of the crater,
(2) n is an empirical constant,
(3) $A\lambda$ is an empirical parameter depending on the scaled height or depth of burst, λ
(4) λ is defined as the height or depth of burst in feet divided by the cube root of the yield in pounds of TNT,
(5) k' is a converting yield to fissions and other units, and
(6) f' is the fraction of the crater mass which mixes with the radioactive elements.

In radiation intensities, equation 8 is

$$F_r = \frac{R_M(1-e^{-\alpha M_r(t)I_r})}{M_r(t)I_r} \tag{10}$$

Harbor fallout

Harbor fallout can be either like sea-water fallout or like land fallout or a mixture of any combination of the two. A low yield burst on the surface of a deep harbor would give a sea-water fallout; a high yield detonation on the surface of a shallow harbor will produce a land fallout.

For the intermediate cases, the fallout will consist of sea water, harbor bottom (soil), bomb structural and target materials, and radioactive elements. The composition of fallout from harbor detonations can be described by their solid to liquid ratio, β.

Experimental data indicate that the additional phase (soil) to the seawater fallout induces only a negligible added interaction for most radionuclides and thus the total effect can be treated as a simple mixture of sea-water and land fallout. Where this is so a single representation can be made for all possible

types of fallout. The general equation, in terms of the ratiation intensity at H+1 hr, is

$$F_r(f)=\left[\exp-\frac{\lambda\beta}{1+\beta}M_r(1)I_r(1)\right]\sum_{j=1}^{j=n}P_j(t)F_j(t)+\frac{R_M}{M_r(1)_rI(1)}\left[1-\exp-\frac{\lambda\beta}{1+\beta}M_r(1)I_r(1)\right] \quad (11)$$

$$F_j(t)=a_{jr}(t)[I_r(1)]^{n_j(t)-1}+\frac{I_r(1)}{K_{jr}(1,t)+I_r(1)} \quad (12)$$

in which $P_j(t)$ is the fraction of the radiation intensity contributed by the radionuclides of element j; the sub r indicates an evaluation in terms of r/hr, the (1) indicates an evaluation in terms of r/hr at 1 hr after burst, and (t) indicates the parameter depends upon the time the surface has been contaminated—i. e. requires evaluation for the time, t, after burst when the decontamination is carried out.

The results of some laboratory scale data from synthetic fallout together with constants for a hypothetical nuclear bomb were used to evaluate other empirical constants. These were used to compute the final levels for a decontamination at 7 days after burst for initial levels ranging from 10 to 10,000 r/hr at 1 hr. The curves for a β of zero (sea-water fallout) to a $\beta \geqslant 1{,}000$ (land fallout) at selected intervals are plotted in figure 1.

At the present time, there is not enough data available to determine a similar three dimensional decontamination surface (or even a partial surface) for any available reclamation method-surface combination. Since large scale reclamations are expensive and difficult, the preferable experimental technique would be to investigate the nature of the decontamination surface for a series of method-surface combinations by use of small-scale experiments in the laboratory and to check these with a few carefully chosen large-scale experiments with sufficient correlation experiments to make proper adjustment of the equation parameters.

CONTAMINABILITY OF TARGETS

The vulnerability of targets to contamination by fallout must consider three parameters:

(*a*) Does the fallout contact the surface?
(*b*) Once in contact, does it remain on the surface?
(*c*) How tenaciously is it attached, i. e., how easy is it to remove?

Such information is important for:

(*a*) Predictions of the relative radiological hazard within the target complex so as to exploit this variability operationally.

(*b*) Predictions of the over-all vulnerability of structures that incorporate shielding.

(*c*) Design and planning criteria for recovery procedures.

(*d*) Design criteria to provide minimum vulnerability of targets.

Factors influencing the contaminability of targets are:

(*a*) Gross geometry and configuration which determine the airflow patterns around the target and/or influence the "drainage" of material from the target surfaces.

(*b*) Physical and chemical characteristics of surface materials, i. e., roughness, porosity, adsorbability, chemical reactivity, etc., which influence the "entrapment" of fallout and ease of loosening, removal, and transport of contaminant by decontamination processes and/or natural weathering.

(*c*) Meteorological conditions which determine the initial distribution of fallout, and its resuspension and/or redistribution.

(*d*) The physical and chemical characteristics of the fallout, i. e., type (deep water, harbor, or dry land), chemical state, particle size, density, etc., which influence the "flight" characteristics, the impact and retention characteristics and the tenacity with which it is held to the surface.

Various laboratory and field tests have been made of the contaminability of surface materials as related to fallout characteristics and angle of inclina-

tion.[1,2,3,4] The contaminability of targets as related to micrometeorology and geometry have not been studied directly, but some information has been derived from experiments with other objectives.[5] As an example, a ship was exposed to fallout from a deep-water detonation.[6,7] The fallout arrived in a 15- to 20-knot wind on the starboard beam.

The following results were obtained:

(*a*) The contamination level (240 readings) on horizontal surfaces varied from 16 percent to 400 percent of the average, i. e., the largest was 25 times higher than the lowest.

(*b*) The gamma radiation level at 3 feet above the deck varied by a factor of 10.

(*c*) The average contamination level for vertical surfaces varied from the average horizontal reading as follows:

1. Forward part of the ship: 40 percent of horizontal average.
2. Aft part of the ship: 20 percent of horizontal average.
3. Lee side: 10 percent of horizontal average.
4. Windward side: approximately equal to horizontal average.

(*d*) Test panels at the stern of the ship had an average contamination level on vertical surfaces three times higher than levels on horizontal surfaces.[8]

Such data cannot be extrapolated or used for predictions without a better understanding of all of the factors involved.

In another example, small buildings and panels of typical building materials were exposed to fallout from land detonations.[4] The contamination levels on typical roofing materials was as much as 300 times higher than that on typical wall panels; or a vertical to horizontal relationship of about 0.3 percent. For panels of the same material, vertical readings were about 10 percent of the horizontal.

The two examples indicate considerable difference in the vertical to horizontal relationships. The characteristics of the fallout appear to have had a considerable influence on this distribution. For instance, the land detonation normally produces a "dry" fallout composed primarily of material from the crater. One can expect masses of 3 to 300 grams of material per square foot to be associated with significant radiation levels at early times. The fallout being a dry powder has little tendency to stick on vertical surfaces.

The fallout from deep-water detonations is largely composed of sea water salts. However, much of the water may evaporate, leaving particles that are damp, semicrystalline masses of a sticky nature. They are capable of sticking to vertical surfaces.

As indicated very little is known of the overall problem of contaminability. It is obvious, however, that two assumptions often made, i. e., ((1) that the fallout is distributed homogeneously on a uniform infinite plane, and (2) that vertical surfaces are not appreciably contaminated) are subject to serious limitations. The ability of a tactical force and/or a civilian population to exploit the variability of the fallout pattern depends upon knowledge we do not have on contaminability.

The contaminability of personnel exposed to the fallout event or working and living in contaminated environments is largely unknown. A study[9] indicating the significance of beta contact hazard to personnel and a requirement for the mass decontamination of personnel, emphasizes the need for additional contaminability information.

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[1] Gevantman, L. H., B. Singer, T. H. Shirasawa, Contaminability of Selected Materials, USNRDL—TR-11.

[2] Gevantman, L. H., J. F. Pestaner, B. Singer, D. Sam, Decontaminability of Selected Materials, USNRDL-TR-13.

[3] Lane, W. B., R. K. Fuller, L. Graham, W. E. Shelberg. Laboratory Studies of the Decontamination of Repeatedly Contaminated Surfaces, USNRDL-TR-59 (confidential).

[4] Strope, W. E., Protection and Decontamination of Land Targets and Vehicles, Operation Jangle, project 6.2, AFSWP-WT-400.

[5] Lee, H., M. B. Hawkins, Some Considerations of the Geometrical Distribution of Fallout Radiation Sources Over Targets, Proceedings of the Shelding Symposium held at USNRDL October 17–18, 1956, vol. II (USNRDL report in preparation), secret.

[6] Molumphy, G. G., Captain, USN, Bigger, M. M., Proof Testing of AW Ship Countermeasures, Operation Castle final report, project 6.4, USNRDL 0012361.

[7] Lee, Hong, Technical Survey Data for Operation Castle, project 6.4, USNRDL TM-49.

[8] Maloney, Joseph C., et al., decontamination and protection, Operation Castle, project 6.5, AFSWP-WT-928.

[9] Broido, A., Teresi, J. D., requirements for mass decontamination of personnel, USNRDL-TR-38, April 1955 (secret RD).

Cost of Reclamation

Considerable data has been collected regarding the effectiveness of reclamation of targets contaminated by local fallout. The feasibility of applying these methods depends upon the following parameters:

(a) The time required to perform the reclamation must be short enough to make an appreciable saving in radiological exposure to mission personnel,

(b) The radiation exposure to reclamation personnel must be justified by the saving in exposure of mission personnel,

(c) The effort (manpower) and logistics required to reclaim the target must be compatible with the total effort available.

Thus, the cost of reclamation as measured in operating time, effort, radiation exposure, equipment, and supplies is an important determination.

It is impossible to generalize on these quantities for they are influenced by many factors.

The type of fallout, whether it be from a deep water, harbor or land detonation, influences the rate and/or method of decontamination. A deepwater-type fallout can be removed only to an extent of about 60 percent for a firehosing, scrubbing operation on ships,[1] the rate being about 40 square feet per minute. The same decontamination procedure at 6 times the rate of operation on a paved area contaminated by dry-land-type fallout will yield a removal of about 98 percent.[2] To achieve an equivalent removal on the ship, a surface removal technique would be required. Typical rates of operation are about 20 feet per minute for paint stripping[3] and about 7 feet per minute for removing a ⅛-inch thick layer of wood from the flight deck.[4]

The amount (or mass) of fallout on a surface influences the rate, particularly for harbor and dry-type fallout that must be transported over horizontal surfces for considerable distances. The following table shows an example of how the rate decreases with increasing masses of dry fallout for motorized flushing.[2]

Dry fallout gm/ft:[2]	*Motorized flushing rate, ft.²/min.*
10	670
33	650
100	580
330	300

The mass of fallout has no effect on the rate of operation for surface removal or earth moving techniques.

The rate of operation is influence by the surface characteristics of the target, rough surfaces, e. g., wood shingles, requiring longer time than smooth, e. g., metal surfaces. The following table is an example of the influence of surface roughness on rate of operation:[2]

Firehosing of dry contaminant

Material	Effectiveness (percent removed)	Rate (ft²/min/hose)
Corrugated metal	97	65
Composition shingles	95	50
Wood shingles	89	35

The rate of reclamation by earth moving is influenced by soil characteristics. Standard earth moving practice has developed considerable information on this subject.

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[1] AFSWP, ITR 1323, preliminary report, Operation Redwing, project 2.9, Standard Recovery Procedure for Tactical Decontamination of Ships. Confidential.

[2] Field Evaluation of Cost and Effectiveness of Basic Decontamination Procedures for Land Target Components, Sartor, J. D., Curtis, H. B., etc., USNRDL–TR in preparation. Unclassified.

[3] Rates approaching 50 square feet per minute are possible if removal of only the surface layer of paint gives the required reduction in radiation intensity.

[4] Proof Testing of AW Ship Countermeasures, Operation Castle, project 6.4 WT-927, Molumphy, Bigger. Confidential.

The degree of mechanization obviously influences rate of operation. The following example compares firehosing rate with that of motor flushing for harbor-type fallout. Also shown are the influence of mechanization on effort and radiation exposure.[2, 5]

Criteria for comparison	Actual performance or cost		
	Firehosing	Motorized flushing	Relative cost FH/MF
1. Operating rate per unit, hr/10^6 ft^2	222	30	7.4
2. Personnel required per unit	5½	2	2.75
3. Effort (direct labor), man-hr/10^6 ft^2	1,210	60	20.0
4. Radiation shielding factor	1.0	0.5	2.0
5. Relative cost in radiation dose	1,210	30	40.0

Target complexity obviously influences rate of operation. For optimum performance, spacings between target components must be large enough to permit mechanized equipment to be used.

A simplified example will help indicate the time, manpower, and basic supplies required for recovery of a target complex. The following criteria are assumed:

(*a*) Target: City of San Francisco.
(*b*) Fallout: Harbor-type at 33 gms/ft^2.
(*c*) Area to be recovered: About 25 square miles consisting of—
1. All paved areas.
2. All industrial and commercial areas and buildings.
3. 50 percent of the park areas.
4. 10 percent of the residential areas and buildings.

(*d*) Methods: Firehosing and earth moving.

The following table indicates an estimate[5] of the cost of reclaiming these critical areas:

Cost of decontaminating critical areas of San Francisco through use of available firefighting and earth moving equipment for removing slurry contaminant

	Firehosing			Earth moving, land areas	Grand total
	Roofs	Paved surfaces	Subtotal		
1. Time to complete decontamination (24-hour days)	16.8	11.7	28.5	13	----------
2. Direct labor (number of men)	----------	----------	4,000	2,800	6,800
3. Total labor, direct and support (number of men)	----------	----------	6,000	4,900	10,900
4. Total effort (8-hour man-days)	101×10^3	70×10^3	171×10^3	64×10^3	235×10^3
5. Labor cost at $10 per man-day	----------	----------	1.71×10^6	0.64×10^6	2.35×10^6
6. Water required for decontamination (gallons)	362×10^6	314×10^6	676×10^6	----------	----------
7. Fuel required (gallons):					
(*a*) Gasoline	145,000	101,000	246,000	95,000	341,000
(*b*) Diesel fuel	----------	----------	----------	195,000	195,000

As can be seen, the reclamation is feasible in what appears to be a reasonable time. The amount of equipment required is within the capability of existing sources in San Francisco. The manpower is not too excessive considering the numbers of people available. The water requirements are within the capability of the normal supply. Fuel consumption is less than normal daily requirements. The greatest problem would undoubtedly be that of organizing, training, supervising, and controlling 11,000 men.

Automatic decontamination devices such as the washdown system have, as an important advantage, the capability of reclamation at very early times with no expenditure of manpower or radiation exposure. They can be extremely effective (i. e., removal of 90–95 percent) even on sea-water-fallout.[4] However, they do require expenditure of funds before the war begins.

[5] Engineering Approach to Radiological Decontamination, Hawkins, M. B. (Paper to be given ASME semiannual meeting, San Francisco, June 1957.) Unclassified.

The Need for a National Program in Nuclear Countermeasures

The full exploitation of nuclear weapons and nuclear power requires that full preventive measures be employed at all times to keep potential exposure below hazardous levels, *and* that the capability of reclamation after any nuclear mishap be high.

The current policies of the AEC and DOD, backed by the very competent Health and Safety Division in the AEC installations, have provided a generally satisfactory national program based on *preventive* and control measures. No similar program exists to fulfill the reclamation requirement, or to prepare the way for successfully coping with a general increase of radioactive background above that imposed from natural sources. Such an increase is inevitable—both on a general scale and on a more limited scale. The general buildup is predominantly related to weapon detonations. The more limited scale is confined to such areas as the exclusion zones of the Nevada test site, the Reactor Test Station, Arco, Idaho, etc. From the other extreme, there is an increasing demand to establish contamination specifications for the general release of previously contaminated equipment into the established industrial channels.

It is proposed that a positive national program of nuclear countermeasures development be undertaken to add preprotection and reclamation capability to the established preventive measures.

Four completely different types of end-use application are apparent:

(1) Increasing the nuclear resistance of military operating forces in the field.

(2) Continental defense of the United States in time of total nuclear war.

(3) Reclamation from a nuclear mishap in times of peace.

(4) Adaptation of the established economic system to absorb the applications of nuclear energy that are already developed, or under development.

The four applications are common in that one is faced with the impact of radiation and radioactivity on the civilian and/or noncombatant society. The control techniques used during the research, development, and production phases are no longer sufficient since these techniques rely solely on control and preventive measures.

The four applications differ in that the criteria relating the nuclear or radiological environment to the permissible dose or acceptable hazard are not completely common.

However, much of the research and development up to the point of final application is interchangeable. A unified research and development program should have a large payoff value on all four fronts. It also appears that the DOD, AEC, FCDA, and possibly the PHS all have a vital interest in such a unified program.

Summary of Nuclear Weapon Countermeasure System Development Program Proposal (Special Test Program Component)

This memorandum summarizes the proposals advanced by the United States Naval Radiological Defense Laboratory as representing the fastest and most economical way of developing a national capability in nuclear weapon countermeasures. This proposal has formed the basis of discussions with Mr. R. L. Corsbie and Dr. A. W. Bellamy, representing the Atomic Energy Commission, concerning the manner in which nuclear weapon tests could be more profitably exploited. The same proposal has been discussed with the FCDA Planning Office, but has not been formally developed.

To insure the greatest level of national readiness in minimum time, the nuclear weapon countermeasure system must be developed with the same care that has gone into the development of the offensive weapon systems. It is proposed that a nuclear weapon countermeasure system development program be established, and that a *proof test* of the proposed system be made at a special test to be conducted in accordance with the attached schedule. This test will:

(1) Proof test proposed standard shelter designs.

(2) Proof test proposed rapid reclamation systems.

(3) Establish an experimental basis for determining criteria required to achieve final recovery.

Such a program cannot succeed if projects are submitted by invitation to all agencies that may have an interest in the subject. The test projects must be carefully planned to proof test an integrated system and must carry through all three time phases: emergency, operational recovery, and final recovery. The United States Naval Radiological Defense Laboratory believes it has the competence to develop such an integrated system. Adequate criteria exist to justify an operational development program of both emergency and operational recovery phases. Adequate criteria do *not* exist to establish feasible systems for the final recovery phase. Therefore, this test must be used to aid in the experimental determination of such criteria including food management and agricultural reclamation. The capability of existing instrumentation and doctrine to provide the required radiation and operational control data will be able to be determined realistically.

This program is only one part of the required national weapon countermeasure program. However, it provides the essential base against which real progress can be evaluated.

The proposals submitted to Mr. Corsbie for Operation Plumbbob are developmental projects covering some aspects of the emergency and operational recovery plans.

Outline of Essential Timetable

R&D program leading to selection of system components and establishment of necessary projects	Test date minus 3 years	
	Test date minus 2 years	Site selection and program scope completed
	Test date minus 1 year	Submission of complete test program plan
Construction, equipment procurement, training		
	TEST DATE	
Operational recovery begins Shelters, situation appraisal, etc.		Emergency phase complete
Transition to final recovery begins	Test date plus 6 months	Final report on emergency phase program
	Test date plus 9 months	Operational recovery complete
	Test date plus 18 months	Final report on operational recovery phase program
Experimental program to measure incorporation of critical elements into the environment and their uptake into animals. Agricultural reclamation experiments.	Test date plus 3 to 5 years	

Evaluation of the state of knowledge relating to radiological countermeasures development[1]

	Continental defense	Peacetime application
Transport mechanism	A	C
Nuclear and chemical properties	A	B
Contamination-decontamination phenomena	C	C
Reclamation methods	B	C
Component development	C	C
Shielding and terrain effects	B	B
Shelter development	B+	(2)
Final recovery	C	C
Systems development and analysis	B	B

[1] This table was developed by Dr. Paul C. Tompkins, USNRDL, Mr. R. Corsbie, and Mr. J. Deal, DBM of the AEC, to guide the development of projects to improve the nuclear weapons defense capability of the Atomic Energy Commission. It has been examined by the technical staff of the USNRDL and is considered to be a fair evaluation of the current state of real knowledge.

[2] Not applicable.

NOTE.—State of knowledge and effectiveness of application based on the ability to apply determinable numbers in a wide range of actual cases:
A—Adequate for practical applications
B—Inadequate for practical applications
C—Little known

(Submitted by Department of Defense)

U. S. NAVAL RADIOLOGICAL DEFENSE LABORATORY

SAN FRANCISCO, CALIF

From: Commanding officer and director.
To: Chief, Bureau of Ships (code 110).
Subject: Congressional hearings before the Joint Committee on Atomic Energy concerning the nature of radioactive fallout and its effects on man, scheduled for May 27 through June 7, 1957.
Reference:
(*a*) Ch, BuShips ltr A18 (110) Ser 110–1447 of June 6, 1957.
(*b*) CO and Dir, USNRDL Conf ltr 900–0801 PCT: icm of May 16, 1957.
(*c*) CO and Dir, USNRDL ltr 900–803 RCL: rts of May 23, 1957.
Enclosure: (1) Biographies of contributors to written statements submitted for subject hearings.

1. As requested by reference (*a*), enclosure (1) forwards biographies of contributors to the written statements submitted for the subject hearings, arranged in an alphabetical listing. The authors and identifying titles of the USNRDL survey of various aspects of radiological fallout from nuclear weapons are given below in the order submitted.

(*a*) Reference (*b*):

I.	Prediction of Fallout	E. A. Schuert C. F. Ksanda
II.	Measurement of Fallout	T. T. Triffet
III.	Physical and Radiochemical Properties of Fallout	N. E. Ballou C. W. Adams
IV.	Environmental Aerosol Analysis	A. L. Baietti
V.	Radiological Countermeasures	C. F. Miller

(*b*) Reference (*c*):

I.	Mass-Activity Relationships Derived from Fallout Measurements.	C. F. Miller
II.	Relation of Radioactive Decay to Countermeasures.	C. F. Miller
III.	Physical Chemistry of Fallout as It Relates to Decontamination Countermeasures.	C. F. Miller
IV.	Contaminability of Targets	M. B. Hawkins
V.	Cost of Reclamation	M. B. Hawkins

VI. General information on a nuclear countermeasures program:	
A. The Need for a National Program in Nuclear Countermeasures.	P. C. Tompkins
B. Summary of Nuclear Weapon Countermeasure System Development Program Proposal.	
C. Evaluation of the State of Knowledge Relating to Radiological Countermeasures Development.	

2. The material submitted by the USNRDL was reviewed and edited by Drs. E. P. Cooper and E. R. Tompkins. Because of their valuable conributions, their biographies are included in enclosure (1).

By direction:

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730 W. 27th Ave., San Mateo, Calif. Home phone FIreside 1–0842, office phone MIssion 8–6900, ext. 240. Date and place of birth: 1922, Sharon, Pa. Education: B. S., Physics, Case Institute of Technology, Cleveland, Ohio, 1943; Grad. work in physics at University of Illinois, 1946–47. Work History: Kellex Corp., Oak Ridge, Tenn., 1943–45; Health Physicist, Clinton Nat. Lab., Oak Ridge, Tenn. 1945–46; Proj. Engr. Kellex Corp. 1947–49; Tech. Asst. to Mgr. Jackson & Moreland Co., Knolls Atomic Power Lab., Schenectady, N. Y., 1950; Naval Radiological Defense Lab., San Francisco 24, Calif., 1950– .

Ballou, Nathan E.

1531 Campus Drive, Berkeley, Calif. Home phone THornwall 5–0259, office phone MIssion 8–6900, ext. 508. Date and place of birth: 1919, Rochester, Minn. Education: B. S., Minnesota State Teachers College, 1941; M. S., Illinois, 1942; Ph. D., Chemistry, Chicago, 1947. Work History: Teaching asst. Chem. Ill., 1941–2; research asst., Metallurgical Lab., Chicago, 1942–43; Clinton Labs, Oak Ridge, 1943–44; assoc. chemist, 1945–46; res. chemist Hanford Engineering Works, Wash., 1944–45; Nat'l Research Council Fellow, Chicago, 1946–47; research assoc. Radiation Lab., Calif., 1947–48; Naval Radiological Defense Lab., San Francisco 24, Calif. 1948–51, Hd. Nucl Phys Chem Br. 1951– .

Cooper, Eugene P.

48 Lakemont Drive, Daly City, Calif. Home phone PLaza 6–0176, office phone MIssion 8–6900, ext. 402. Date and place of birth: 1915, Somerville, Mass. Education: B. S., Mass. Inst. Tech., 1937; Munich, 1935; Ph. D., theoretical physics, California, 1942. Work History: Asst. prof. physics, North Carolina, 1941–43; research physicist, Franklin Inst., Philadelphia, 1943–45; USN Ord. Test Sta., Inyokern, Calif., 1945–47; assoc. prof. physics, Oregon, 1947–48; research physicist, USN Ord. Test Sta., Pasadena, Calif., 1948–51; Assoc. Sci. Director, Naval Radiological Defense Lab., San Francisco 24, Calif., 1952– .

Hawkins, Myron B.

2600 La Honda Ave., El Cerrito, Calif. Home phone BEacon 3–3591, office phone, MIssion 8–6900, ext. 517. Date and place of birth: 1920, Indianapolis, Ind. Education: B. S. M. E., Purdue, 1942. Work History: Processing engr., Bendix Products Div., Ind., 1942–44; Assoc. Engr. Oak Ridge Nat. Lab., 1944–48;

Mech. Engr., Isotopes Div., Atomic Energy Cmn. 1948; Treas, and Ch. Engr. Scientific Service Inc., 1948–50; Naval Radiological Defense Lab., San Francisco 24, Calif., 1950–51; Hd Tech. Dev. Br. 1951– .

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292 Glen Drive, Sausalito, Calif. Home phone EDgewater 2–2257, office phone, MIssion 8–6900, ext. 421. Date and place of birth: 1921, Washington, D. C. Education: B. S., Univ. of Maryland, with additional 12 sem. hrs in Physics, 1942; 26 sem. hrs. at Geo. Wash. U. in 1951. Work History: Physicist, BuShips 1941–42, Washington, D. C.; Weapon Capabilities Br., Naval Radiological Defense Lab., San Francisco 24, Calif., 1951– .

Miller, Carl F.

24 Roosevelt Circle, Palo Alto, Calif. Home phone: None. Office phone: MIssion 8–6900, ext. 423. Date and place of birth: 1918, Osceola, Wisconsin. Education: B. S., Math., State Teachers College, River Falls, Wisc. 1940; M. S., Chemistry, Univ. of Calif. 1948; Ph. D., Chemistry, Iowa State College, Ames, Iowa, 1951. Work History: Jr. Chemist, Iowa State Col., 1948–51; Naval Radiological Defense Laboratory, San Francisco 24, Calif., 1952; Hd, Countermeasures Evaluation Br., 1956– .

Schuert, Edward A.

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Triffet, Terry

3912 Nelson Drive, Palo Alto, Calif. Home phone: YOrkshire 7–4963; office phone, MIssion 8–6900, ext. 478. Date and Place of Birth: 1922, Enid, Oklahoma. Education: B. A., Univ. of Oklahoma (Lib. Arts) 1945; B. S. Eng., Univ. of Colorado, 1948; M. S. Eng., Univ. of Colorado, 1950; Ph. D., Eng., Stanford, Univ., 1957. Work History: Univ. of Colorado, instructor, 1947–50; Gen. Engr., U. S. Naval Ordnance Test Station; Gen. Engr., Naval Radiological Defense Laboratory, San Francisco 24, Calif., 1955– .

Tompkins, Edward R.

1341 Hull Drive, San Carlos, Calif. Home phone: LYtell 3–0935; office phone, MIssion 8–6900, ext. 470. Date and place of birth: 1908, Winterset, Iowa. Education: A. B., Greeley State College, 1931: summers, Washington, 1930, Wisconsin, 1935; M. A., California, 1941; Ph. D., biochem., 1942. Work History: Teacher, Denison High School, Iowa, 1932–37; Teaching Asst., Bichem., Calif., 1938–39; Res. Chemist, Armour Research Foundation, Chicago, 1942–43; Research Chemist, Metallurgical Lab., Chicago, 1943; Research Chemist and Grp. Ldr., Clinton Lab., Oak Ridge, 1943–47; consultant, Advisory Field Service Br., Isotopes Div., 1947–48; Dir. Res., Sci. Service, Inc., 1948–51; Hd., Chem. Tech. Div., Naval Radiological Defense Laboratory, San Francisco 24, Calif., 1951– .

Tompkins, Paul C.

2765 Summit Drive, Hillsborough, Calif. Home phone: DIamond 4–4774; office phone, MIssion 8–6900, ext. 400. Date and place of birth: 1914, Walla Walla, Wash. Education: A. B., Whitman College, 1935; Chicago, 1936–37; Ph. D., biochemistry, California, 1941. Work History: Asst. biochem., Calif., 1940–41; chem., Stanford, 1941–42; Research Assoc., 1942–43; Metallurgical Lab., Chicago, 1943–45; Sr. Chemist, Clinton Lab., 1945–47; Prin. Biochemist, Oak Ridge Nat. Lab., 1948–49; Staff Advisor to Sci. Director, Naval Radiological Defense Laboratory, San Francisco 24, Calif., 1949–50; Assoc. Sci. Dir., 1950–51; Sci. Dir., 1952– .

DEPARTMENT OF THE NAVY

OFFICE OF NAVAL RESEARCH

Washington, D. C.

From: Chief of Naval Research.
To: Chief of Legislative Liaison.
Subject: Congressional hearings before the Joint Committee on Atomic Energy concerning The Nature of Radioactive Fallout and its Effects on Man, scheduled for May 27 through June 7, 1957.
Reference: (a) OLL: INV: ACJ: gms memo 6–1182 of June 3, 1957.

1. In accordance with reference (a), the following biography is submitted:

Lockhart, Luther B., Jr.

115 Devon Drive, Falls Church, Va., Home phone JEfferson 4–4083, Office phone, JOhnson 3–6600, extension 340. Date and place of birth: 1917, Atlanta, Ga. Education: A. B., Emory, 1938; Sigma XI, Phi Beta Kappa; Ph. D. (Org. Chem.), North Carolina, 1892; Ethel-Dow Corp. Fellowship, North Carolina, 1939–40: research assoc., naval research project, North Carolina, 1942–43. Work history: research chemist, Naval Research Laboratory, 1943–; head, radiochemistry section, 1948–54; head, high polymers branch, 1954–present.

A. B. METSGER,
Deputy and Assistant Chief of Naval Research.

HEADQUARTERS, AIR WEATHER SERVICE,
MILITARY AIR TRANSPORT SERVICE,
UNITED STATES AIR FORCE,
Washington, D. C., May 17, 1957.

STATEMENT BY LEROY H. CLEM[1] OF THE AIR WEATHER SERVICE ON FALLOUT PREDICTION

The prediction of radioactive fallout is a combined meteorological and radiochemical problem. The mission of the Air Weather Service does not include any treatment of the radiochemical portion of the problem but is confined to the indication under emergency wartime conditions of the effect of only the meteorological variables on the transport of radioactive debris during its fall back to earth within a few hundred miles of the site of a nuclear detonation. To accomplish this mission requires accepting certain rather gross assumptions about such nonmeteorological variables as the initial distribution of contaminated particles in the stabilized cloud and the particle fall rates. Thus, given these gross assumptions, the only additional factors with which the Air Weather Service forecasters are concerned are the time and location of the occurrence of a nuclear detonation and the best estimate of the wind field through which the radioactive debris will fall.

The idealized meterological solution to this problem involves the computation of a whole family of three-dimensional trajectories starting from various levels of the atomic cloud. By considering changing wind conditions, these trajectories represent the fall paths of a variety of particles (of differing size and shape) which were present at the various levels in the cloud when it stopped rising. Such a solution is beyond the present state of the science of meterology. Therefore, the Air Weather Service, as well as other agencies, had to make certain simplifying meterological assumptions in order to be able to handle the forecast problem.

The generally accepted method is based on the following main assumptions:

(*a*) The winds at selected levels in the middle of layers (e. g., 10,000 feet thick) are representative of the windflow that would effect the drift of the particles while they are falling through that layer.

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[1] Bachelor of science degree, Brown University; certificate for graduate meteorological engineering, New York University; master of science degree in meteorology, Massachusetts Institute of Technology; 13 years of professional experience in meteorology; during World War II served as an aerological officer in the Navy; several years experience with the U. S. Weather Bureau; serves as the expert on fallout-prediction problems, high level wind problems and forecast capabilities. Assigned to the Technical Service Branch, Technical Requirements and Services Division of Scientific Services of Headquarters, Air Weather Service. (Submitted by Department of Defense.)

(*b*) The wind sounding (observed or forecast) for the upper-level winds applicable at the point of detonation is representative of the windflow throughout the course of the ensuing fallout.

It is believed that assumption (*a*) does not introduce much error; however, assumption (*b*) is both the heart of the technique and the most questionable of these two assumptions. Although experience has shown that persistence forecasts (implied in (*b*)) of upper-level winds are equal to or better than other available forecast methods for the first few hours, variability of the wind, when coupled with the inaccuracies of wind observations and forecasts, introduce sizable errors in the method. The average error in the resultant 6-hour forecast fallout plot derived from this method is of the order of ± 10 to ± 30 degrees in direction and 30 to 40 percent in distance. The forecast errors involving fallout coming from the higher levels of radioactive clouds formed by large weapons are relatively smaller than those for fallout from the lower levels of the clouds.

However (even in view of these inaccuracies), by using the stated meteorological assumptions, it is possible to compute the most probable geographical area that may be contaminated by radioactive fallout following a surface or near-surface burst of a nuclear weapon. There are more sophisticated and time-consuming computational techniques available, involving more complex assumtions than (*b*) which are supposed to take care of some of the variability of the wind. However, evaluation tests have indicated that the slight decrease in the resultant errors from using these more complex methods does not justify the extra computational time and effort involved. The Air Weather Service method, based on a very simple and versatile 6-hour wind-vector technique, is equally useful in this country as well as overseas and can be evaluated for any desired area on the earth's surface or at altitudes normally used by aircraft. This method consists of adding the wind vectors from the layers through which the particle will fall and then (with an assumed fall-rate) a fallout plot is developed from this. This results in a time-space plot which delineates the areas which may receive fallout and the expected time of occurence. It should be noted that no attempt is made in this method to forecast levels of radiation intensities, relative or absolute.

Although this method for forecasting the close-in areas which are expected to be contaminated by falling radioactive debris does not achieve the accuracy and precision ideally desired (because of many unresolved factors, some of which are nonmeteorological), its accuracy (± 10 to ± 30 degrees in direction and 30 to 40 percent in distance from ground zero) is in line with the current state of the science. There are no techniques available today which can provide more than very generalized answers in a forecast situation involving large weapons.

STATEMENT BY COL. B. G. HOLZMAN[1] AND COL. NORAIR M. LULEJIAN,[2] AIR FORCE RESEARCH AND DEVELOPMENT COMMAND

Question. How does your organization predict fallout, given weapon yield, height of burst, type of terrain, and meteorological conditions? How reliable do you feel these forecasts are?

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[1] Born: Los Angeles, Calif., January 25, 1910. 1931: B. S., California Institute of Technology. 1933–34: Ph. D., candidate graduate study, California Institute of Technology. June 1945 to July 1945: Alamagordo, N. Mex.: meteorological adviser on first atomic test. August 1945 to January 1946: Asheville, N. C.: Chief, Research and Evaluation Division, Weather Service. February 1946 to August 1946: Washington, D. C., and Bikini, Marshall Islands: Atomic bomb tests, staff weather officer, Joint Task Force 1. September 1946 to August 1947: Washington, D. C.: Headquarters, air staff, research and development officer for atmospheric sciences. September 1947 to June 1948: Washington, D. C., Eniwetok, Marshall Islands: Atomic bomb tests, staff, weather officer, Joint Task Force 7; executive officer, long-range detection (AFOAT–1). July 1948 to June 1950: Washington, D. C.: Chief, Geophysical Sciences Branch, research and development, Headquarters, USAF. June 12, 1950, to August 23, 1951: Washington, D. C.: Chief, Research Division, assistant for atomic energy, headquarters, USAF. January, February, 1951: Nevada: Operations consultant to AEC, Nevada ranger, atomic tests. August 23, 1951, to June 17, 1952: Washington, D. C.: National War College. June 17, 1952, to September 1, 1952: Baltimore, Md.: Headquarters, Air Research and Development Command. September 1, 1952, to May 30, 1955: Albuquerque, N. Mex.: Deputy for Research and Development, and Chief of Staff, Special Weapons Center, Kirtland Air Force Base. May 30, 1955, to present: Baltimore, Md.: Director, Air Weapons, Headquarters, Air Research and Development Command. (Submitted by Department of Defense.)

[2] Native of Hawthorne, N. J., is a graduate of Columbia University (B. S. in 1939); worked as a research chemist for Ortho Products, Inc., in Elizabeth, N. J., for a period of

Footnote continued on following page.

Answer. The Air Force Research and Development Command has developed a simple method to predict the general area within which fallout may occur. An attempt was made for several years to develop a more rigorous treatment of the fallout problem; however, this approach was abandoned because of the inherent time and space variability of the atmospheric winds which introduces large errors in all fallout predictions. It is the opinion of Air Force Research and Development Command that in view of the large errors introduced by the variability of the winds, it is pointless to attempt a precise model of the radioactive distribution in the atomic cloud. The fallout pattern influenced by different types of terrain are completely masked by the very large errors introduced because of variability of the winds. Experience during past atomic-test operations has verified the fact that even when zero-hour-measured winds were used, thus eliminating all wind-forecast errors, the actual fallout never occurred in the exact region indicated by the observed winds. There was always a displacement varying up to 45° of the actual fallout from its calculated position. The reason for this is simple. The observed winds are valid over the target area for a relatively short time. Fallout may reach a distance of 150–250 miles downwind and it may take 6–18 hours for the local fallout to be completely deposited on the ground. During this 6–18 hours the winds change both in direction and in speed. There is not only this time variation, but also the space variation. In other words, the winds aloft over ground zero are not necessarily the same as winds 150 miles downwind. Furthermore, the method of measuring winds by the use of balloons introduces another serious error because of the inability to specify the true wind profile directly over ground zero.

It is questionable whether the time and space variability of the winds could be forecast with an accuracy exceeding ±15° in the direction of the winds. An error of 15° would spell the difference between an airbase or a city receiving either no radiation at all or possibly lethal concentrations of radiation from the local fallout of a single weapon.

We have considered fallout from the offensive and defensive points of view. For defensive purposes before we can make any reasonable estimates of the local fallout plot we need to know the total yield as well as the fission yield of the weapons, height of burst, and coordinates of ground zero. Even with all this information it will be impossible to delineate the fallout pattern with sufficient accuracy to predict whether a given military installation downwind of ground zero would receive no, little, or lethal concentrations of radiation. For offensive use a further complexity is introduced because most probably very little if any reliable meteorological information will be available over enemy territory. Therefore, in spite of our knowledge of the yield, height of burst, and ground zero on bombs, the fallout pattern over enemy territory would be equally if not more difficult to predict.

When many multimegaton weapons are exploded, the calculation of reasonably accurate fallout patterns becomes even more difficult. The validity of the requirement for ascertaining fallout patterns under these conditions becomes even more questionable because lethal contamination will occur over large overlapping areas regardless of the meteorology.

However, the above should not be construed to mean that we are unable to make generally correct predictions; for instance, as to what countries or large areas shall lie inside or outside the dangerous part of fallout patterns from distant bursts. A choice of burst height that prevents the fireball from touching the earth, essentially eliminates local fallout.

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2 years; entered military service in September 1942, and enrolled as an air cadet in meteorology in January 1943 at Grand Rapids, Mich.; received his commission as second lieutenant in September 1943; returned from overseas early in 1946 and served with Headquarters Air Weather Service until September 1946, after which time he was transferred to Headquarters, USAF, in the Pentagon. He left Headquarters, USAF, to enter a 3-year course in radiological engineering in September 1948; assigned to Headquarters, ARDC, in Baltimore since June of 1952. He is Chief of the Nuclear Applications Division of the Directorate of Air Weapons. (Submitted by Department of Defense.)

STATEMENT BY DR. DONALD M. SWINGLE[8] OF THE ARMY SIGNAL CORPS, EVANS, S. C., LABORATORY, FOR SUBMISSION TO THE JOINT COMMITTEE ON ATOMIC ENERGY

Question. How does your organization predict fallout, given the weapon yield, height of burst, type of terrain, and meteorological conditions? How reliable do you feel these forecasts are?

Answer. 1. The principles of fallout prediction used in the Signal Corps method are outlined below:

(*a*) The prediction procedure described assumes a surface burst of a nuclear weapon. Given the weapon yield, the dimensions of the cloud are estimated from a survey of previous test data. (See discussion below.)

(*b*) The cloud is split into layers, on the basis of available data on particle-size distribution, and the particle sizes in each layer are considered.

(*c*) Where each particle size in each part of the cloud will land is determined by trigonometry, considering rate of fall and the effect of wind. It is assumed that the latest available wind data is representative.

(*d*) Then the ground position of arriving fallout is plotted and overlapping contours are added together.

(*e*) A meteorological contour analysis of the resulting pattern is made and this is analyzed for dose-rate information.

(*f*) The time of arrival on the ground of each particle size for each original slice is calculated and analyzed for *time of arrival* and *time of ending*, if wanted, of fallout material.

(*g*) From consideration of the time of arrival and dose rate, the *total dose* for 48 hours is approximated.

In the above procedure, terrain has not been specifically considered.

2. The effective radiological activity predicted would be affected by the estimated height of the cloud. The ratio of fission to total yield of the burst will affect the estimated dose rates and the total dose.

3. If the height of the cloud is known, the height to which wind data is required is immediately known. Assuming the height of the cloud is not known, the height and diameter may be estimated from the meteorological conditions. The criterion for estimating the cloud height is the height of the tropopause. This is accomplished by adjusting the basic pattern design for location of burst and for the season.

4. The accuracy of fallout prediction is directly related to the accuracy of the basic information. The degree of reliability of the several factors and the variation of wind with time and space prohibit exacting prediction. The Signal Corps is studying the question of attainable accuracy. But data for vertification of pattern design is limited. Considering the factors outlined above and when the fission to total yield is known, it is hoped to attain fallout prediction within a factor of 2 for points within contour lines of 100 roentgens per hour or greater.

5. The details of a Signal Corps draft method of fallout prediction are being reviewed and the method is being modified. Consideration is being given to the effect of time and space variations of the wind. The present method utilizes an effective wind consideration at the point of burst to be applied throughout the pattern computation.

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[8] Physicist GS–14. Chief, Meteorological Techniques Secton, Meteorological Branch, Physical Sciences Division, Evans Signal Laboratory, U. S. Army Signal Engineering Laboratories, Fort Monmouth, N. J. Graduated Theodore Roosevelt High School, Washington High School, Washington, D. C., 1939; bachelor of science in math-science from Wilson Teachers College, 1943; master of science in meteorology from New York University, 1947; master of arts in engineering science and applied physics from Harvard University, 1948; doctor of philosophy in engineering science and applied physics from Harvard University, 1950. American Meteorological Society, World Meteorological Organization Working Groups, Institute of Radio Engineers, American Geophysical Union, American Association for the Advancement of Science, American Institute of Electrical Engineers, ad hoc groups under established coordinating agencies. Professional engineer in States of New York and New Jersey. During World War II participated in the first modification of existing radars for the specific purpose of weather observation. He has been intimately connected with later development of equipment and the development of radar techniques for observations of storms, precipitation areas, and other specialized parameters. Techniques concerned with local meteorology of particular importance to Army operations is another concern of his. Leading Army scientist concerned with methods of predicting fallout. His present efforts are directed toward a practical field method of prediction which will lead to information of sufficient detail for Army use. (Submitted by Department of Defense.)

MAY 24, 1957.

TECHNICAL PRESENTATION FOR THE JOINT COMMITTEE ON ATOMIC ENERGY HEARINGS ON THE SUBJECT, THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN, MAY 27–29 AND JUNE 3–7, 1957

Specifically on—

Topic VI. Atmospheric Transport, Storage, and Removal of Particulate Radioactivity
Topic VII. Local Fallout
Topic VIII. Delayed Fallout

Submitted by James G. Terrill, Jr.,[a] Chief, Radiological Health Program, Division of Sanitary Engineering Service, Public Health Service, United States Department of Health, Education, and Welfare

VI. ATMOSPHERIC TRANSPORT, STORAGE, AND REMOVAL OF PARTICULATE RADIOACTIVITY

Public Health Service fallout activities have emphasized the collection of data on the actual exposure of people which data can be used to modify operational procedures to reduce the exposures and to serve as a basis for studying possible chronic radiation effects.

B. Local fallout

Local fallout is initially of concern as an acute external gamma or beta irradiation hazard. For this reason our off-site radiological safety operations in Nevada and in the Pacific are based on external gamma readings obtained with portable survey instruments. This system of operation is based on the assumption that beta concentrations during this period are substantially in proportion to the gamma intensities. This assumption has been confirmed, in general, by results of beta measurements of air samples collected during the fallout periods in Nevada. Local fallout may, and has become of concern as an internal beta emitter after its decay to a level at which the gamma irradiation is no longer of concern from the standpoint of acute effects. Up to this time the Service has not attempted to measure alpha concentrations in local (or delayed) fallout although the amounts are presumed to be low.

A report of local fallout sufficiently detailed to be used for public health purposes is the Report of Off-Site Radiological Safety Activities from Operation Teapot conducted at the Nevada test site in the spring of 1955, prepared jointly by the Las Vegas Branch Office of the Atomic Energy Commission and the Public Health Service.[1] Comments concerning the predictability of local fallout and observed patterns of local fallout will be based on this report.

The Teapot report outlines Public Health Service responsibilities and the supporting services, including air support, provided by other agencies.

Data gathered during this operation make it possible to:

1. Compare predicted fallout with the fallout as it actually occurred;
2. Compare the radioactive cloud path with the deposition of activity on the ground; and
3. Report on observed patterns of local fallout in terms of external gamma radiation.

1. The predictability of local fallout.—Fourteen devices were detonated during Operation Teapot. In reviewing the data on predicted and measured fallout from these detonations, it was found that in 5 cases the prediction is in substantial agreement with measured fallout, while in 6 cases the actual deposition of fallout was significantly at variance with the prediction. Three devices were air detonated and no fallout prediction per se was used. Chart I illustrates a case where the fallout prediction compares favorably with the fallout which actually occurred. Chart II shows a typical deviation from the predicted fallout, while chart III shows a major deviation from the prediction.[1]

[a] Graduated from the University of Cincinnati in 1937 with a degree in civil engineering. Studied public health engineering at the Massachusetts Institute of Technology Graduate School from 1938–41. Since 1941 he has been active in the Public Health Service. Participated in the first Bikini tests. During the period 1948–51 he studied radiological defense under the sponsorship of the Armed Forces special weapons project at the U. S. Navy Postgraduate School and the University of California. He participated in and directed the Public Health Service activities related to the Nevada and Pacific test operations during 1953–57. Active in radiological committees of the American Society of Civil Engineers and the American Public Health Association. Member of the National Committee on Radiation Protection and the Nuclear Standards Board of the American Standards Association. Presently chief of the radiological health program, Division of Sanitary Engineering Services, Public Health Service. (Submitted by witness.)

[1] Report of Off-Site Radiological Safety Activities, Operation Teapot, Nevada test site, spring 1955.

It should be emphasized that these data are for gamma radiation only and represent only particulate material fallen from the cloud. They do not take into account isotopes, such as iodine, that may be in a gaseous form and may not follow the fallout pattern. We plan to study this as well as other problems related to fallout exposure during and following Operation Plumbbob as a part of our off-site operation carried out under agreement with Albuquerque Operations Office of AEC. This work also has the concurrence of the Division of Biology and Medicine of AEC.

CHART 1

FALLOUT PATTERNS

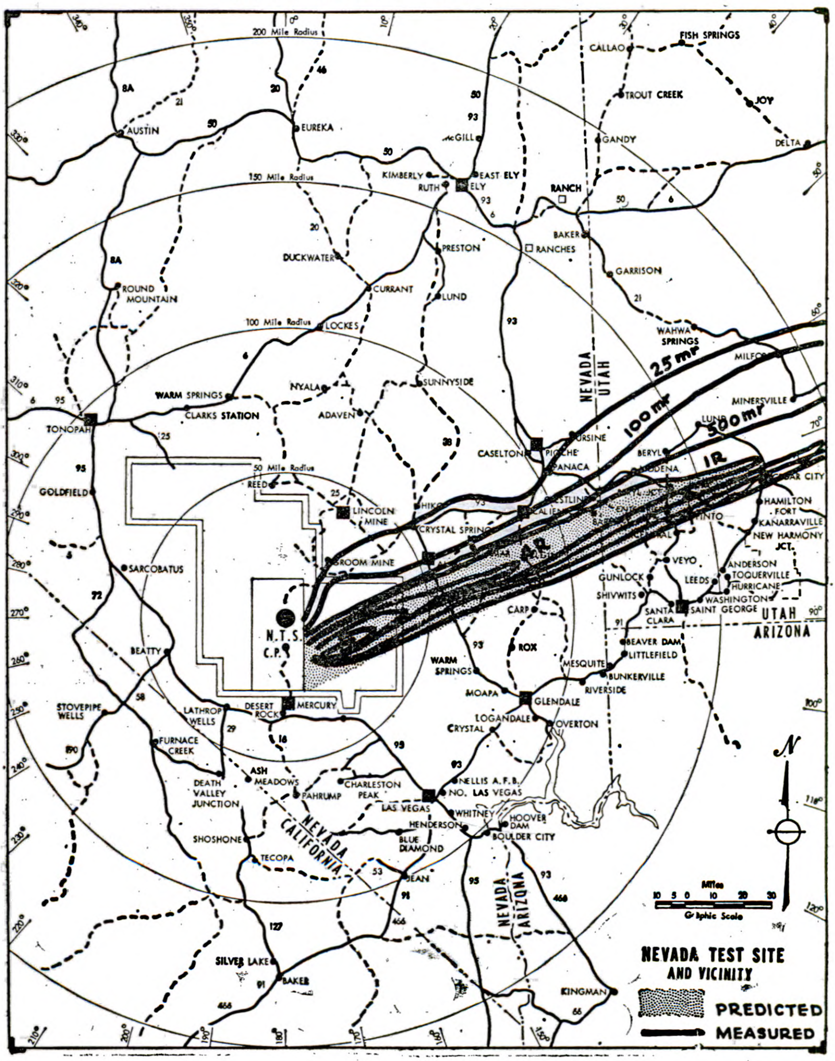


CHART 2

FALLOUT PATTERNS

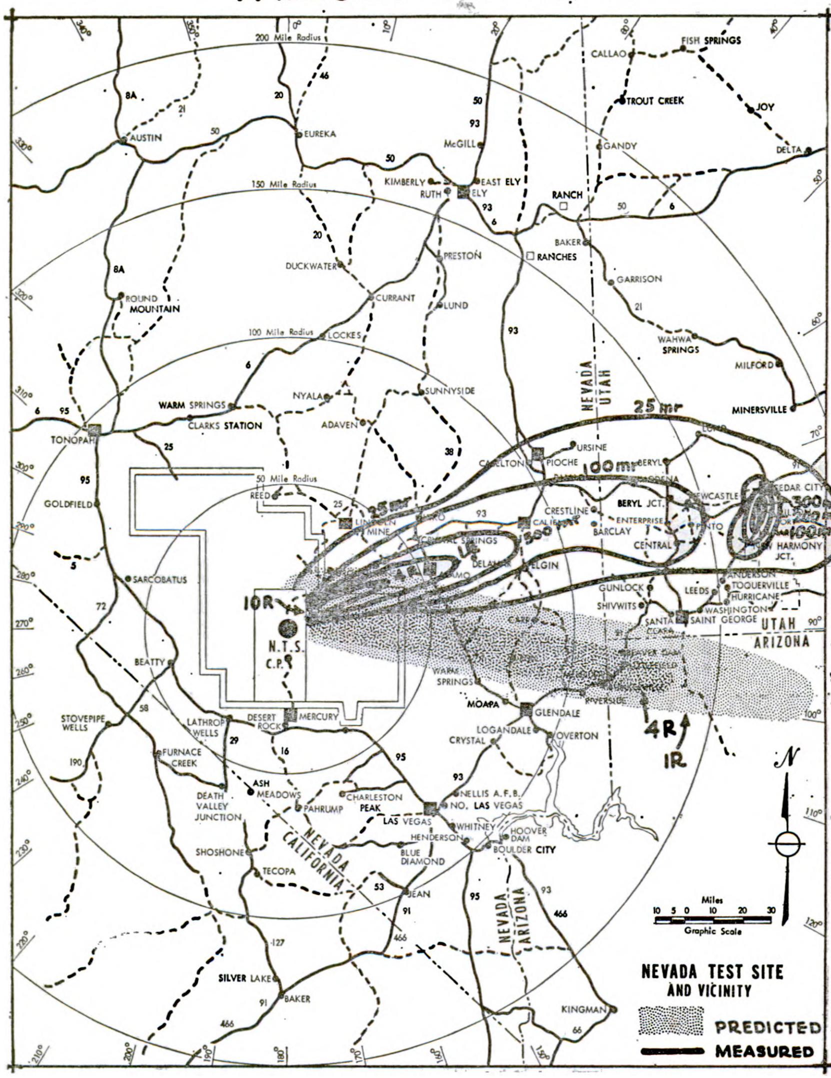
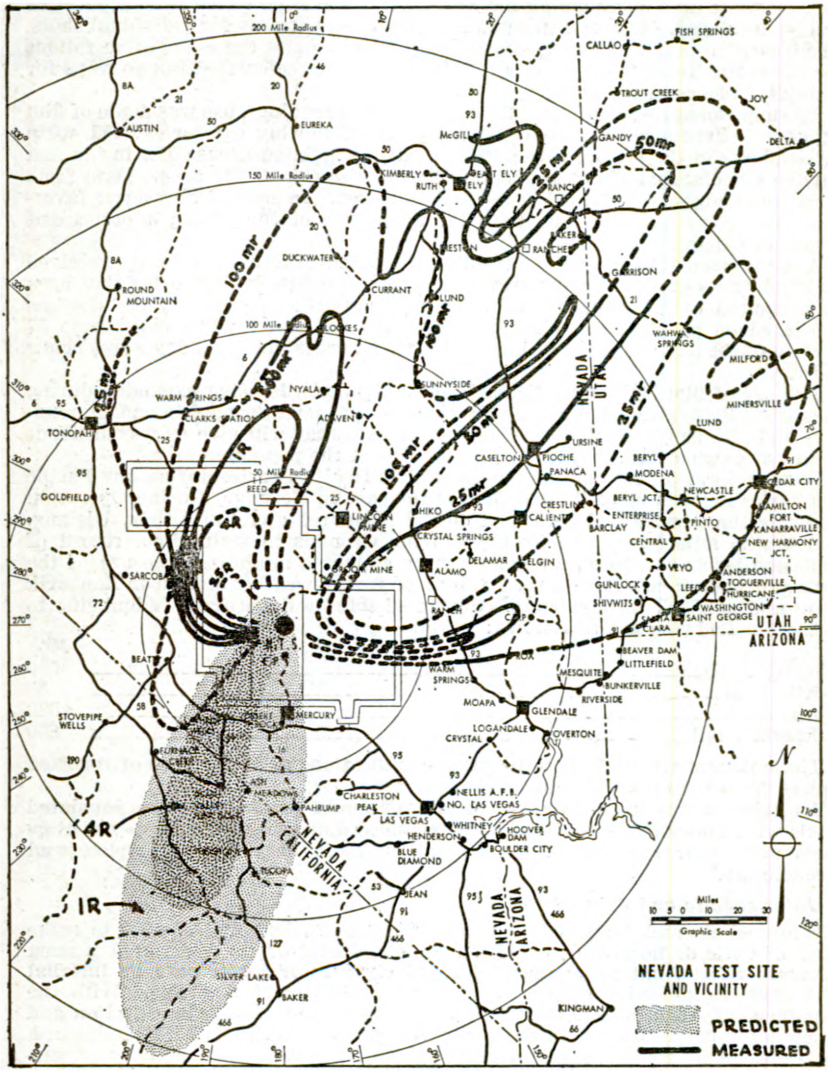


CHART 3

FALLOUT PATTERNS



Data from this report also indicates that cloud tracking with planes will generally give only an indication of the direction of fallout and cannot be relied upon for precise knowledge concerning deposition on the ground.[1] Most of the time it will give an idea of the direction from the point of detonation in which the fallout will occur, but this is not always the case. For public health emergency action it is not possible to depend entirely on cloud tracking as this will not always result in a reduction in exposure.

2. *Observed patterns of local fallout.*—A good deal of data has been obtained in the off-site area surrounding the Nevada test site from which fallout patterns can be developed. Charts I through III show such patterns for individual shots. The Teapot report contains a similar map which shows the cumulative fallout for the entire Teapot series and a tabulation of doses calculated in two ways for populated places in the area.[1]

To supplement these data calculated from meter readings, use was made of film badges.[1] Film-badge stations consisted of the following categories: 171 worn by residents in the off-site area; 106 posted in populated areas; 152 inside and outside schools; and 126 at nonpopulated points in the off-site area. Data from these film badges is contained in the Teapot report. In general they agree favorably with computed data and have the advantage of comprising a permanent record of exposures.

A comparison of the data from the film badges indicate that the dosage received by inhabitants in a particular area is less than the dose indicated for that area as measured by the same method. Approximately 94 percent of the dosages measured on people were within the 0 to 100 mr. range while only about 57 percent of the film badges posted in the populated areas indicated exposures below 100 mr.[1]

The use of film badges, particularly on individuals, is being expanded during Operation Plumbbob. We are also supplementing monitoring instrument readings and film badges with recording instruments that will give us a continuous record of gamma radiation levels in a number of the populated areas.

Data on local fallout obtained by the Public Health Service during any Pacific test series is much more limited than is the case in Nevada. During Operation Redwing the Service had personnel on the populated atolls of Utirik, Ujelang, and Wotho adjacent to the Pacific Proving Grounds to maintain a record of radiation levels and initiate any necessary action to minimize exposure of the natives to radiation.[2] Data was also obtained from a weather station on Rongarik Atoll and at JTF7 headquarters. Computed infinite doses from fallout due to Operation Redwing are as follows:

	Mr.
Ujelang Atoll	560
Utirik Atoll	50
Wotho Atoll	620
Rongarik Atoll	850

These figures are subject to the same qualifications as in the case of reported figures from Operation Teapot.

An attempt was made to supplement instrument readings on the populated atolls with film-badge data, but, due to technical difficulties which were not overcome until near the end of the operation, these data are incomplete and inconclusive.[3]

C. Intermediate and delayed fallout

Intermediate and delayed fallout are at concentrations and of ages to make them of little or no significance from the standpoint of acute external gamma exposure effects, but make them of relatively greater importance as internal beta emitters and with respect to the long-range biological effects. With the assistance of AEC, in 1956 and 1957, a routine system of sample collection and reporting in cooperation with State departments of health has been established. Our nationwide radiation surveillance network measures beta activity of particulates collected from air samples. Data from this network may be used to indicate the concentrations of radioactive materials which could expose humans to direct and indirect internal radiation hazards. Daily ambient gamma readings are also taken on a Geiger counter type of survey instrument.

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[1] See footnote, p. 328.
[2] Unpublished report, Radiation Exposures Received on Populated Atoll as a Result of Operation Redwing.
[3] Unpublished report, Report on Experimental Film Badge Study During Operation Redwing.

Two references describe the collection and measurement of radioactivity deriving from the troposphere.[4 5] The Public Health Service, for a number of years, has been developing methods which assist the States in determining environmental radiation levels and interpreting those data in terms of Public Health significance.

Reference 4 summarizes the results of this operation and demonstrates the increasing amounts of fallout found in the United States from our, and foreign, nuclear tests. Reference 5 presents a more detailed study, principally in relation to rainfall, in the Cincinnati, Ohio, area.

Radioactivity in air, at any one location, is a daily variable and cannot quantitatively be predicted from a knowledge of test schedules. For public health evaluation there appears to be no substitute for routine measurement techniques. Deposition on the ground, to a large degree, is related to rainfall. Distribution of radioactivity, geographically, is then largely dependent upon local topography and meteorology. Rainfall may contain much more activity than do the surface waters which are fed by the rainfall. The protective factors offered by the watershed may give as high as 90 percent removal of gross radioactivity.

Fallout from many nuclear tests is now always present in the air we breathe and the water supplies for ourselves, our animals, and our plants. Since there are many variables it is necessary to make measurements and keep records on those factors in the environment which directly affect man in order to make a public health evaluation of the hazards.

VII. LOCAL FALLOUT: THE MECHANISMS BY WHICH IT CAN AFFECT MAN AND THE MEASURES HE CAN TAKE TO MINIMIZE EXPOSURE

B. Shelter and shielding and their effects

By the nature of the radiation involved, it has been observed that persons can protect themselves from the acute, external effects of the beta component of fallout simply by staying under cover at the time of fallout so that none or little falls on them. Virtually direct contact with the skin is necessary to produce beta burns. We have also observed that remaining in a building will provide some protection from gamma radiation as a result of the shielding effect of the structure and the distance from the fallout afforded by being in the building.

Some data on the gamma exposure protection afforded by this means was obtained during Operation Teapot by placing film badges inside and outside of school buildings.[1] A tabulation of the results is given in the Teapot report.

The significant feature of this data is the apparent protection offered by the school buildings. The upper exposure limit is reduced by a considerable factor on a gross basis and while about 95 percent of the inside exposures fall within the 0–100 mr range, only about 79 percent of the outside badges are below 100 mr.

During Operation Plumbbob the Service is going to attempt a much more complete documentation of the shielding effects of buildings. Film badges will be placed inside and outside of several different types of buildings and at several locations within the buildings. Film badge data will be supplemented insofar as possible with data from recording instruments which will give a continuous plot of time versus intensity of gamma radiation.

C. Other immediate emergency measures that can reduce hazard

The Public Health Service has operated under radsafe criteria in the Pacific and in Nevada which illustrate the type of emergency action which may be taken in the event of unexpectedly heavy fallout.[1 6] These were devolped jointly with JTF7 and the Nevada test organization respectively.

Both of these criteria recommend remaining indoors or under cover during periods of fallout to avoid direct contact with falling or settling radioactive particles. If exposed to fallout, personal decontamination is recommended including dusting and shaking off or laundering clothes and bathing with particular attention being given to washing under the arms, the groin, face, and hair. Covering of food and water to prevent ingestion of fallout particles is recommended.

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[4] A Brief Review of the Public Health Service Radiation Surveillance Network, May 22, 1957.

[5] The Distribution of Radioactivity From Rain, by L. R. Setter and C. P. Straub, presented at the American Geophysical Union Meeting, April 29–May 1, 1957, Washington, D. C.

[6] Radsafe Emergency Instructions for Populated Islands.

An emergency measure recommended in the Pacific is to stand in the lagoon immersed as far as possible in the water while continuing to wash off exposed portions of the body. This recommendation is based on the fact that the fallout settles from the surface and allows water to attenuate the radiations. This fact has been checked in the field by PHS personnel.

In extreme emergencies, evacuation of contaminated areas may be indicated. This procedure is practical only if the evacuation will result in lower exposures than would result by staying within a shelter and if the location and intensity of the fallout pattern is known so that persons will, in the least possible time, be moved to areas of lesser contamination rather than into an area of higher contamination.

D. Dose and dose-rate versus time

During Redwing the PHS collected data at intervals ranging from once daily to once each hour with a gamma survey instrument at each of the atolls of concern.[3]

This data shows a phenomenon that has not, to our knowledge, been discussed to any great extent and that is the fact that, in the case of the larger weapons, the arrival of fallout may be extended over a period of several hours. Thus, although the fission products are decaying, this not apparent because of the continued arrival of new fallout. Typically, the radiation intensity will build up quite rapidly to a maximum, remain at or near this maximum for a period of several hours, and then start to decrease slowly. Thus a significant amount of exposure may be received before apparent decay starts.

VIII. DELAYED FALLOUT

A. The relative importance of internal emitters compared with external radiation in general for the long-run fallout situation

Elsewhere in our presentation mention has been made of the Public Health Service surveillance programs for air, water and milk. From the data which the Service has collected, and from other published information, we are following the obvious conclusion that, especially in relation to fallout, we must develop the trends of the amounts of internal emitters in man's environment and in his food chain. Because of the masking effect of natural background, external exposure effects relatable to fallout appear to be small in long-term potential when compared to the probabilities of accumulative buildup of internal emitters.

B. Deposition on and migration in soil and transport by surface waters

A number of Public Health Service studies are directly associated with the problems of migration and transport. Specific reference is made to the cooperative studies on high level radioactive waste performed by our staff from the Robert A. Taft Sanitary Enigineering Center at Oak Ridge National Laboratory.[7]

In relation to the problem of transport in surface waters, background and operational studies made by the PHS at the Columbia and Savannah River systems have a direct bearing, and provide research support data.[8] [9]

D. The effect of fallout on water supplies for human, agricultural, and industrial use

The Public Health Service has studied efficiency of normal water-treatment methods for the removal of radioisotopes from water supplies, and has made observations on the natural protective mechanisms.[10]

Depending, of course, on the exact nature of the radioactive compounds water-treatment methods offer limited protection. The degree of protection is on the order of 10 percent to 98 percent removal, or a decontamination factor ranging from 1.1 to 50.

The protective factors found in nature such as removal in watershed areas, are also within this range.[5] We have observed that in order to achieve removals of a much higher degree, as might prove necessary in the event of massive fallout during time of war or nuclear accident, the potential cost of effective water

[7] ORNL 1684, Radioactive Waste Disposal Research, by R. J. Morton et al., sec. I–60, Health Physics Division Semiannual Program Report, for period ending January 31, 1954.
[8] Columbia River Studies.
[9] Interim Report on the Savannah River Studies, July 1951–July 1952. U. S. Department of HEW, Public Health Service, 1954.
[10] Limitations of Water Treatment Methods for Removing Radioactive Contaminants, by C. P. Straub, Public Health Reports, No. 70, 897 (1955).

treatment, such as ion exchange removal, increases tremendously. The requirements in treatment materials in quantity alone is probably prohibitive. At the present time we cannot state that modern water-treatment methods applicable to the general population offer substantial protection against fallout.

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3. Unpublished report, Report on Experimental Film Badge Study During Operating Redwing. (See p. 452.)
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5. The Distribution of Radioactivity From Rain, by L. R. Setter and C. P. Straub, presented at the American Geophysical Union meeting, April 29–May 1, 1957, Washington, D. C. (See p. 466.)
6. Radsafe Emergency Instructions for Populated Islands, JTF–7. (See p. 475.)
7. ORNL 1684, Radioactive Waste Disposal Research, by R. J. Morton, et al., section I–60, Health Physics Division semiannual progress report for period ending January 31, 1954.
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11. The Detectability of Low-Level Radioactivity in Water, by A. S. Goldin, J. S. Nader, and L. R. Setter, Journal American Water Works Association, volume 45, No. 1, January 1953.
12. Measurement of Low-Level Radioactivity in Water, by L. R. Setter and A. S. Goldin, Journal American Water Works Association, volume 48, No. 11, November 1956.
13. Unpublished office memo on Measurement of Radioactivity in Water, Bottom Silts, and Biological Materials, by J. E. Flanagan, Jr., January 30, 1957.

REPORT OF OFF-SITE RADIOLOGICAL SAFETY ACTIVITIES—OPERATION TEAPOT, NEVADA TEST SITE, SPRING, 1955

Prepared for the Test Division, Santa Fe Operations Office, United States Atomic Energy Commission; prepared by J. B. Sanders, Branch Manager, Las Vegas Branch Office, AEC; O. R. Placak, Off-Site Radiological Safety Officer, PHS; M. W. Carter, Deputy Off-Site Radiological Safety Officer, PHS

PURPOSE

The purpose of this report is to present a concise summary of off-site rad-safe activities during Operation Teapot and to serve as a source of information to interested AEC and health agency personnel. All pertinent data necessary to evaluate the exposure effects of the operation in populated areas are included. In the interests of brevity, selected data only are given for nonpopulated areas. Complete monitoring logs and detailed film badge results covering these areas are, however, available from the files of the Las Vegas Branch Office, AEC.

PLAN OF REPORT

This report is composed of the following general sections:
1. AEC radiological criteria for the protection of the public.
2. Off-site Rad-Safe Organization.
3. Methods and equipment used.
4. Public relations.
5. Résumé of individual shots are also included.

These individual sections cover the following materials: A summary of monitoring runs and dosages, airway closures, cloud tracking, and low-level terrain

surveys; a table which includes the dosages at all populated places where the external gamma dosage rate reading was greater than 0.1 mr/hr. and selected values in nonpopulated areas such as the maximum dosage and the dosage at points where the fallout crossed main highways; maps of fallout prediction, cloud tracking, low-level terrain survey, and ground-survey data.

6. Summaries.

In addition to the above, the following summaries and maps are included:

Integrated dosage for populated areas.
Film-badge data.
Milk-sampling data.
Water-sampling data.
Air-sampling data.
Maps: Integrated dosages from survey data.

1. AEC RADIOLOGICAL CRITERIA FOR THE PROTECTION OF THE PUBLIC

The Division of Biology and Medicine accepted the responsibility for establishing such criteria and procedures as were deemed necessary by the Atomic Energy Commission to protect the health and welfare of the general populace from the consequences of tests at the Nevada test site. The operational procedures adopted during Operation Teapot to meet these criteria were the responsibility of the Test Manager and were carried out by the Off-Site Rad-Safe Organization, under the direct supervision of the Support Director.

The basic criterion was that the whole-body gamma effective biological dose (EBD) for the off-site population should not exceed 3.9 roentgens over a period of 1 year. This total dose may result from a single exposure or a series of exposures.

The effective biological dose is an estimate of the biological damage dose taking into account the length of time for delivery of a given dose and the reduction of dose due to (*a*) shielding afforded by buildings, and (*b*) the process of weathering.

The EBD, as computed from integrations of dose rate readings, is the sum of three-fourths of the maximum theoretical radiation dose from time of fallout to 15 days later and one-half of the maximum theoretical dose from the 15th day to 1 year.

Values of gamma dose rate readings that will satisfy this criterion for particular situations are given in graphs I, II, and III.

Personnel should be requested to remain indoors with windows and doors closed when the gamma dose rate reading, measured by a survey meter held 3 feet above ground, reaches the values given in graph I at the times indicated.

Personnel decontamination should be practiced when the gamma dose rate readings, measured by a survey meter held 4 inches from the contaminated area, equals or exceeds the values given in graph II.

Vehicles should be cleaned inside and out when the gamma dose rate readings, measured by a survey meter held 4 inches from the surface, equals or exceeds the values given in graph III.

It is recommended that when the predicted fallout across a main highway will be equivalent to a 10 roentgen infinity gamma dose or higher, that vehicles will be held until after fallout has essentially ceased.

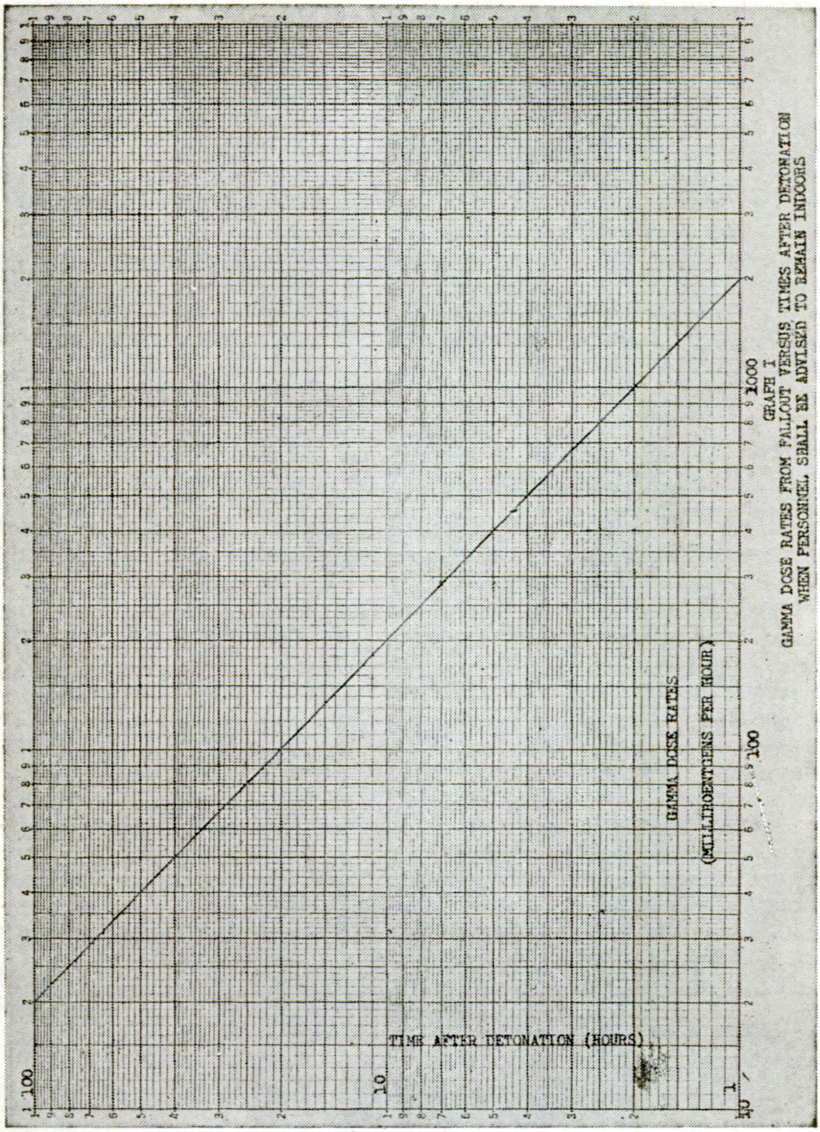
The above criteria do not apply to domestic or wild animals since levels of radiation which would be significant to them would have to be higher than those specified.

A more complete discussion of criteria is contained in a Division of Biology and Medicine, AEC, publication entitled "Atomic Energy Commission Radiological Safety Criteria and Procedures for Protecting the Public During Weapons Testing at the Nevada Test Site" dated February 1955.

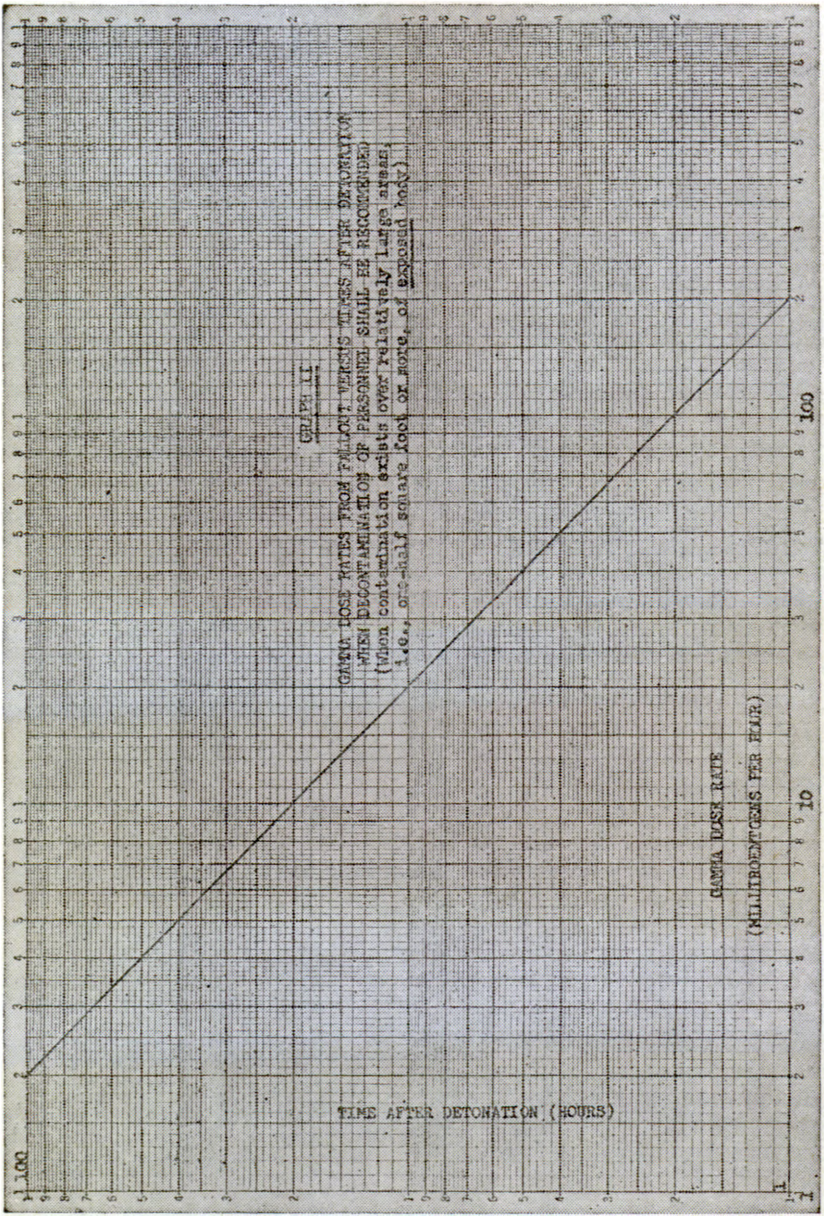
2. OFF-SITE RAD SAFE ORGANIZATION

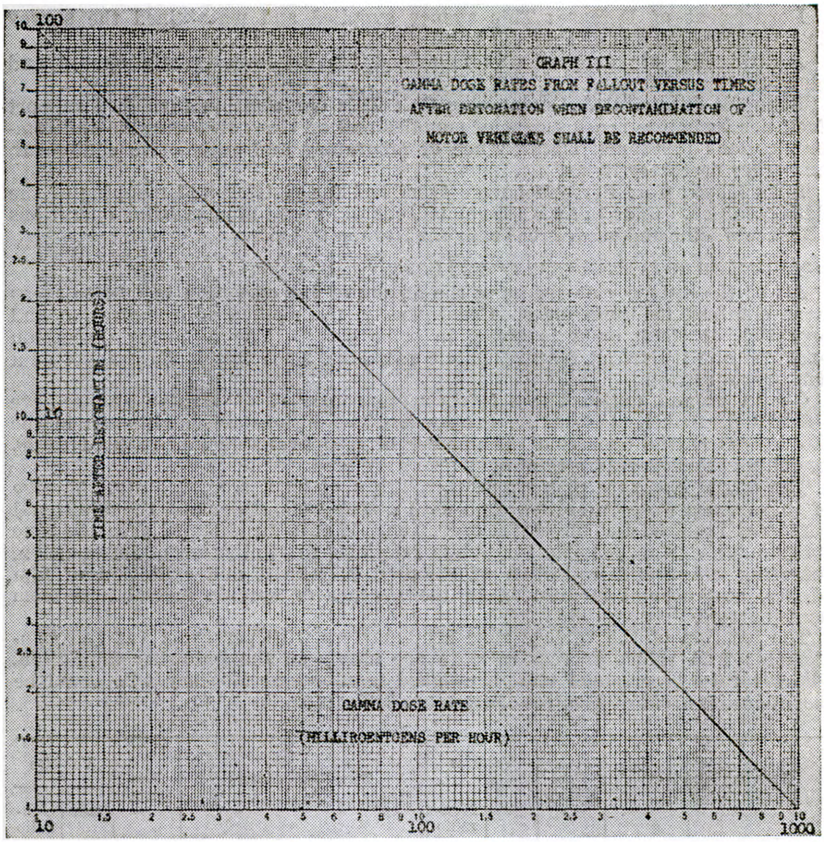
Off-site rad safe operations were a responsibility of the Test Manager and the Support Director, and were directed by the Deputy Director for Support, who was the Off-Site Operations Chief, and the PHS officer in charge, who was the Deputy Off-Site Operations Chief.

The various functions of the Off-Site program as outlined in the scope of work immediately following were carried out by personnel from the AEC, PHS, DOD, Reynolds Electrical and Engineering Co., and Silas-Mason Co.



GRAPH I
GAMMA DOSE RATES FROM FALLOUT VERSUS TIMES AFTER DETONATION WHEN PERSONNEL SHALL BE ADVISED TO REMAIN INDOORS


GRAPH II
GAMMA DOSE RATES FROM FALLOUT VERSUS TIMES AFTER DETONATION
WHEN DECONTAMINATION OF PERSONNEL SHALL BE RECOMMENDED
(When contamination exists over relatively large areas,
i.e., one-half square foot or more, of exposed body)
TIME AFTER DETONATION (HOURS)
GAMMA DOSE RATE
(MILLIROENTGENS PER HOUR)
1
10
100



Scope of program

The off-site rad safe program dated January 10, 1955, was designed to accomplish the following objectives:

1. To accurately delineate the duration and extent of the fallout pattern as determined by ground surveys.
2. To verify the above by low-level terrain surveys using aerial monitoring.
3. To determine, by aerial tracking, the intensity and direction of the radioactive cloud.
4. To determine the actual exposures to people and livestock, by the above methods, by film-badge exposure records and by air, milk, and water samples.
5. To obtain data, at points close to the Nevada Test Site, to improve the formulae used in fallout prediction.
6. To conduct a continuing public relations and education program.
7. To record, map, and report the data obtained.

Responsibilities of various agencies

Atomic Energy Commission: The AEC was responsible for the overall administration of the program and of the work of other agencies outlined in more detail below. This includes policy decisions, budget requirements, procurement of materials and supplies, and all other support requirements.

Public Health Service: All ground monitoring crews were composed of regular and reserve PHS personnel with the exceptions noted under Silas Mason Co. and Los Alamos Scientific Laboratory employees W. S. Johnson and C. P. Skillern, who assisted in prior planning and during the first five shots.

This group was directly responsible for accomplishing the objectives set forth in items 1, 4, 5, 6, and 7.

Department of Defense: This agency supplied and maintained the survey instruments used and also supplied and processed all film badges.

Additionally, air support was furnished for low-level terrain survey and cloud-tracking purposes.

Reynolds Electrical & Engineering Co.: This organization furnished support facilities including procurement of supplies and services, stenographic and communications personnel, maintenance of laboratory and automotive equipment, and other necessary items.

Silas Mason Co.: Personnel were furnished for plotting and mapping of the data obtained. Four Silas Mason employees were used in monitoring operations at Lincoln mine and other areas near the Nevada test site.

Organization and operation of ground and air support units

All of the data for determining the effects of the operation in off-site areas were obtained by these units. Consequently, the method of organization and operation of these units is given, in some detail. The following discussion excludes one important feature, public relations, since this is the subject of a subsequent section.

Offsite ground units: These teams were composed of regular and reserve USPHS personnel. Originally, there were 33 positions filled, although in the late stages of the operation the number of men available was reduced to 20. Including replacements, a total of 66 men were used.

The offsite rad safe plan established areas of local responsibility. Twelve of these zones were organized, each with a zone commander and the additional personnel required for successful operation. Within an assigned area the zone commander was responsible for public relations, dissemination of information, reporting grievances, collection of samples, placement and collection of film badges, and normal monitoring in the absence of specific instructions from headquarters.

The location of the various zone headquarters with personnel (at full complement) and vehicular requirements are shown in the following tabulation. The laboratory personnel and unassigned monitors at Mercury who could be dispatched to areas of greatest concern are indicated.

Zone headquarters	Personnel	Vehicles	
		Radio	Nonradio
1. Tonopah, Nev.	2	1	------
2. Mercury, Nev.	1	1	------
3. Las Vegas, Nev.	1	1	------
4. Glendale, Nev.	2	1	------
Mesquite, Nev.	1	1	------
5. St. George, Utah	2	1	------
6. Cedar City, Utah	2	1	------
7. Beaver, Utah	2	1	------
8. Alamo, Nev.	2	1	------
9. Caliente, Nev.	2	1	------
10. Pioche, Nev.	2	1	------
11. Ely, Nev.	3	1	1
Eureka, Nev.	1	1	------
12. Lincoln mine (Tempiute), Nev.	2	1	1
Headquarters	4	1	------
Unassigned monitors at headquarters	4	4	------

A complete roster of all off-site personnel is contained in appendix I.

Off-site communications were maintained by telephone and radio, telephones being used only when the radio network was not operating. This dual system of communications operated well and satisfactory contact with field personnel was maintained.

The radio net was composed of a net control station at Mercury, fixed and semifixed relay stations and mobile receiving and transmitting sets in the monitoring vehicles. The semifixed stations were mounted in trailers and could be relocated as required by a particular operation. The normal period of operation was from 0800 to 1600 each day. Operation subsequent to a shot was continuous until the all-clear announcement by net control.

The type, normal location, and personnel of the radio net at the start of the operation are given in the following tabulation.

Type station	Base station	Number of personnel
Control station	Mercury, Nev	2
Fixed	Currant, Nev	2
Do	St. George, Utah	2
Do	Lincoln Mine, Nev	2
Semifixed	Sunnyside or Warm Springs, Nev	1
Do	Glendale, Nev	2
Do	Alamo, Nev	2
Do	Caliente, Nev	2
Do	Pioche, Nev	2
Do	Ely, Nev	2
Do	Eureka or Geyser, Nev	2

A normal abbreviated sequence of events for ground monitoring operations follows:

Preshot:
1. Decision to proceed tentatively at evening weather briefing.
2. Field stations alerted—background samples started.
3. Radio stations and unassigned monitors dispatched as required.
4. Decision to proceed confirmed at early morning briefing.
5. Additional monitors dispatched as required.
6. Special preparations made, such as for roadblocks and evacuation.
7. Monitors advise people in their area.

Postshot:
8. All roads and populated areas in fallout area monitored as directed by headquarters.
9. All other monitors operate in manner prescribed in general instructions.
10. Monitoring of roads discontinued when no further information can be obtained.

D+1 following day:
11. Remonitoring performed as required.
12. Air samples, milk and water samples were collected as required.
13. Samples and data dispatched to laboratory for processing and counting.
14. Film badges collected as directed by headquarters.
15. Complaints and grievances investigated.
16. Field men contlnle public relations program.
17. Headquarters prepares report of the operation.

Off-site air support unit: This unit was staffed by Air Force personnel and including replacements was composed of approximately 20 airmen. The mission of the unit was:

1. To make low-level terrain surveys along the path of the fallout following a shot, and preshot reconnaissance surveys to check isolated areas for persons or animals.
2. To track the radioactive cloud or clouds at various altitudes and to record and plot the data obtained.
3. To assist the CAA in directing closure of airways in which a radiation hazard might develop based on preshot predictions and to recommend necessary changes based on actual cloud-tracking operation following a shot.

The aircraft assigned to the unit and their functional use are tabulated below. The detection devices used were T-1B and MX-5 type instruments. The air-to-ground conversion curve is shown as graph IV.

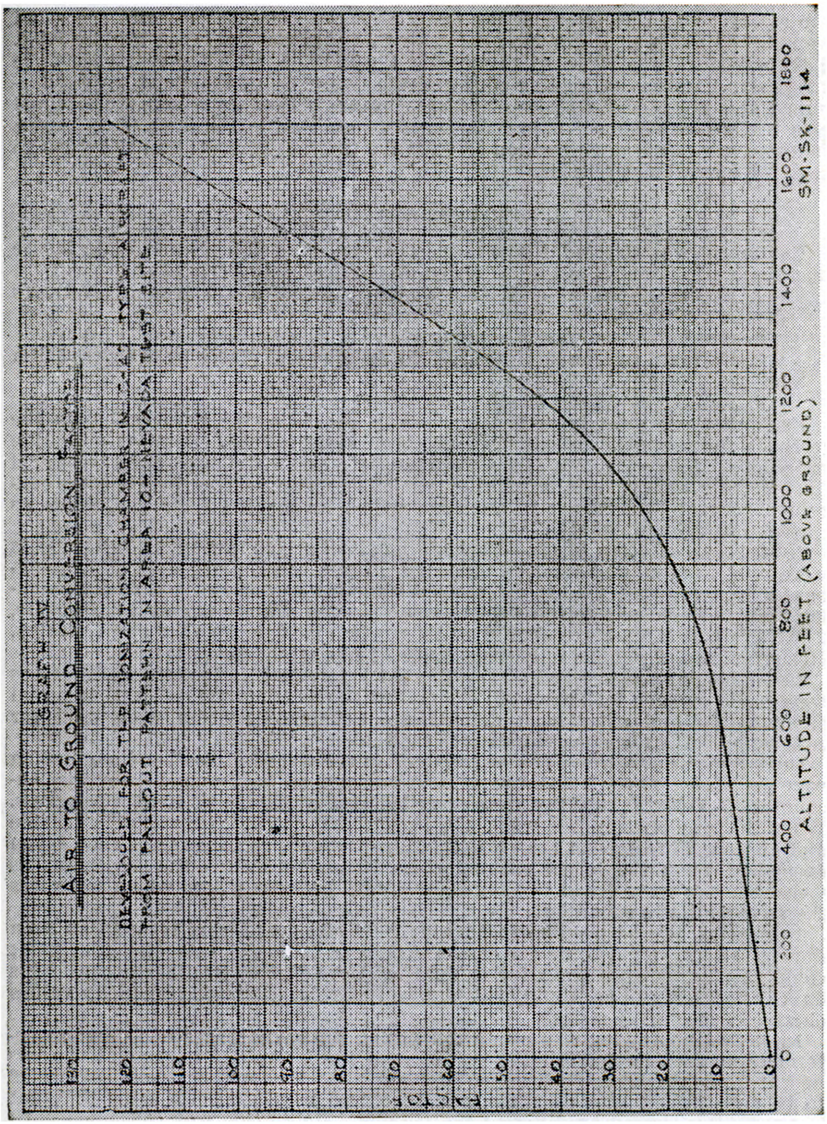
Type of aircraft	Number available	Use
C-47	2	Low level terrain surveys at 200 to 600 eet.
B-25	1	Cloud tracking. 10,000 to 15,000 feet.
B-29	1	Cloud tracking, 20,000 to 25,000 feet.
B-50	1	Cloud tracking, 27,000 to 32,000 feet.

A brief résumé of the technique of low level terrain survey and cloud tracking operations follows:

Low level terrain survey

Prior to the initial shot a terrain survey was conducted on D–1 for the purpose of locating persons and/or animals in the vicinity of the test site. On subsequent shots, the D–1 survey was conducted only upon request of the offsite operations chief or when reports indicated new concentrations or considerable changes in known livestock locations. Information was kept current by noting such positions whenever a low level survey of any type was flown.

On shot day, after sufficient time had been allowed for the fallout to occur, a low level mission was flown to delineate the fallout zones. To accomplish this mission, the aircraft was initially maneuvered to a point near ground zero to cross the suspected fallout zone at an angle of about 90°. From this starting point the fallout path was repeatedly crossed at altitude varying from 200 to 600 feet above the terrain, the interval between successive crossings varying from 3 to 10 miles, depending on the local terrain features. If possible, an altitude of about 300 to 500 feet above the terrain was maintained, since these proved to be the best operating levels for accurate readings on the radio altimeter. Since the conversion of the air reading of the radiac meter to a corresponding surface reading is dependent upon the altitude of the aircraft above the terrain, it was necessary that this factor be known as accurately as possible. In converting these readings, correlation curve (graph IV) was used. This curve was plotted from data obtained by flying a C–47 at various known altitudes above areas with known radiation intensities. The correlation between readings obtained by air surveys and those obtained by ground monitors was in reasonable agreement throughout the operation. The aerial survey proved invaluable in obtaining data in regions inaccessible to ground parties, thereby making it possible to more completely determine the actual fallout patterns.


AIR TO GROUND CONVERSION FACTOR
ALTITUDE IN FEET (ABOVE GROUND)
SM-SK-1114
0
200
400
600
800
1000
1200
1400
1600
1800
0
10
20
30
40
50
60
70
80
90
100
110
120
130

Cloud tracking

In order that the tracking aircraft could avoid deep penetrations of the cloud, the cloud was approached from one side at an angle of approximately 30° until a reading of 10 mr/hr or higher was obtained on the radiac meter. At this point the aircraft was turned out of the cloud as sharply as possible, and the cloud approached again at a different point, in this case the suspected leading edge. This procedure was repeated throughout the mission, with the result that the successive positions of leading edge and the two sides of the cloud were determined and provided a definite cloud track when plotted on a map. The cloud was tracked either until it had dispersed to such an extent that it no longer followed any particular direction or until the tracking aircraft had to return to base for operational reasons.

3. EQUIPMENT AND METHODS

Equipment was selected, located, and operated in such a manner as to insure maximum effectiveness in the collection of physical data pertaining to:

1. Surface levels of activity (normally, 3 feet above ground level), as determined by the use of survey meters.
2. Concentration of airborne activity.
3. External gamma dose received by persons and places by the use of film badges.
4. Activity contained in milk and water.

Each of these procedures is described in the following paragraphs, as the methods for sampling fallout have not been standardized. Detailed operating procedures along with data forms were prepared and distributed to all personnel during their briefing and orientation period. These written instructions contained general background information which augmented their usefulness as routine operational guides.

1. Surface radiation levels.—Portable monitoring (survey type) instruments were used to measure radiation intensity. These rates, along with other pertinent data, were then used to calculate the gamma dosage received at a particular point. Each monitoring vehicle was supplied with 4 survey instruments, 2 MX–5's and 2 T1–b's (range 0 to 20 mr/hr and 0 to 50 r/hr respectively). Measurements of gamma only were made at hip height above terrain.

Instruments were checked and calibrated before issue. Periodic calibrations were made on each instrument in the field with the minimum calibration period being before and after each detonation. Cobalt 60 sources were used for calibration both at headquarters and in the field.

During monitoring runs, the instruments in use were left "on" and monitoring was performed from inside the vehicle as long as background only was encountered. General readings were recorded at a maximum of 10-mile intervals. When the level encountered was twice background, monitoring was done outside and at least 25 feet from the vehicle. More frequent readings were then taken dependent upon the levels encountered. Distances were quite important as measurements were found to vary significantly between points which were less than one-tenth of a mile apart.

In general, it was possible to have at least one monitoring team in an area during fallout. Such being the case, the time of fallout at this particular point could then serve as a basis for estimating fallout times in other areas. This data is necessary to accurately calculate a radiation dose using intensity values obtained from survey meters.

Intensive monitoring was conducted during the early stages of fallout to determine as soon as possible the pattern and the intensities in populated areas and at strategic places such as major highways. Remonitoring was performed to be sure fallout was complete, and to obtain measurements using different instruments operated by different individuals. Monitoring was continued until it was thought no further useful data could be collected or because another detonation was imminently scheduled. It was necessary in a few instances to compromise slightly between completing today's shot activities and preparation for tomorrow's shot.

2. Airborne concentrations.—Staplex high volume air samplers were used with an MSA comfo-all dust filter for the collection of airborne contaminants. The rate of flow was in the range 1.1 to 1.3 cubic meters per minute. The standard sampling period was 28 hours beginning at shot time. Background samples, however, were run prior to each shot. The 28-hour sampling period included 7

filter changes ranging from time periods of 1 to 16 hours. More frequent changes were made when fallout occurred or when the flow rate decreased appreciably.

All filter samples were returned to the laboratory for gross beta counting. Proportional counters in conjunction with Sr-90+Y-90 standards were used for laboratory counting. For purposes of calculation, activity measured was extrapolated to midcollection time. The method of calculation resulted in a visual presentation of fallout pattern and distribution. Air activity concentrations were finally expressed as total microcuries per cubic meter of air averaged over the sampling period.

Approximately 230 individual filter samples were collected and counted following each detonation.

3. Film badges.—DuPont film packet type 559 film badges, consisting of two film components (type 502 and type 606), were used. The badges were placed in communities, along highways at 10- to 15-mile intervals, in strategic desert locations, on representative people in various towns and ranches, and in all known schools in the off-site area with the exception of the Las Vegas school system. At schools, badges were placed inside the building, outside, and usually on at least one member of the faculty.

Personnel badges were clipped on, while the "area" badges were mounted with either masking tape or placed in glassine envelopes which were tacked to a post, tree, or building. Of course, the badges themselves were sheathed in a watertight plastic envelope.

Badges were changed at frequent intervals. The collected badges were returned to the laboratory and the dosage calculated by comparing their net optical density to that if similar film packets exposed to a CO-60 standard.

Approximately 600 film badges (per change) were in place at the various stations before, during, and following the test series. Using this procedure, data are available on background; incremental additions of activity; overall exposure during the time period covered; the effect of certain structures on reducing gamma radiation; and, by comparison with dosages calculated from monitoring readings, the effectiveness of survey meters for obtaining information from which dosages are calculated.

4. Activity contained in milk and water.—Milk samples were collected from selected herds in each zone, from retail stores, from all processing plants in the off-site area, and any herd which was thought to be affected by fallout. Such a program should reveal the maximum activity contained in milk as well as the average levels to which the general population would be exposed. Samples were collected periodically with enough flexibility in sampling to insure adequate samples to define any given situation.

Water samples were collected from surface and subsurface water supplies, irrigation canals, and stock-watering ponds. Specific sampling stations were established and sampled routinely. Here again, samples were taken from any watering point which could possibly be affected by fallout.

The first sets of water and milk samples were collected prior to the start of the series. After assay, these data were used as the normal background radiation level. It could then be determined if the series produced contamination and the magnitude of such contamination.

Milk and water samples were sent to the laboratory for assay. Yet ashing, using nitric acid and hydrogen peroxide, was the method of sample preparation. Residues were transferred to counting dishes; a wetting agent added; dried under infrared lamps; and counted in proportional counters. Sr-90+Y-90 standards were used. The results are presented as microcuries per milliliter at the time of collection and/or the time of fallout.

5. Personnel monitoring.—Every person actively engaged in the off-site program was supplied with Du Pont type 559 film packet and a 1 roentgen Cambridge dosimeter. The dosage recorded by the film badge was used as the official record exposure, as required by the test manager. The dosimeter was to be used for information of use to the individual and not as a part of the official record.

6. Laboratory equipment.—Laboratory equipment consisted of four proportional counters which were fed methane gas. Three were connected to count rate meters and Esterline-Angus recorders while the fourth was connected to a decade scaler. The probes and sample housings were specifically designed to accommodate the type samples collected.

Two of the probes were used with a scanning device which enabled the counting of up to 100 micocuries on a single sample. These arrangements were used on the more active air samples.

All laboratory counting equipment was designed and built by the instruments division of the Los Alamos Scientific Laboratory in Los Alamos, N. Mex.

4. PUBLIC RELATIONS

It was recognized that adequate public relations is necessary to the successful operation of the Nevada test site. The off-site program was designed to facilitate good public relations. This was accomplished by contacts and talks prior to the series, by the system of zone commanders who were largely responsible for good relations within a specified area, by following up each incident reported immediately and, of course, by the general program carried out by the Joint Office of Test Information.

The public relations program during the operation laid the general ground work for a continuing public relations program to be carried out in the interim periods.

In general, relations with the off-site populace were good. People were particularly appreciative of the fact that monitors were permanently stationed in their communities. Opinions expressed to monitors indicated that local populations felt more secure with this arrangement with regard to radiation hazards and that they appreciated having a local contact to go to for information or with complaints. Off-site personnel were able to carry out a continuous educational program since full advantage of their presence in the community was taken and they were asked to be on the programs of civic clubs and other organizations, to furnish material for radio programs and newspapers and to aid in school programs.

Prior arrangements.—Prior to the start of the series, all of the large population centers in the area were visited by off-site personnel to inform people of the forthcoming tests and the manner in which off-site problems would be handled.

Immediately before the start of the series most of these communities were revisited by a group consisting of the Test Manager, Scientific Advisor, Test Director, Support Director, Information Director, Off-Site Operations Chief, and the senior PHS officer. A series of talks were given in Caliente, Pioche, Ely, and Tonopah, Nev., and St. George and Salt Lake City, Utah. In these talks the value of continental nucler tests to the country was stressed and the precautionary measures to be taken with regard to public safety were outlined. People were informed of the plans to station monitors in their community and that these men were expected to become a part of the community during their stay and to be of service to it in regard to public safety, information or in any other way.

From 7 to 10 days before the initial detonation, the monitors with their equipment moved into the community, familiarized themselves with the area, made acquaintances and actively took over the job of public relations.

Liaison activities.—Arrangements were made to keep those health officials who might be primarily concerned, informed of the activities at the test site. The States normally involved were Nevada, Utah, California, and Arizona, and the State health officers of these States were advised routinely by phone of any fallout situation that might affect areas under their jurisdiction. The personnel advised in these instances were:

Nevada: Dr. Daniel J. Hurley, State health officer.
Utah: Dr. George A. Spendlove, State health officer.
California: Dr. John M. Heslep, designate of State health officer.
Arizona: Dr. C. G. Salsbury, State health officer.

In addition to these arrangements, contacts were made with affected USPHS officials and with local health officials.

Activities of zone personnel.—Zone personnel conducted a public-relations program on an informal and down-to-earth basis. They formed a wide acquaintance in their respective areas, participated in local events and took their instructions to become a part of the community seriously; as for example, the monitor at Glendale who became a Sunday school teacher, or the one in Alamo who plastered a ceiling in one of the hotel rooms. Such intimate association with the people in the area was good practical public relations, and while it may not have altered completely basic public opinion regarding the tests, it at least made the explanations of zone personnel more acceptable.

Every opportunity to reach the public through talks and film showings was accepted. Practically every person throughout the off-site area saw at least one film and listened to at least one discussion by monitors. This was accomplished through civic clubs, schools and PTA, and other groups. In this connection, it should be stated that the new film Atomic Tests in Nevada received enthusiastic reception. From the remarks made to zone personnel, it appears that general feeling was that, for the first time, the public was being shown exactly what happened during a shot.

A complete listing of public relations contacts is not available, but the partial list of film showings tabulated in table 1 will indicated the scope of this activity:

TABLE 1.—*Public relations—Movies*

Zone	Location	Date	Film	Attendance
Alamo	Alamo	Feb. 9	Target Nevada	100
	do	Feb. 10	Atoms in Agriculture (shown twice).	25
Caliente	Lincoln County High School.	May	Atomic Tests in Nevada, and Atoms in Agriculture.	200
	Elementary school	do	do	80
	Lincoln County High School.	do	Nuclear Reactors	30
	Elementary school (science and physics class).	May 12	do	38
Cedar City			Atomic Tests in Nevada.	1,180
			A Is for Atom	870
			Operation Ivy	36
			Target Nevada	36
Ely	Lions, Rotary, and chamber of commerce.	Feb. 9	Target Nevada, and A Is for Atom.	109
	Ely Woman's Club	Feb. 10	A Is for Atom	51
	Ely Elks' Club	do	do	30
	Roadrunners' Motorcycle Club.	Feb. 13	Operation Ivy, and A Is for Atom.	
Ely	Ely PTA	Feb. 14	Operation Ivy, and A Is for Atom.	75
	VFW and auxiliary	Feb. 17	do	50
	Ruth-Kimberly PTA	Feb. 21	Operation Ivy	40
	Society of Professional Engineers.	Feb. 22	do	20
	Steptoe Hospital staff	Feb. 25	do	
	Shut-ins	Feb. 26	Operation Ivy, and A Is for Atom.	14
	Fire department	Mar. 1	do	30
	Duckwater	Mar. 4	do	40
	Baker PTA	Mar. 5	do	60
	Eureka School	Mar. 11	A Is for Atom	70
	Austin School	Mar. 15	Operation Ivy	60
Glendale	Mesquite School	Apr. 7	A Is for Atom, and Target Nevada.	60
	Bunkerville School	Apr. 8	Target Nevada	105
	Overton School	Apr. 11	Target Nevada, and Atom Tests in Nevada.	175
	Mesquite Theatre	Apr. 15	do	150
	Overton High School	Apr. 21	A Is for Atom	40
	Overton Veterans' Club (attending: sportsmen, firemen, and California civil defense).	Apr. 25	Atomic Tests in Nevada, and Target Nevada.	88
Lincoln Mine	Lincoln Mine Theater	Apr. 24-30	Atomic Tests in Nevada.	500
Pioche	Pioche		A Is for Atom	20
	Volunteer fire department.		do	
	Pioche		Operation Ivy	
	do		Operation Doorstep	
	Young women's literary club.		A Is for Atom	
	Latter-day Saints Church	Apr. 17	Atomic Tests in Nevada.	35
St. George	Glendale, Utah, PTA	Mar. 16	Target Nevada	35
	Kanab PTA	Apr. 4	Atomic Tests in Nevada.	45
	Kanab High School	Apr. 5	do	160
	Orderville PTA	Apr. 11	do	35
	St. George firemen	do	do	12
	Ladies' relief society Latter-day Saints.	do	do	200
	do	Apr. 12	do	40
	Elementary school	Apr. 13	do	230
	Dixie College	do	do	180
	VFW	do	Atomic Test in Nevada, and Target Nevada.	82

TABLE 1.—*Public relations—Movies*—Continued

Zone	Location	Date	Film	Attendance
St. George—Continued				
	Chamber of commerce	do	Atomic Tests in Nevada	40
	High school	Apr. 14	Atomic Tests in Nevada, and Target Nevada.	400
	Ladies' relief and faculty	Apr. 15	Atomic Tests in Nevada	60
	Lady Elks	do	do	88
	National Guard	Apr. 14	do	73
	Virgin PTA	Apr. 21	do	60
	Community church	Apr. 22	do	18
Tonopah	High school		Target Nevada (shown 3 times).	260
	Mizpah Hotel		Target Nevada	170
	Goldfield Elks	Feb. 28	do	60
	Fish Lake		do	50
	Manhattan	Feb. 23	do	60
	Round Mountain	Feb. 24	do	50
	Wellington Rotary		do	50
	Tonopah (2 clubs)		Atomic Energy	80
	Round Mountain		do	50
	Goldfield		do	60
	Wellington		do	50
Other:				
Beatty, Nev			Target Nevada, and Atomic Energy.	80
Do		Apr. 11	Atomic Tests in Nevada	60
Do	High school	Apr. 14-15	Atomic Tests in Nevada (shown twice).	175
Chattanooga, Tenn	Division of Health and Safety, TVA.	May 9	do	60
Fort Oglethorpe, Ga	Kiwanis Club	May 10	Atomic Tests in Nevada	26
Florence, Ala	Lions' Club	May 16	do	45
Los Angeles, Calif	ASCE. sanitary section	May 25	do	50
Total people seeing films.				[1] 7,550

[1] Not a full count. Conservative estimate made when attendance figure was missing.

In addition to these semiformal contacts, a large number of individual contacts were made. One interesting example of this indicates the public relations value of the film badge program. During a routine change of a personnel film badge in Goldfield, Nev., the wearer remarked that "there must be some fine people at the test site, since they were taking such precautions even in a small place like Goldfield." It must be recognized, however, that although relations throughout the off-site area were generally good, there are some specific areas of difficulty. An example of this is the attitude of the newspaper editor in Tonapah, who, contrary to editorial opinion in general, has maintained a highly critical attitude toward test activities.

Other informational material was distributed. The news releases of the Joint Office of Test Information were widely used by monitors. However, the most valuable piece of educational material was the little yellow booklet, Atomic Test Effects in the Nevada Test Site Region. Thousands of these were distributed through schools, post offices, motels, and by other means throughout southern Nevada and Utah, and in parts of Arizona and California. This was very well received. In fact, some people thought so highly of it that they requested copies to distribute on their own. Many of these booklets were picked up by tourists and were probably carried to all parts of the Nation.

Special investigations.—It was inevitable that numerous incidents requiring investigation should arise. These were of three types, as they effected material things, people, or livestock. All that came to the attention of the off-site program were investigated and are documented in the files.

With respect to material things, the greatest number of complaints were from prospectors. An explanation of the transient nature of radioactivity from fallout was generally acceptable. In all cases where blast damage was reported, forms for damage claims were mailed and these are being processed in the customary manner. In those cases where contamination from radiation were reported, such as on vehicles, the zone personnel investigated and were generally able to satisfy people during these visits that no hazard existed.

A number of cases of radiation damage to people were reported. These were investigated by the Cedar City Zone commander, Dr. Clinton G. Powell, who is a

PHS doctor. This procedure was so useful that it became apparent that it was a mistake to require medical personnel to also act as zone commanders. In any future operation, a qualified doctor with radiation experience should be available within the off-site program for the sole purpose of investigating claims of personal radiation injury.

Prior contacts were made with the local doctors. All investigations were made by working with local doctors. This procedure eliminated any chance of criticism about professional ethics, increased the patient's confidence in the procedure and did much to educate the local physicians in regard to radiation matters.

The general procedure was to have the patient brought to the local doctor's office. If necessary, off-site monitors provided the transportation. There both doctors examined the patient and arrived at a decision. Any costs were billed through Reynolds Electrical & Engineering, Inc.

In no case, of those examined, were there symptoms that could be definitely attributed to radiation injury. Many cases turned out to be some common ailment, diaper rash, in one case. However, the reports of eye irritation were so persistent that this matter should be investigated in order to prove or refute the widespread belief that this is due to test activities.

Reports of injury to livestock were reported by zone personnel and investigated during the series by veterinarians (Maj. Grant Kuhn and Col. Bernard Trum) from the AEC-University of Tennessee Agricultural Farm at Oak Ridge or by Dr. Wendell Brooksby, of the Utah State Agricultural College. There is little doubt that reputed livestock damage will continue to be reported for some time after the tests since livestock culture is such an important part of the economic life of the area. This suggests the desirability of the continuous services of a veterinarian with radiological training and of a sound investigative program.

5. RÉSUMÉ OF INDIVIDUAL SHOTS

This section includes a brief summary of results for each nuclear device detonated during the test series. While only condensed highlights are presented here, complete data are contained in the files of the Las Vegas branch office.

Each shot summary has been arranged, for ease of comparison, according to the outline below:

1. General information
2. Airway closure pattern
3. Cloud tracking
4. Low-level terrain survey
5. Ground monitoring results
6. Selected gamma dosages
7. Number of ground readings taken (above 0.1 mr./hr.)
8. Comparison of maps
9. Airborne radioactivity concentrations
10. Table—External gamma dose in populated areas and at selected nonpopulated points
11. Maps
    - (a) Prediction
    - (b) Cloud track
    - (c) Low-level terrain survey
    - (d) Ground monitoring
    - (e) Combined air and ground results

In some cases, all of the above maps are not available. For exemple, for certain shots it was not possible to obtain aerial results due to the extremely light fallout. On 1 occasion there were 2 detonations on the same day. In this case the results from the two individual detonations have been combined.

The maps are given in terms of infinite dose and/or effective biological dose.

Comparison of maps is done on an extremely gross basis. The major points of comparison are direction of fallout and relative magnitude of fallout, as determined from length of specific isodose contours.

Following the summary for Zucchini (the last shot of the series) is map 1 which represents the cumulative fallout plot for Operation Teapot. The map is plotted in terms of infinite dose and reflects both aerial and ground monitoring results.

For orientation purposes, as to the shot names, the times and dates of detonation, the type of detonation, and the firing area, the following tabulation is presented.

Devices detonated during Operation Teapot

Name	Detonated		Type	Area
	Time	Date		
Wasp	1200	Feb. 18	Airdrop	7—Yucca.
Moth	0545	Feb. 22	300-foot tower	3—Yucca.
Tesla	0530	Mar. 1	do	9—Yucca.
Turk	0520	Mar. 7	500-foot tower	2—Yucca.
Hornet	0520	Mar. 12	300-foot tower	3—Yucca.
Bee	0505	Mar. 22	500-foot tower	7—Yucca.
ESS	1230	Mar. 23	Subsurface	10—Yucca.
Apple	0455	Mar. 29	500-foot tower	4—Yucca.
Wasp Prime	1000	Mar. 29	Airdrop	7—Yucca.
HA	1000	Apr. 6	High altitude	Yucca.
Post	0430	Apr. 9	300-foot tower	9—Yucca.
MET	1115	Apr. 15	400-foot tower	Frenchman.
Apple II	0510	May 5	500-foot tower	1—Yucca.
Zucchini	0500	May 15	do	7—Yucca.

Wasp

Wasp was an airdrop which was detonated at approximately 800 feet on February 18, 1955, at 12 noon. The shot took place in test area 7 in Yucca Flat.

The airway closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles, was ordered closed at all altitudes from 7:15 to 10 a. m.
2. A sector, bounded by radii at 100° and 210°, extending 175 and 120 nautical miles, respectively, from the end of the 210° radius extend due east to JF3015, then due north to intersection with 100° radius, was ordered closed from the surface to 30,000 feet from 8:45 to 11 a. m.
3. The following area was ordered closed from 10,000 feet to 30,000 feet from 11 a. m. to 5:30 p. m. From FF3030 due south to FE3000, then due east to ME0000, then follow Arizona-New Mexico border to 4 corners, then west to HH3000, then return to initial point.

Due to mechanical difficulty with the drop aircraft, the time of the shot was moved ahead, causing a corresponding shift of 4 hours in all closure times.

The cloud was tracked, as indicated on the accompanying map, from H plus 80 minutes to H plus 2 hours. In addition to reports from the B–25 tracker, reports from sampler aircraft were used to chart the cloud movement. Maximum cloud height observed was 20,000 feet. The cloud was tracked to a point southwest of Las Vegas (Goodsprings, Nev.), at which point it was so scattered as to become relatively undefined.

On D–1 day a low-level survey was made by one C–47 to locate and record the position of persons and/or animals in remote areas within the predicted fallout zone. On D–day the survey aircraft took off at approximately H plus 3 hours 45 minutes and reported no significant off-site contamination.

Ground-monitoring runs, which indicated activity substantially above background, were made along U. S. 95 between 10 miles east of Mercury Road and Indian Springs Air Force Base; on Nevada 52 between 12 miles south of U. S. 95 and the southern end of the highway; and along Nevada 85 from 22 to 32 miles southeast of Pahrump, Nev.

The maximum effective biological dose for populated area was 0.7 mr. at Cactus Springs, Nev. The maximum effective biological dose at a nonpopulated point was 55 mr. on Nevada 85, 28 miles southeast of Pahrump, Nev.

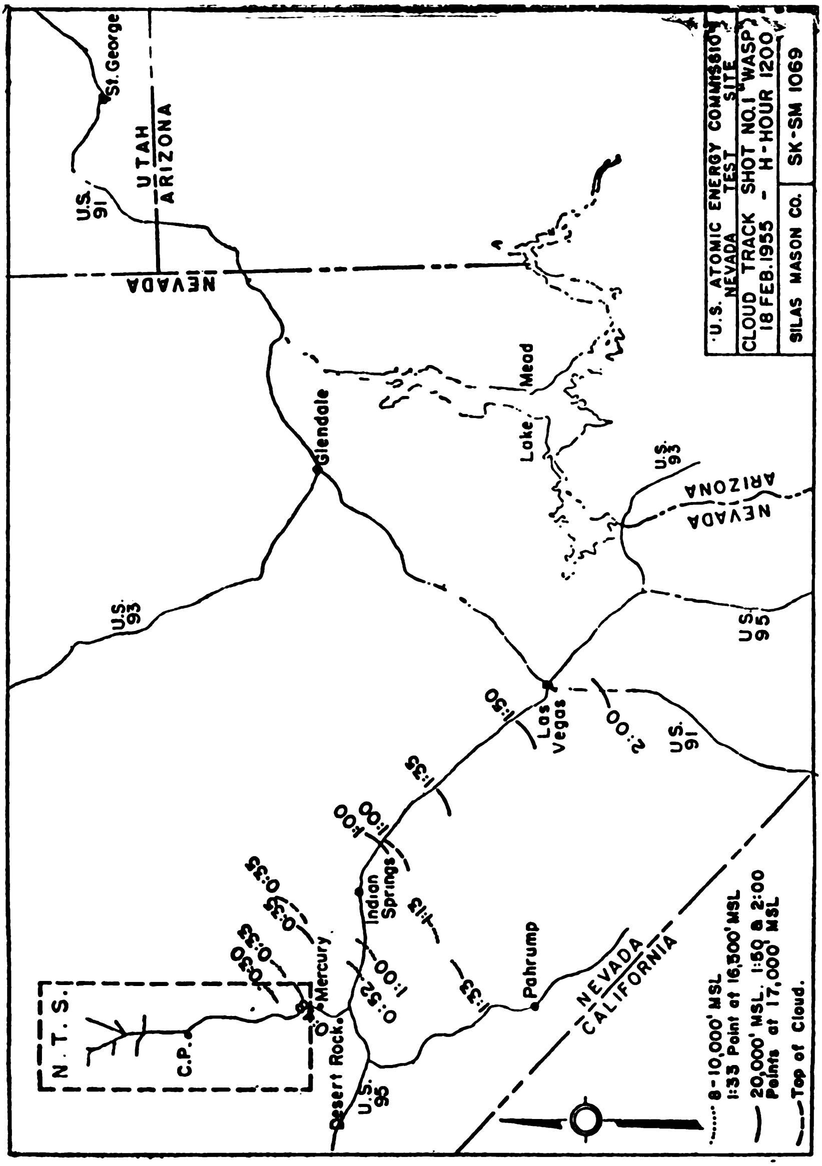
Only 10 individual monitoring readings above 0.1 mr./hr. were recorded.

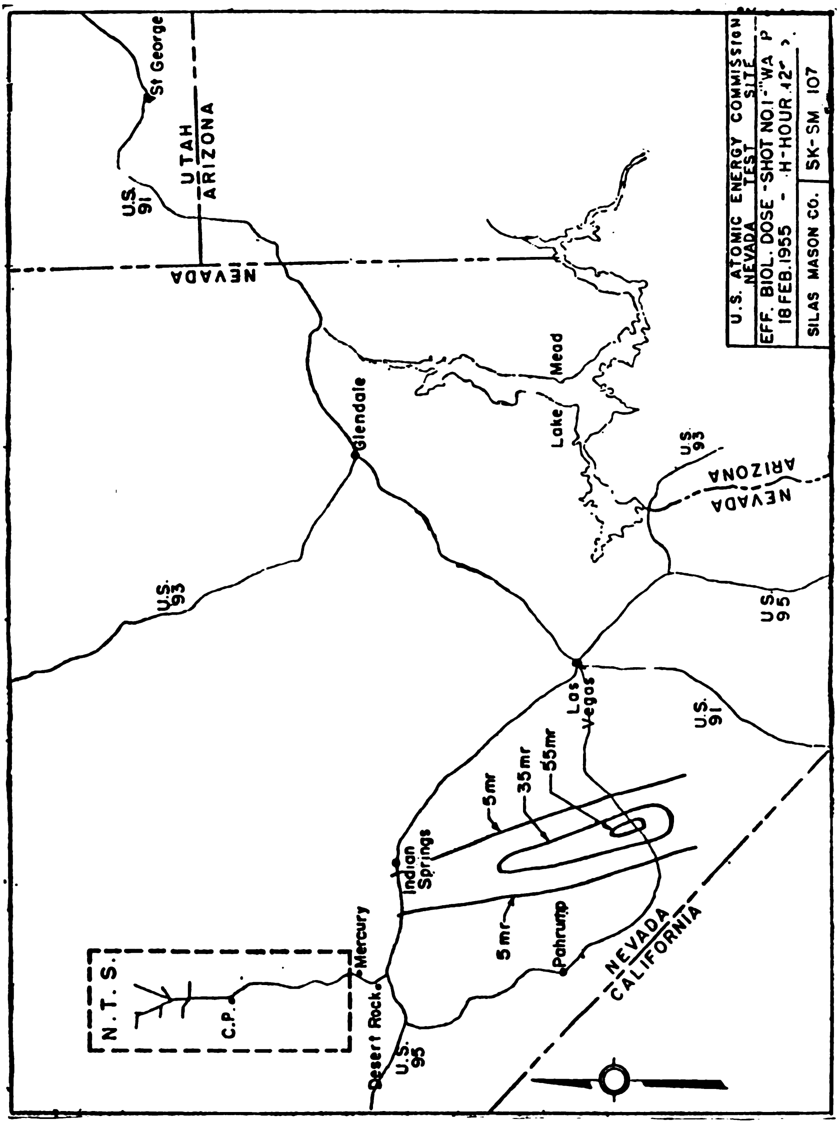
Fallout was very light as indicated by ground monitoring and also by the lack of data from the low-level terrain survey.

No air samples were collected which indicated an air activity concentration in excess of $10^{-4} \mu c/m^3$.

Wasp: External gamma dose in populated areas and at selected nonpopulated points

Location	Time of instrument reading (H plus hours)	Gamma ground level (mr./hr.)	Time of fallout (H plus hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Indian Springs, Nev	1.6	0.1	1.0	0.5	0.8
Cactus Springs, Nev	1.9	.1	1.0	.7	1.2
Nonpopulated points:					
U. S. 95, 15 miles east of Mercury Road	1.7	1.5	1.0	8.4	14.0
Nevada 85, 28 miles southeast of Pahrump, Nev	3.6	5.5	3.0	55.0	110.0


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK SHOT NO. 1 "WASP"
18 FEB. 1955 - H-HOUR 1200
SILAS MASON CO.
SK-SM 1069
St. George
UTAH
ARIZONA
U.S. 91
NEVADA
Glendale
Lake Mead
U.S. 93
ARIZONA
NEVADA
U.S. 95
Las Vegas
2:00
1:50
1:35
1:00
1:00
Indian Springs
1:13
0:35
0:35
0:33
0:30
Mercury
Pahrump
1:33
1:00
0:52
N. T. S.
C.P.
Desert Rock
U.S. 95
NEVADA
CALIFORNIA
8-10,000' MSL
1:33 Point at 16,500' MSL
20,000' MSL. 1:50 & 2:00
Points at 17,000' MSL
Top of Cloud.


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
EFF. BIOL. DOSE - SHOT NO. 1 "WA P"
18 FEB. 1955 - H-HOUR 42"
SILAS MASON CO.
SK-SM 107
St George
UTAH
ARIZONA
U.S. 91
NEVADA
Lake Mead
Glendale
U.S. 93
NEVADA
ARIZONA
U.S. 95
Las Vegas
U.S. 91
5mr
35mr
55mr
Indian Springs
5mr
Pahrump
Mercury
NEVADA
CALIFORNIA
N. T. S.
C.P.
Desert Rock
U.S. 95
Desert Rock

Moth

Moth was a 300-foot tower detonation which was fired at 5:45 a. m. on February 22, 1955. The shot took place in test area 3 in Yucca Flat.

The airway closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles, was ordered closed at all altitudes from 5:15 to 8 a. m.

2. A sector bounded by radii at 120° and 180°, length of radius 125 nautical miles, was ordered closed from 15,000 to 26,000 feet between the hours of 6:45 and 8:15 a. m.

3. Finally, a large rectangular area, primarily in Arizona, was closed at the same altitudes as the above sector from 8 to 11:45 a. m. The boundaries of this area, as defined by Georef coordinates, were:

Continue the 120° radius to KE0030, then due south to Tucson, Ariz.

Continue the 180° radius to EE0050, then southeast to HC0000.

As the reports of the tracking aircraft came in, the following changes were made in the closure pattern:

At 6:30 a. m., the radius bounding the sector defined in (2) above was shifted from 120° to 90° out to KH0000, then due south to Tucson, Ariz.

At 6:45 a. m., the area south of the north edge of Airway Green 4 was opened at all altitudes.

At 6:48 a. m., all areas were opened to traffic at 24,000 feet and above.

The cloud was tracked, as indicated on the accompanying map, from H plus 18 minutes to H plus 3 hours 25 minutes. Reports from B–25 tracker and sampler aircraft were used. The maximum cloud height observed was 24,880 feet. The maximum height of the cloud decreased as it progressed on a general bearing of approximately 130°, stabilzing at roughly 22,000 feet. The path followed by the cloud took it north and east of Las Vegas, Nev., and extended for approximately 110 nautical miles when the tracking aircraft were recalled.

As an indication of the magnitude of radiation to be expected should a commercial aircraft penetrate the cloud, two F–84 aircraft were flown into the cloud and were checked for contamination upon return. The pertinent data are presented in the following tabulation:

	Aircraft No. 1	Aircraft No. 2
Time of penetration	H+3 hours	H+3 hours.
Duration in cloud	30 seconds	13 minutes.
Altitude	22,500 feet	20,000 feet.[1]
Maximum intensity	800 mr./hr	500 mr./hr.
Average intensity	300 mr./hr	400 mr./hr.
Time of landing	H+5 hours	H+5 hours.
Readings on various aircraft components:		
Drive brakes	100 mr./hr	480 mr./hr.
Wing tips	48 mr./hr	140 mr./hr.
Nose	50 mr./hr	180 mr./hr.
Impeller section of engine	100 mr./hr	480 mr./hr.
Pilot dose	30 mr	150 mr.

[1] Descending along cloud to 10,000 feet.

The low-level terrain survey was conducted by 1 C–47-type aircraft which took off at H plus 7 hours 45 minutes, completing the mission at H plus 10 hours. The contamination pattern disclosed by this survey is plotted on the accompanying map.

Monitoring runs, which indicated activity substantially above background, were made on the desert road running north from Indian Springs, Nev., along the desert road from Groom Road to Frenchman Flat; on the game preserve road running north from U. S. 95; on U. S. 93–91 in the vicinity of Dry Lake, Nev.; and along Nevada 40.

The maximum effective biological dose for a populated area was 130 mr. at Dry Lake, Nev. The maximum effective biological dose at a nonpopulated point was 5,900 mr. 22.8 miles north of Indian Springs, Nev., on a desert road.

Approximately 60 individual monitoring readings above 0.1 mr./hr. were recorded.

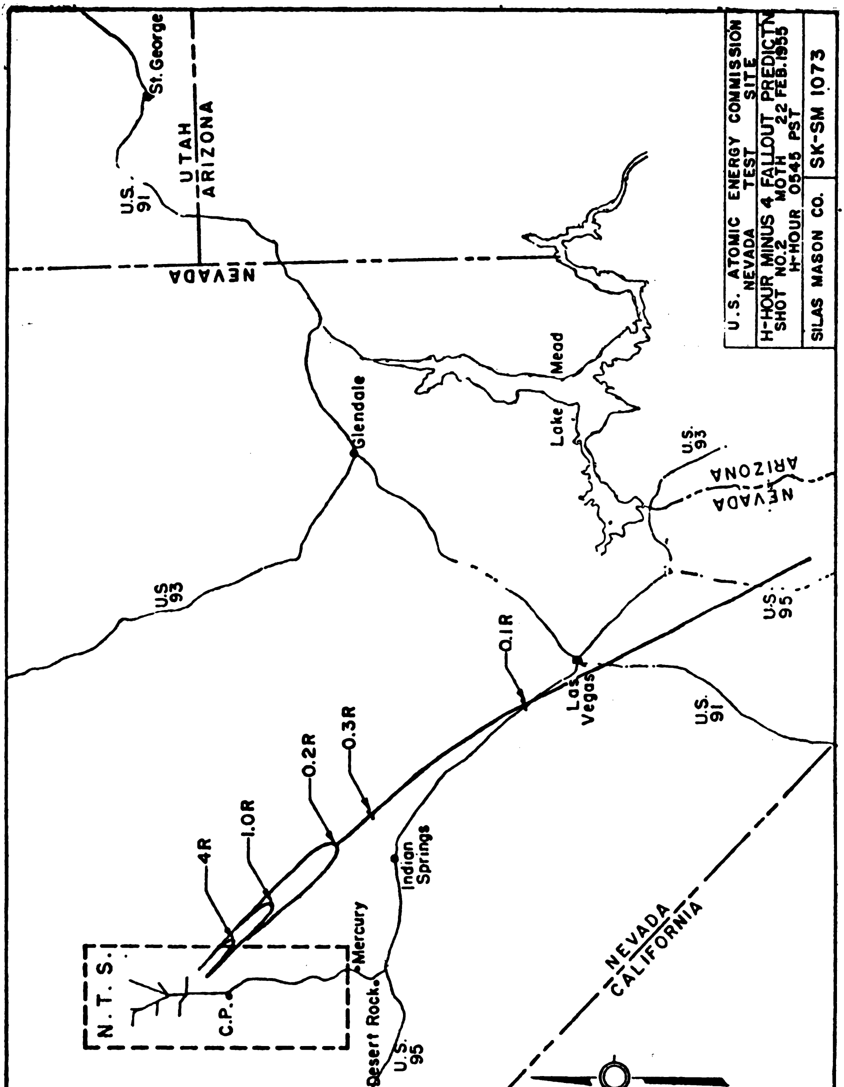
The H minus 4-hour prediction map indicated fairly good agreement with the factual maps. With very little shear in the fallout pattern, expected times of fallout could be readily predicted. The small amount of fallout to the northeast

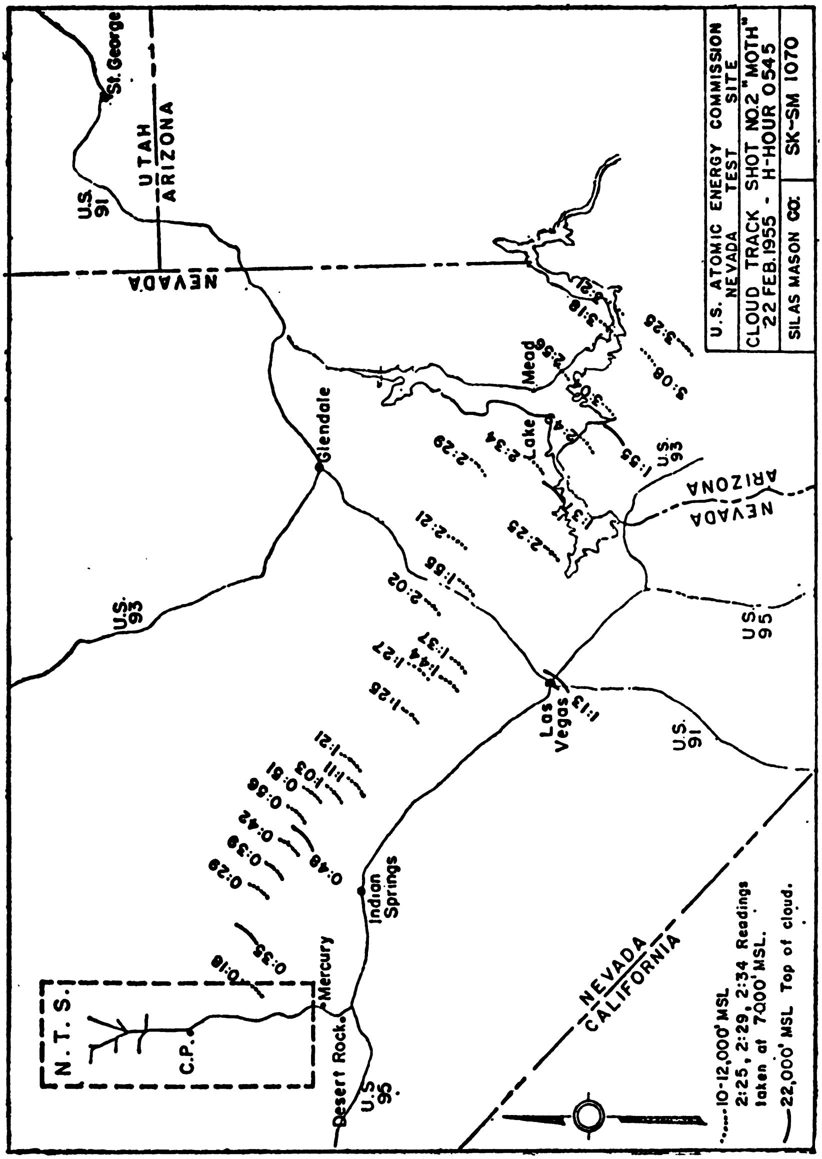
was not detected by the low-level terrain survey. The ground survey as well as the aerial survey showed that the 1 r. infinite contour crossed U. S. 93–91 at a nonpopulated place.

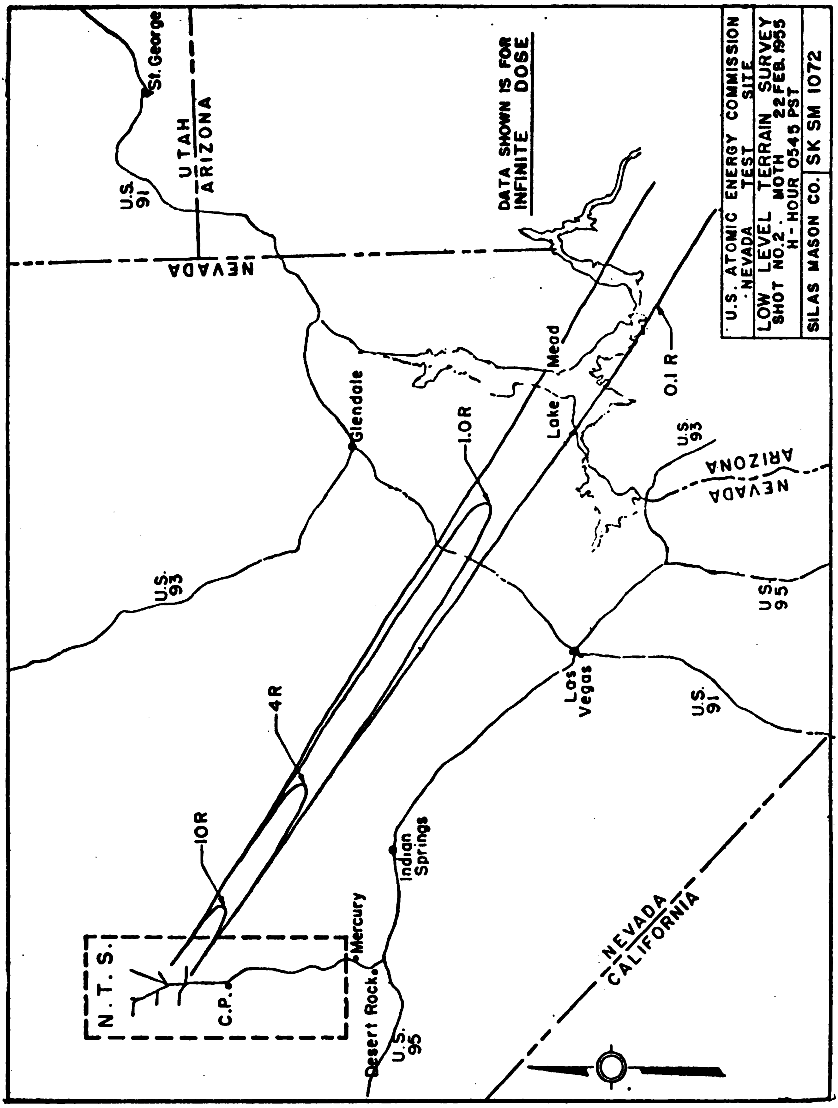
The maximum air radioactivity concentration measured was $4.4 \times 10^{-3} \mu c/m^3$. at Cedar City, Utah. This represents the average air concentration for a 28-hour period starting at shot time.

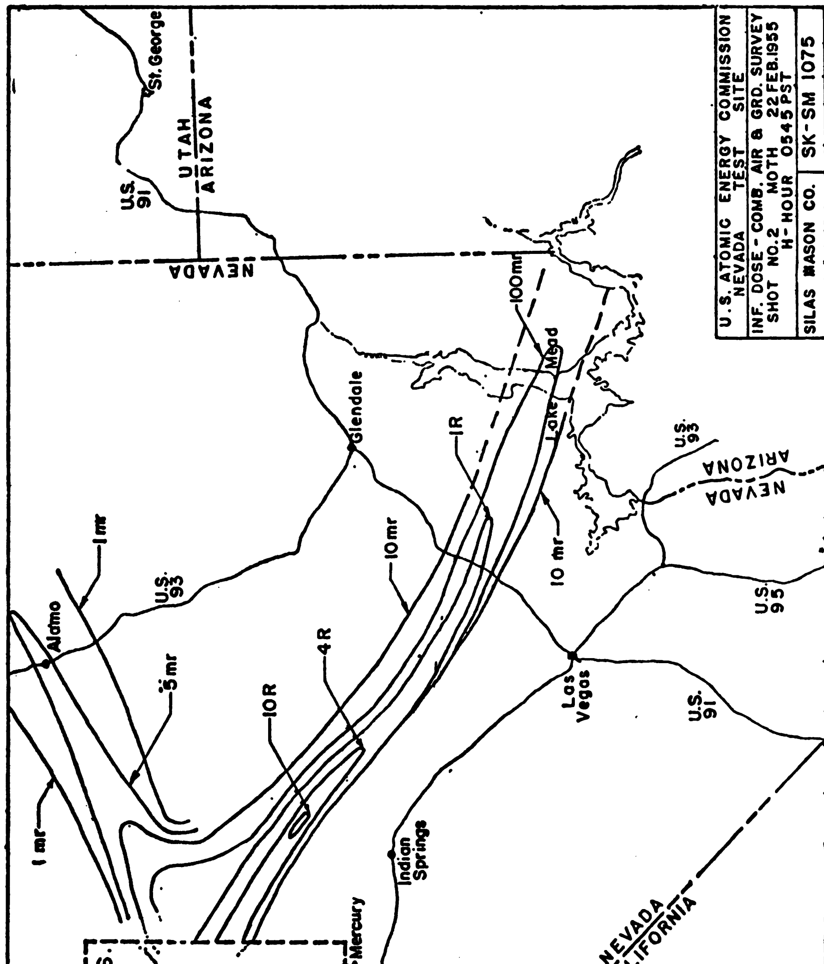
Moth: External gamma dose in populated areas and at selected nonpopulated points

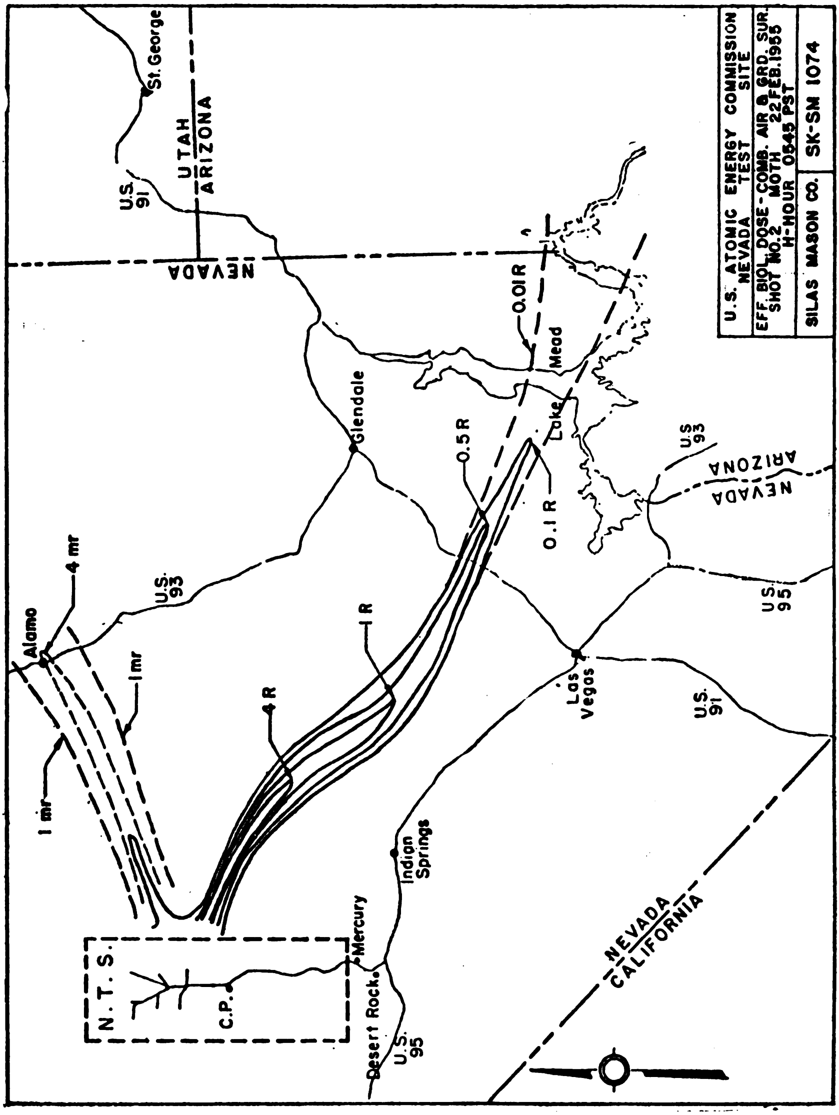
Location	Time of instrument reading (H+hours)	Gamma ground level (mr./hr.)	Time of fallout (H+hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Dry Lake, Nev	6.3	6.0	2.2	130.0	235.0
Crystal, Nev	4.5	.5	2.5	7.0	13.0
Alamo, Nev	11.7	.1	1.5	4.4	7.7
Nonpopulated points:					
U. S. 91–93, 1 mile southwest of Dry Lake, Nev	6.4	29.0	2.2	650.0	1,200.0
22.8 miles north of Indian Springs, Nev	4.5	310.0	.8	5,900.0	10,000.0


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
H-HOUR MINUS 4 FALLOUT PREDICTN
SHOT NO.2 MOTH 22 FEB.1955
H-HOUR 0545 PST
SILAS MASON CO.
SK-SM 1073
St. George
UTAH
ARIZONA
NEVADA
U.S. 91
Glendale
Lake Mead
U.S. 93
NEVADA
ARIZONA
U.S. 93
U.S. 95
0.1R
Las Vegas
U.S. 91
0.3R
0.2R
1.0R
4R
Indian Springs
Mercury
N. T. S.
C.P.
Desert Rock
U.S. 95
NEVADA
CALIFORNIA


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK SHOT NO.2 "MOTH"
22 FEB.1955 - H-HOUR 0545
SILAS MASON CO.
SK-SM 1070
St. George
UTAH
ARIZONA
U.S. 91
NEVADA
Glendale
U.S. 93
Mead
Lake
ARIZONA
NEVADA
U.S. 95
Las Vegas
U.S. 91
Indian Springs
Mercury
N. T. S.
C.P.
Desert Rock
U.S. 95
NEVADA
CALIFORNIA
10-12,000' MSL
2:25, 2:29, 2:34 Readings taken at 7,000' MSL.
22,000' MSL Top of cloud.


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
LOW LEVEL TERRAIN SURVEY
SHOT NO. 2. MOTH 22 FEB. 1955
H-HOUR 0545 PST
SILAS MASON CO.
SK SM 1072
DATA SHOWN IS FOR
INFINITE DOSE
St. George
U.S. 91
UTAH
ARIZONA
NEVADA
Glendale
U.S. 93
Lake Mead
I.0 R
O.I R
4 R
10 R
NEVADA
ARIZONA
U.S. 93
U.S. 95
Las Vegas
U.S. 91
Indian Springs
Mercury
Desert Rock
U.S. 95
N. T. S.
C.P.
NEVADA
CALIFORNIA


U. S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
INF. DOSE - COMB. AIR & GRD. SURVEY
SHOT NO.2 MOTH 22 FEB.1955
H-HOUR 0545 PST
SILAS MASON CO.
SK-SM 1075
St. George
UTAH
ARIZONA
NEVADA
U.S. 91
U.S. 93
U.S. 95
Glendale
Alamo
Lake Mead
Las Vegas
Indian Springs
Mercury
Desert Rock
C.P.
N. T. S.
NEVADA
CALIFORNIA
1 mr
5 mr
10 mr
100 mr
1R
4R
10R


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
EFF. BIOL. DOSE-COMB. AIR & GRD. SUR.
SHOT NO.2 MOTH 22 FEB.1955
H-HOUR 0545 PST
SILAS MASON CO.
SK-SM 1074
St. George
U.S. 91
UTAH
ARIZONA
NEVADA
0.01 R
Mead
Lake
Glendale
0.5 R
0.1 R
U.S 93
ARIZONA
NEVADA
4 mr
U.S. 93
Alamo
1 mr
1 R
U.S 95
4 R
Las Vegas
U.S. 91
1 mr
Indian Springs
Mercury
N. T. S.
C.P.
Desert Rock
U.S. 95
NEVADA
CALIFORNIA

Tesla

Tesla was a 300-foot tower detonation which was fired at 5:30 a. m., on March 1, 1955. The shot took place in test area 9 in Yucca Flat.

The airway closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles, was ordered closed at all altitudes from 5 a. m. to 8 a. m.
2. A sector bounded by radii at 65° and 125°, length of radius 60 nautical miles, was ordered closed at all altitudes from 5 a. m. to 10 a. m.
3. An extension of this sector to a radius of 90 nautical miles was ordered closed from 13,000 to 19,000 feet from 7:30 a. m. to 10 a. m.
4. A further extension of this sector to a radius of 160 nautical miles was ordered closed from 20,000 to 28,000 feet from 6:30 a. m. to 10 a. m.
5. At 7:47 a. m. the altitudes in sector (3) above were changed to close only between 16,000 to 19,000 feet and the circle in (1) above was opened at all altitudes except for the sector area (65°–125°).

The cloud was tracked as indicated on the accompanying map from H plus 35 minutes to H plus 2 hours and 55 minutes. In addition to reports from the B–25 tracker, reports from sampler aircraft were used to chart the cloud track. Maximum cloud height observed was 30,000 feet, which stabilized rather quickly to 27,000 feet. The cloud was tracked to a point near the northernmost part of Lake Mead, at which time aircraft were recalled.

The low level terrain survey was performed by one C–47 aircraft, which took off at H plus 7 hours and completed the mission at H plus 11 hours and 45 minutes.

The monitoring runs, which indicated activity substantially above background, were made on the Game Preserve Road north of U. S. 95; on U. S. 91 between Beaver Dam, Ariz., and Washington, Utah; along Utah 64 south of St. George, Utah; on Utah 15; on Utah 17; on Utah 18; along the Mormon Mesa Road north of U. S. 91; and on U. S. 93 approximately 27 miles south of Alamo, Nev.

The maximum effective biological dose for a populated area was 177 mr. at Santa Clara, Utah. The maximum effective biological dose at nonpopulated point was 34,000 mr. 10.5 miles south of Groom Road on Papoose Lake.

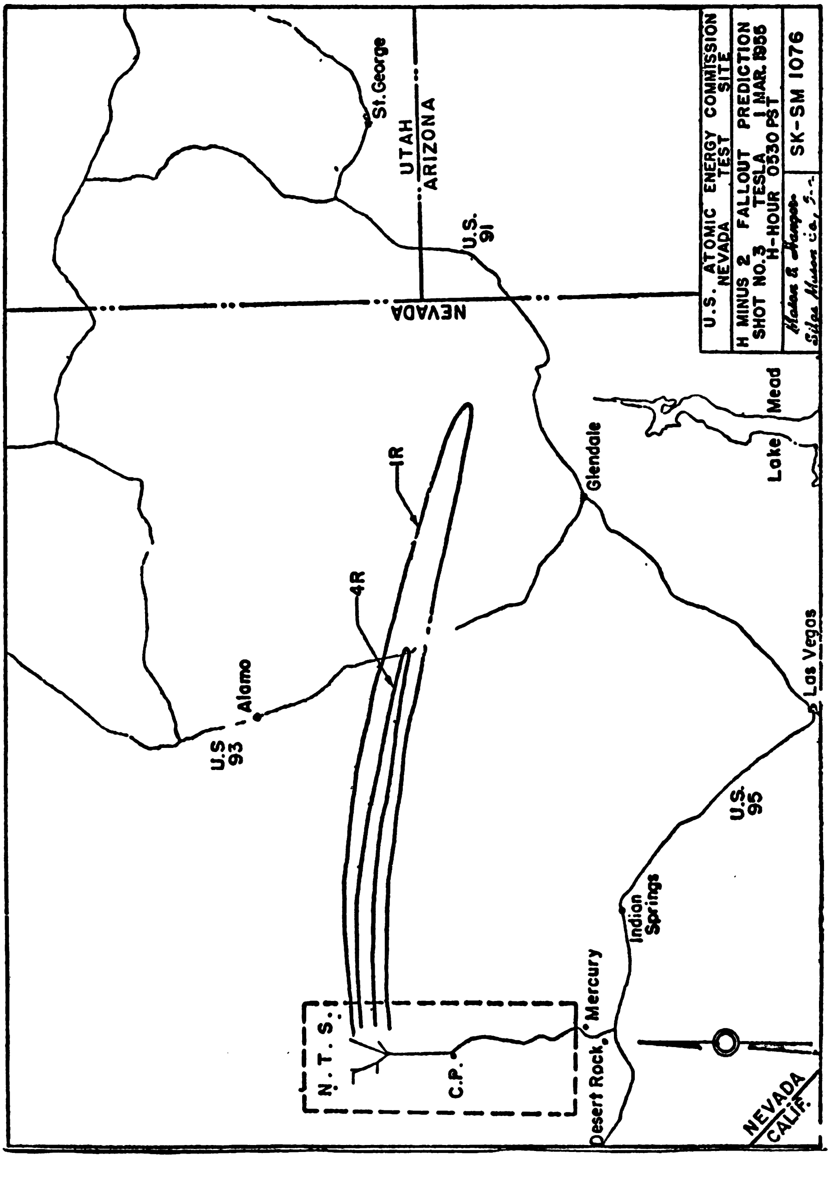
Approximately 190 individual monitoring readings above 0.1 mr/hr were recorded.

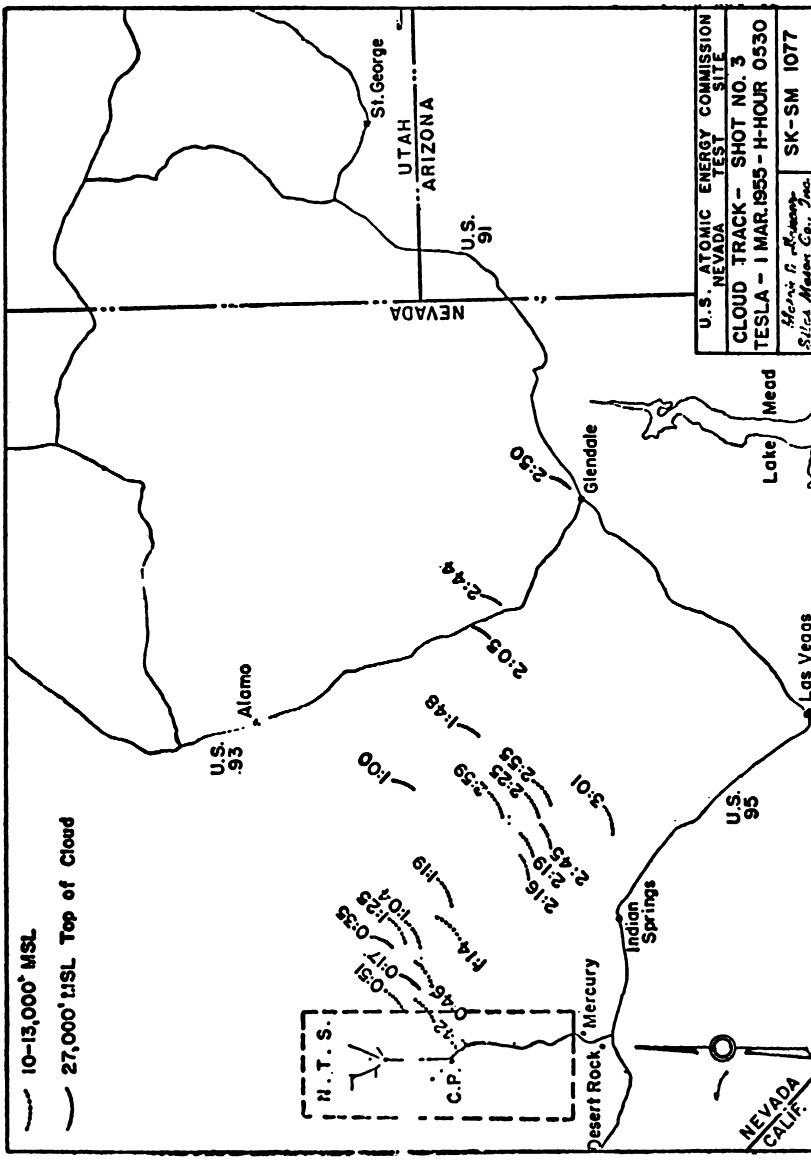
A comparison of the prediction map and the factual maps indiactes good agreement in both direction and magnitude of fallout. Ground monitoring did not indicate fallout in Carp, Nev., as shown on the terrain survey map. This fact was substantiated by the several film badges located in Carp for the entire series. The maximum series dose indicated by a film badge in Carp, Nev., was 60 mr. The 1 r. infinite-dose contour crossed U. S. 93 at a nonpopulated place.

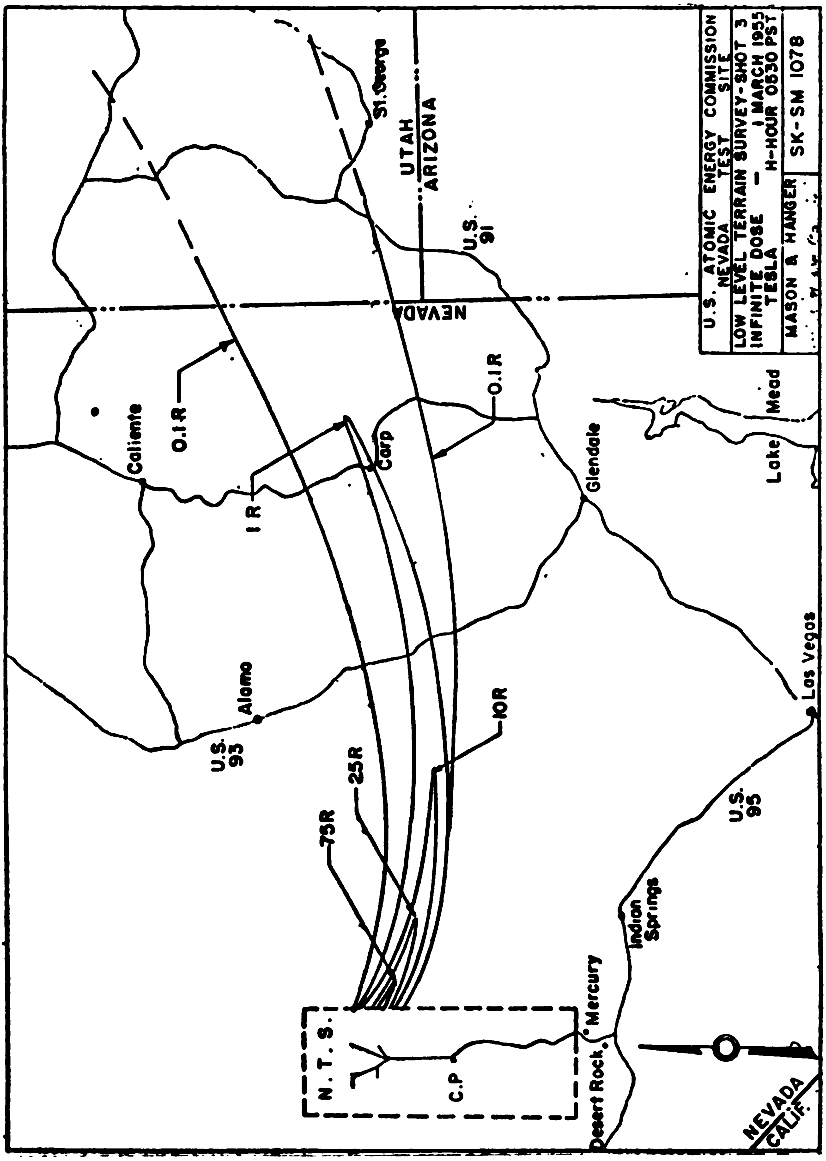
The maximum air concentration measured was $4.0\times10^{-2}\mu c/m^3$, at St. George, Utah. This represents the average air concentration for a 28-hour period starting at shot time.

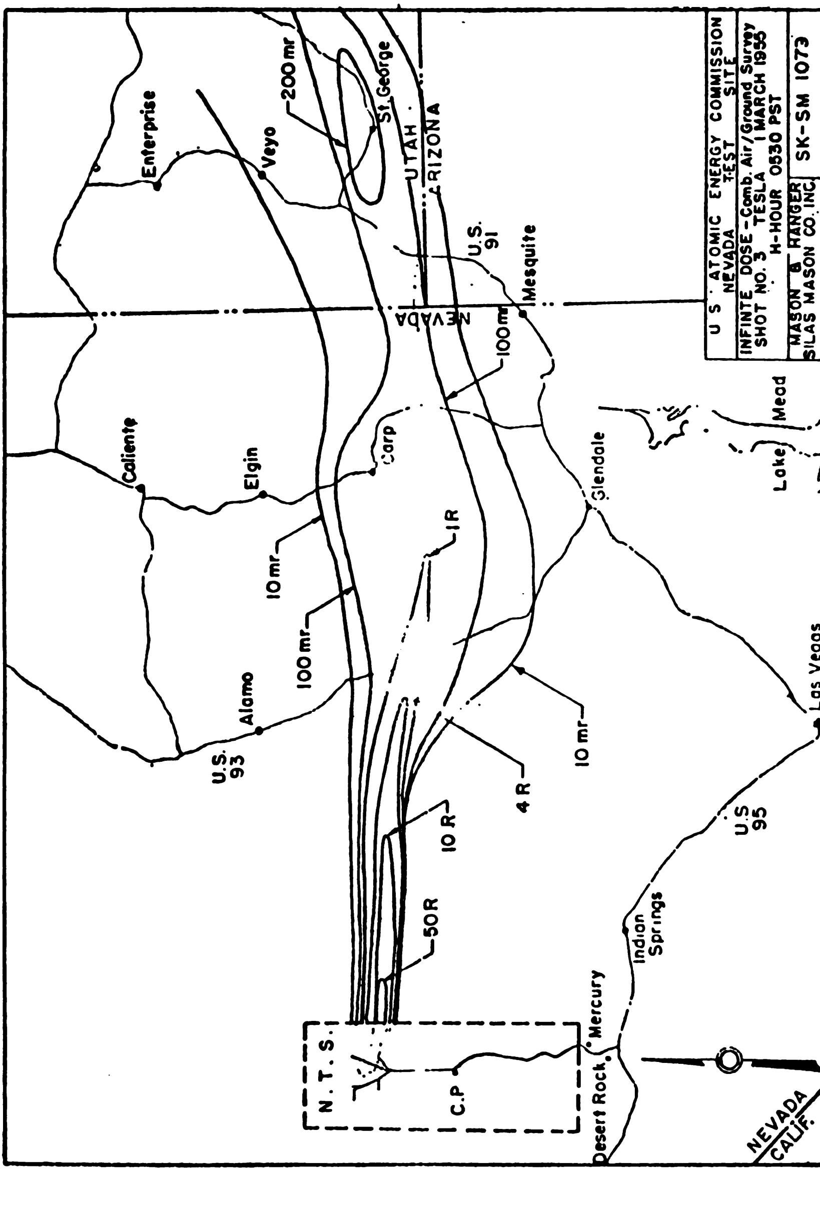
Tesla: External gamma dose in populated areas and at selected nonpopulated points

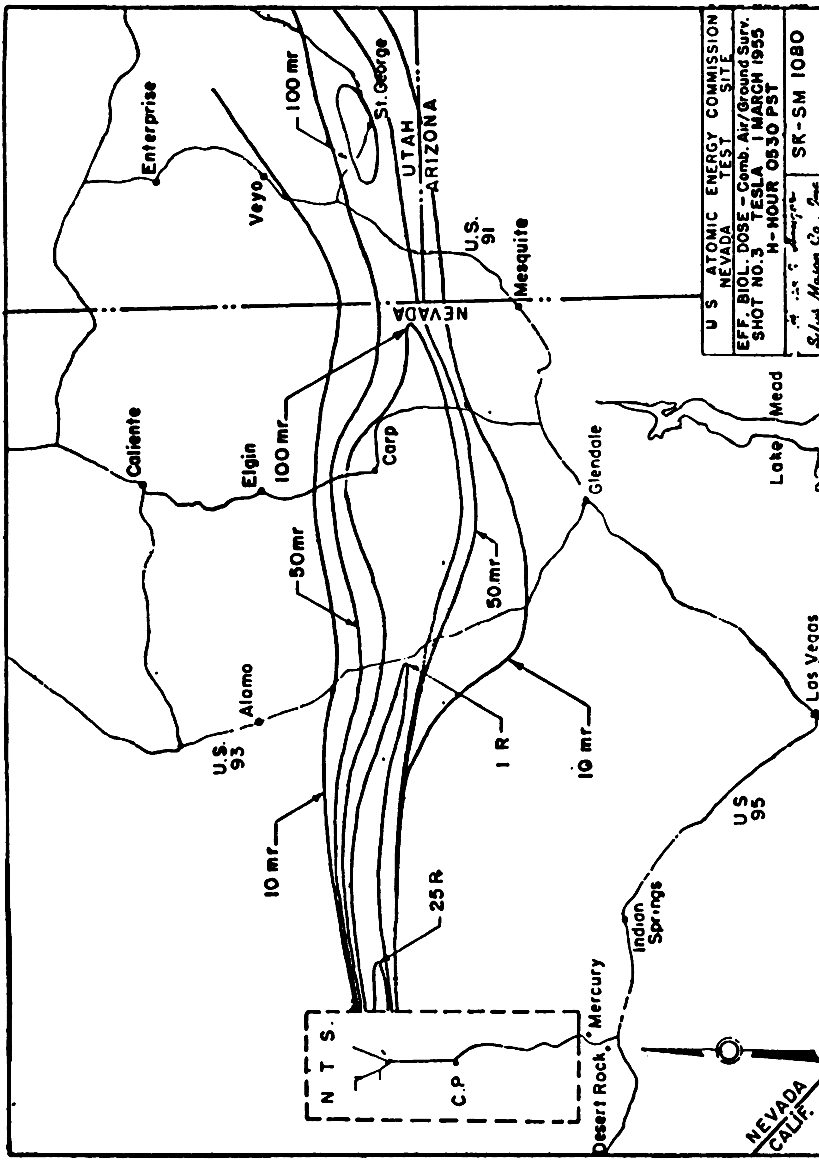
Location	Time of instrument reading (H+hours)	Gamma ground level (mr./hr.)	Time of fallout (H+hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Santa Clara, Utah	11.3	6.0	9.0	177	350
St. George, Utah	10.2	4.0	9.2	103	210
Veyo, Utah	10.5	.5	9.0	14	27
Gunlock, Utah	10.9	.3	8.8	8	17
Washington, Utah	11.5	3.0	9.4	87	175
Hurricane, Utah	11.8	1.7	10.0	51	100
Toquerville, Utah	12.0	1.8	10.0	52	105
Anderson Junction, Utah	12.1	1.1	9.9	34	69
Leeds, Utah	12.2	1.4	9.8	44	88
Alamo, Nev	7.3	.3	4.0	7	12
Ash Springs, Nev	9.2	4.5	4.0	133	246
Nonpopulated points:					
U. S. 93, 27 miles south of Alamo, Nev	5.2	55.0	3.25–5.25	845	1,550
10.5 miles south of Groom Road on Papoose Lake	8.0	966.0	1.2	34,000	56,700


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
H MINUS 2 FALLOUT PREDICTION
SHOT NO. 3 TESLA 1 MAR. 1955
H-HOUR 0530 PST
SK-SM 1076
St. George
UTAH
ARIZONA
NEVADA
U.S. 91
Glendale
Lake Mead
1R
4R
Alamo
U.S 93
Las Vegas
U.S. 95
Indian Springs
Mercury
Desert Rock
N. T. S.
C.P.
NEVADA
CALIF.


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK - SHOT NO. 3
TESLA - 1 MAR. 1955 - H-HOUR 0530
SK-SM 1077
10-13,000' MSL
27,000' MSL Top of Cloud
St. George
UTAH
ARIZONA
U.S. 91
NEVADA
Glendale
Lake Mead
Las Vegas
Alamo
U.S. 93
U.S. 95
Indian Springs
Mercury
Desert Rock
N. T. S.
C.P.
NEVADA
CALIF.
2:50
2:44
2:05
1:48
1:00
3:01
1:19
0:35
0:51
0:17
0:46


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
LOW LEVEL TERRAIN SURVEY-SHOT 3
INFINITE DOSE — 1 MARCH 1955
TESLA H-HOUR 0530 PST
MASON & HANGER
SK-SM 1078
St. George
UTAH
ARIZONA
U.S. 91
NEVADA
O.I.R
Caliente
Carp
O.I.R
I R
Glendale
Lake Mead
Alamo
U.S. 93
Las Vegas
10R
25R
75R
U.S. 95
Indian Springs
Mercury
Desert Rock
N. T. S.
C.P
NEVADA
CALIF.


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
INFINTE DOSE-Comb. Air/Ground Survey
SHOT NO. 3 TESLA 1 MARCH 1955
H-HOUR 0530 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1073
Enterprise
Veyo
200 mr
St. George
UTAH
ARIZONA
U.S. 91
Mesquite
NEVADA
100 mr
Caliente
Elgin
Carp
10 mr
100 mr
1 R
Alamo
U.S. 93
4 R
10 mr
10 R
50 R
Glendale
Lake Mead
Las Vegas
U.S. 95
Indian Springs
Mercury
Desert Rock
N.T.S.
C.P.
NEVADA
CALIF.


U S ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
EFF. BIOL. DOSE - Comb. Air/Ground Surv.
SHOT NO.3 TESLA 1 MARCH 1955
H-HOUR 0530 PST
SK-SM 1080
Enterprise
Veyo
St. George
UTAH
ARIZONA
U.S. 91
Mesquite
NEVADA
100 mr
Caliente
Elgin
Carp
Glendale
Lake Mead
50 mr
Alamo
U.S. 93
1 R
10 mr
Las Vegas
US 95
25 R
Indian Springs
Mercury
Desert Rock
N T S.
C.P.
NEVADA
CALIF.

TURK

Turk was a 500-foot tower detonation which was fired at 5: 20 a. m. on March 7, 1955. The detonation took place in test area 2 in Yucca Flat.

The airway-closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles, was closed at all altitudes from 5 a. m. to 9: 30 a. m.
2. An area southwest of the test site, bounded as described below, was ordered closed from 25,000 to 30,000 feet from 7 a. m. to 12: 30 p. m. From EH0000 due south to EG0000, then southwest to the east edge of airway Amber 1 at Bakersfield, then along the east edge of Amber 1 to AH0000, then due east to EH0000.
3. An area north and east of the test site, bounded as described below, was ordered closed from 32,000 feet up from 6: 30 a. m. to 12: 30 p. m. From EH 0000 on a bearing of 42° true for 320 nautical miles, then due south to JG4500, then due west to EG0000, then due north to EH0000.

At 10: 23 a. m. conditions were such that closure time in areas 2 and 3 was cut back to 11 a. m. instead of 12: 30 p. m.

The cloud was tracked as shown on the accompanying map from H plus 30 minutes to H plus 4 hours and 55 minutes. Tracking was performed by the following aircraft:

B–50: initially at 28,000 feet and subsequently at 31,000 and 25,000 feet.
B–29: 20,000 to 23,000 feet.
B–25: 11,000 to 14,000 feet.
Samplers (F–84): 36,000 to 42,000 feet.

Maximum cloud height observed was 42,500 feet. The wind pattern at shot time was such that the cloud became broken and dispersed in a very short time, with two general zones containing most of the cloud components. At altitudes up to about 28,000 feet, the cloud generally drifted into the northwest quadrant from GZ, with the maximum distance being approximately 85 nautical miles at 20,000 to 23,000 feet bearing from CP 315° true. The second zone was between 40° and 105° true at high altitudes (above 30,000 feet), extending to 105 nautical miles at 105° true and about 130 nautical miles at about 75° true. In many instances, the cloud appeared to have several different leading edges, and at times doubled back on its previous path.

A low-level terrain survey was flown on D-day by one C–47 from about H plus 6.5 to H plus 11. The fallout pattern indicated by this flight was quite scattered, and on D plus 1, additional flights were made by the C–47 and by a helicopter.

The pattern shown on the accompanying map was drawn using data from all three flights. It is evident that, just as the cloud-track pattern was widespread and, in several directions, so also was the fallout pattern.

Monitoring runs, which indicated activity substantially above background, were made along U. S. 93 in the vicinity of Pioche, Nev.; on Utah 21 between Beaver, Utah, and the Utah-Nevada State line; on U. S. 95 between Beatty, Nev., and Goldfield, Nev.; along Utah 15 in the Toquerville, Utah, area; along U. S. 6 between Tonopah, Nev., and Ely, Nev.; in the vicinity of Ely, Nev.; along Nevada 25 between Crystal Springs, Nev., and Warm Springs, Nev.; and on the desert roads northeast and west of the Nevada test site.

The maximum effective biological dose for a populated area was 157 mr. at Warm Springs, Nev. The maximum effective biological dose at a nonpopulated point was 30,500 mr. 15 miles west of Nevada test site on a desert road.

Approximately 220 individual monitoring readings above 0.1 mr./hr. were recorded.

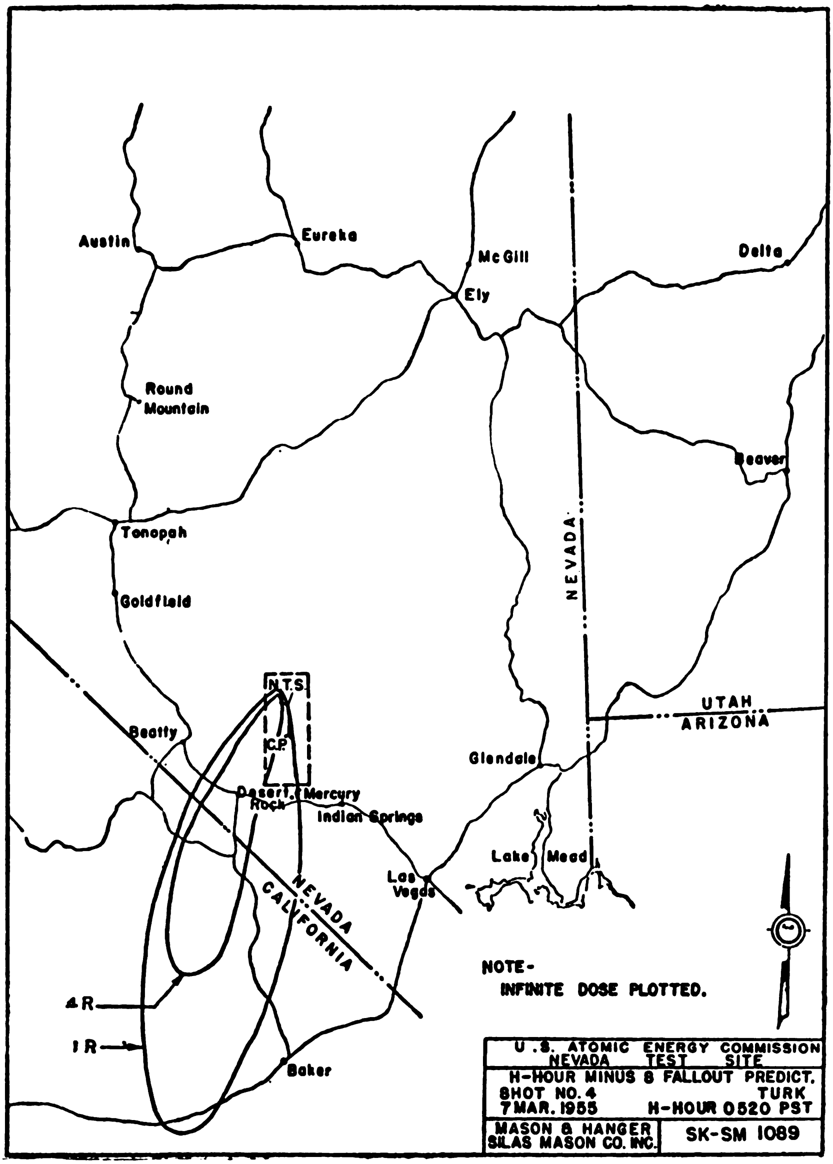
A comparison of the prediction map and the factual maps indicates an extreme overprediction. This came about as a result of a frontal system change which occurred near shot time. This frontal change resulted in a drastic reduction in wind speeds and a rapid shifting of wind directions.

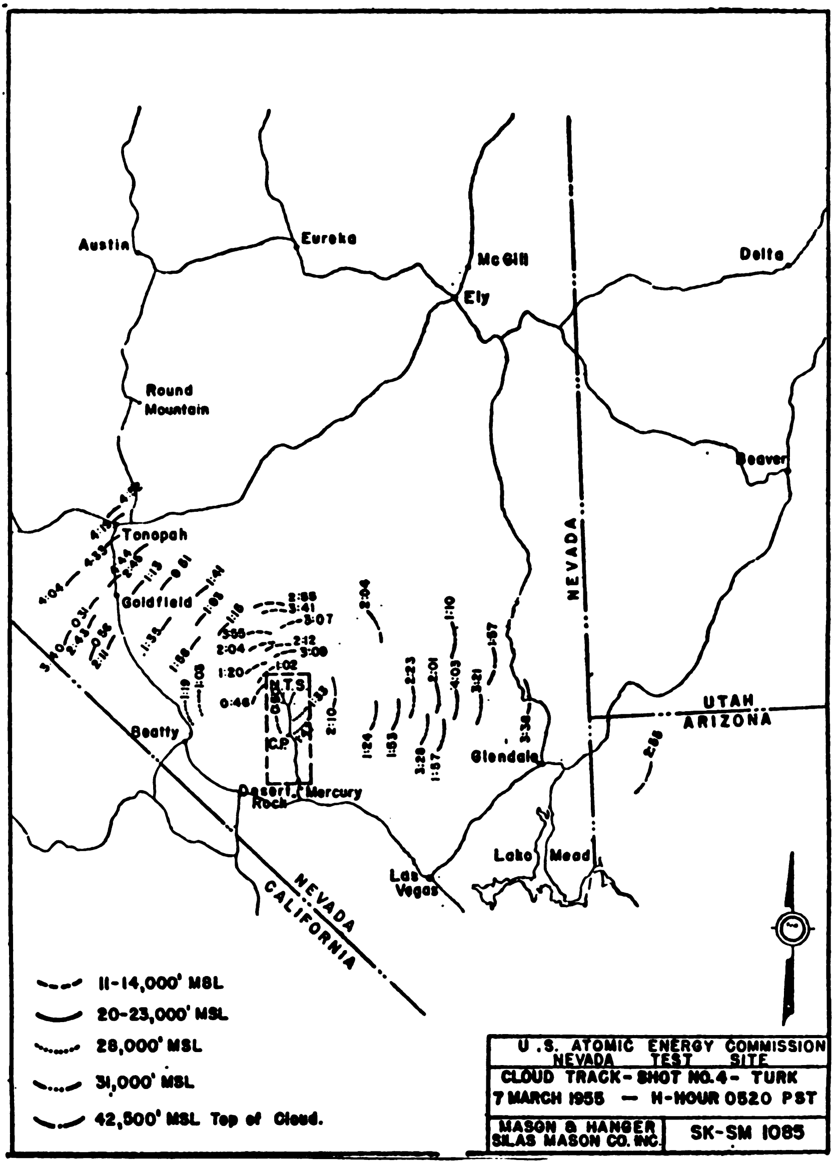
The ground-survey map shows the wide scattering of fallout under the meteorological conditions mentioned above. Also, the effects of terrain on the fallout pattern are quite pronounced. It is apparent from this map that the 1 r. isodose contour did not intersect any major highways.

The maximum air radioactivity concentration measured was $1.7\times10^{-3}\mu c/m^3$. at Currant, Nev. This represents the average air concentration for a 29-hour period starting three-quarters of an hour after shot time.

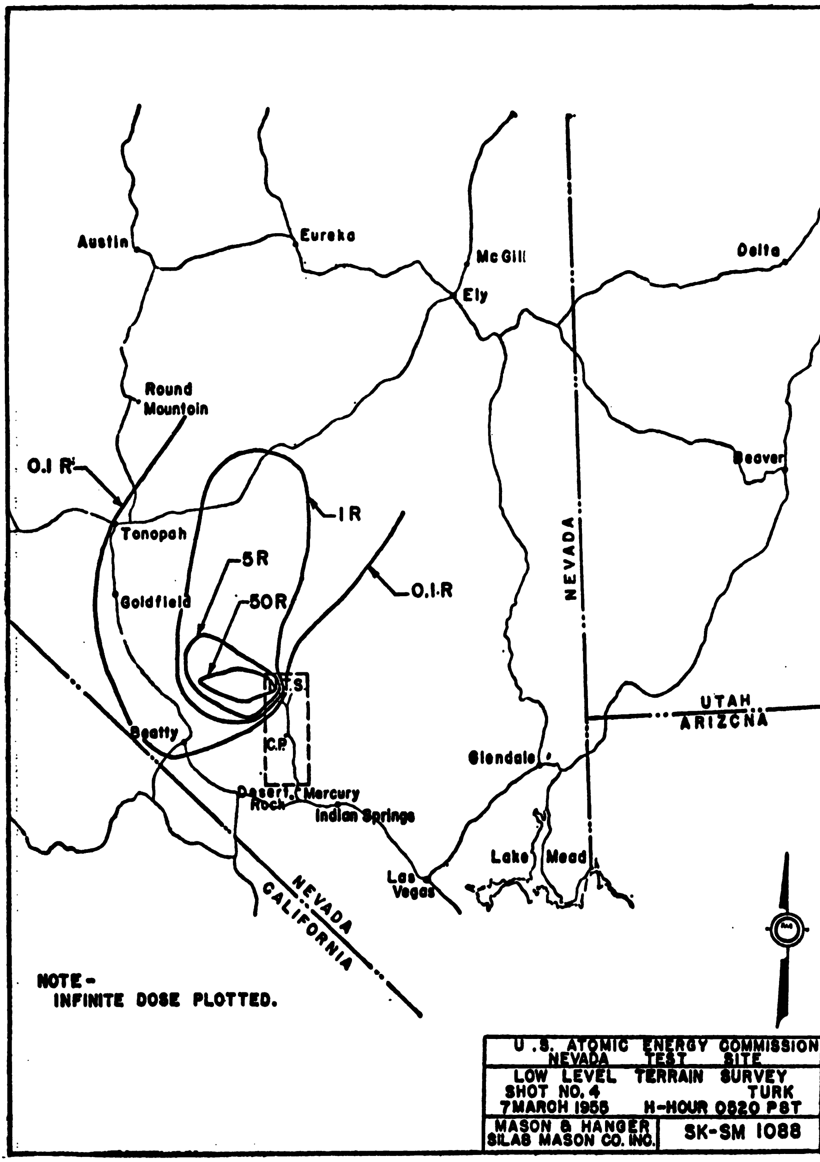
Turk: External gamma dose in populated areas and at selected nonpopulated points

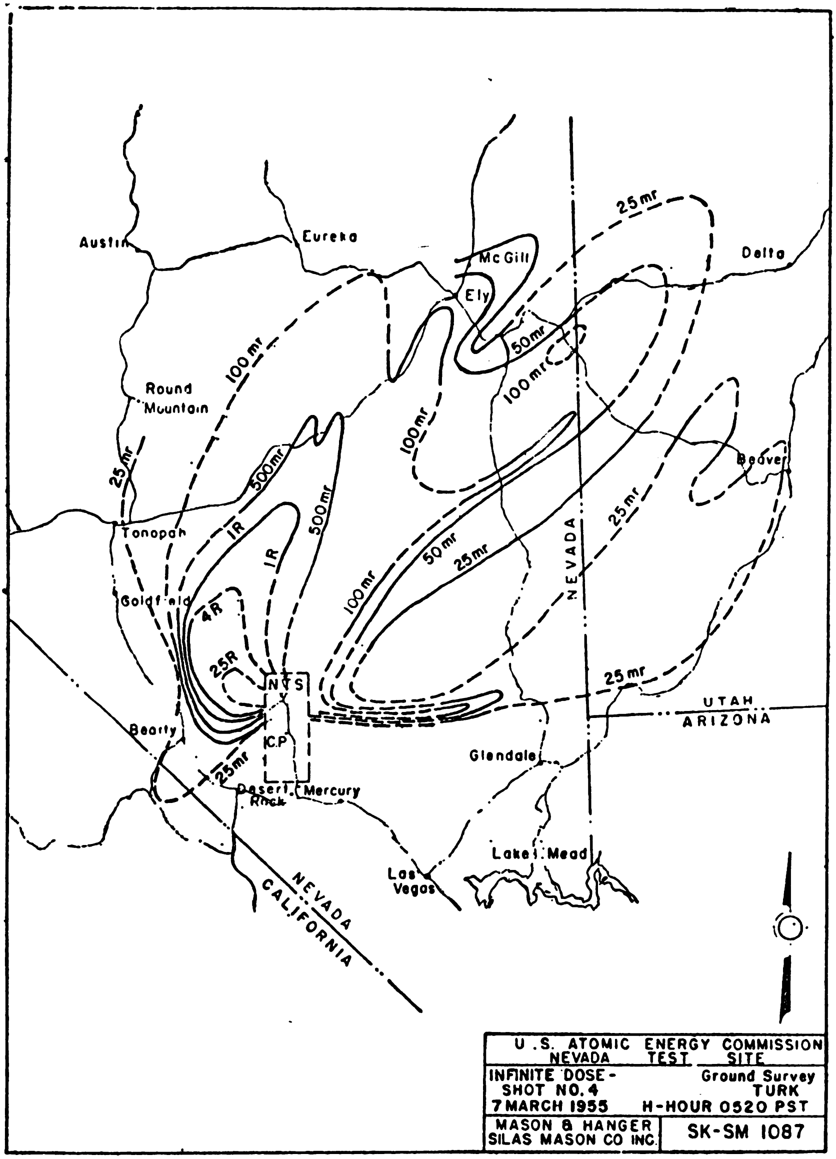
Location	Time of instrument reading (H+hours)	Gamma ground level (mr./hr.)	Time of fallout (H+hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Rockville, Utah	11.3	0.10	11.0	3	6
Toquerville, Utah	12.2	.37	11.0	11	24
Lincoln Mine, Nev	16.7	.70	8.0	34	65
Frisco, Utah	29.8	.20	15.0	17	35
Desert Game Experiment Station, Utah	30.7	.23	14.5	19	41
Garrison, Utah	32.0	.38	15.0	34	71
Baker, Utah	32.1	.58	15.0	53	113
Pioche, Nev	13.9	.30	11.7	11	21
Ursine, Nev	14.2	.25	13.0	9	18
McGill, Nev	33.8	.17	18.0	16	32
Ely, Nev	28.4	1.00	17.5	74	156
New Ruth, Nev	34.4	.37	17.5	34	74
Preston, Nev	30.0	1.00	15.5	80	172
Lund, Nev	28.5	1.18	15.1	93	192
Currant, Nev	29.4	.98	14.6	79	164
Lockes Ranch, Nev	31.6	1.48	13.5	132	279
Wahwa Springs, Utah	30.1	.10	15.0	8	17
Warm Springs, Nev	34.8	1.50	12.0	157	324
Nonpopulated points:					
6 miles south of Lockes on U. S. 6	31.0	3.00	13.4	253	549
15 miles west of Nevada test site on desert road	31.2	300.00	2.0	30,500	64,000


Austin
Eureka
McGill
Ely
Delta
Round Mountain
Beaver
Tonopah
Goldfield
NEVADA
UTAH
ARIZONA
N.T.S.
C.P.
Beatty
Glendale
Desert Rock
Mercury
Indian Springs
Lake Mead
Las Vegas
NEVADA
CALIFORNIA
NOTE-
INFINITE DOSE PLOTTED.
4 R
1 R
Baker
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
H-HOUR MINUS 8 FALLOUT PREDICT.
SHOT NO. 4 TURK
7 MAR. 1955 H-HOUR 0520 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1089



Austin
Eureka
McGill
Ely
Delta
Round Mountain
Beaver
Tonopah
Goldfield
NEVADA
UTAH
ARIZONA
N.T.S.
C.P.
Beatty
Glendale
Desert Rock
Mercury
Lake Mead
Las Vegas
NEVADA
CALIFORNIA
11-14,000' MSL
20-23,000' MSL
28,000' MSL
31,000' MSL
42,500' MSL Top of Cloud.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK-SHOT NO.4- TURK
7 MARCH 1955 — H-HOUR 0520 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1085


Austin
Eureka
McGill
Ely
Delta
Round Mountain
0.1 R
1 R
Tonopah
5 R
Goldfield
50 R
0.1 R
N.T.S.
C.P.
Beatty
Desert Rock
Mercury
Indian Springs
Las Vegas
Lake Mead
Glendale
Beaver
NEVADA
UTAH
ARIZONA
NEVADA
CALIFORNIA
NOTE-
INFINITE DOSE PLOTTED.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
LOW LEVEL TERRAIN SURVEY
SHOT NO. 4 TURK
7 MARCH 1955 H-HOUR 0520 PST
MASON & HANGER SILAS MASON CO. INC.
SK-SM 1088



Hornet

Hornet was a 300-foot tower detonation which was fired at 5:20 a. m. on March 12, 1955. The shot took place in test area 3 in Yucca Flat.

The airway closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles, was closed at all altitudes from 5 a. m. to 9 a. m.
2. A sector, bounded by radii at 70° and 130°, length of radius 125 nautical miles, was ordered closed from 21,000 to 32,000 feet from 6:50 a. m. to 12 noon.

3. That part of the above sector extending between radial distances of 125 nautical miles and 200 nautical miles was ordered closed from 30,000 feet to 40,000 feet from 6 a. m. to 11:30 a. m.

4. At 9:15 a. m., the situation was such that sectors 2 and 3, above, were opened after 10:30 a. m.

The cloud was tracked at three levels: 10,000 to 14,000; 23,000 to 30,000; and 36,000 feet, by B-35, B-29, and sampler aircraft respectively. The track obtained is shown on the accompanying map. Maximum cloud height observed was 39,300 feet. The cloud was tracked to a distance of about 140 nautical miles from the CP, becoming so dispersed at this distance as to render further tracking impractical.

A low-level terrain survey mission was flown by one C-47 aircraft from H plus 6 hours and 40 minutes to H plus 11 hours. The fallout pattern obtained from the data reported is shown on the accompanying map. The fallout pattern was determined out to a distance of 100 nautical miles at which point low radiation levels made continuation impracticable.

Monitoring runs, which indicated activity substantially above background, were made on the game preserve road north of U. S. 95; on the Mormon Mesa road north of U. S. 91; on U. S. 91 between the Nevada-Utah State line and Glendale, Nev.; along U. S. 93 between Panaca, Nev., and Glendale, Nev.; on Nevada 12; on Nevada 40; along the desert road north of Indian Springs, Nev.; along the roads in the Moapa Indian Reservation; and on the desert roads just east of the Nevada test site.

The maximum effective biological dose for a populated area was 293 mr. at Glendale, Nev. The maximum effective biological dose at a nonpopulated point was 3,502 mr. 29.5 miles north of Indian Springs, Nev.

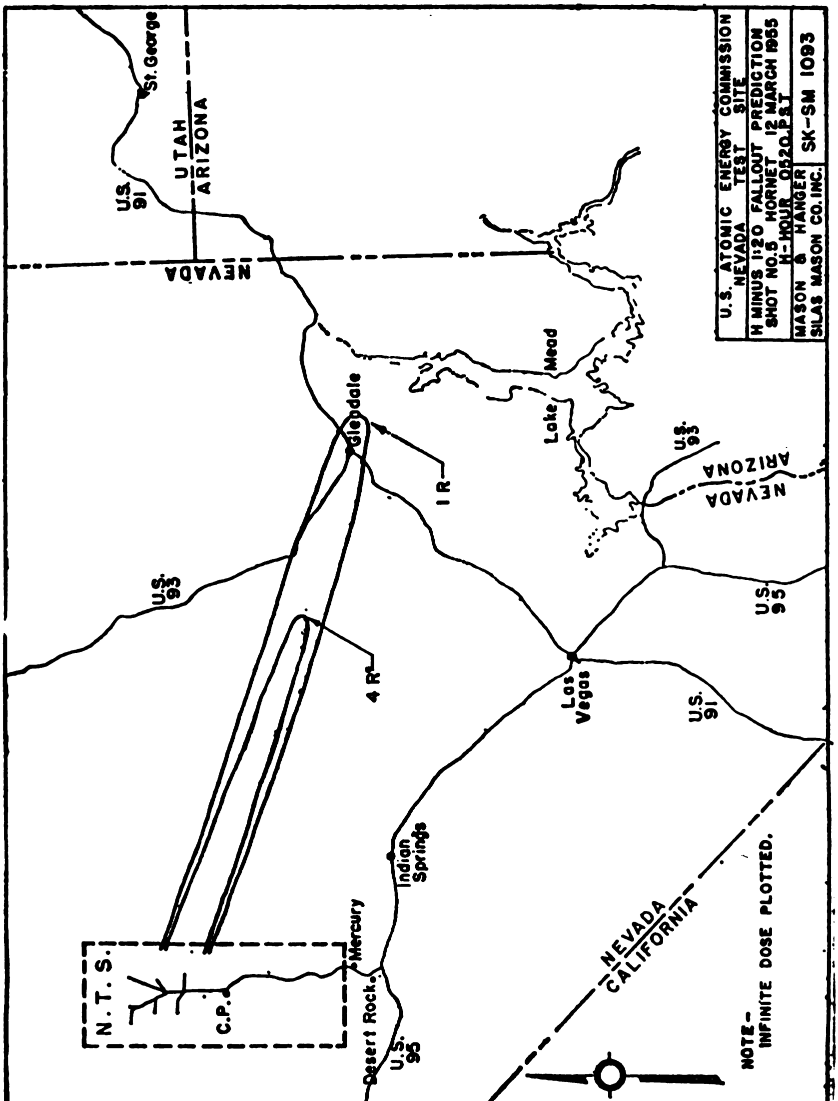
Approximately 300 individual monitoring readings above 0.1 mr/hr were recorded.

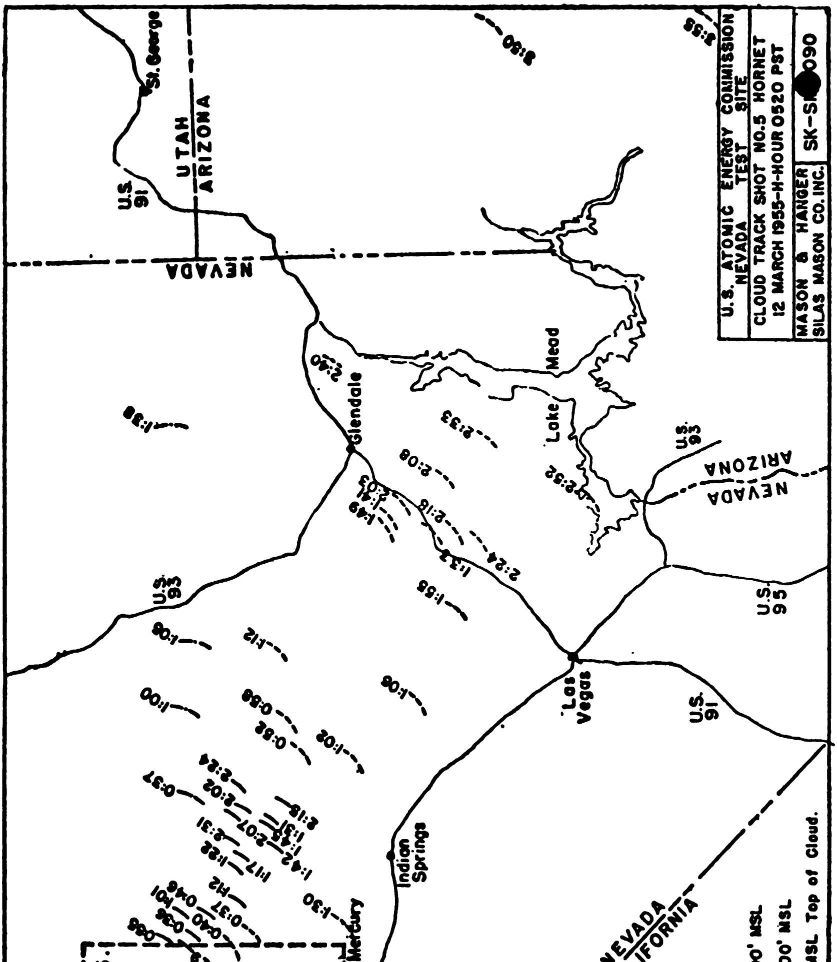
A comparison of the prediction map and the factual maps indicates good agreement in direction and relatively good agreement in magnitude. However, the 4 r. and 1 r. contours actually extended about 50 percent of the predicted distance. The ground survey map shows the results of directional shear which resulted in fallout in the Alamo, Nev., area. It also indicated the uneven isodose contours that may be expected, particularly at relatively low doses. It is apparent from the ground survey maps that the 1 r. isodose contour did not intersect any major highways.

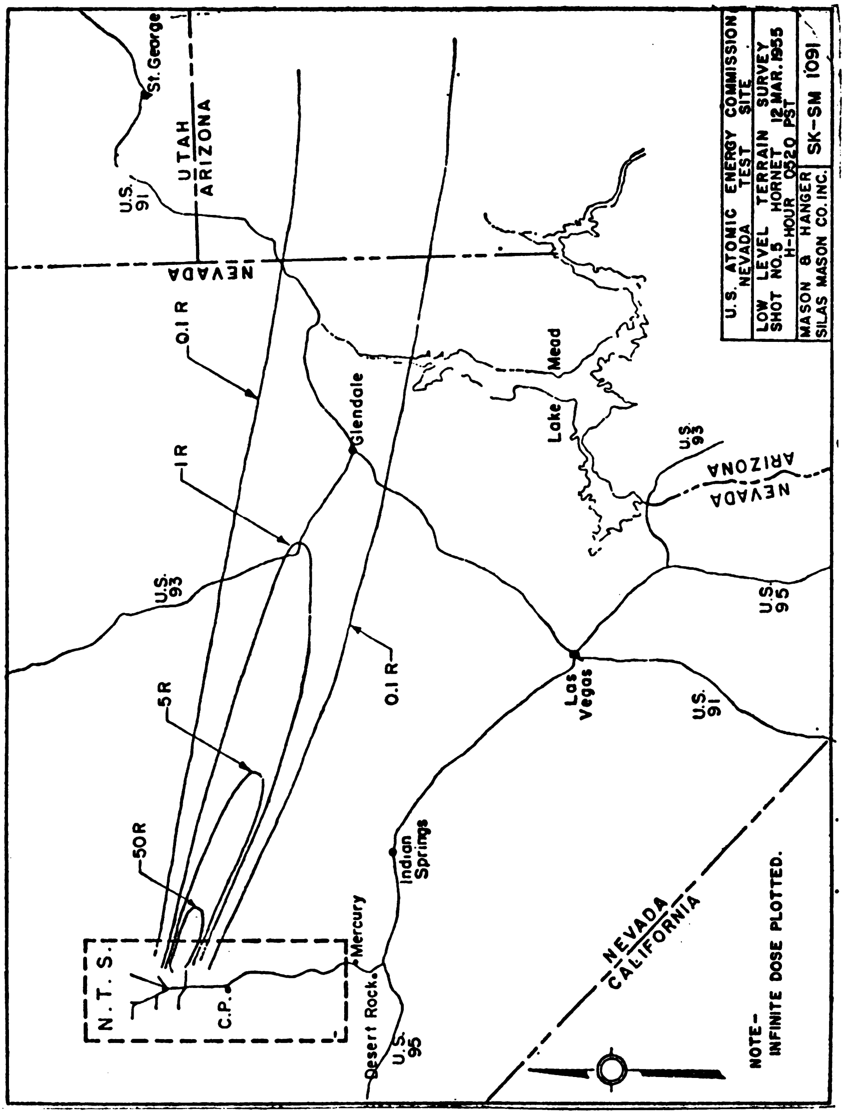
The maximum air radioactivity concentration measured was 1.2 by 10^{-2} $\mu c/m^3$ at Glendale, Nev. This represents the average air concentration for a 28-hour period starting at shot time.

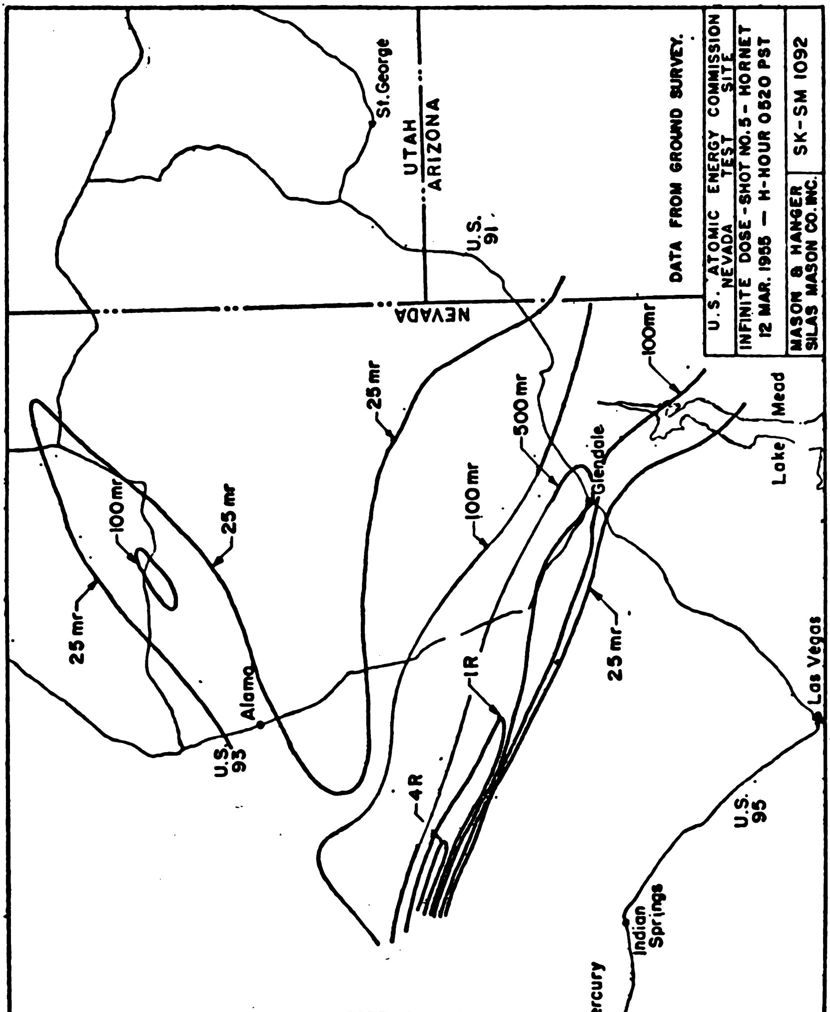
Hornet: External gamma does in populated areas and at selected nonpopulated points

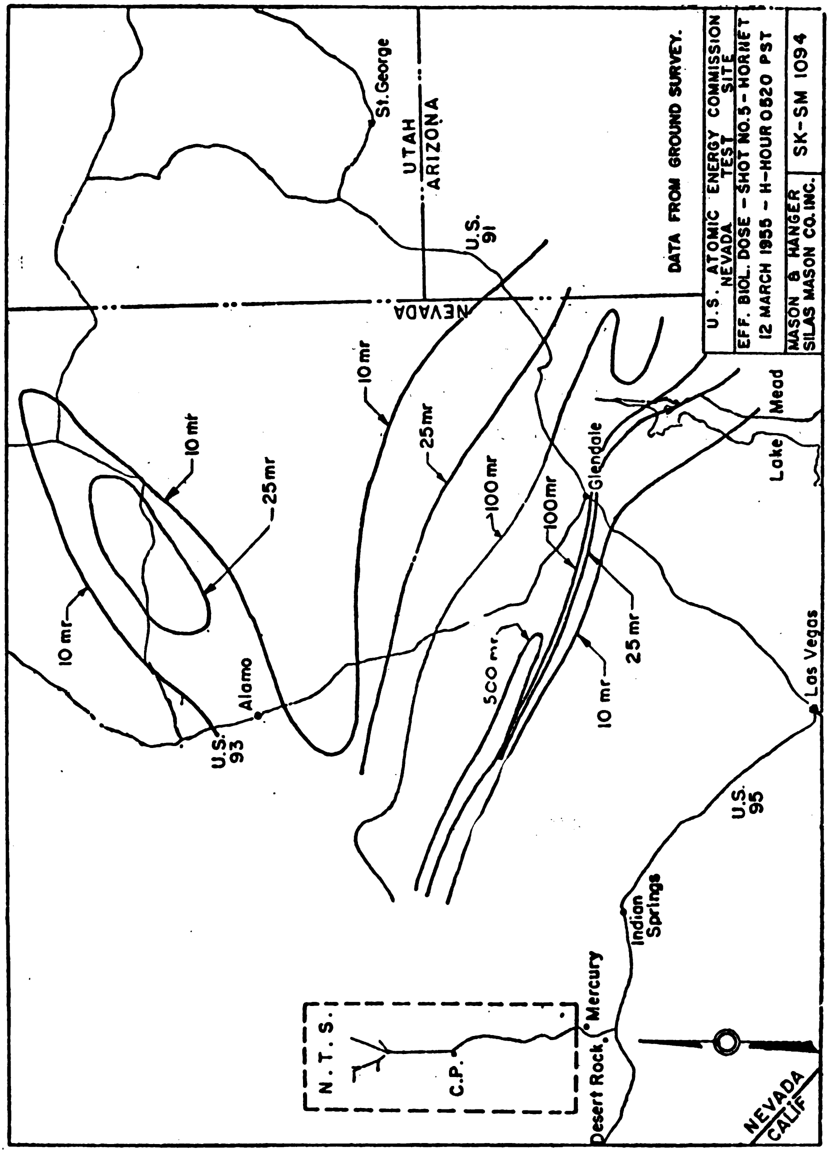
Location	Time of instrument reading (H+hours)	Gamma ground level (mr./hr.)	Time of fallout (H+hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Riverside, Nev	8.3	2.0	8.0	43	84
Bunkerville, Nev	11.7	.6	8.6	19	37
Mesquite, Nev	12.0	.3	8.7	10	19
Littlefield, Ariz	12.5	.3	9.3	10	20
Glendale, Nev	7.7	14.0	6.0	293	564
Logandale, Nev	9.1	.7	7.3	17	33
Carp, Nev	10.4	.4	8.5	10	20
Moapa Indian Reservation, Nev	6.5	10.0	5.5	176	336
Warm Springs Ranch, Nev	5.2	7.0	5.0	97	184
Caliente, Nev	8.5	2.8	8.5	60	119
Panaca, Nev	9.5	.7	9.9	17	33
Alamo, Nev	6.9	.14	5.3	3	5
Ash Springs, Nev	7.3	.5	5.3	9	18
Nonpopulated points:					
U. S. 91, 5 miles east of Glendale, Nev	11.9	10.0	7.1	342	664
29.5 miles north of Indian Spring, Nev	4.8	210.0	1.8	3,502	6,124


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
H MINUS 1:20 FALLOUT PREDICTION
SHOT NO.5 HORNET 12 MARCH 1955
H-HOUR 0520 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1093
St. George
UTAH
ARIZONA
U.S. 91
NEVADA
Glendale
Lake Mead
1 R
U.S. 93
NEVADA
ARIZONA
U.S. 93
4 R
U.S. 95
Las Vegas
U.S. 91
Indian Springs
Mercury
Desert Rock
U.S. 95
N. T. S.
C.P.
NEVADA
CALIFORNIA
NOTE-
INFINITE DOSE PLOTTED.


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK SHOT NO.5 HORNET
12 MARCH 1955-H-HOUR 0520 PST
MASON & HANGER
SILAS MASON CO. INC.
10-14,000' MSL
23-30,000' MSL
36,000' MSL Top of Cloud.
St. George
UTAH
ARIZONA
NEVADA
CALIFORNIA
Glendale
Lake Mead
Las Vegas
Indian Springs
Mercury
Desert Rock
N.T.S.
C.P.
U.S. 91
U.S. 93
U.S. 95


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
LOW LEVEL TERRAIN SURVEY
SHOT NO.5 HORNET 12 MAR. 1955
H-HOUR 0520 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1091
St. George
UTAH
ARIZONA
NEVADA
U.S. 91
U.S. 93
U.S. 95
0.1 R
1 R
5 R
50 R
0.1 R
Glendale
Lake Mead
Las Vegas
Indian Springs
Mercury
Desert Rock
C.P.
N. T. S.
NEVADA
CALIFORNIA
NOTE-
INFINITE DOSE PLOTTED.


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
INFINITE DOSE - SHOT NO.5 - HORNET
12 MAR. 1955 — H-HOUR 0520 PST
MASON & HANGER
SILAS MASON CO.INC.
SK-SM 1092
DATA FROM GROUND SURVEY.
St.George
UTAH
ARIZONA
U.S. 91
NEVADA
25 mr
500 mr
100 mr
100mr
25 mr
100mr
25 mr
25 mr
1R
4R
Glendale
Lake Mead
Las Vegas
Alamo
U.S. 93
U.S. 95
Indian Springs
Mercury
Desert Rock
N. T. S.
C.P.
NEVADA
CALIF.


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
EFF. BIOL. DOSE - SHOT NO. 5 - HORNET
12 MARCH 1955 - H-HOUR 0520 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1094
DATA FROM GROUND SURVEY.
St. George
UTAH
ARIZONA
NEVADA
U.S. 91
U.S. 93
U.S. 95
Alamo
Glendale
Las Vegas
Lake Mead
Indian Springs
Mercury
Desert Rock
C.P.
N. T. S.
NEVADA
CALIF
10 mr
25 mr
100 mr
10 mr
25 mr
10 mr
25 mr
100 mr
10 mr
25 mr
500 mr

Bee

Bee was a 500-foot tower detonation which was fired at 5:05 a. m. on March 22, 1955. The shot took place in test area 7 in Yucca Flat.

The airway closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles, was ordered closed at all altitudes from 4:45 a. m. to 5:30 a. m.
2. A sector as described below was ordered closed from 5:45 a. m. to 10:15 a. m. at altitudes from 17,000 to 40,000 feet. The boundaries of the sector were: From CP along 100° radius to LF 5745, then south to LD 4730, east to Phoenix, Ariz., then along north edge of airway Green 5 to FD 2740, and finally back to CP along 160° radius.
3. At 7:25 a. m. airway Amber 2 was closed at all altitudes within the above sector for 1 hour.
4. At 7:30 a. m. airway Red 15 was ordered closed at all altitudes between Amber 2 and Green 4 for 1 hour.
5. At 8:40 a. m. Amber 2 was opened at all altitudes.

The cloud was tracked by B–25, B–29, and sampler aircraft to a maximum distance of about 130 nautical miles on a general bearing of about 130°. The plot made from reports of these aircraft is shown on the accompanying map. The cloud stabilized at about 39,000 feet, and separated into several layers below the top altitude.

No D-day low-level terrain survey was made by the C–47 as ground parties were reporting fairly low intensities. On D-plus-one day, a survey was made to cover the areas inaccessible to ground parties. It was not possible to obtain reliable results from this survey because the cloud from Ess (detonated the day after Bee) drifted along virtually the same path as that from Bee, such that fallout from both shots would be expected in the same areas.

Monitoring runs, which indicated activity substantially above background, were made on U. S. 93 between Las Vegas, Nev., and a point 30 miles southeast of Boulder City, Nev.; along U. S. 91 between a point 2 miles northeast of Nellis Air Force Base and the intersection of South Fifth Street (Las Vegas proper); in North Las Vegas, Nev.; in the northeast section of Las Vegas, Nev.; on the game preserve road north of U. S. 95; along the desert road north of Indian Springs, Nev.; and on several of the desert roads east of the Nevada test site.

The maximum effective biological dose for a populated area was 185 mr. at North Las Vegas, Nev. The maximum effective biological dose at a nonpopulated point was 26,400 mr. 4.7 miles south of Papoose Lake.

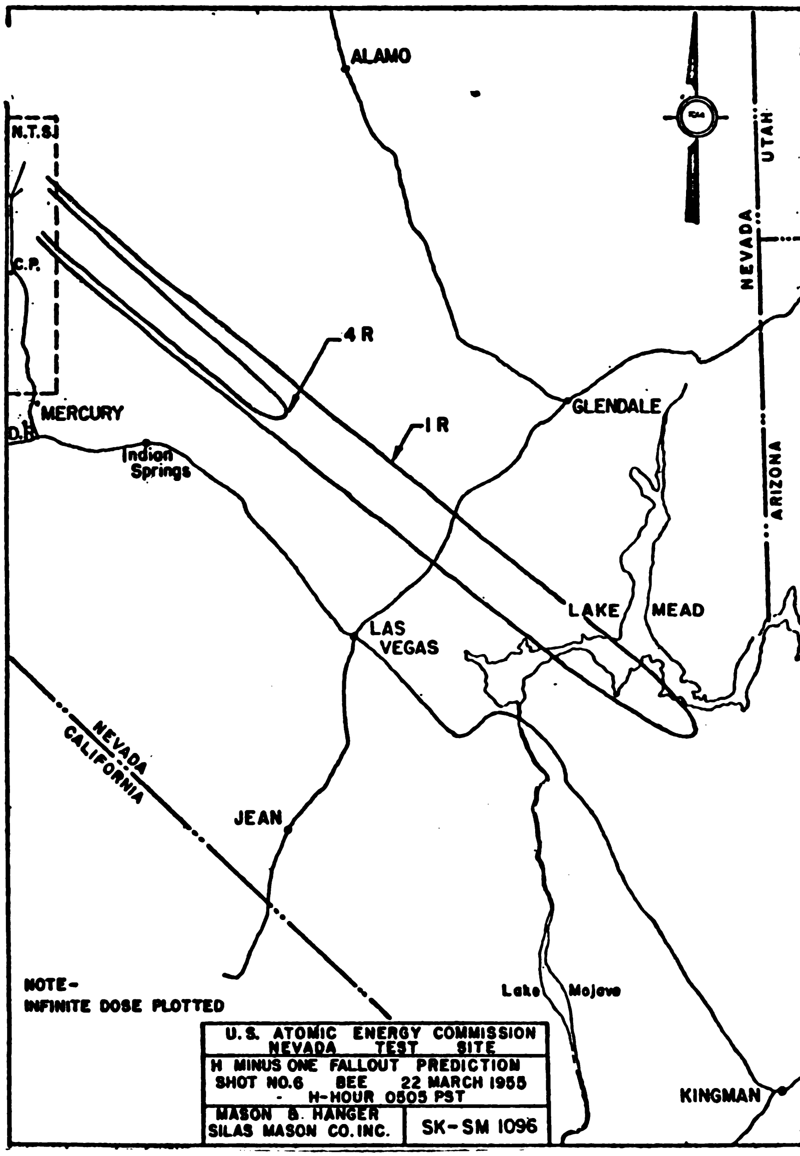
Approximately 170 individual monitoring readings above 0.1 mr./hr. were recorded.

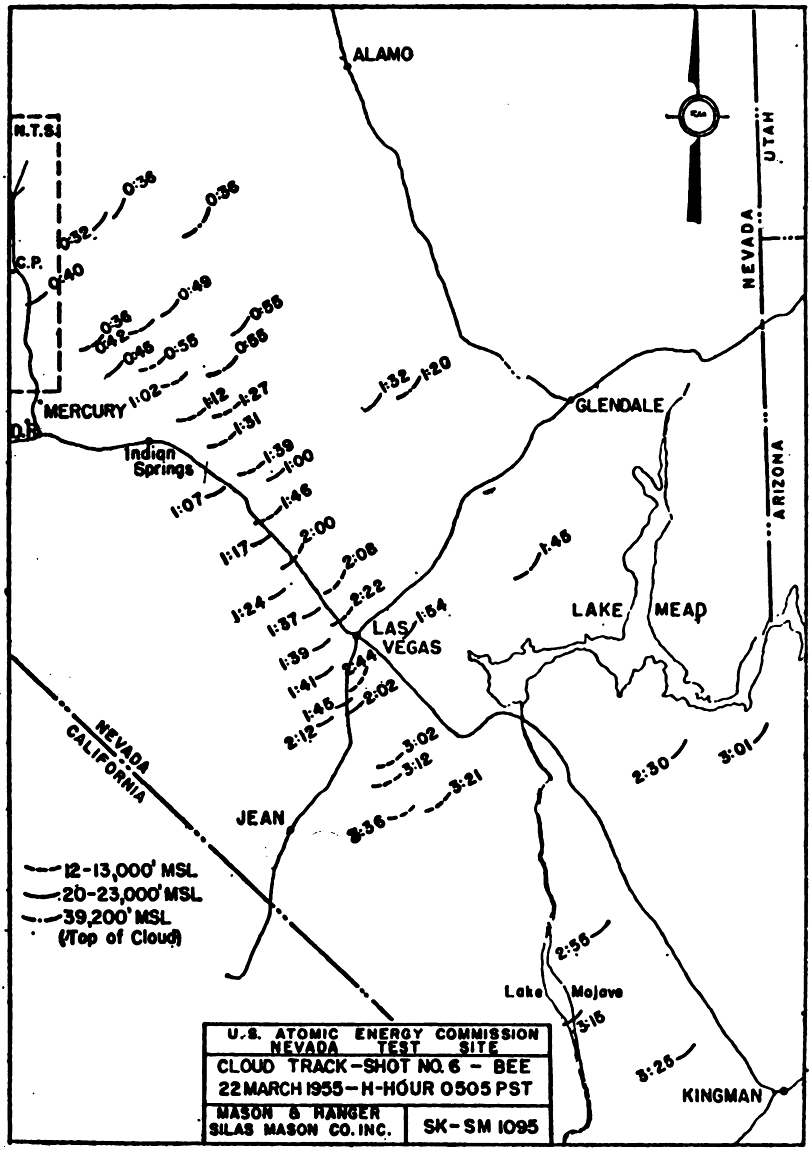
A comparison of the prediction map and the factual maps indicates rather poor agreement in direction and especially in magnitude. Fallout occurred south of the predicted path and the 1 r. infinite isodose contour extended out only about 20 percent of the predicted distance. It is apparent from the factual map that the 1 r. isodose contour did not intersect any major highways.

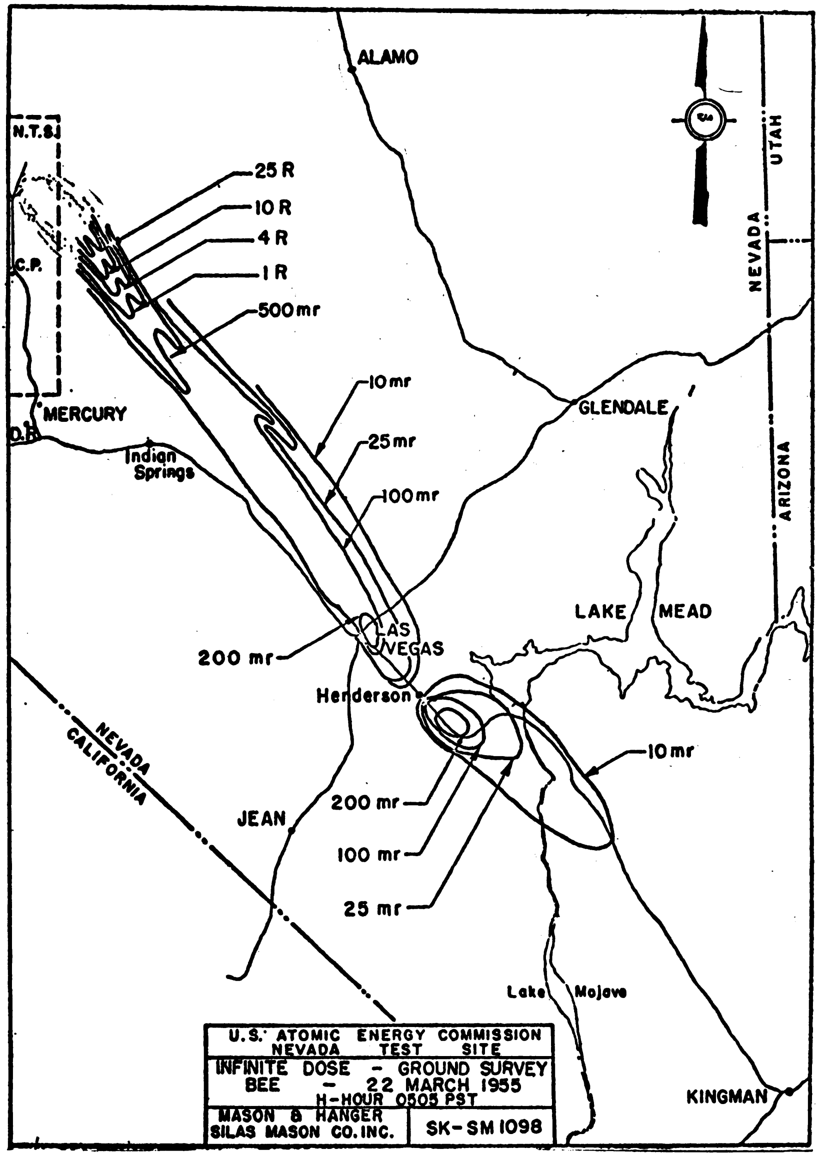
The maximum air radioactivity concentration measured was $3.5\times10^{-3}\mu c/m^3$, at Nellis Air Force Base, Nev. This represents the average air concentration for a 12-hour period starting at shot time.

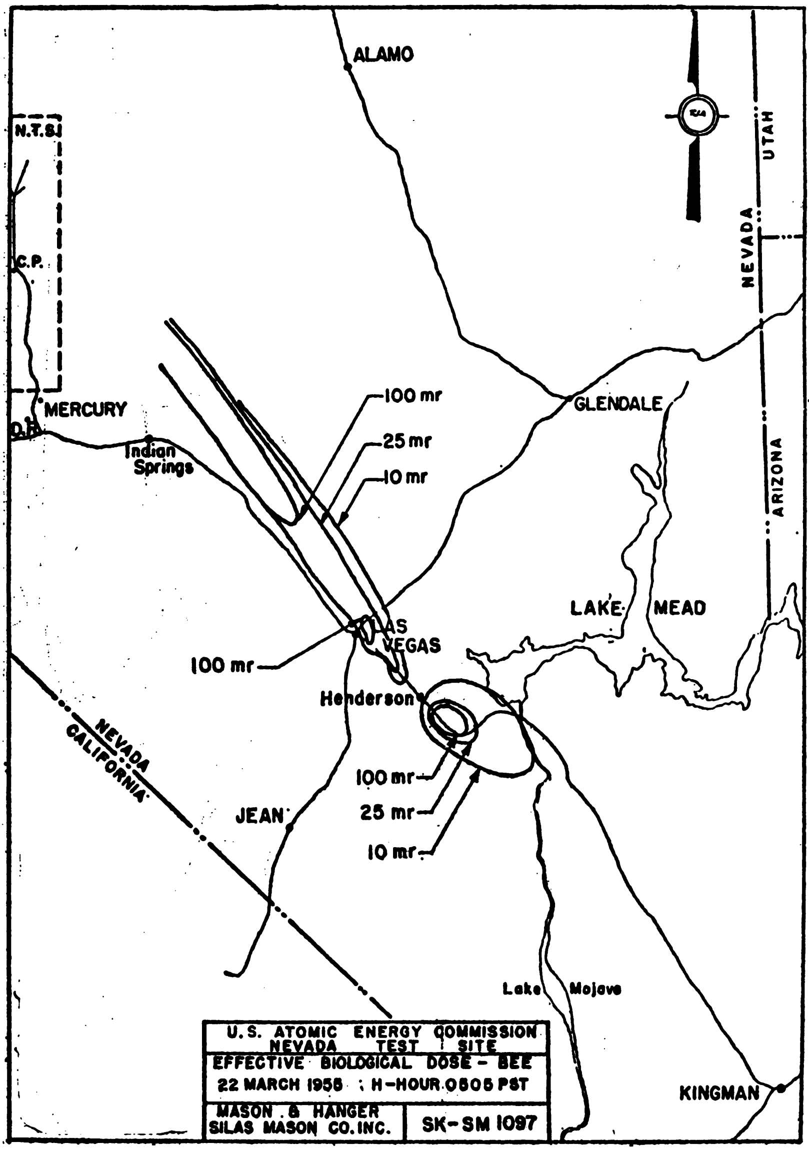
Bee: External gamma dose in populated areas and at selected nonpopulated points

Location	Time of instrument readings (H+hours)	Gamma ground level (mr./hr.)	Time of fallout (H+hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Las Vegas, Nev.	5.5	9.0	4.2	140	260
North Las Vegas, Nev.	5.1	13.0	4.1	185	345
Nellis Air Force Base, Nev.	6.4	1.2	4.1	23	42
Henderson, Nev.	5.7	.5	4.8	8	15
Boulder City, Nev.	7.3	4.0	5.2	83	156
Whitney, Nev.	5.8	.5	4.5	8	15
Hoover Dam, Nev.	9	.4	5.2	11	20
Nonpopulated points:					
U. S. 93–95, 3 miles south of Henderson, Nev.	5.1	18	4.9	246	465
4.7 miles south of Papoose Lake	55.4	60	.54	26,400	42,110


ALAMO
N.T.S.
C.P.
MERCURY
D.R.
Indian Springs
4 R
1 R
GLENDALE
NEVADA
UTAH
ARIZONA
LAKE MEAD
LAS VEGAS
NEVADA
CALIFORNIA
JEAN
NOTE-
INFINITE DOSE PLOTTED
Lake Mojave
KINGMAN
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
H MINUS ONE FALLOUT PREDICTION
SHOT NO.6 BEE 22 MARCH 1955
- H-HOUR 0505 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1096


ALAMO
N.T.S.
C.P.
MERCURY
Indian Springs
GLENDALE
LAS VEGAS
LAKE MEAD
JEAN
Lake Mojave
KINGMAN
NEVADA
UTAH
ARIZONA
NEVADA
CALIFORNIA
12-13,000' MSL
20-23,000' MSL
39,200' MSL (Top of Cloud)
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK-SHOT NO. 6 - BEE
22 MARCH 1955-H-HOUR 0505 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1095


ALAMO
N.T.S.
25 R
10 R
4 R
1 R
500mr
C.P.
MERCURY
D.R.
Indian Springs
10mr
25mr
100mr
GLENDALE
UTAH
NEVADA
ARIZONA
LAKE MEAD
LAS VEGAS
200 mr
Henderson
10mr
NEVADA
CALIFORNIA
JEAN
200 mr
100 mr
25 mr
Lake Mojave
KINGMAN
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
INFINITE DOSE - GROUND SURVEY
BEE - 22 MARCH 1955
H-HOUR 0505 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1098


ALAMO
N.T.S.
C.P.
UTAH
NEVADA
MERCURY
Indian Springs
GLENDALE
100 mr
25 mr
10 mr
ARIZONA
LAKE
MEAD
LAS VEGAS
100 mr
Henderson
NEVADA
CALIFORNIA
100 mr
25 mr
10 mr
JEAN
Lake Mojave
KINGMAN
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
EFFECTIVE BIOLOGICAL DOSE - BEE
22 MARCH 1955 ; H-HOUR 0505 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1097

Ess

Ess was an underground detonation which was fired at 12 p. m. on March 23, 1955. The shot took place in test area 10 in Yucca Flat.

No airway closure pattern was established for Ess.

The cloud was tracked as shown on the accompanying map from H plus 35 minutes to H plus 4 hours and 20 minutes. Tracking was accomplished between 10,000 and 13,000 feet by a B–25 type aircraft.

A low-level terrain survey was flown from H plus 3 hours to H plus 5 hours by 1 C–47. The results of this survey are shown on the accompanying map.

Monitoring runs, which indicated activity substantially above background, were made on U. S. 93 between a point 21 miles south of Alamo, Nev., and Glendale, Nev.; on the desert road north of Indian Springs, Nev.; and along several of the desert roads east of the Nevada test site.

The maximum effective biological dose for a populated area was 30 mr. at Beaver Dam, Ariz. The maximum effective biological dose at a nonpopulated point was 2,510 mr. 22 miles north of Indian Springs, Nev.

Approximately 105 individual monitoring readings above 0.1 mr./hr. were recorded.

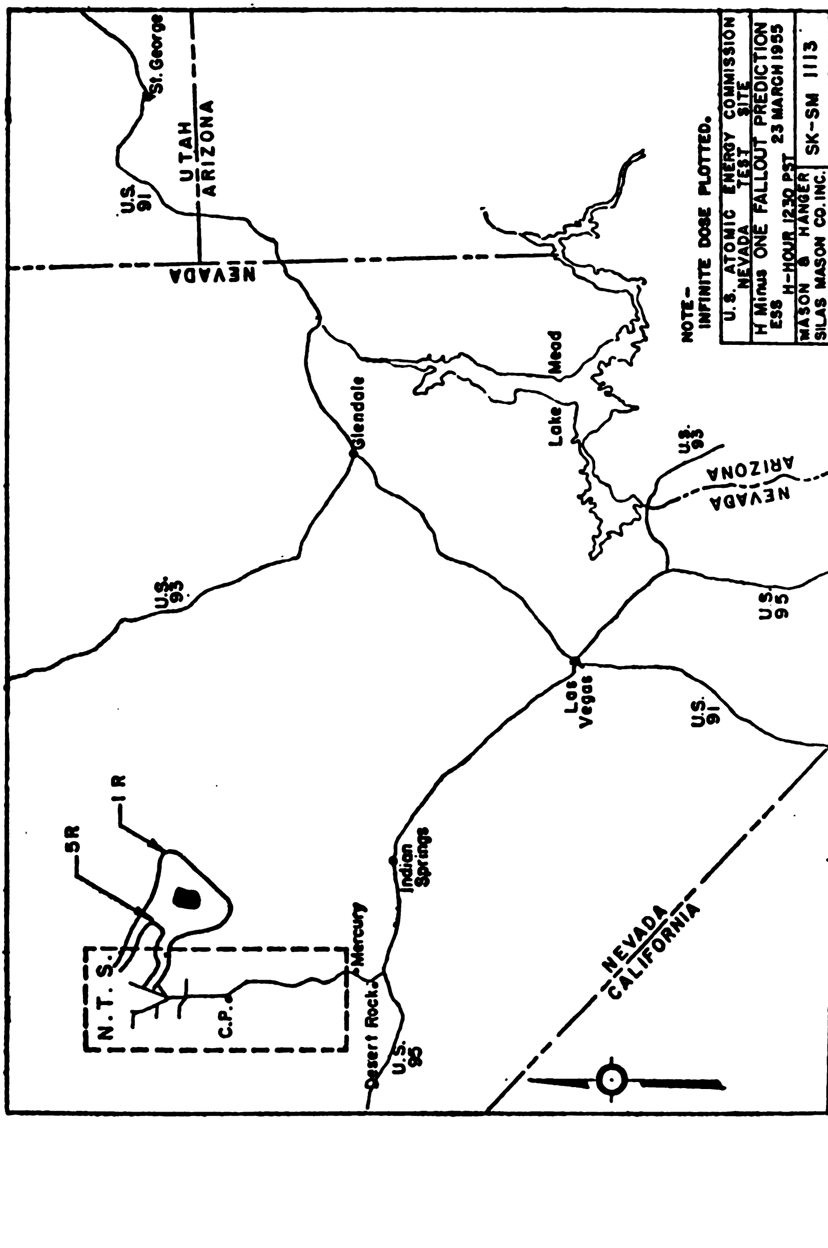
A comparison of the prediction map and the factual maps indicates good agreement in direction, but the 1 r. isodose contour was at least twice as long as was predicted.

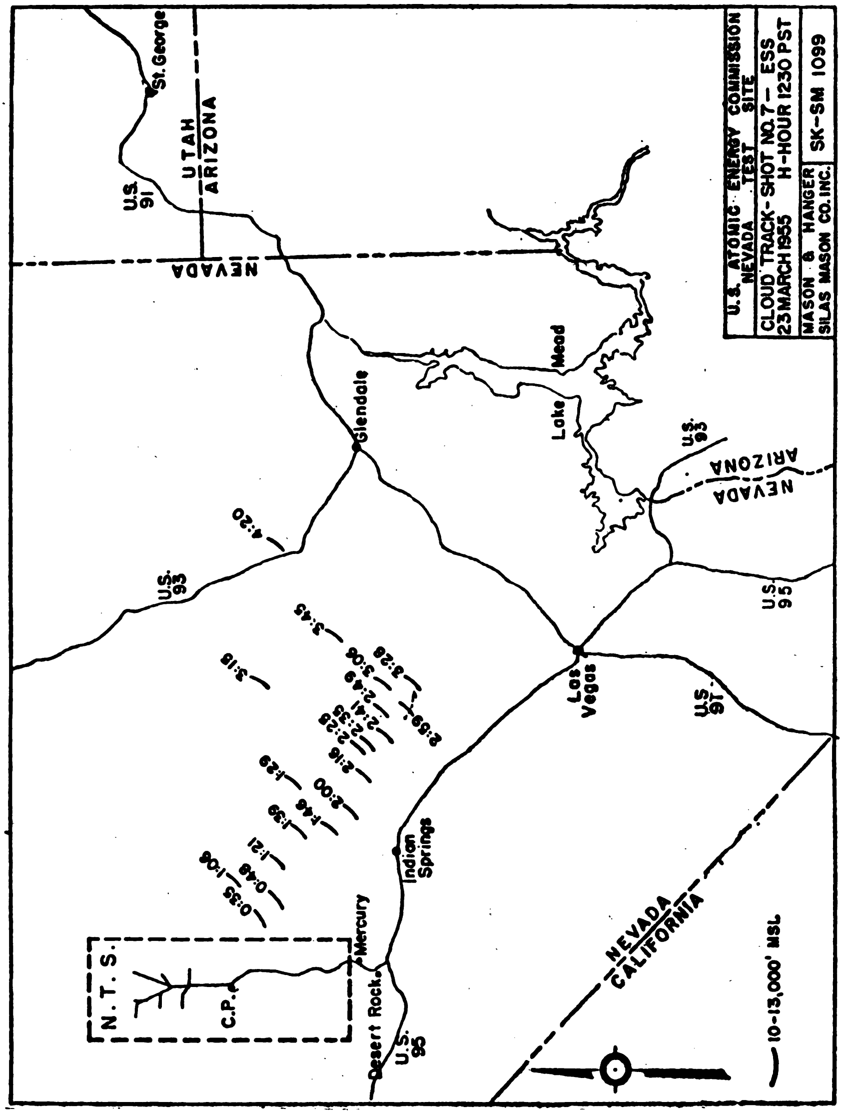
The low-level terrain survey map shows roughly twice the infinite doses that are plotted from ground monitoring results. The leading edge of the cloud did not cross the eastern boundary of the bombing and gunnery range until approximately H plus 4 hours. The haze from the cloud could still be seen in the Valley to the east and northeast of Indian Springs, Nev., at H plus 5 hours. The higher dose indicated by the low-level terrain survey are, therefore, probably due to radiation from that part of the cloud still in the valley when the survey was made. The ground monitoring plot also indicates extensive shear and the effects of terrain features on fallout pattern. It is apparent from the maps that the 1 r. isodose contour did not intersect any major highways.

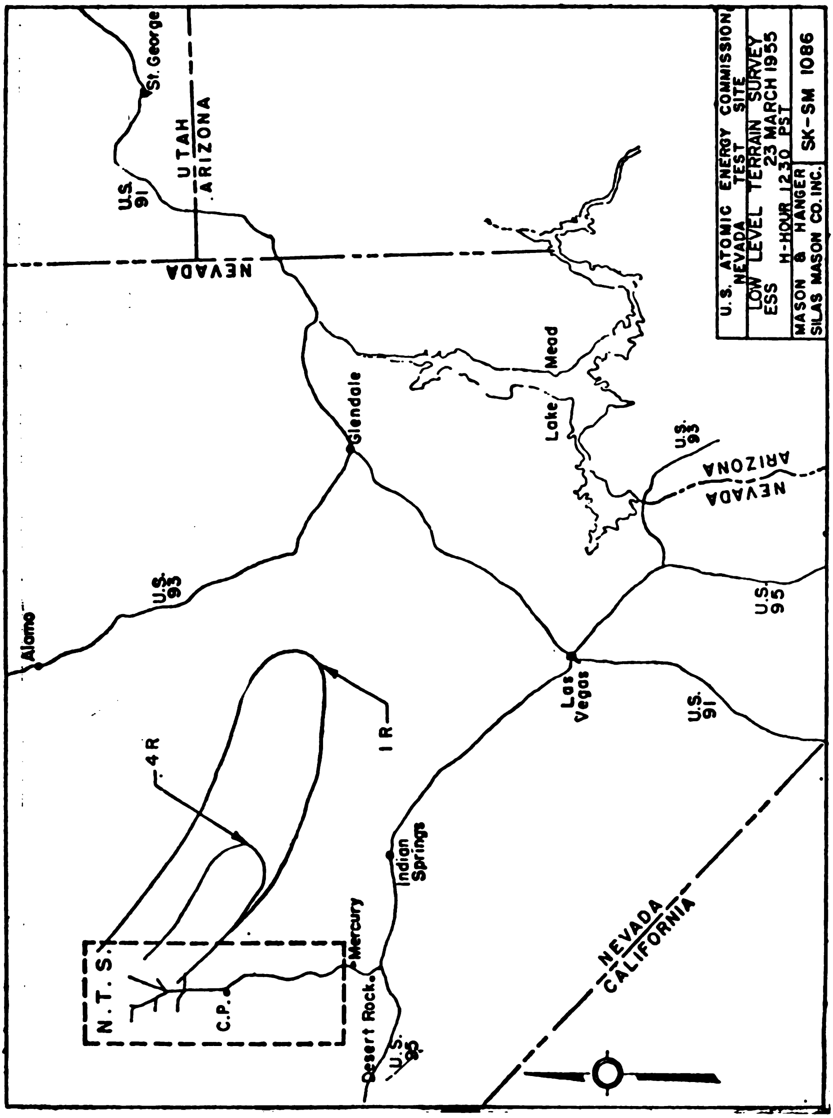
The maximum air radioactivity concentration measured was $4.5\times10^{-3}\mu c/m^3$, at Mesquite, Nev. This represents the average air concentration for a 43-hour period starting 1.5 hours after detonation.

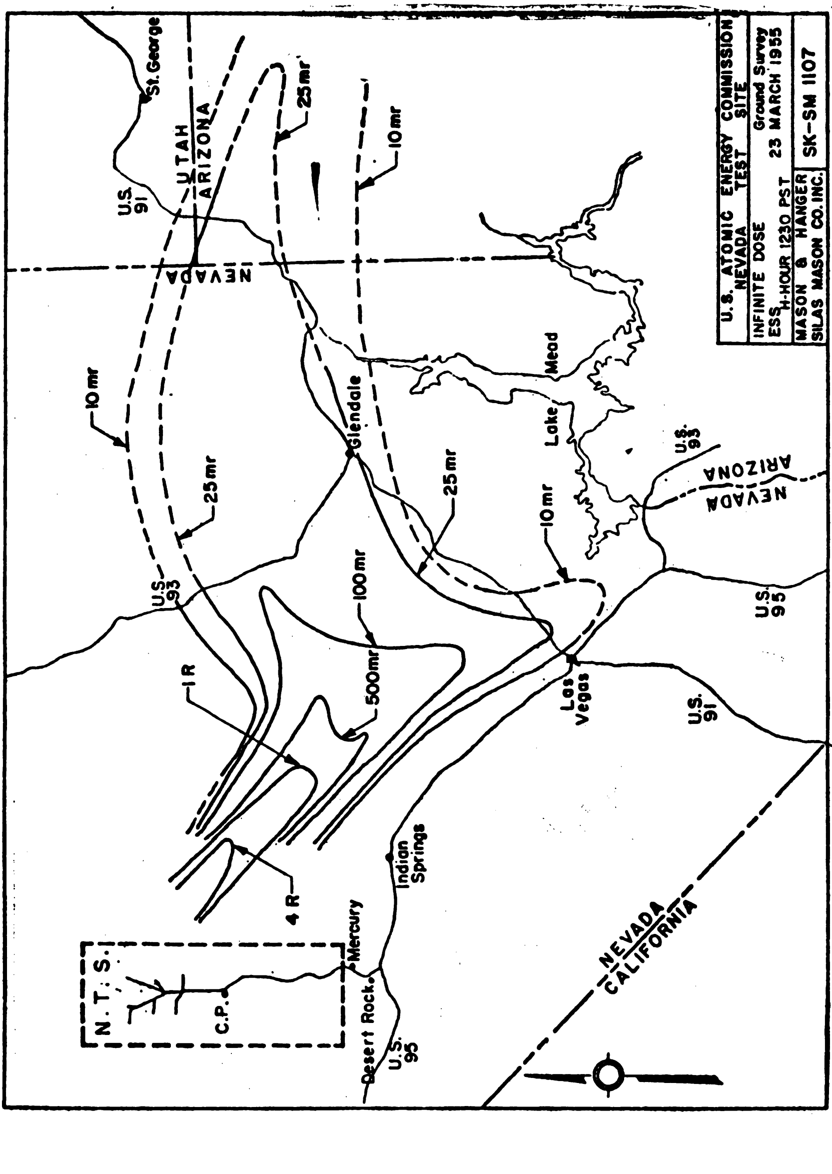
Ess: External gamma dose in populated areas and at selected nonpopulated points

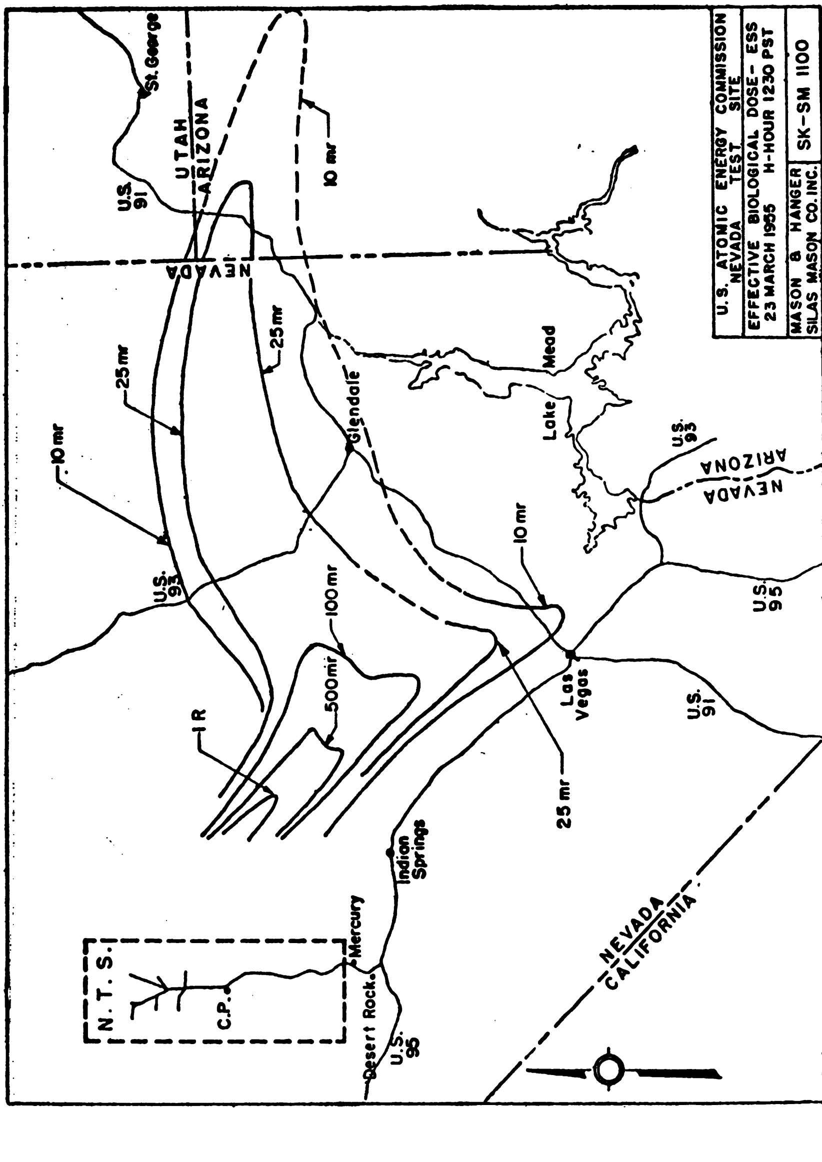
Location	Time of instrument reading (H+hours)	Gamma ground level (mr./hr.)	Time of fallout (H+hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Nellis Air Force Base, Nev	3.5	0.8	3.5	8	14
Lake Mead Base, Nev	4.0	1.5	3.5	16	30
North Las Vegas, Nev	6.5	.3	3.5	7	12
Glendale, Nev	7.2	1.5	6.0	25	48
Moapa, Nev	20.9	.2	6.0	14	27
Beaver Dam, Ariz	23.6	.4	10.0	30	60
Nonpopulated points:					
U. S. 93, 38 miles south of Alamo, Nev.	6.3	3.0	6.0	51	100
22 miles north of Indian Springs, Nev.	5.3	140.0	2.0	2,510	4,400


NOTE-
INFINITE DOSE PLOTTED.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
H Minus ONE FALLOUT PREDICTION
ESS 23 MARCH 1955
H-HOUR 1230 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1113
St. George
UTAH
ARIZONA
NEVADA
U.S. 91
Glendale
Lake Mead
U.S. 93
U.S. 95
Las Vegas
Indian Springs
Mercury
Desert Rock
N. T. S.
C.P.
5R
1R
CALIFORNIA


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK - SHOT NO. 7 - ESS
23 MARCH 1955 H-HOUR 1230 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1099
St. George
UTAH
ARIZONA
NEVADA
U.S. 91
U.S. 93
U.S. 95
Glendale
Lake Mead
Los Vegas
Indian Springs
Mercury
Desert Rock
N. T. S.
C.P.
NEVADA
CALIFORNIA
10-13,000' MSL


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
LOW LEVEL TERRAIN SURVEY
ESS 23 MARCH 1955
H-HOUR 1230 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1086
St. George
UTAH
ARIZONA
U.S. 91
NEVADA
Glendale
Lake Mead
U.S. 93
ARIZONA
NEVADA
U.S. 93
Alamo
U.S. 95
Las Vegas
U.S. 91
1 R
4 R
Indian Springs
Mercury
N. T. S.
C.P.
Desert Rock
U.S. 95
NEVADA
CALIFORNIA


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
INFINITE DOSE
ESS
Ground Survey
H-HOUR 1230 PST
23 MARCH 1955
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1107
St. George
UTAH
ARIZONA
NEVADA
U.S. 91
25mr
10mr
Glendale
Lake Mead
U.S. 93
100mr
500mr
1 R
4 R
Las Vegas
U.S. 95
Indian Springs
Mercury
Desert Rock
N. T. S.
C.P.
NEVADA
CALIFORNIA


U. S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
EFFECTIVE BIOLOGICAL DOSE - ESS
23 MARCH 1955 H-HOUR 1230 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1100
St. George
UTAH
ARIZONA
NEVADA
U.S. 91
U.S. 93
U.S. 95
10 mr
25 mr
100 mr
500 mr
1 R
Glendale
Lake Mead
Las Vegas
Indian Springs
Mercury
Desert Rock
C.P.
N. T. S.
CALIFORNIA

Apple

Apple was a 500-foot tower detonation which was fired at 4:55 a. m. on March 29, 1955. The shot took place in test area 4 in Yucca Flat.

The airway closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles was closed at all altitudes from 4:30 a. m. to 2 p. m. The extended closure time was necessary since a second shot, Wasp Prime, was scheduled for approximately 10 a. m.
2. A sector, with radii at 70° and 130° extending to the Utah-Colorado and Arizona-New Mexico borders, was closed from 22,000 feet and above from 6 a. m. to 1 p. m.
3. At 6:20 a. m., the closed altitudes were changed to read 18,000 to 33,000 feet, inclusive.
4. At 9:10 a. m., the 130° radius was moved to 110°, extending to the north edge of airway Green 4, and along this path to the Arizona-New Mexico border.

The cloud was tracked by one B–50 and one B–25 aircraft at the levels of 21,000 and 13,000 feet, respectively, with additional reports from sampler aircraft. The maximum distance to which the cloud was tracked was 166 nautical miles on a bearing of approximately 90°. At the lower level, the general bearing was between 60° and 70°. Maximum cloud height observed was 31,000 feet.

A low-level terrain survey was flown by one C–47 starting at about H plus 6 hours and 35 minutes. Results are plotted on the accompanying map.

Monitoring runs, which indicated activity substantially above background, were made on U. S. 91 between St. George, Utah, and Cedar City, Utah; along U. S. 93 between Alamo, Nev., and Pioche, Nev.; on Utah 18 between Central, Utah, and Beryl, Utah; along Utah 56 west of Cedar City, Utah, and continuing on Nevada 25 to the junction with U. S. 93; along Nevada 25 in the vicinity of Lincoln Mine, Nevada; on the desert road north of Indian Springs, Nev.; and along several of the desert roads north and east of the Nevada test site.

The maximum effective biological dose for a populated area was 1,300 mr. at Alamo, Nev. The maximum effective biological dose at a nonpopulated point was 6,500 mr., 4.3 miles south of Groom Lake on Kelly Mine Road.

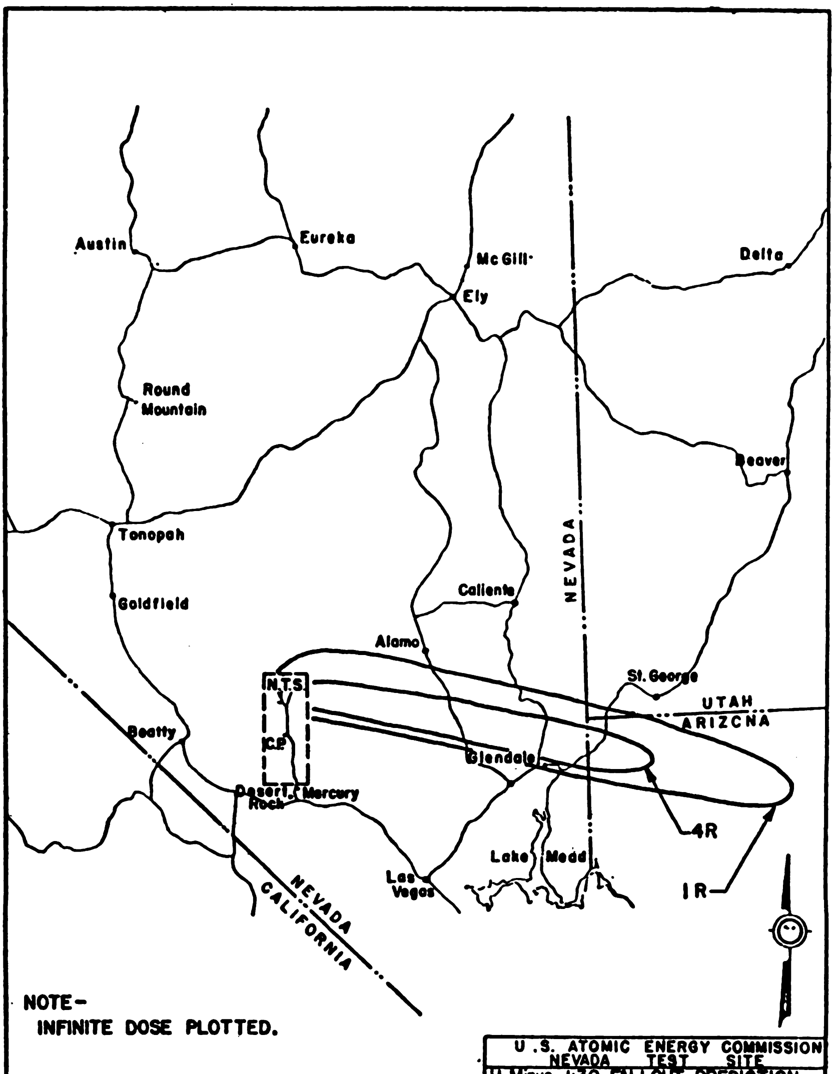
Approximately 395 individual monitoring readings above 0.1 mr./hr. were recorded.

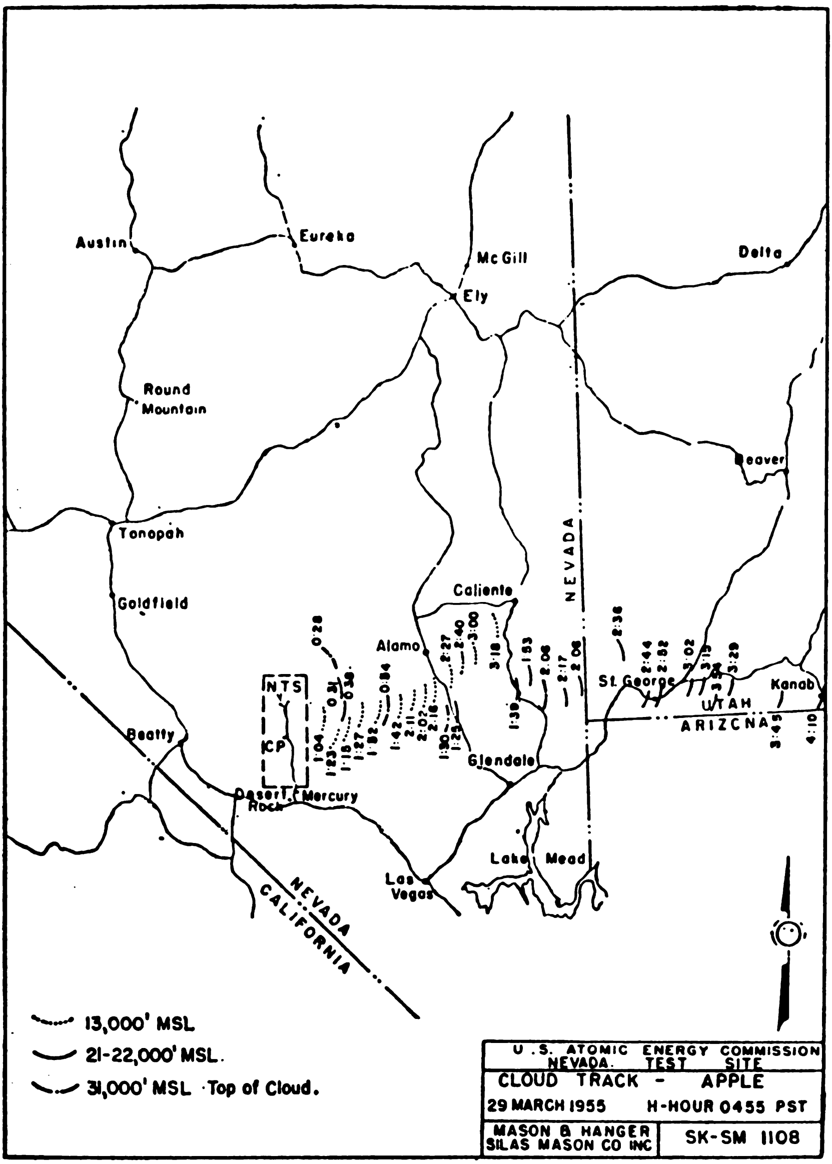
A comparison of the prediction map and the factual maps indicates that the predicted direction was off by 20° and that the 1 r. infinite isodose contour extended only about one-third of the predicted distance. The low-level terrain survey map is in good agreement with the map depicting ground monitoring results. The 1 r. infinite dose contour crossed U. S. 93 at Alamo, Nev.

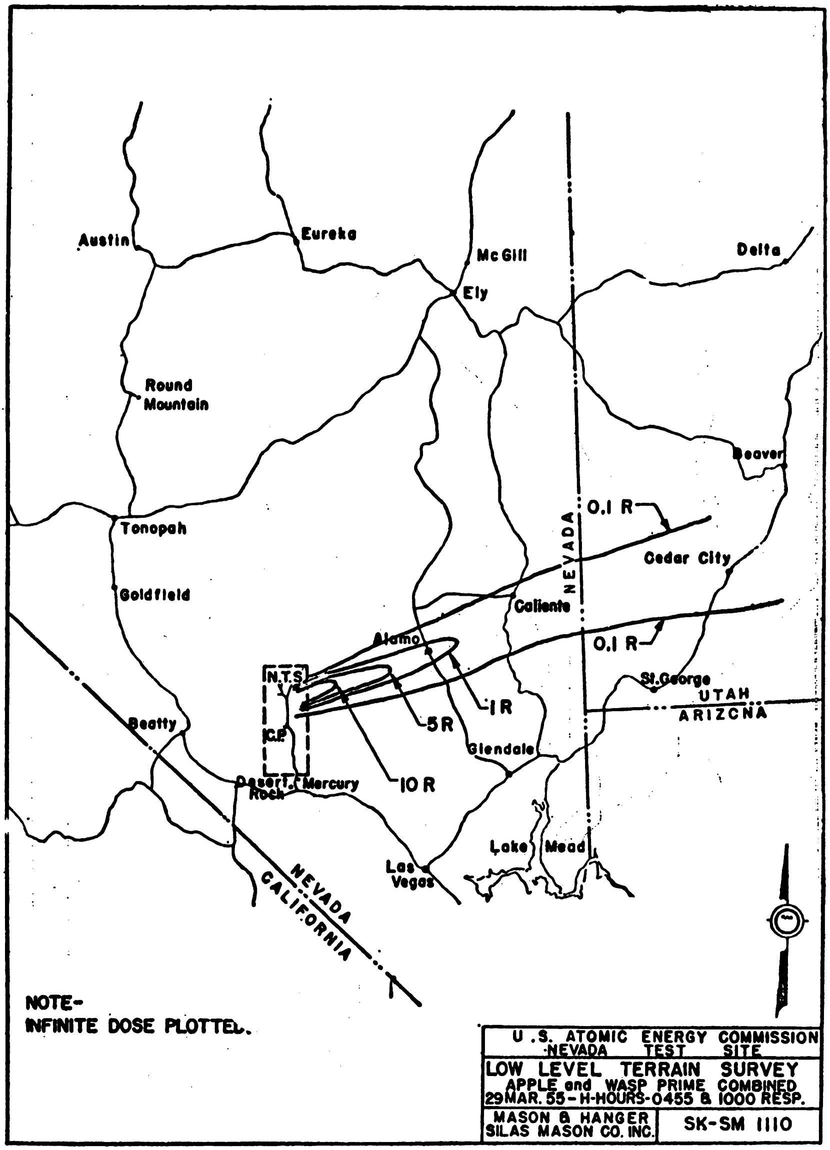
The maximum air radioactivity concentration measured was $4.0 \times 10^{-3} \mu c/m^3$, at Alamo, Nev. This represents the average air concentration for a 28-hour period starting at shot time.

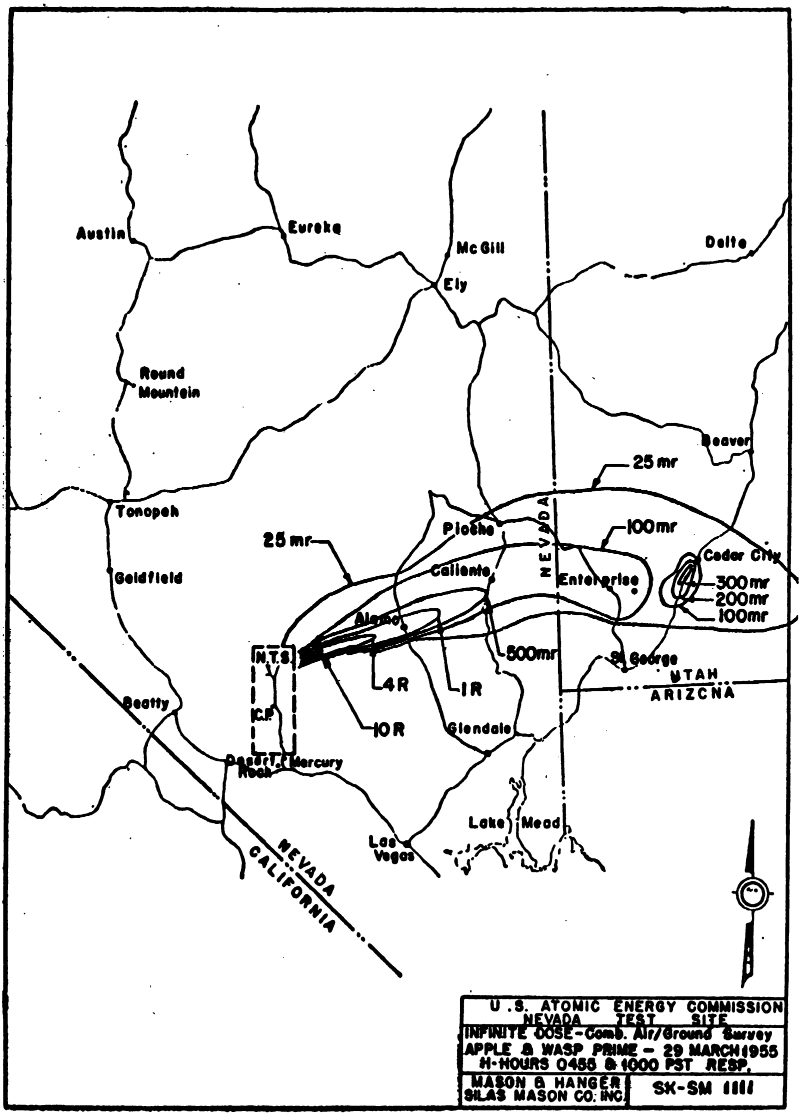
Apple: External gamma dose in populated areas and at selected nonpopulated points

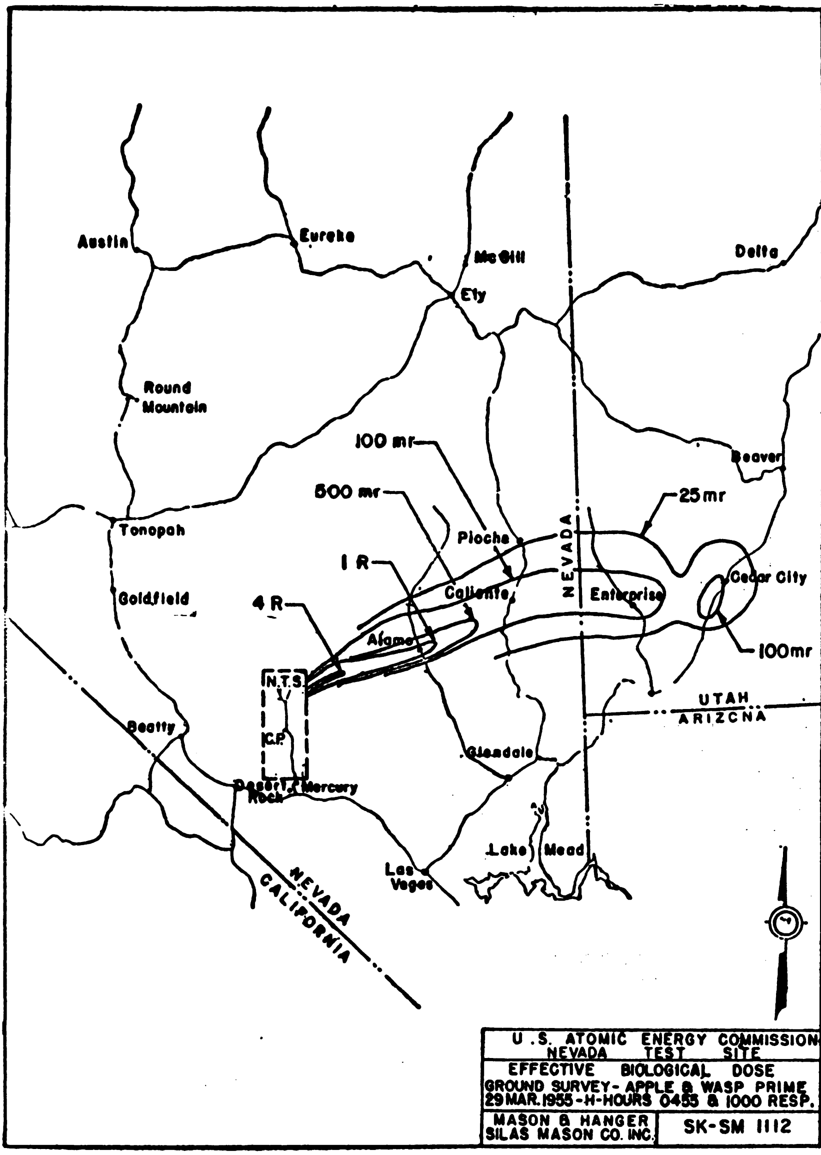
Location	Time of instrument reading (H+hours)	Gamma ground level (mr./hr.)	Time of fallout (H+hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Lincoln Mine, Nev.	6.1	0.5	2.3	10	19
Alamo, Nev.	2.8	160.0	2.6	1,300	2,300
Crystal Springs, Nev.	9.4	1.5	2.7	50	91
Hiko, Nev.	12.2	1.4	2.7	63	110
Pioche, Nev.	5.5	1.0	4.6	15	30
Panaca, Nev.	5.9	2.5	4.6	41	80
Modena, Utah	6.7	2.0	5.5	37	70
Ursine, Nev.	12.1	.3	5.1	10	22
Caliente, Nev.	4.9	9.0	4.1	120	230
Beryl Junction, Utah	6.5	5.0	5.9	86	170
Enterprise, Utah	6.8	9.5	5.8	180	340
New Harmony, Utah	5.5	4.0	4.9	59	110
Kanarraville, Utah	5.7	10.0	5.1	150	290
Hamilton Fort, Utah	5.9	6.0	5.3	95	180
Cedar City, Utah	6.0	3.0	5.4	48	92
Newcastle, Utah	6.7	10.0	6.1	175	340
Ely, Nev.	10.4	.12	6.7	4	7
Currant, Nev.	9.5	.10	5.0	3	5
Orderville, Utah	34.5	.16	7.9	19	37
Glendale, Utah	34.6	.16	8.3	19	37
Alton, Utah	35.0	.16	8.3	19	37
Nonpopulated points:					
U. S. 93, 1 mile south of Alamo, Nev.	28.2	12.0	2.7	1,500	2,700
4.8 miles south of Groom Lake	54.6	18.0	.9	6,500	11,000


Austin
Eureka
Mc Gill
Ely
Delta
Round Mountain
Beaver
Tonopah
Goldfield
Caliente
NEVADA
Alamo
N.T.S.
C.P.
St. George
UTAH
ARIZCNA
Beatty
Glendale
Desert Rock
Mercury
4R
Lake Mead
Las Vegas
IR
NEVADA
CALIFORNIA
NOTE-
INFINITE DOSE PLOTTED.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
H Minus 1:30 FALLOUT PREDICTION
APPLE 29 MARCH 1955
H-HOUR 0455 PST
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1109


Austin
Eureka
McGill
Ely
Delta
Round Mountain
Beaver
Tonopah
Goldfield
Caliente
NEVADA
Alamo
St. George
Kanab
UTAH
ARIZONA
NTS
CP
Beatty
Glendale
Desert Rock
Mercury
Lake Mead
Las Vegas
NEVADA
CALIFORNIA
13,000' MSL
21-22,000' MSL.
31,000' MSL ·Top of Cloud.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK - APPLE
29 MARCH 1955
H-HOUR 0455 PST
MASON & HANGER
SILAS MASON CO INC
SK-SM 1108


Austin
Eureka
McGill
Ely
Delta
Round Mountain
Beaver
Tonopah
0.1 R
Cedar City
Goldfield
NEVADA
Caliente
Alamo
0.1 R
N.T.S.
St.George
UTAH
ARIZONA
1 R
5 R
C.P.
Beatty
Glendale
Desert Rock
Mercury
10 R
Lake Mead
Las Vegas
NEVADA
CALIFORNIA
NOTE-
INFINITE DOSE PLOTTED.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
LOW LEVEL TERRAIN SURVEY
APPLE and WASP PRIME COMBINED
29 MAR. 55 - H-HOURS-0455 & 1000 RESP.
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1110


Austin
Eureka
McGill
Ely
Delta
Round Mountain
Beaver
25 mr
Tonopah
Pioche
100mr
25 mr
Goldfield
Caliente
NEVADA
Enterprise
Cedar City
300mr
200mr
100mr
Alamo
N.T.S.
500mr
St George
UTAH
ARIZONA
4R
1R
C.P.
10R
Beatty
Glendale
Desert Rock
Mercury
Lake Mead
Las Vegas
NEVADA
CALIFORNIA
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
INFINITE DOSE-Comb. Air/Ground Survey
APPLE & WASP PRIME - 29 MARCH 1955
H-HOURS 0455 & 1000 PST RESP.
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1111



Wasp Prime

Wasp Prime was an airdrop which was detonated at 10 a. m. on March 29, 1955. The shot took place in test area 7 in Yucca Flat.

The airway closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles, was ordered closed at all altitudes from 4:30 a. m. to 2 p. m. This was in conjunction with the flash circle for Apple.
2. A sector, with radii at 50° and 110°, length of radius 150 nautical miles, was ordered closed from 15,000 to 35,000 feet from 1 a. m. to 4 p. m.

3. At 10:30 a. m., the flash circle was opened between 110° and 360°. The end closure time on the sector and remaining part of the circle was changed to 1 p. m.

4. At 10:55 a. m., the lower closed altitude was raised from 15,000 to 18,000 feet.

A C–45 aircraft was assigned to track the cloud as the B–25 used earlier in the day for Apple was contaminated. The aircraft developed engine trouble and aborted the mission. A second C–45 was dispatched and tracked the cloud at 10,000 feet. Normal procedure could not be followed since the C–45 was not capable of flying at sufficiently high altitudes. In place of normal procedure, the cloud was tracked visually from below by the tracking aircraft. Additional reports were submitted by sampler aircraft. At low levels, the direction of the cloud track was along a bearing of 55° to 60°, while at higher levels, the approximate bearing was 74°. The maximum distance to which the track was followed was about 70 nautical miles.

No separate monitoring, either air or ground, was conducted for this detonation for two reasons. First, no fallout was expected and, secondly, as the cloud tracks were essentially the same in direction, any fallout would be determined by normal operational monitoring for Apple. Thus, the low level terrain survey and the ground monitoring maps for Apple are labeled "Apple and Wasp Prime Combined."

HA

HA was an air detonation which was fired at 10 a. m. on April 6, 1955, at an altitude of approximately 36,000 feet. The shot took place over Yucca Flat.

The airway closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles, was ordered closed at all altitudes from 8:30 to 1 p. m.

2. A sector with a radii at 110° and 180°, length of radius 150 nautical miles, was ordered closed from 25,000 feet up from 10 a. m. to 1 p. m.

3. At 11 a. m., all restrictions were removed.

Due to the height of the detonation, no regular cloud-tracking missions were flown. At 10:12 a. m. the cloud height was estimated by the sampler control aircraft to be 55,000 to 60,000 feet. Later data, at 10:20 a. m., indicated a drop to 45,000 to 47,000 feet.

No low level terrain survey missions were flown as no fallout was reported from ground monitoring teams.

Monitoring runs, made in the off-site area, indicated only background.

The only air radioactivity concentration measured in excess of $10^{-4}\mu c/m^3$ was $1.35\times10^{-4}\mu c/m^3$, at Lincoln Mine, Nev. This represents the average air concentration for a 24-hour period starting at shot time.

Post

Post was a 300-foot tower detonation which was fired at 4:30 a. m. on April 9, 1955. The shot took place in test area 9 in Yucca Flat.

The airway closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles, was ordered closed at all altitudes from 4 a. m. to 11:30 a. m. The extended closure time was due to the scheduling of another detonation, Met, later in the morning.

2. At 6:10 a. m., closure time on the flash circle was extended to 2 p. m. due to a delay in possible firing of Met.

3. At 8:10 a. m., closure time was cut back to 9 a. m. due to cancellation of Met.

The cloud was tracked by 1 B–25 aircraft flying at 13,000 feet, with additional reports coming from sampler aircraft. Maximum cloud height observed was 15,000 feet, with subsequent settling to 14,500 feet. The cloud was tracked to a maximum distance of 36 nautical miles at a bearing of approximately 165° from the command post. The plot of the cloud track is shown on the accompanying map.

A low level terrain survey was made by 1 C–47 aircraft from approximately H plus 5 hours to H plus 7 hours and 15 minutes. The survey indicated no significant off-site contamination.

Monitoring runs, which indicated activity substantially above background, were made on Mercury Road; along U. S. 95 between Lathrop Wells, Nev., and Indian Springs, Nev. on Nevada 16 between U. S. 95 and U. S. 91; and on the desert roads north and east of the Nevada test site.

The maximum effective biological dose for a populated area was 47 mr. at Camp Desert Rock, Nev. The maximum effective biological dose at a nonpopulated point was 162 mr., 3.5 miles south of Papoose Lake.

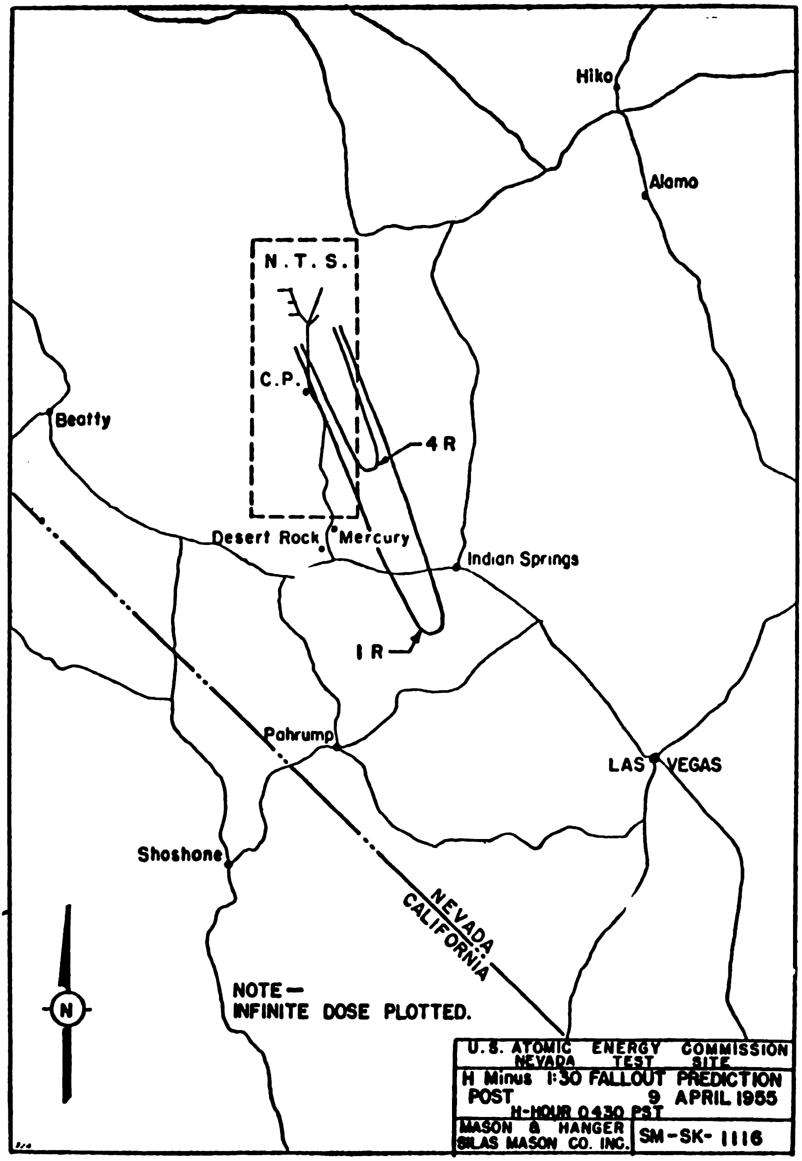
Approximately 75 individual monitoring readings above 0.1 mr/hr. were recorded.

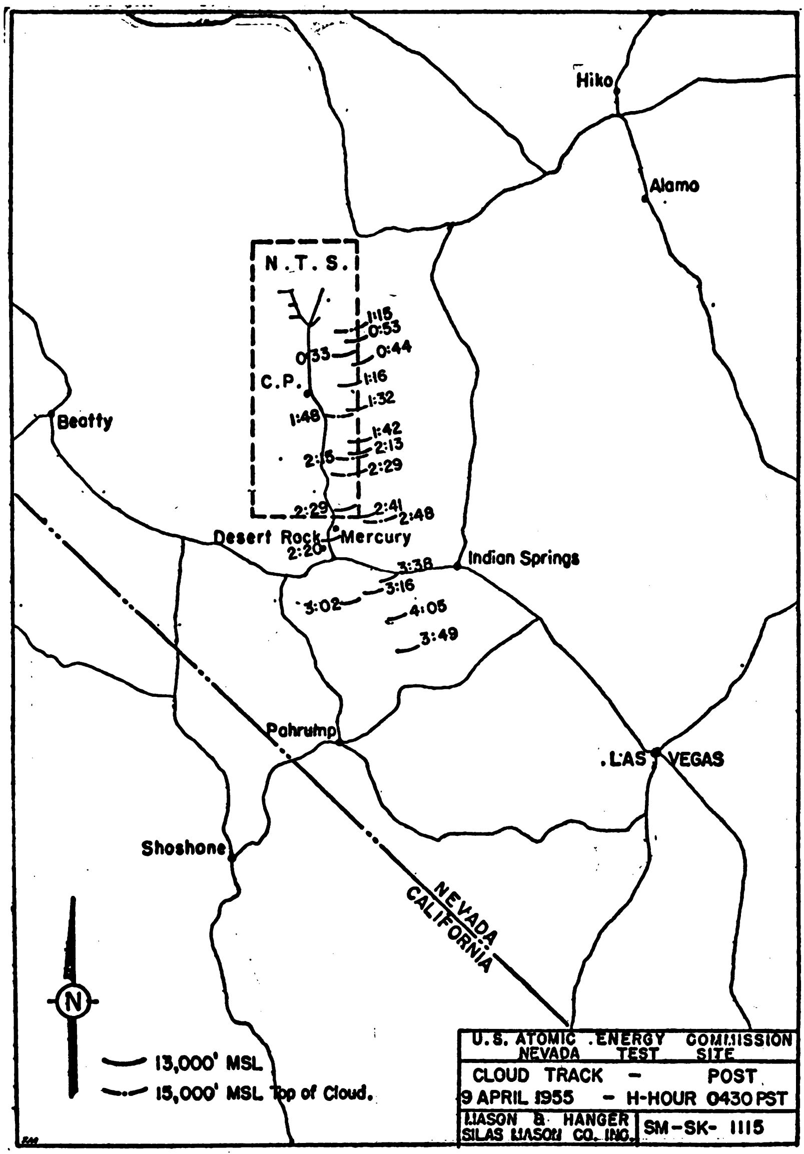
The ground survey map indicates extensive scattering of fallout in several directions. An interesting point is the presence of fallout to the northeast, although the cloud was tracked in a southerly direction. Only very light fallout occurred outside the Nevada test site.

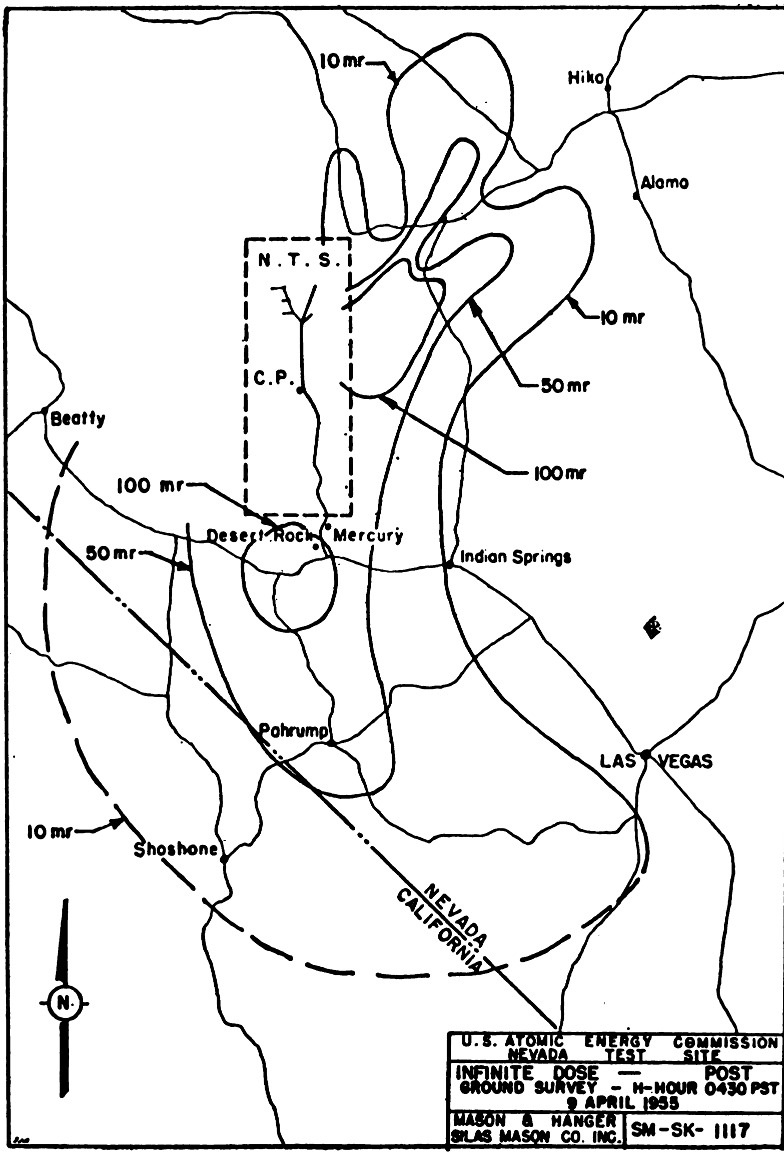
The maximum air radioactivity concentration measured was $9.0 \times 10^{-1} \mu c/m^3$, at Indian Springs, Nev. This represents the average air concentration for a 17.5-hour period starting at shot time.

Post: External gamma dose in populated areas and at selected nonpopulated points

Location	Time of instrument reading (H+hours)	Gamma ground level (mr./hr.)	Time of fallout (H+hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Pahrump, Nev	10.0	0.12	10.0	3	6
Quartz Mine (near Mercury, Nev.)	10.7	1.3	9.6	35	71
Cactus Springs, Nev	9.6	.14	9.6	3	7
Indian Springs, Nev	9.8	.3	9.8	7	15
Lathrop Wells, Nev	11.6	.7	10.0	21	42
Desert Rock, Nev	12.8	1.4	9.3	47	96
Mercury, Highway	13.0	1.2	9.3	42	84
Nonpopulated points:					
U. S. 95, 10 miles west of Mercury Highway	11.2	2.5	9.6	73	150
3.5 miles south of Papoose Lake	14.5	3.0	8.0	162	300


Hiko
Alamo
N. T. S.
C. P.
Beatty
4 R
Desert Rock
Mercury
Indian Springs
I R
Pahrump
LAS VEGAS
Shoshone
NEVADA
CALIFORNIA
N
NOTE—
INFINITE DOSE PLOTTED.
U. S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
H Minus 1:30 FALLOUT PREDICTION
POST 9 APRIL 1955
H-HOUR 0430 PST
MASON & HANGER
SILAS MASON CO. INC.
SM-SK- 1116


Hiko
Alamo
N. T. S.
C.P.
1:15
0:53
0:33
0:44
1:16
1:32
1:48
1:42
2:13
2:15
2:29
2:29
2:41
2:48
Beatty
Desert Rock
2:20
Mercury
Indian Springs
3:38
3:16
3:02
4:05
3:49
Pahrump
LAS VEGAS
Shoshone
NEVADA
CALIFORNIA
N
13,000' MSL
15,000' MSL Top of Cloud.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK - POST
9 APRIL 1955 - H-HOUR 0430 PST
MASON & HANGER
SILAS MASON CO. INC.
SM-SK- 1115


10 mr
Hiko
Alamo
N.T.S.
C.P.
10 mr
50 mr
100 mr
Beatty
100 mr
Desert Rock
Mercury
50 mr
Indian Springs
Pahrump
LAS VEGAS
10 mr
Shoshone
NEVADA
CALIFORNIA
N
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
INFINITE DOSE — POST
GROUND SURVEY - H-HOUR 0430 PST
9 APRIL 1955
MASON & HANGER
SILAS MASON CO. INC.
SM-SK- 1117

MET

Met was a 400-foot tower detonation which was fired at 11:15 a. m. on April 15, 1955. The shot took place in Frenchman Flat.

The airway closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles, was ordered closed at all altitudes from 10:30 a. m. to 4 p. m.
2. A sector, with radii at 40° and 100°, length of radius 200 nautical miles, was ordered closed from 15,000 to 20,000 feet from 11 a. m. to 4 p. m.
3. A continuation of and including this sector, extending northeast to JL 2049 then southeast to the Arizona-New Mexico border at LG 4700, then west to KG 5000, then northwest along 100° radius to sector 2, above, was closed from 21,000 to 42,000 feet from 11 a. m. to 8 p. m.
4. An area from JL 2049 along the southern edge of airways Red 49 and Green 3 to Medicine Bow, then along the western edge of Green 3 to Denver, continuing along the western boundaries of Denver and the western edge of Amber 3 to Colorado Springs, and finally southwest to LG 5700 was ordered closed from 25,000 to 42,000 feet from 2:30 p. m. to 8 p. m.
5. At 12 noon, the top altitude in 3 and 4 above was changed from 42,000 feet to 44,000 feet, and the beginning closure time in 4 above was moved back to 2 p. m.
6. At 12:30 p. m. the bottom altitude in sector 2 above was lowered to 10,000 feet.
7. At 2:50 p. m., the flash circle was opened except that portion in sector 2 and the lower altitude in sectors 2 and 3 was raised to 21,000 feet effective at 3:15 p. m.
8. At 4 p. m., area 4 was reduced by opening the area south of airway Victor B effective at 4:15 p. m.

The cloud was tracked by one B–25, two B–50's, and sampler aircraft. Maximum cloud height observed was 42,800 feet, settling quickly to 41,300 feet. Base of the mushroom was reported at 28,000 feet. The cloud was tracked to a maximum distance of 200 nautical miles on an approximate true bearing of 65° from the C. P. In general, all levels (13,000, 23,000, 28,000, and 42,000 feet) tracked followed the same bearing.

A low-level terrain survey was made by one C–47 aircraft from approximately H plus 3 hours and 15 minutes to H plus 7 hours. The fallout pattern is plotted on the accompanying map. Due to the relatively late detonation time, the aerial survey had to be cut short, as the aircraft could not conduct the low-level survey after sunset. For this reason, sufficient data to close the 1 r. infinite dose contour was not obtained.

Monitoring runs, which indicated activity substantially above background, were made along U. S. 93 between 36 miles north of Glendale, Nev., and the junction of Nevada 25; on Utah 21 between Beaver, Utah, and 7 miles west of Milford, Utah; on U. S. 91 between 5 miles north of Cove Fort, Utah, and Parowan, Utah; along Nevada 25 and Utah 56 between U. S. 93 and Newcastle, Utah; along Nevada 55 between 4 miles south of Elgin, Nev., to Caliente, Nev.; on Utah 18 between Enterprise, Utah, and Beryl Junction, Utah; along Utah 98 from Beryl Junction, Utah, to Beryl, Utah; on Utah 19 between Lund, Utah, and the junction of Utah 56; on the desert road north of Indian Springs, Nev.; along the game preserve road north of U. S. 95; and on several other desert roads northeast of the Nevada test site.

The maximum effective biological dose for a populated area was 2,880 mr. at Elgin, Nev. The maximum effective biological dose at a nonpopulated point was 44,600 mr. 24 miles north of Indian Springs, Nev.

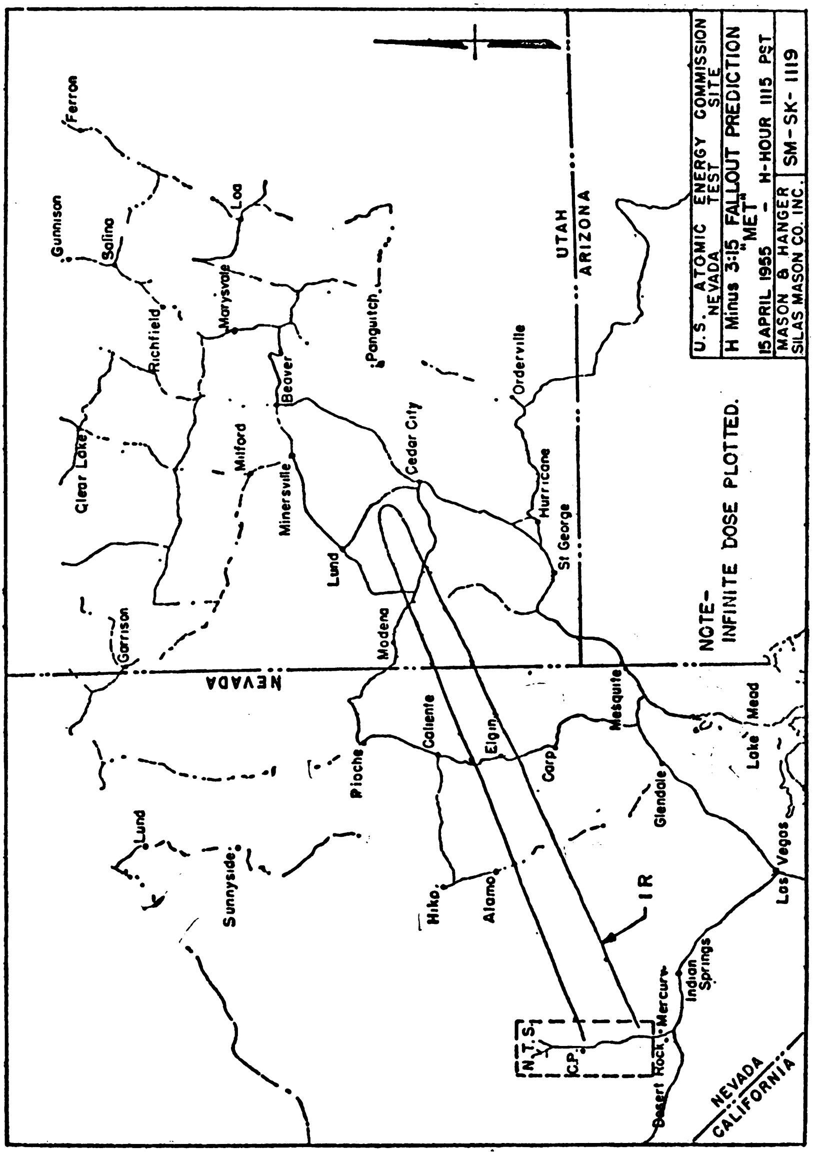
Approximately 340 individual monitoring readings above 0.1 mr./hr. were recorded.

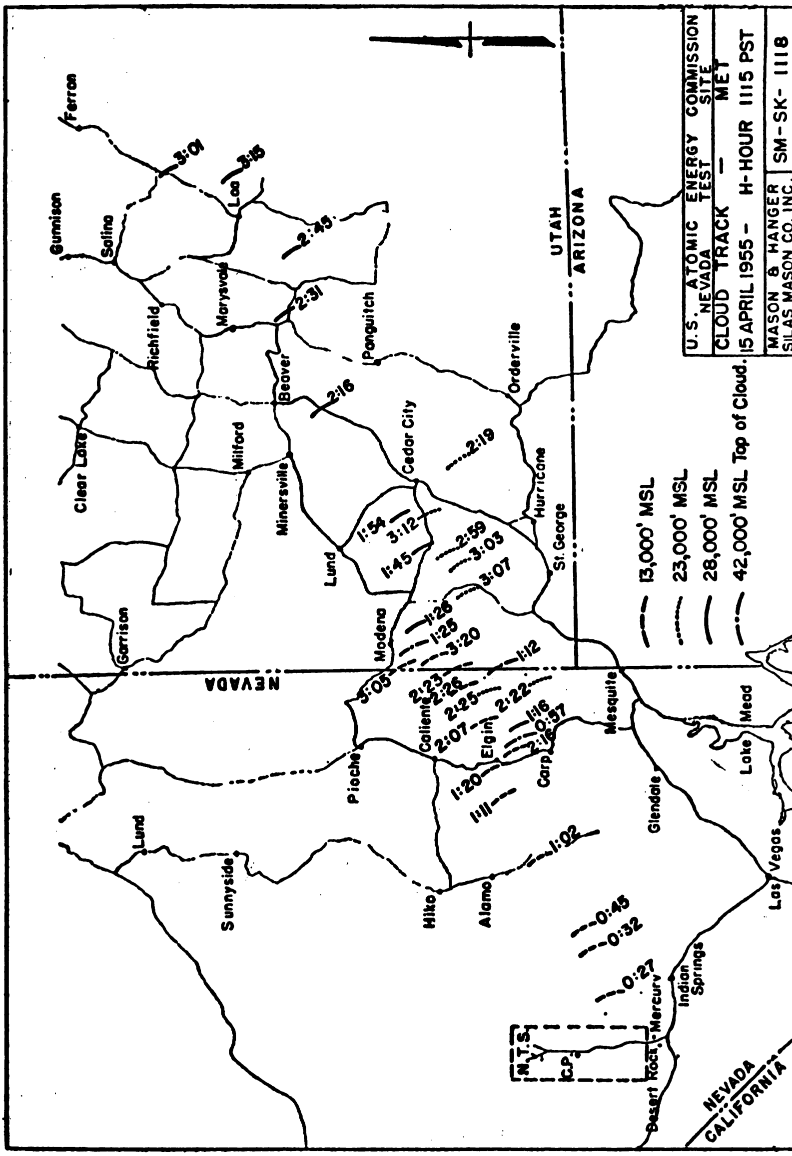
In general, all the maps are in good agreement as to both direction and magnitude. The actual 1 r. infinite isodose contour was a few miles longer than predicted, terminating northeast of Utah 21 rather than southwest. It is apparent that the 10 r. infinite isodose line crossed U. S. 93 southeast of Alamo, Nev.

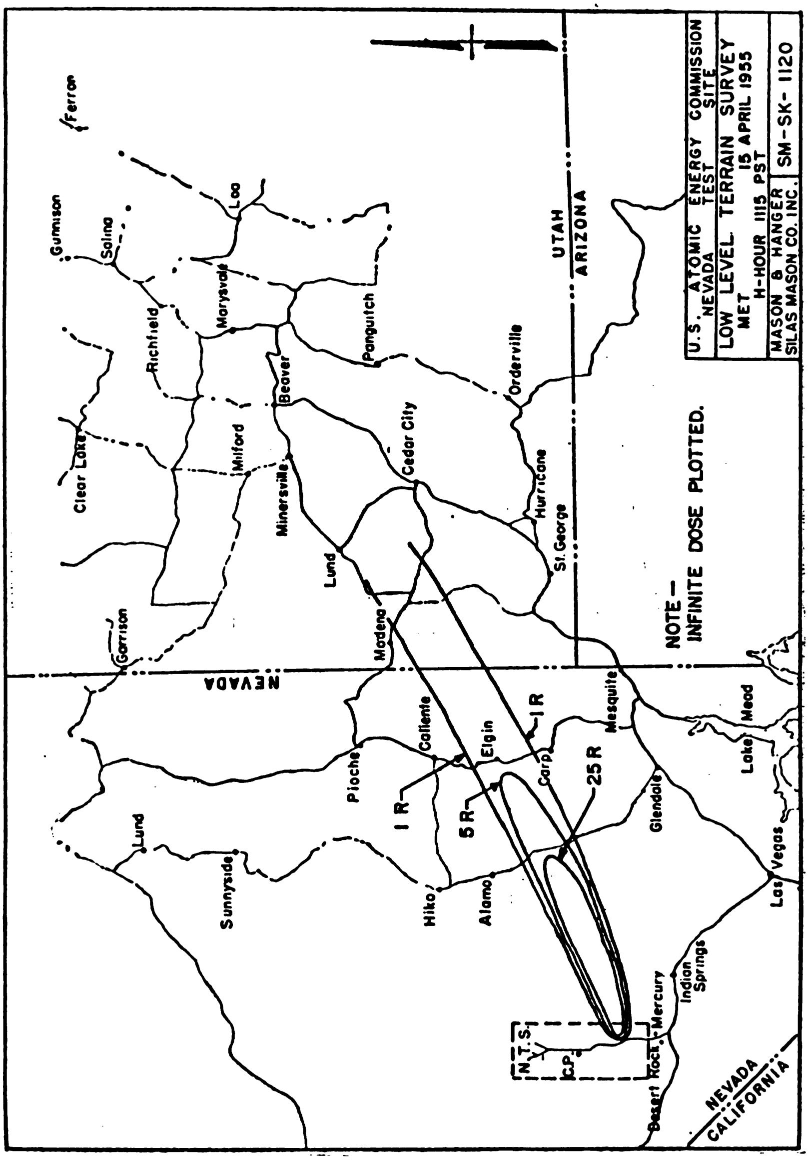
The maximum air radioactivity concentration measured was $6.1\times10^{-2}\mu c/m^3$, at Beaver, Utah. This represents the average air concentration for a 24.8-hour period starting at shot time.

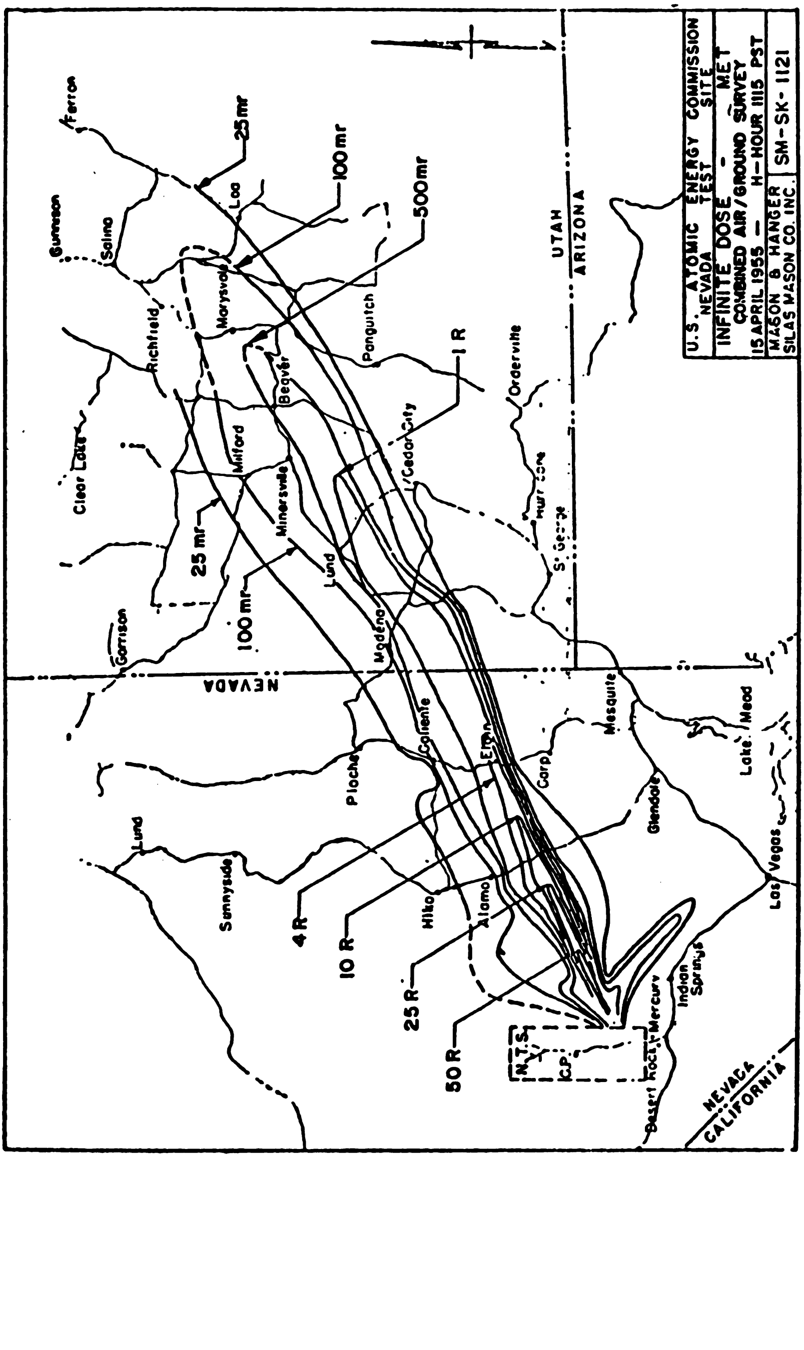
Met: External gamma dose in populated areas and at selected nonpopulated points

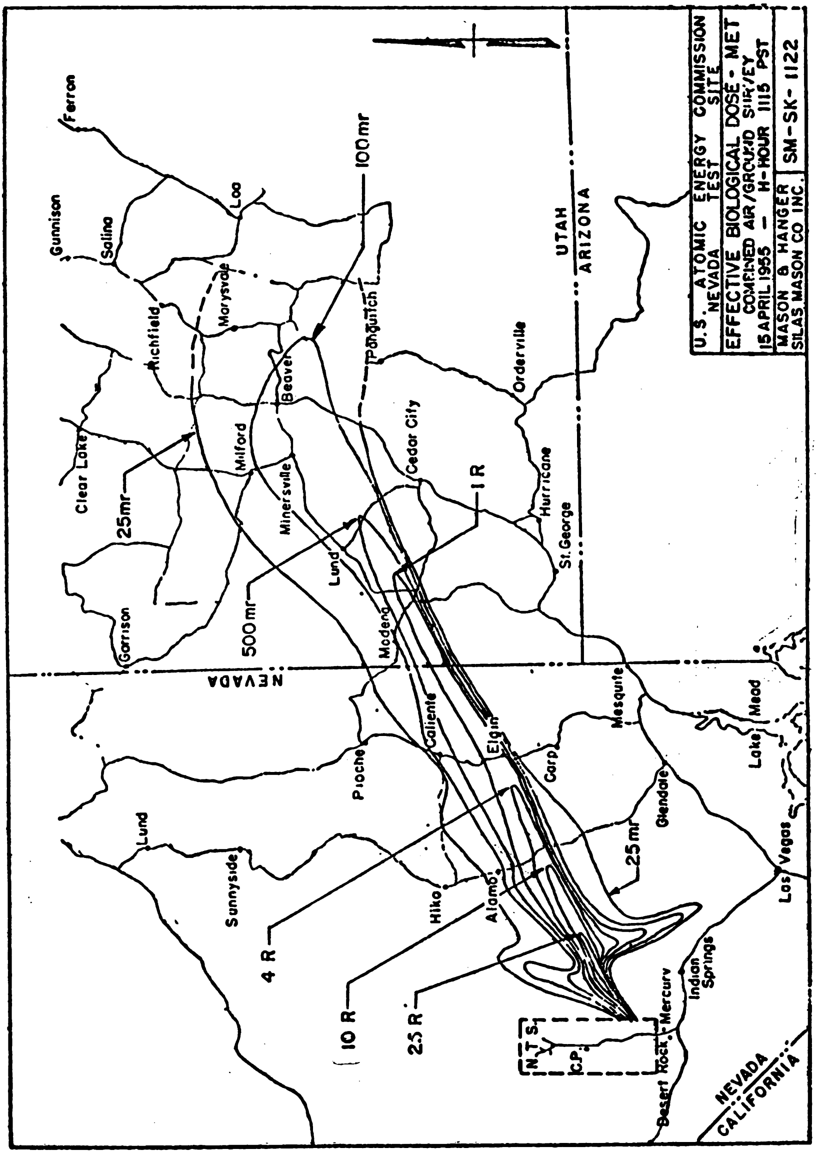
Location	Time of instrument reading (H+hours)	Gamma ground level (mr./hr.)	Time of fallout (H+hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Alamo, Nev	3.6	4.2	2.6	45	82
Buckhorn Ranch, Nev	2.6	140.0	2.6	980	1,750
Caliente, Nev	6.1	8.0	4.3	140	260
Elgin, Nev	5.1	200.0	3.9	2,880	5,340
Panaca, Nev	6.5	5.0	4.9	91	170
Modena, Utah	6.6	3.5	5.9	62	120
Enterprise, Utah	6.0	.13	6.0	2	4
Beryl Junction, Utah	28.8	3.6	6.2	370	700
Beryl, Utah	6.6	6.0	6.6	102	200
Zane, Utah	6.8	16.0	6.8	275	525
Lund, Utah	7.3	9.0	7.3	170	330
Cedar City, Utah	7.8	.3	7.6	6	12
Paragonah, Utah	25.9	.2	8.5	16	32
Parowan, Utah	26.0	.1	8.3	5	10
Beaver, Utah	25.3	3.5	9.3	270	540
Minersville, Utah	22.1	2.5	8.6	170	330
Milford, Utah	22.7	1.0	8.7	70	140
Cove Fort, Utah	24.3	.4	9.9	29	58
Newcastle, Utah	29.3	.1	6.5	7	14
Nonpopulated points:					
U. S. 93, 16 miles south of Alamo, Nev	2.9	950.0	2.6	7,630	13,800
24 miles north of Indian Springs, Nev	24.6	270.0	.9	44,600	74,300


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
H Minus 3:15 FALLOUT PREDICTION
"MET"
15 APRIL 1955 - H-HOUR 1115 PST
MASON & HANGER
SILAS MASON CO. INC.
SM-SK-1119
NOTE-
INFINITE DOSE PLOTTED.
UTAH
ARIZONA
NEVADA
NEVADA
CALIFORNIA
Ferron
Gunnison
Salina
Loa
Marysvale
Richfield
Panguitch
Beaver
Orderville
Milford
Cedar City
Clear Lake
Minersville
Hurricane
Lund
St George
Garrison
Modena
Caliente
Elgin
Mesquite
Lake Mead
Pioche
Carp
Glendale
Lund
Sunnyside
Hiko
Alamo
IR
Las Vegas
Indian Springs
Mercury
Desert Rock
N.T.S.
C.P.


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK — MET
15 APRIL 1955 - H-HOUR 1115 PST
MASON & HANGER
SILAS MASON CO. INC.
SM-SK-1118
13,000' MSL
23,000' MSL
28,000' MSL
42,000' MSL Top of Cloud.
UTAH
ARIZONA
NEVADA
NEVADA
CALIFORNIA
Ferron
Gunnison
Salina
Loa
Marysvale
Richfield
Panguitch
Beaver
Orderville
Clear Lake
Milford
Minersville
Cedar City
Hurricane
St. George
Lund
Modena
Garrison
Pioche
Caliente
Elgin
Carp
Mesquite
Lake Mead
Glendale
Lund
Sunnyside
Hiko
Alamo
Las Vegas
Indian Springs
Mercury
Desert Rock
N.T.S.
C.P.


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
LOW LEVEL TERRAIN SURVEY
MET 15 APRIL 1955
H-HOUR 1115 PST
MASON & HANGER
SILAS MASON CO. INC.
SM-SK-1120
NOTE—
INFINITE DOSE PLOTTED.
UTAH
ARIZONA
NEVADA
NEVADA
CALIFORNIA
Ferron
Gunnison
Salina
Loa
Richfield
Marysvale
Panguitch
Clear Lake
Milford
Beaver
Minersville
Cedar City
Orderville
Hurricane
St. George
Lund
Modena
Garrison
Caliente
Elgin
Pioche
Carp
Mesquite
1 R
1 R
5 R
25 R
Lund
Sunnyside
Hiko
Alamo
Glendale
Lake Mead
Las Vegas
Indian Springs
Mercury
N.T.S.
C.P.
Desert Rock


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
INFINITE DOSE - MET
COMBINED AIR/GROUND SURVEY
15 APRIL 1955 — H-HOUR 1115 PST
MASON & HANGER
SILAS MASON CO. INC.
SM-SK-1121
UTAH
ARIZONA
NEVADA
CALIFORNIA
25mr
100mr
500mr
1 R
4 R
10 R
25 R
50 R
Ferron
Gunnison
Salina
Loa
Marysvale
Richfield
Panguitch
Beaver
Ordervllle
Clear Lake
Milford
Minersville
Cedar City
Lund
Modena
Garrison
Caliente
Pioche
Elgin
Carp
Mesquite
Lake Mead
Glendale
Las Vegas
Sunnyside
Hiko
Alamo
Indian Springs
Mercury
N.T.S.
C.P.


U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
EFFECTIVE BIOLOGICAL DOSE - MET
COMBINED AIR/GROUND SURVEY
15 APRIL 1955 — H-HOUR 1115 PST
MASON & HANGER
SILAS MASON CO INC.
SM-SK-1122
UTAH
ARIZONA
NEVADA
CALIFORNIA
Ferron
Gunnison
Salina
Loa
Marysvale
Richfield
Panguitch
Beaver
Clear Lake
Milford
Minersville
Cedar City
Orderville
Hurricane
St. George
Lund
Modena
Garrison
Pioche
Caliente
Elgin
Carp
Mesquite
Lake Mead
Glendale
Las Vegas
Sunnyside
Hiko
Alamo
Indian Springs
Mercury
Desert Rock
N.T.S.
C.P.
100mr
25mr
500mr
1 R
4 R
10 R
25 R

APPLE TWO

Apple Two was a 500-foot tower detonation which was fired at 5:10 a. m. on May 5, 1955. The shot took place in test area 1 in Yucca Flat.

The airway closure pattern was as follows:

1. A circular area around Yucca Flat, with a radius of 60 nautical miles, was ordered closed at all altitudes from 4:45 a. m. to 9:30 a. m. The southern half of this circle was to be opened at H plus 10 miutes.
2. A sector, radii at 315° and 20°, length of radius 140 nautical miles, was closed from 14,000 to 24,000 feet from 6:30 a. m. to 9 a. m.
3. A sector, radii at 335° and 30°, length of radius 200 nautical miles, was closed from 24,000 to 44,000 feet from 6 a. m. to 10 a. m.
4. A continuation of this sector 3, above, extending the radius to 400 nautical miles, was closed from 24,000 to 44,000 feet from 8:30 a. m. to 12 noon.
5. At 6:30 a. m. the 30° bearing in sectors 3, and 4, was changed to 50°, and the extreme length of radius was reduced to 300 nautical miles.
6. At 8 a. m. the end closure time in 3. was changed to 12 noon, and the start closure time in 4. was changed to 9 a. m.
7. At 10:10 a. m., sector 4. was opened at all altitudes.

Cloud track data were received from one B–25, two B–50's, and sampler aircraft. Maximum cloud height observed was 40,500 feet. Considerable shear was present and the various levels tracked showed a spread in bearing from about 340° to 60°. The cloud was tracked to a miximum distance of about 120 nautical miles at all levels. The plot of the several tracks is shown on the accompanying map.

A preshot survey was flown on D–3 days since the zone of predicted fallout was in a direction not extensively surveyed by air previously. A low level terrain survey was flown by one C–47 aircraft from H plus 5 hours to approximately H plus 10 hours and 30 minutes. Results of this survey are plotted on the accompanying map.

Monitoring runs, which indicated activity substantially above background, were made along U. S. 93 between 45 miles north of Pioche, Nev., and Ely, Nev.; on Nevada 25 between U. S. 6 and several miles west of Lincoln Mine, Nev.; on U. S. 6 between 1 mile east of Warm Springs, Nev., and Ely, Nev.; along Nevada 20 between Currant, Nev. and U. S. 50; on U. S. 50 between 55 miles west of Eureka, Nev., and Nevada 73; along Nevada 73 between U. S. 50 and Nevada 21; on Utah 21 between Nevada 73 and 25 miles east of Garrison, Utah; along Nevada 38 between Sunnyside, Nev., and 3 miles south of Sunnyside, Nev.; and along several of the desert roads north of the Nevada test site.

The maximum effective biological dose for a populated area was 2,580 mr. at Reed, Nev. The maximum effective biological dose at a nonpopulated point was 6,270 mr. in Kawich Valley northwest of the Nevada test site.

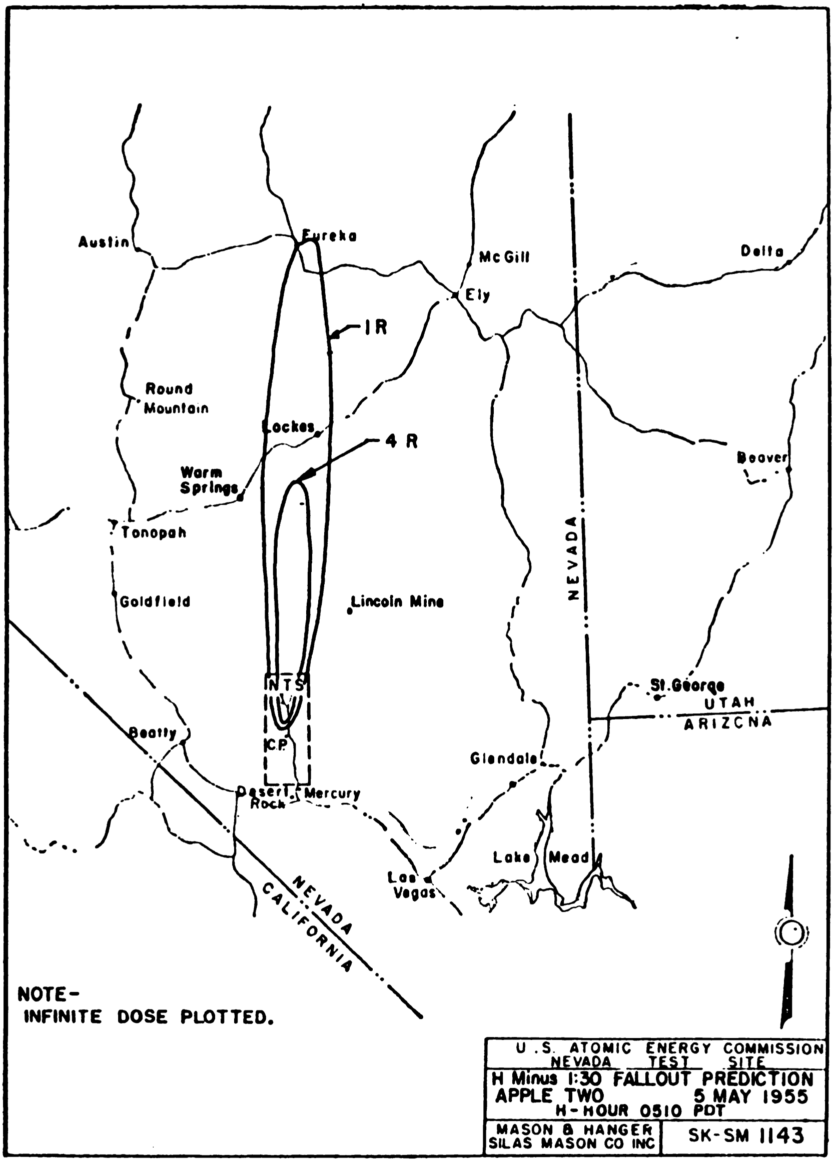
Approximately 385 individual monitoring readings above 0.1 mr/hr., were recorded.

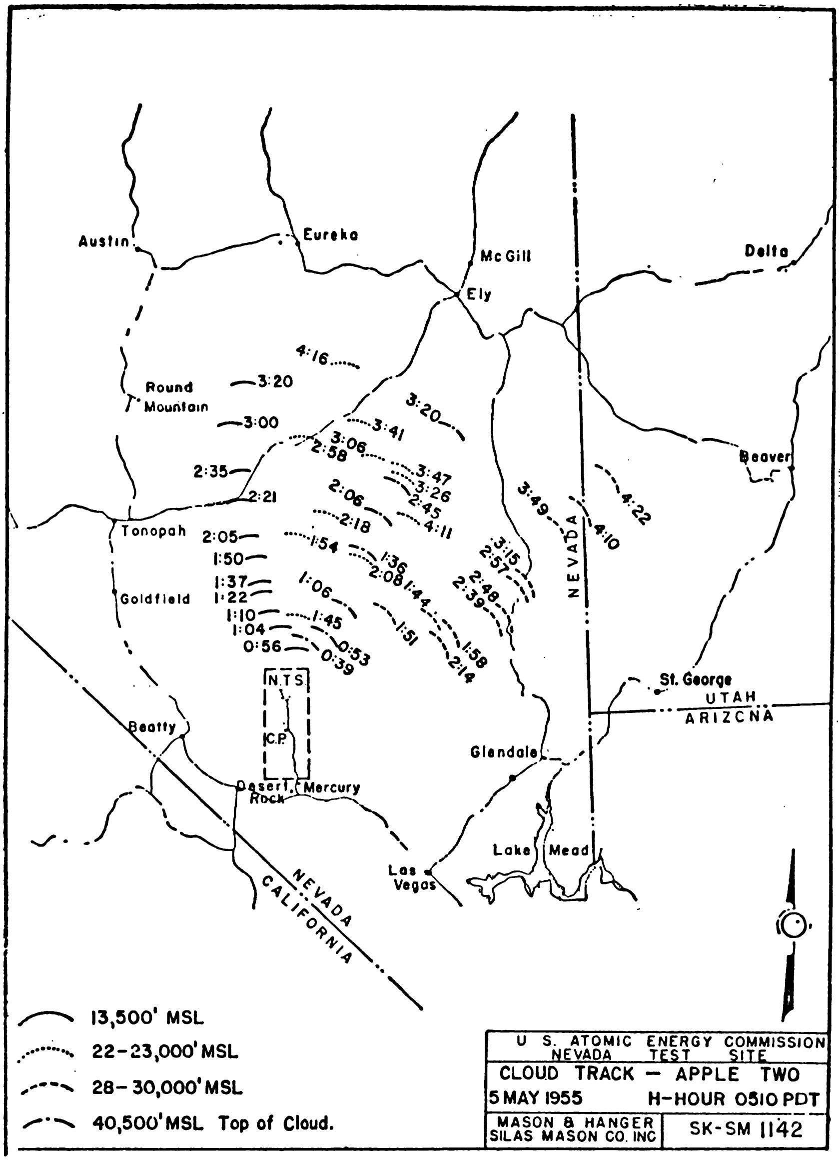
A comparison of the prediction map and the factual maps indicates good directional agreement with an overprediction in magnitude (length of isodose contours). The cloud track map shows one reason for the overprediction, and that is shear. The cloud was dispersed to a great extent laterally. The ground survey infinite dose map shows the 1 r. contour crossing U. S. 6 about midway between Tonopah and Ely, Nev. The shear, previously mentioned, is also evident in the construction of the isodose lines.

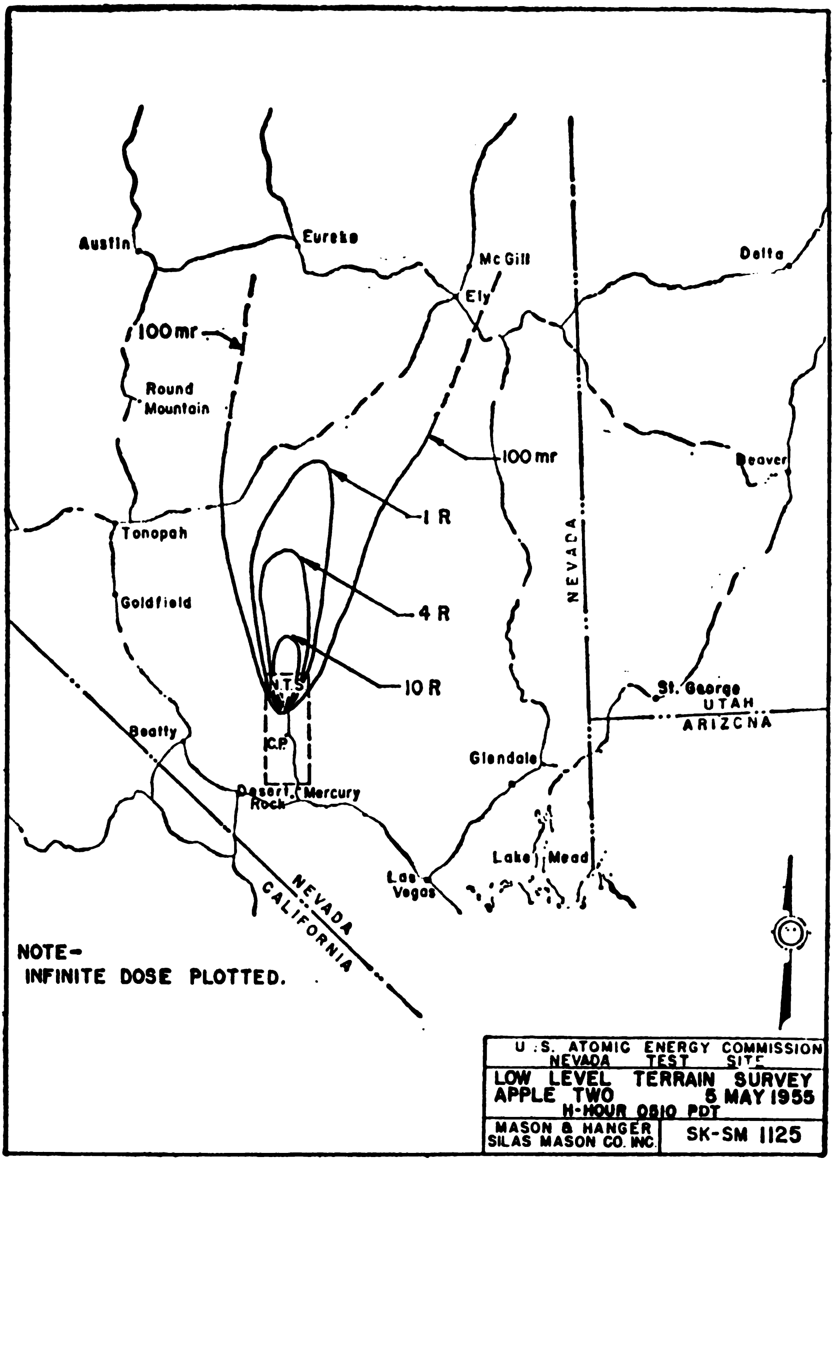
The maximum air radioactivity concentration measured was 5.9×10^{-2} $\mu c/m^3$, at Ely, Nev. This represents the average air concentration for a 28-hour period starting at shot time.

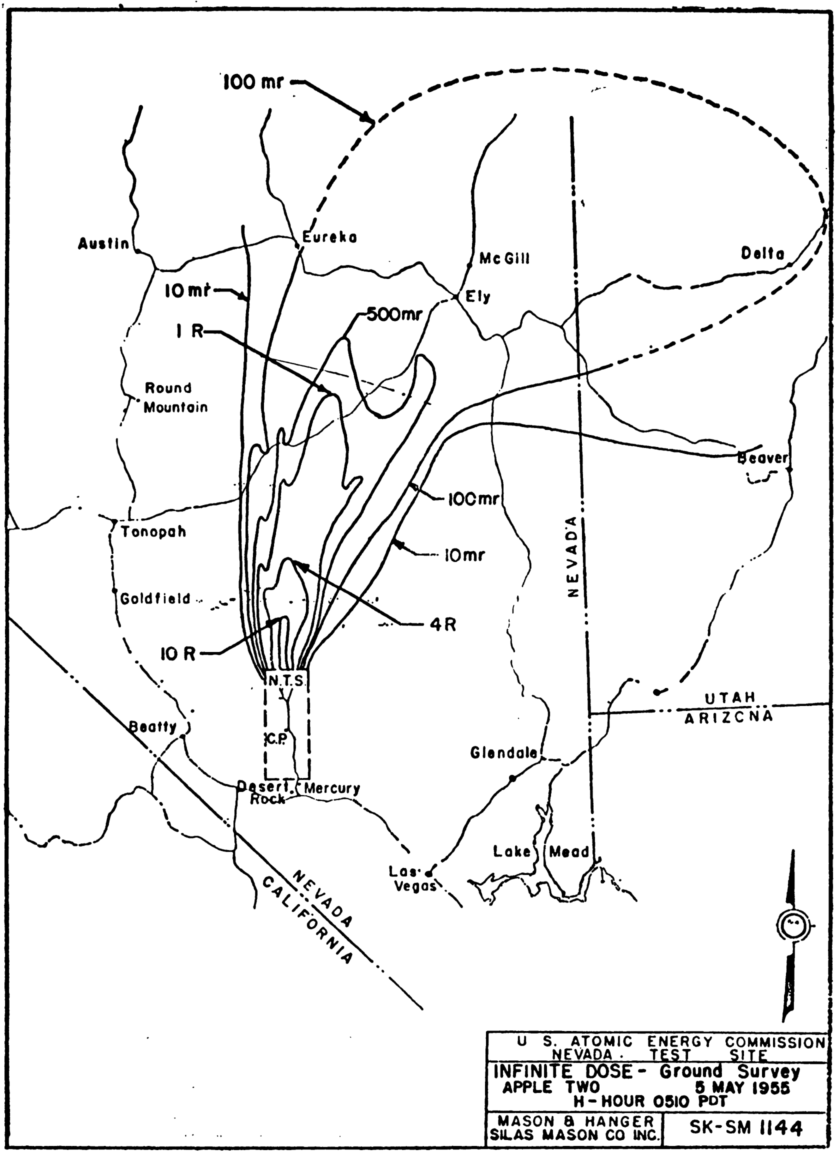
Apple Two: External gamma dose in populated areas and at selected nonpopulated points

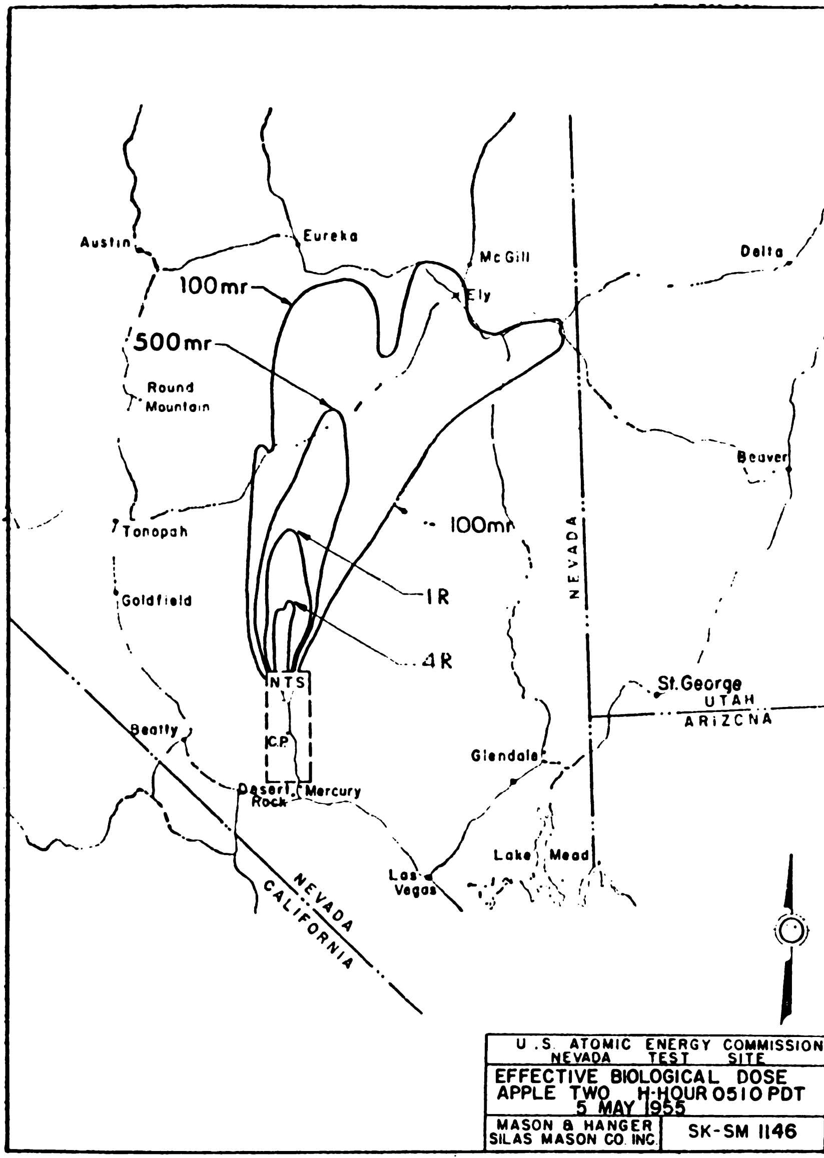
Location	Time of instrument reading (H+hours)	Gamma ground level (mr./hr.)	Time of fallout (H+hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Adaven, Nev	4.2	18.0	4.1	200	370
Nyala, Nev	5.8	30.0	4.4	500	930
Lincoln Mine, Nev	15.0	.3	2.6	18	32
Fallini Ranch, Nev	5.0	13.0	4.2	250	460
Reed, Nev	6.8	110.0	2.5	2,580	4,590
Sunnyside, Nev	5.6	.4	5.6	5	10
Warm Springs, Nev	7.5	.3	4.2	6	11
Lockes Ranch, Nev	5.3	38.0	5.3	530	1,010
Currant, Nev	6.5	8.0	6.4	130	260
Duckwater, Nev	7.3	16.0	6.8	300	590
Lund, Nev	11.9	7.5	7.3	250	490
Ely, Nev	13.7	6.5	8.7	250	490
Baker, Nev	36.1	.8	9.1	95	190
Garrison, Utah	36.3	.5	9.0	60	120
Eureka, Nev	9.1	.8	9.1	18	36
Nonpopulated points:					
U. S. 6, 4 miles west of Lockes Ranch, Nev	5.4	55.0	5.3	790	1,490
Kawich Valley, Nev	4.2	440.0	1.8	6,270	10,900


Austin
Eureka
McGill
Ely
Delta
1 R
Round Mountain
Lockes
4 R
Warm Springs
Beaver
Tonopah
NEVADA
Goldfield
Lincoln Mine
NTS
St. George
UTAH
ARIZONA
Beatty
C.P.
Glendale
Desert Rock
Mercury
Lake Mead
Las Vegas
NEVADA
CALIFORNIA
NOTE-
INFINITE DOSE PLOTTED.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
H Minus 1:30 FALLOUT PREDICTION
APPLE TWO 5 MAY 1955
H-HOUR 0510 PDT
MASON & HANGER
SILAS MASON CO INC
SK-SM 1143


Austin
Eureka
McGill
Ely
Delta
Round Mountain
Tonopah
Goldfield
Beatty
Beaver
St. George
UTAH
ARIZONA
NEVADA
Glendale
Lake Mead
Las Vegas
Desert Rock
Mercury
N.T.S.
C.P.
NEVADA
CALIFORNIA
4:16
3:20
3:00
2:35
2:21
2:05
1:50
1:37
1:22
1:10
1:04
0:56
0:39
0:53
1:45
1:06
1:54
2:18
2:06
2:58
3:06
3:41
3:20
3:47
3:26
2:45
4:11
1:36
2:08
1:44
1:51
1:58
2:14
2:48
2:39
3:15
2:57
3:49
4:10
4:22
13,500' MSL
22-23,000' MSL
28-30,000' MSL
40,500' MSL Top of Cloud.
U. S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK — APPLE TWO
5 MAY 1955
H-HOUR 0510 PDT
MASON & HANGER
SILAS MASON CO. INC
SK-SM 1142


Austin
Eureka
McGill
Ely
Delta
100 mr
Round Mountain
100 mr
Beaver
1 R
Tonopah
4 R
Goldfield
10 R
N.T.S
NEVADA
St. George
UTAH
ARIZONA
Beatty
C.P.
Glendale
Desert Rock
Mercury
Lake Mead
Las Vegas
NEVADA
CALIFORNIA
NOTE-
INFINITE DOSE PLOTTED.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
LOW LEVEL TERRAIN SURVEY
APPLE TWO 5 MAY 1955
H-HOUR 0510 PDT
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1125


100 mr
Austin
Eureka
McGill
Delta
10 mr
Ely
500mr
1 R
Round Mountain
Beaver
100mr
Tonopah
10mr
NEVADA
Goldfield
4R
10 R
N.T.S.
C.P.
Beatty
UTAH
ARIZONA
Glendale
Desert Rock
Mercury
Lake Mead
Las Vegas
NEVADA
CALIFORNIA
U. S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
INFINITE DOSE - Ground Survey
APPLE TWO 5 MAY 1955
H-HOUR 0510 PDT
MASON & HANGER
SILAS MASON CO INC.
SK-SM 1144


Austin
Eureka
McGill
Ely
Delta
100mr
500mr
Round Mountain
Beaver
Tonopah
100mr
NEVADA
Goldfield
1R
4R
NTS
St. George
UTAH
ARIZONA
Beatty
C.P.
Glendale
Desert Rock
Mercury
Lake Mead
Las Vegas
NEVADA
CALIFORNIA
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
EFFECTIVE BIOLOGICAL DOSE
APPLE TWO H-HOUR 0510 PDT
5 MAY 1955
MASON & HANGER
SILAS MASON CO. INC.
SK-SM 1146

Zucchini

Zucchini was a 500-foot tower detonation which was fired at 5 a. m. on May 15, 1955. The shot took place in test area 7 in Yucca Flat.

The airway closure pattern was as follows:

1. Starting the evening of May 14 at 10:11 p. m., the warning circle was set up as a 265-nautical-mile radius at the Grand Canyon Airport. The flash circle was a standard 60-nautical-mile radius of the coordinates 37° N. 116° W. and closed at all altitudes from 4:30 to 9:30 a. m. The low-level closure was between the bearings of 140° and 180° for a distance of 120 nautical miles and closed at the altitudes from the surface through 13,000 feet. The time of closure was 6:30 a. m. to 9 a. m. The middle-level closure was between bearing lines of 100° and 180°, determined by extending the 180° line for 150 nautical miles and extending the 100° line for 340 nautical miles with an arc of 340-nautical-mile radius drawn off the 100° bearing; then at a distance of 150 nautical miles on the 180° line, a direct line extending through the Phoenix low-frequency radio range station to intersect the 340-nautical-mile arc. The first segment of the middle level was the area extending from the test site out 150 nautical miles. This area was closed at 13,000 feet through 23,000 feet, inclusive. The time of closure was 5:45 a. m. to 10 a. m. The second segment was all the area between 150 nautical miles and 340 nautical miles and closed at the altitudes of 16,000 to 23,000 feet, inclusive, from 7:15 a. m. to 10 a. m. The high-level closure was between the bearings of 50° and 140°. The 140° line was extended to intersect the north edge of airway Green 4, then direct to the 4 corners, direct to the Grand Junction low-frequency-range station. The 50° line was extended across the east course of the Delta, Utah, radio-range station and direct to Grand Junction. The first segment of the high-level closure was all the area west of a line drawn from Delta to the Grand Canyon Airport. The altitudes closed were 23,000 to 40,000 feet from 5:30 to 8:45 a. m. The second segment was all the area east of the Delta-Grand Canyon line and closed at the altitudes of 23,000 to 40,000 feet from 6:45 a. m. to 12 noon.
2. At 6:07 a. m., tracking aircraft reports indicated the low-level trajectories had changed, and the 140° and 180° bearing lines were changed to read 100° and 140°. The closure time remained the same.
3. At 6:26 a. m., the middle-level closure was changed to correspond with the high-level closure. This area was then closed at the altitudes of 13,000 to 40,000 feet, inclusive.
4. At 7:05 a. m., the combined high- and middle-level areas were extended from Delta, Utah, directly to the south edge of airway Green 3 at Fort Bridger, Wyo., then east along the south edge of airway Green 3 to Medicine Bow, then south along the west edge of Green 10 to Denver, then west to the north edge of airway Victor 8 to Grand Junction.
5. At 8:26 a. m., closures remaining effective at 9 a. m. were the second segment of the high- and middle-level closure lying east of the Delta-Grand Canyon line. This area was then closed at altitudes 20,000 to 40,000 feet, inclusive, from 9 a. m. to 12 noon. All tracking aircraft were returned to base and no further changes were made.

The cloud was tracked by 1 B-25 at 13,000 feet, 2 B-50's at 23,000 and 28,000 feet, respectively, and by sampler aircraft at approximately 35,000 feet. Maximum cloud height reported was 37,700 feet, stabilizing at 36,300 feet. The cloud was tracked by the B-50's and sampler aircraft on an approximate bearing of 69° for a distance of 218 nautical miles. The low-level portion was followed 145 nautical miles on a 118° bearing.

A low-level terrain survey was made by 1 C-47 aircraft from H plus 5 hours and 30 minutes to H plus 10 hours and 30 minutes. The accompanying map shows that fallout occurred along an approximate bearing of 105°.

Monitoring runs, which indicated activity substantially above background, were made along Utah 18 from 3 to 38 miles south of Enterprise, Utah; on U. S. 91-93 from Glendale, Nev., to 4 miles south of Apex, Nev.; along U. S. 91 from Glendale, Nev., to Paragonah, Utah; along U. S. 93 from Glendale, Nev., to 38 miles north of Glendale; on Warm Springs Ranch, Nev., Road; along the Mormon Mesa Road; on the roads in the Moapa Indian Reservation; on Nevada 40; on Nevada 12; and along several of the roads east of the Nevada test site.

The maximum effective biological dose for a populated area was 700 mr. at Moapa, Nev. The maximum effective biological dose at a nonpopulated point was 19,900 mr., 7.5 miles south of Papoose Lake.

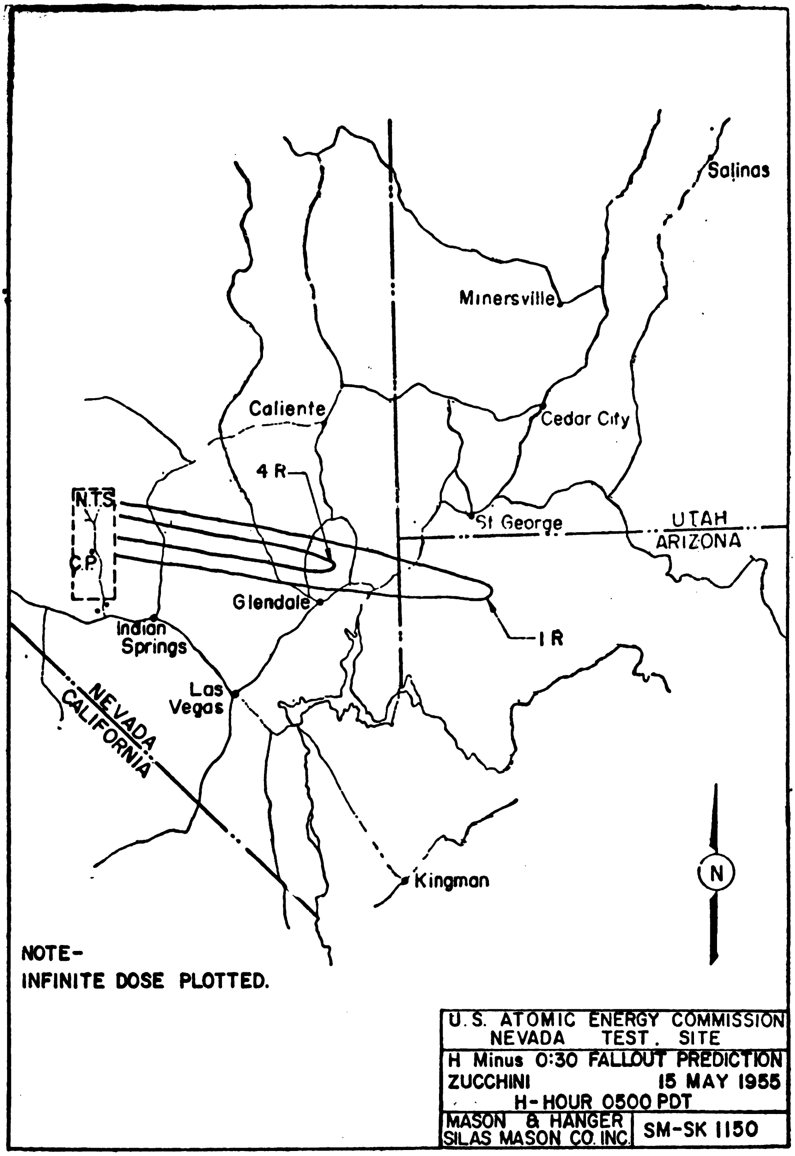
Approximately 375 individual monitoring readings above 0.1 mr./hr. were recorded.

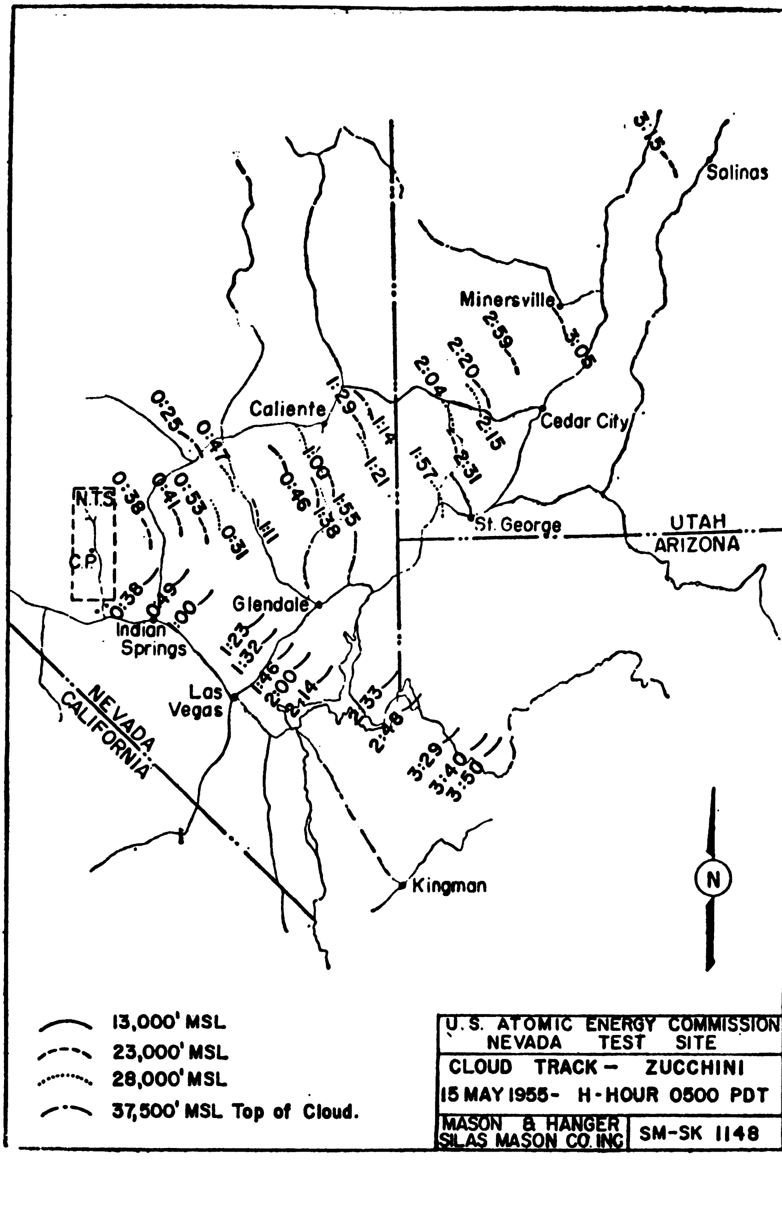
A comparison of the prediction map and the factual maps indicates an over-prediction in magnitude and a 5° to 10° difference in direction. The low-level terrain survey map shows good agreement with ground survey results with the exception that it indicates shorter isodose lines. The ground survey map shows an additional light fallout pattern to the northeast. One r. infinite isodose contour intersected U. S. 91 and U. S. 93 in the vicinity of Glendale, Nev.

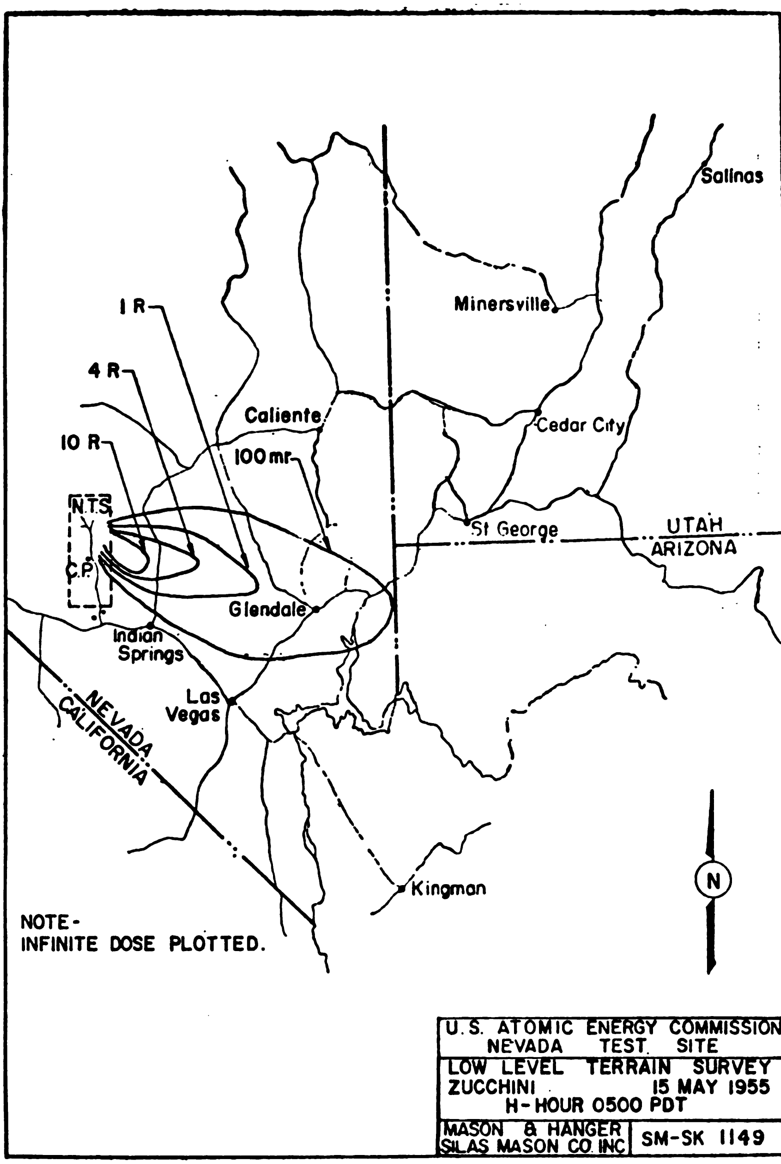
The maximum air radioactivity concentration measured was 7.8×10^{-6} $\mu c/m^3$, at Cedar City, Utah. This represents the average air concentration for a 28.2-hour period starting at shot time.

Zucchini: External gamma dose in populated areas and at selected nonpopulated points

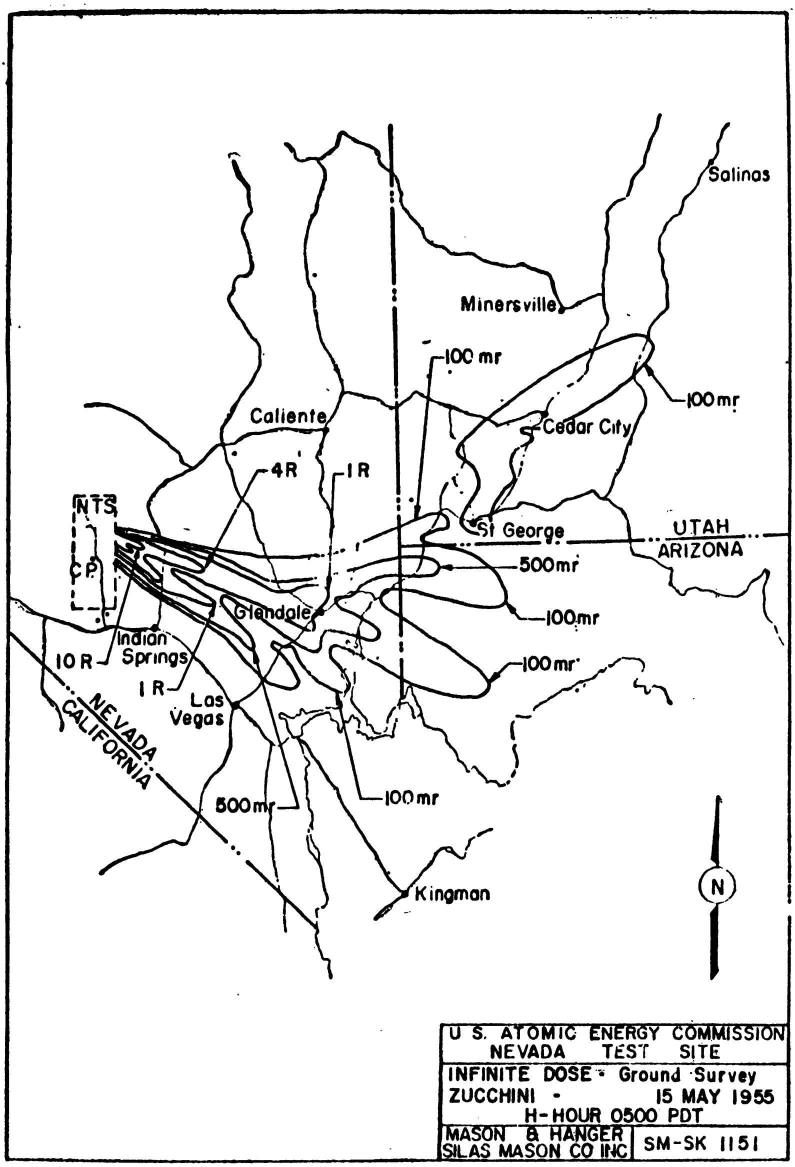
Location	Time of instrument reading (H+hours)	Gamma ground level (mr./hr.)	Time of fallout (H+hours)	Effective biological dose (mr.)	Infinite dose (mr.)
Populated areas:					
Warm Springs Ranch, Nev	3.4	50.0	3.4	470	850
Indian Springs, Nev	6.6	.18	1.2	4	6
Apex, Nev	4.1	6.5	3.5	75	140
Dry Lake, Nev	3.8	7.5	3.3	80	150
Crystal, Nev	3.5	11.0	3.5	100	190
Glendale, Nev	13.5	9.0	3.9	420	780
Moapa, Nev	3.9	65.0	3.6	700	1,290
Mesquite, Nev	12.0	3.0	6.0	110	210
Carp, Nev	4.8	.8	3.8	10	21
Logandale, Nev	5.3	20.0	4.5	290	540
Overton, Nev	5.6	17.0	4.7	260	480
Cedar City, Utah	7.0	4.8	6.6	90	170
Kanarraville, Utah	8.7	2.8	6.2	70	130
Pintura, Utah	9.0	2.2	5.9	55	110
Leeds, Utah	9.3	1.8	5.7	50	91
Washington, Utah	9.6	3.9	5.2	110	210
St. George, Utah	8.6	3.0	5.0	76	140
Summit, Utah	11.6	2.2	7.0	70	140
Parowan, Utah	11.7	5.5	7.3	180	350
Paragonah, Utah	11.8	4.6	7.4	150	300
Santa Clara, Utah	8.5	.8	4.8	20	38
Beaver Dam, Ariz	7.3	13.0	4.6	270	500
Nonpopulated points:					
U. S. 93, 12 miles north of junction with U. S. 91	3.1	82.0	3.1	700	1,270
7.5 miles south of Papoose Lake	32.7	85.0	0.5	19,900	32,500

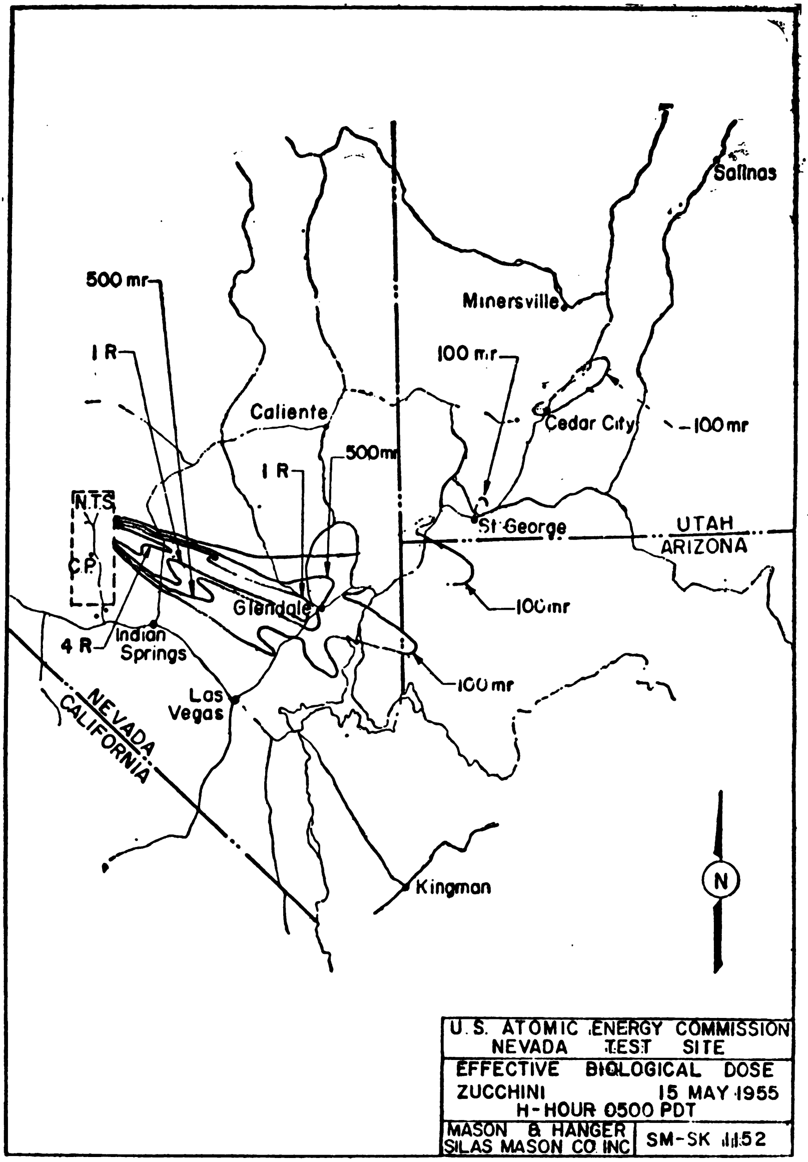

Salinas
Minersville
Caliente
Cedar City
4 R
N.T.S.
C.P.
St George
UTAH
ARIZONA
Glendale
Indian Springs
I R
Las Vegas
NEVADA
CALIFORNIA
Kingman
N
NOTE-
INFINITE DOSE PLOTTED.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST. SITE
H Minus 0:30 FALLOUT PREDICTION
ZUCCHINI 15 MAY 1955
H-HOUR 0500 PDT
MASON & HANGER
SILAS MASON CO. INC.
SM-SK 1150


Solinas
Minersville
Cedar City
Caliente
St. George
UTAH
ARIZONA
N.T.S.
C.P.
Glendale
Indian Springs
Las Vegas
NEVADA
CALIFORNIA
Kingman
N
13,000' MSL
23,000' MSL
28,000' MSL
37,500' MSL Top of Cloud.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
CLOUD TRACK — ZUCCHINI
15 MAY 1955 - H-HOUR 0500 PDT
MASON & HANGER SILAS MASON CO. INC
SM-SK 1148



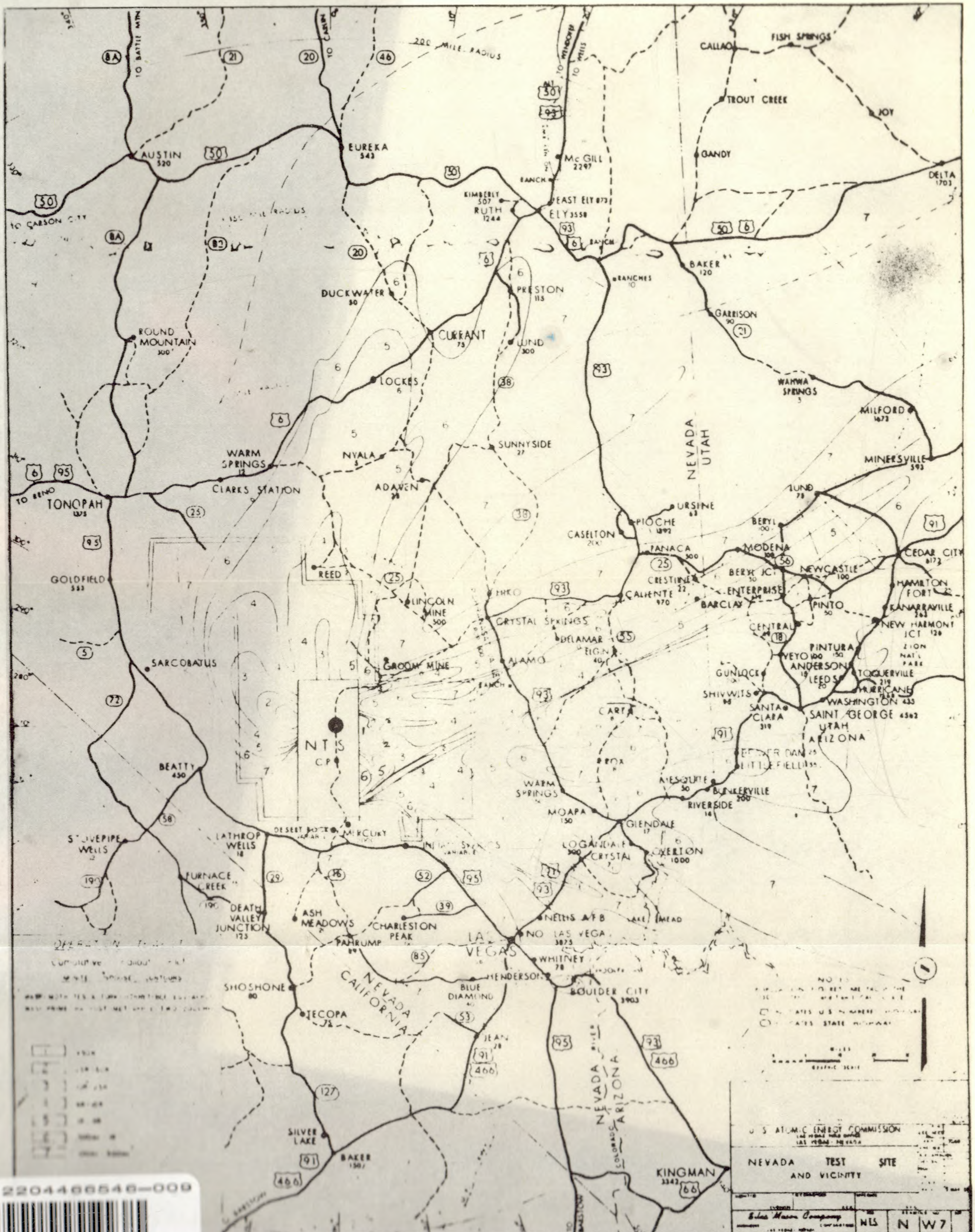
Salinas
Minersville
1 R
4 R
10 R
Caliente
Cedar City
100 mr
N.T.S.
C.P.
St George
UTAH
ARIZONA
Glendale
Indian Springs
Las Vegas
NEVADA
CALIFORNIA
Kingman
N
NOTE-
INFINITE DOSE PLOTTED.
U.S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
LOW LEVEL TERRAIN SURVEY
ZUCCHINI 15 MAY 1955
H-HOUR 0500 PDT
MASON & HANGER
SILAS MASON CO. INC
SM-SK 1149


Salinas
Minersville
100 mr
100 mr
Caliente
Cedar City
4R
1 R
NTS
CP
St George
UTAH
ARIZONA
500mr
100mr
Glendale
Indian Springs
10 R
100mr
1 R
Las Vegas
NEVADA
CALIFORNIA
500mr
100mr
Kingman
N
U S. ATOMIC ENERGY COMMISSION
NEVADA TEST SITE
INFINITE DOSE - Ground Survey
ZUCCHINI - 15 MAY 1955
H-HOUR 0500 PDT
MASON & HANGER
SILAS MASON CO INC
SM-SK 1151



6. SUMMARIES

In this section are summaries of the film badge program, accumulated dosage in populated areas, water and milk results, and air results. Each of these subjects is discussed in a subsection while accumulated dosage in populated areas is presented as table 2. The values presented are those measured by survey instruments and by film badges. Survey instrument readings have been expressed as effective biological dose and as infinite dose. Every populated place in which either type measurement was made is included.


200 MILE RADIUS
TO BATTLE MTN
TO CARLIN
TO WENDOVER
TO WELLS
TO CARSON CITY
TO RENO
CALLAO
FISH SPRINGS
TROUT CREEK
JOY
GANDY
DELTA
1703
AUSTIN
520
EUREKA
543
McGILL
2297
KIMBERLY
507
RUTH
1244
EAST ELY
ELY 3558
BAKER
120
GARRISON
90
DUCKWATER
50
PRESTON
115
CURRANT
75
LUND
300
LOCKES
6
ROUND MOUNTAIN
300
WAH WAH SPRINGS
MILFORD
1673
MINERSVILLE
593
SUNNYSIDE
27
NYALA
ADAVEN
WARM SPRINGS
CLARKS STATION
TONOPAH
1375
NEVADA
UTAH
URSINE
PIOCHE
1392
CASELTON
200
LUND
BERYL
MODENA
PANACA
CEDAR CITY
6173
GOLDFIELD
553
REED
CRESTLINE
BERYL JCT
NEWCASTLE
ENTERPRISE
HAMILTON FORT
HIKO
CALIENTE
2970
BARCLAY
PINTO
KANARRAVILLE
LINCOLN MINE
500
CRYSTAL SPRINGS
NEW HARMONY JCT
DELAMAR
ELGIN
CENTRAL
PINTURA
ZION NAT'L PARK
SARCOBATUS
GROOM MINE
ALAMO
VEYO
ANDERSON
LEEDS
TOQUERVILLE
GUNLOCK
HURRICANE
SHIVWITS
WASHINGTON
SANTA CLARA
319
SAINT GEORGE
UTAH
ARIZONA
CAREY
NTS
CP
BEATTY
450
MESQUITE
BUNKERVILLE
200
RIVERSIDE
WARM SPRINGS
MOAPA
150
GLENDALE
STOVEPIPE WELLS
LATHROP WELLS
DESERT ROCK
MERCURY
INDIAN SPRINGS
LOGANDALE
300
CRYSTAL
OVERTON
1000
FURNACE CREEK
DEATH VALLEY JUNCTION
125
ASH MEADOWS
PAHRUMP
CHARLESTON PEAK
NELLIS AFB
NO LAS VEGAS
LAS VEGAS
WHITNEY
HENDERSON
BOULDER CITY
3903
SHOSHONE
80
TECOPA
BLUE DIAMOND
NEVADA
CALIFORNIA
JEAN
NEVADA
ARIZONA
COLORADO RIVER
SILVER LAKE
BAKER
KINGMAN
3342
U. S. ATOMIC ENERGY COMMISSION
LAS VEGAS, NEVADA
NEVADA TEST SITE
AND VICINITY
NTS
N
W7

FILM BADGE PROGRAM

Film badges have proved to be a practical method for large-scale area monitoring. During Operation Teapot, an area of approximately 50,000 square miles was effectively monitored by the use of film badge stations. These stations consisted of the following categories: 171 worn by residents in the off-site area; 106 in populated areas; 152 inside and outside schools; and 126 at nonpopulated points along all of the major highways and most of the less-traveled roads. Badges were changed at frequent intervals, with a total of 4,420 individual badges used during the operation.

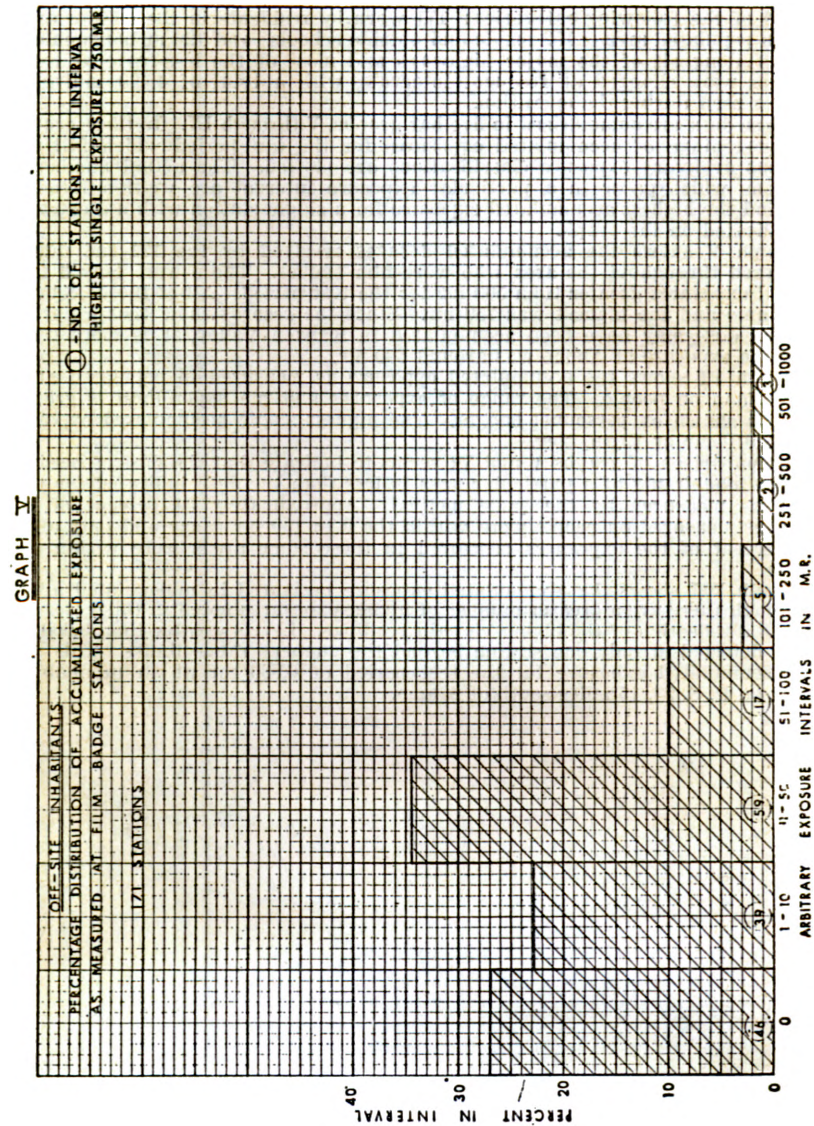
These data are summarized in five bar graphs, V through IX. In these graphs, the percentage of accumulated exposures for the test period which fall into various arbitrary exposure classifications are plotted. The actual number of stations which occur in a particular exposure interval are written into the bar and the highest accumulated exposure for a single station within a category is indicated on the graph.

Graph V shows the accumulated dosages received by the 171 individuals scattered throughout the area who wore film badges. There were only 3 individual exposures greater than 500 mr for the test period. All of these occurred in Alamo and the highest was 750 mr. Since the records show that a portion of this dose may have been accumulated in Kawich Valley, it is more likely that the exposure at Alamo was around 600 mr.

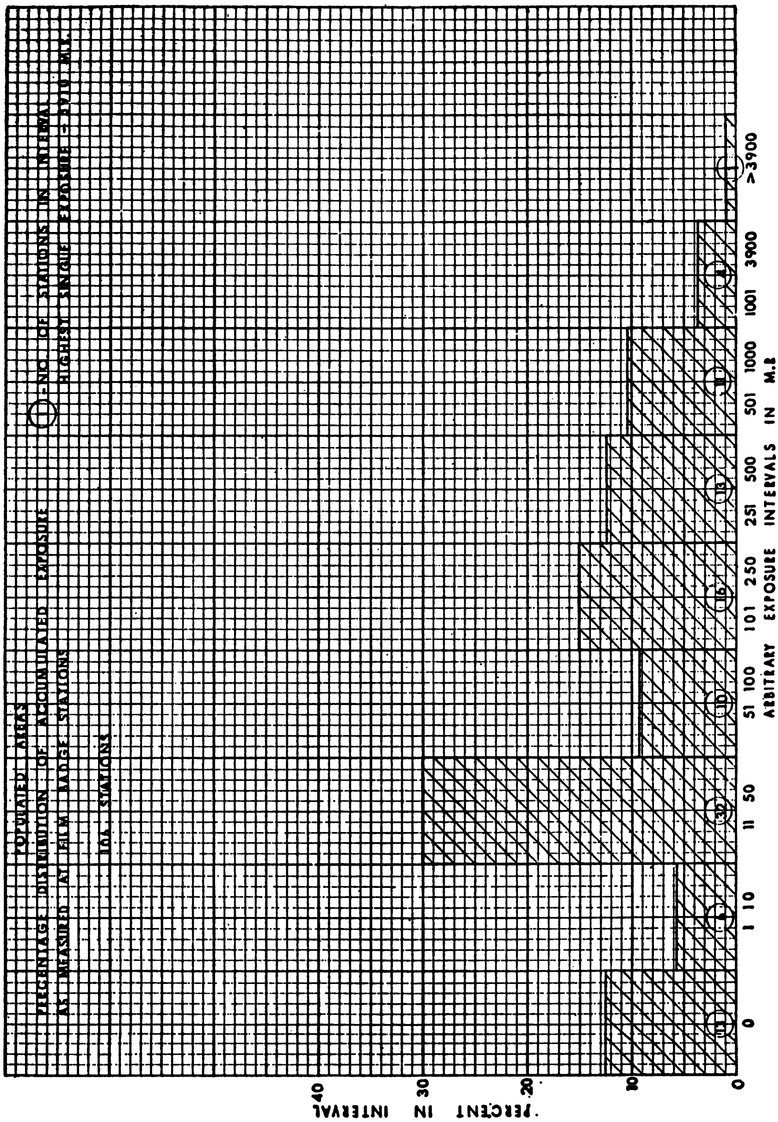
Graph VI shows the accumulated exposure as measured in populated areas other than on people. The range of exposures and the upper limit is greater than is shown on personnel badges. The highest single accumulated exposure was 3,910 mr at Elgin, Nev.

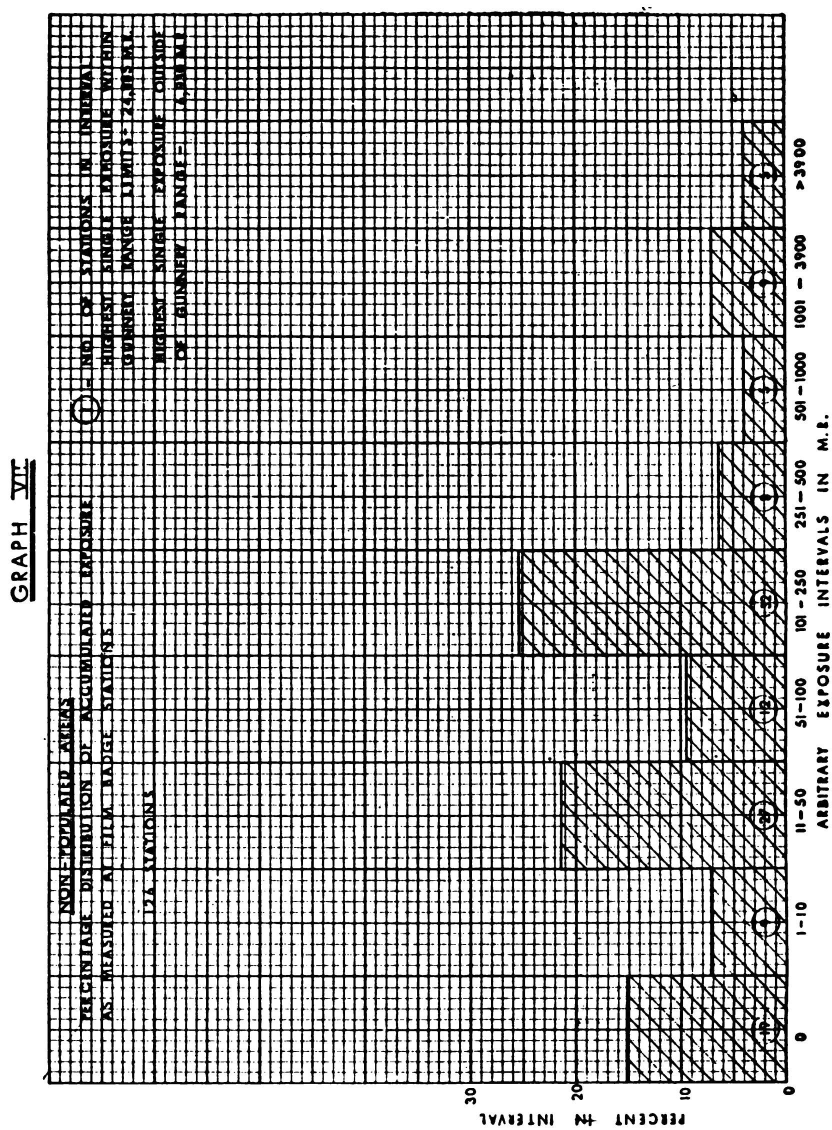
A comparison of graphs V and VI seems to indicate that the dosage received by the inhabitants of a particular area is less than the dose indicated for that area as measured by the same method. Approximately 94 percent of the dosages measured on people were within the 0 to 100 mr range and only about 57 percent of the exposures measured in these same areas were within this range.

Graph VII shows the accumulated exposure measured in unpopulated areas. Although there are a greater number of higher exposures noted in this category, this is a reflection of the higher close-in readings. Levels greater than 3,900 mr were detected at only one station beyond the gunnery range limits. This was an accumulated exposure for the test period of 6,930 mr at an uninhabited point on U. S. 93, 15 miles south of Alamo.


GRAPH V
OFF-SITE INHABITANTS
PERCENTAGE DISTRIBUTION OF ACCUMULATED EXPOSURE
AS MEASURED AT FILM BADGE STATIONS
171 STATIONS
1 - NO. OF STATIONS IN INTERVAL
HIGHEST SINGLE EXPOSURE - 750 MR
46
39
59
17
5
2
3
0
1 - 10
11 - 50
51 - 100
101 - 250
251 - 500
501 - 1000
ARBITRARY EXPOSURE INTERVALS IN M.R.
0
10
20
30
40
PERCENT IN INTERVAL

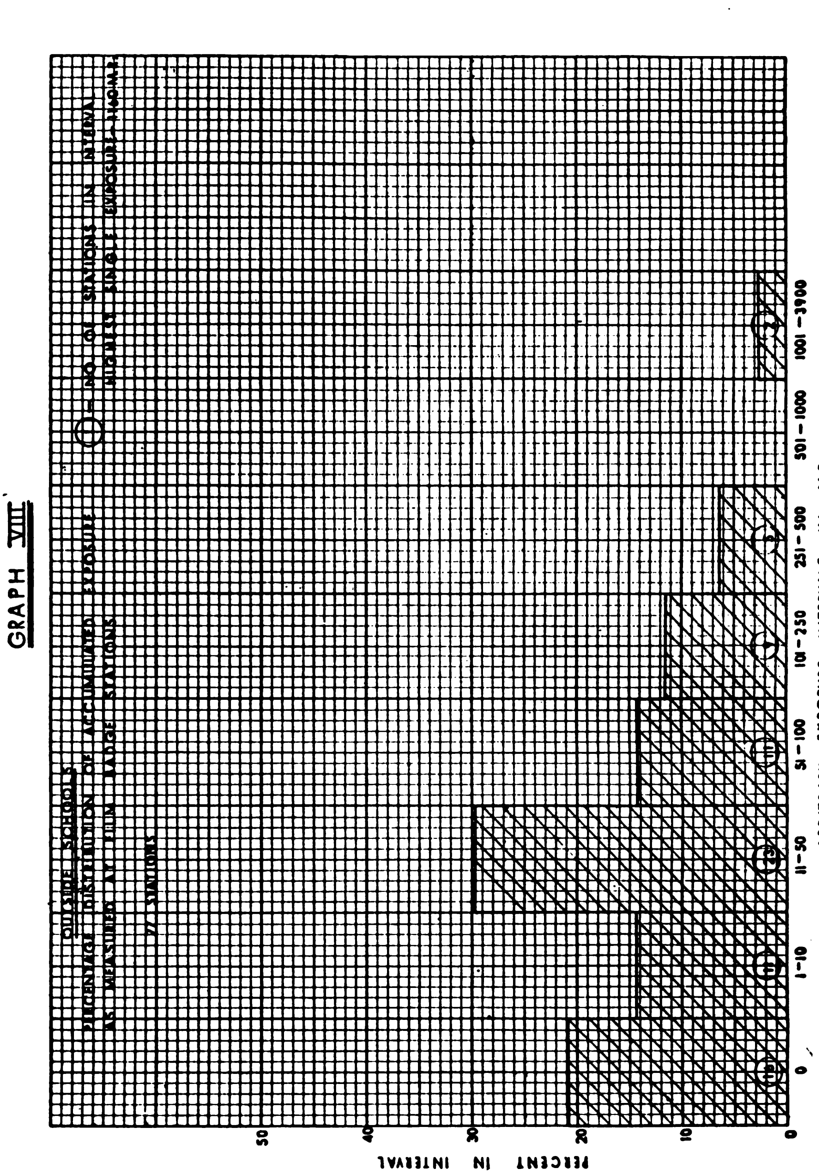
GRAPH VI


POPULATED AREAS
PERCENTAGE DISTRIBUTION OF ACCUMULATED EXPOSURE
AS MEASURED AT FILM BADGE STATIONS
104 STATIONS
NO. OF STATIONS IN INTERVAL
HIGHEST SINGLE EXPOSURE
0
1 10
11 50
51 100
101 250
251 500
501 1000
1001 3900
>3900
ARBITRARY EXPOSURE INTERVALS IN M.R
0
10
20
30
40
PERCENT IN INTERVAL

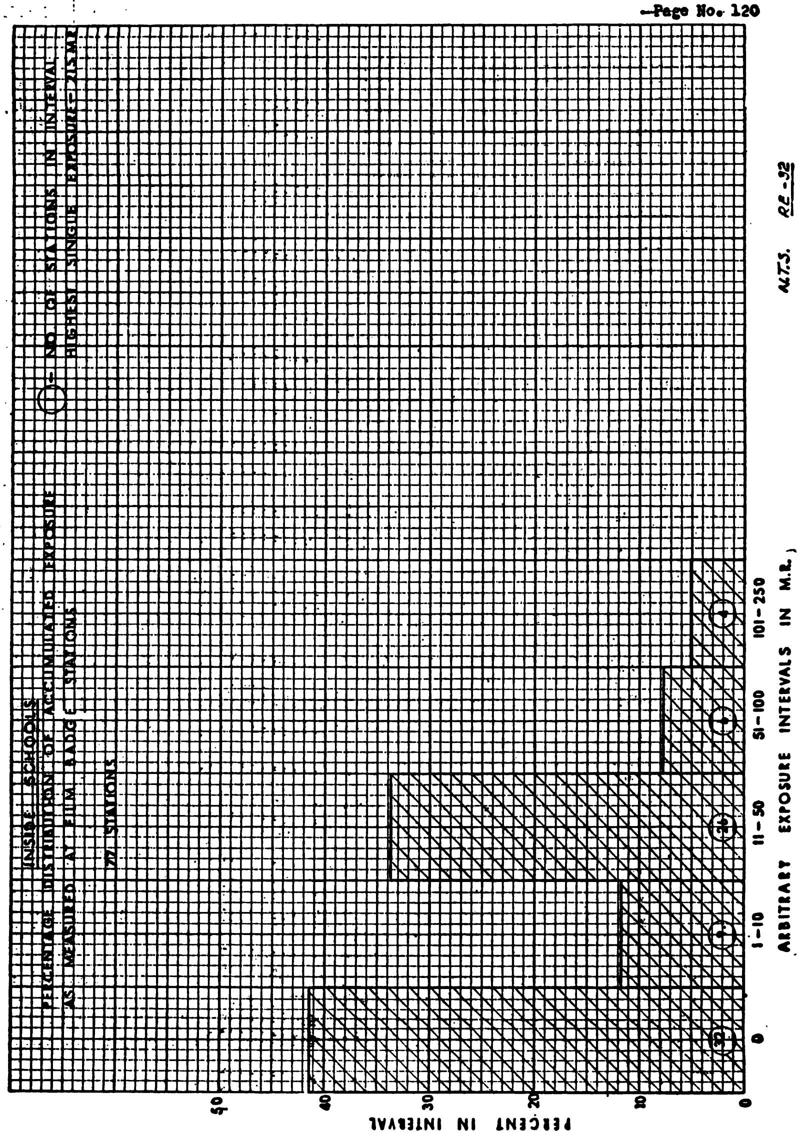

GRAPH VII
NON-POPULATED AREAS
PERCENTAGE DISTRIBUTION OF ACCUMULATED EXPOSURE
AS MEASURED AT FILM BADGE STATIONS
126 STATIONS
NO. OF STATIONS IN INTERVAL
PERCENT IN INTERVAL
0
10
20
30
0
1-10
11-50
51-100
101-250
251-500
501-1000
1001-3900
>3900
ARBITRARY EXPOSURE INTERVALS IN M.R.

Graphs VIII and IX show the accumulated exposures outside and inside of schools, respectively. The highest exposure outside was 1,160 mr at Alamo and the highest inside exposure noted was at Fallini Ranch School. The significant feature of these graphs is the apparent protection afforded by school buildings. The upper exposure limit is reduced by a considerable factor on a gross basis and while about 95 percent of the inside exposures fall within the 0–100 mr range, only about 77 percent of the outside badges are so limited.

One other factor of interest should noted. When the badges are considered on an individual basis rather than by stations for the whole period, the percentage of negative badges becomes very large. The entire range of percentages for individual badges is tabulated below.


GRAPH VIII
OUTSIDE SCHOOLS
PERCENTAGE DISTRIBUTION OF ACCUMULATED EXPOSURE
AS MEASURED AT FILM BADGE STATIONS
77 STATIONS
○ = NO. OF STATIONS IN INTERVAL
HIGHEST SINGLE EXPOSURE
0
1-10
11-50
51-100
101-250
251-500
501-1000
1001-3900
ARBITRARY EXPOSURE INTERVALS IN M.R.
0
10
20
30
40
50
PERCENT IN INTERVAL

—Page No. 120


GRAPH IX
INSIDE SCHOOLS
PERCENTAGE DISTRIBUTION OF ACCUMULATED EXPOSURE
AS MEASURED AT FILM BADGE STATIONS
77 STATIONS
- NO. OF STATIONS IN INTERVAL
HIGHEST SINGLE EXPOSURE
PERCENT IN INTERVAL
0
10
20
30
40
50
0
1-10
11-50
51-100
101-250
ARBITRARY EXPOSURE INTERVALS IN M.R.
N.T.S.
RE-32

Range interval, mr	Category in percent				
	Personnel exposure	Populated places	Unpopulate areas	Schools	
				Outside	Inside
0	82.8	71.6	67.7	79.1	89.2
1-10	8.9	8.5	8.2	9.3	4.8
11-50	6.7	11.3	12.7	7.3	5.1
51-100	.6	2.8	3.0	2.0	.6
101-250	.6	2.8	4.2	1.5	.3
251-500	.4	1.8	1.3	.5	
501-1,000		.7	1.1	.3	
1,001-3,900		.5	1.2		
3,900			.6		

If one considers the extensive area covered during Operation Teapot, the total number of badges and the large number of badge stations, and the numbers of people wearing badges off site, then film badge monitoring would appear to have the following desirable characteristics:

1. Film badges offer a permanent record of accumulated radiation exposure.
2. The exposure measurements obtained from film badges are not subject to error due to mathematical manipulation.
3. Film badges are convenient to use.
4. Extensive areas can readily be monitored by the use of film badges.
5. Film badges require no attention during the time they are recording radiation exposure.
6. From all available information, it appears that film badges can be left in the field for extended periods without adversely affecting the accumulated exposure data obtained.
7. People wearing film badges have a positive feeling that steps are being taken to look after their welfare. The typical attitude seemed to be twofold: (*a*) that they were important enough to be considered; and (*b*) the feeling of being protected.
8. Film badge records have an official status in radiation protection programs.

Based on performance during Operation Teapot, film badges are an effective, practical, and economical way to monitor both extensive land areas and people during a continental test series.

Inevitably, when data which presumably measure the same thing are collected, a comparison of results seems indicated. Comparative data at film badge stations in the Lincoln Mine zone are shown in the following table for illustrative purposes. The results shown are given in terms of effective biological dose, infinite dose, and accumulated dose as measured on film badges.

If one were to draw conclusions, these could only be broad generalizations, such as that the film dosage is *roughly comparable to the effective biological dose*. Striking exceptions are apparent. To attempt to be more specific, as for example, trying to arrive at a valid, constant factor relating the various types of data is not warranted. The reasons for this are outlined below. All of these various data, however, are useful and the fact that they cannot be strictly correlated does not impair their individual value for various purposes.

Film badges and monitoring data do not measure exactly the same thing although it might seem so at first glance. The measurements made by these various methods might be stated as follows:

Film badge results are a measure of the total accumulated radiation exposure within a given energy range, at a fixed point, and for a given time interval. This time is generally the period of the test series plus a week or two prior to and after the series.

Comparison of accumulated dosage readings in the Lincoln Mine area

Film badge station	Monitoring results		Film badge dose, milli-roentgen	Film badge station	Monitoring results		Film badge dose, milli-roentgen
	Effective biological dose, milli-roentgen	Infinite dose, milli-roentgen			Effective biological dose, milli-roentgen	Infinite dose, milli-roentgen	
74	717	1,197	450	127	26	47	180
75	2,087	3,567	2,000	128	5,100	8,830	7,330
76	105	190	180	129	84	146	250
80	200	370	245	130	29,295	48,395	24,650
88	1,801	3,340	2,110	131	5,015	8,290	11,940
89	2,135	3,903	2,320	132	1,037	1,765	2,100
90	7	13	1,625	133	6,562	11,120	24,885
91	79	146	200	134	95	168	115
92	200	360	140	135	2,247	4,070	3,370
93	94	168	180	141	500	930	515
94	367	625	360	143	2,580	4,590	2,990
126	52	105	100	476	81	133	235

Infinite dose is the maximum calculated exposure from time of fallout to infinite time at the place of measurement, based on an instrument reading taken at that place and extrapolated to infinity.

Effective biological dose is a percentage of the infinite dose obtained by inserting certain impirical factors for shielding, weathering, etc.

The reasons why these various means of estimating accumulated dose are not absolutely comparable are:

1. Both EBD and film badge doses are a variable percentage of the infinite dose. The relationship of EBD to infinite dose varies from approximately 60 percent when the time of fallout is 30 minutes to about 50 percent when the time of fallout is 9 hours. The relationship of film badge exposure to infinite dose varies with the total time interval covered by the film badge series, at a particular point.
2. Infinite dose and EBD are mathematical extrapolations of a single reading by instruments, which will vary with the accuracy of the reading and the approximation of fallout time.
3. Dosage determination by film badge measurements does not require a knowledge of fallout time. Film badges measure directly the actual dose present at a point during the time of exposure If changes in radiation level occur during this time, such as the blowing away or covering up of radioactive material or the secondary deposition of radioactive material at a later time, these changes are reflected in the film badge readings but not in the infinite dose or EBD results.
4. When two closely spaced shots proceed in the same direction, the monitoring readings from the second shot may reflect residual radioactivity from the previous shot. Extrapolation of the readings may result in higher infinite dose and EBD.
5. Fallout patterns are directional and frequently sharply defined within a given area. Unless the monitoring reading is taken at the exact point of film badge exposure, the results cannot be compared.
6. It is possible to fail to record a significant portion of the total exposure on film badges by discontinuing the program too soon. This would only occur under special circumstances. These would be a situation in a close-in area where the major portion of the exposure occurred on the last shot and the badges were collected immediately thereafter. It could be theoretically possible to fail to record several hundred mr. of total exposure under these conditions. It does not appear, however, that this could be too much of a factor beyond the gunnery range limits.
7. Film badges are a positive means of measurement of personnel exposure, Where film badges are worn by people, they measure the actual dose received as these people move about in their daily routine. As far as the individual's exposure is concerned, it is unnecessary to speculate as to his probable location to know his exposure, as is necessary when using monitoring measurements.

The above material indicates that monitoring and film badge measurements are both necessary and supplementary data. The overall picture of the total ex-

posure resulting from either type of data is similar in nature, but it is impractical to attempt to relate the two types of measurement through some specific and constant factor or factors.

The large amount of data that can be obtained with relative ease and economy by film badge measurements indicates that these measurements should become a permanent part of offsite planning. The major change that should be instituted is to increase the area coverage (school and personnel coverage was probably adequate) and at the same time to reduce the workload by less frequent badge changes.

Although the offsite program was concerned primarily with exposures of people and animals, numerous close-in monitoring runs were made to aid the Fallout Prediction Unit. In future operations, this assistance could be supplemented by lines of film badges across the gunnery range to aid in this objective.

Aside from the items mentioned above, and minor details regarding recordkeeping, the film badge program as operated was so successful and so productive that it appears that future programs should be based on the same general plan.

TABLE 2.—*Accumulated dosage in populated areas*

Location	Effective biological dose (mr.)	Infinite dose (mr.)	Film badge record	
			Time interval	Accumulated dosage (mr.)
Acoma, Nev			Apr. 1–May 16	[1] 3,130
Adaven, Nev	200	370	Mar. 11–May 16	[1] 245
Alamo, Nev	1,359	2,407	Feb. 12–June 7	1,160
Alton, Utah	19	37		
Anderson Junction, Utah	34	69	Feb. 11–May 20	20
Apex, Nev	75	140	Feb. 11–June 7	110
Ash Springs, Nev	142	264		
Austin, Nev			Feb. 12–May 19	40
Baker, Nev	148	303	Feb. 11–May 23	[2] 40
Barclay, Nev			Feb. 12–May 16	1,860
Bear Valley Junction, Utah			Feb. 11–May 21	390
Beatty, Nev			Feb. 21–May 17	[1] 50
Beaver, Utah	270	540	Feb. 14–May 23	100
Beaver Dam, Ariz	300	560	Feb. 11–June 10	145
Beryl, Utah	102	200	Feb. 21–May 20	50
Beryl Junction, Utah	456	870	Feb. 14–May 20	635
Boulder City, Nev	83	156	Feb. 12–May 17	30
Bristol Silver Mine, Nev			Feb. 21–May 17	[1] 20
Buckhorn Ranch, Nev	980	1,750		
Bunkerville, Nev	19	37	Feb. 11–June 10	25
Caliente, Nev	320	609	Feb. 14–May 16	370
Carp, Nev	20	41	Feb. 10–June 15	60
Caselton, Nev			Feb. 11–May 16	0
Cedar City, Utah	144	274	Feb. 11–May 20	20
Central, Utah			Mar. 24–May 20	[1] 0
Clarks Station, Nev			Feb. 11–May 21	10
Cove Fort, Utah	29	58	Feb. 14–Mar. 10	[1] 0
Crestline, Nev			Feb. 11–May 16	450
Crystal, Nev	107	203	Feb. 11–June 7	60
Crystal Springs, Nev	50	91		
Currant, Nev	212	429	Feb. 11–May 21	295
Desert Rock, Nev	47	96		
Dry Lake, Nev	210	385	Feb. 11–June 7	245
Duckwater, Nev	300	590	Feb. 11–May 21	345
East Ely, Nev			Feb. 12–May 9	[1] 205
Elgin, Nev	2,880	5,340	Feb. 14–May 24	3,910
Ely, Nev	328	653	Feb. 12–May 9	[1] 205
Enterprise, Utah	182	344	Feb. 14–May 20	220
Eureka, Nev	18	36	Feb. 11–May 19	[2] 70
Fallini Ranch, Nev	250	460	Feb. 12–May 16	395
Frisco, Utah	17	35		
Garrison, Utah	94	191	Feb. 11–May 23	15
Glendale, Nev	738	1,392		
Glendale, Utah	19	37	Feb. 11–May 19	10
Goldfield, Nev			Feb. 10–May 18	0
Groom Mine, Nev			Feb. 12–May 14	[1] 345
Gunlock, Utah	8	17	Feb. 11–May 20	20
Hamilton Fort, Utah	95	180		
Hamlin Valley, Utah			Feb. 20–May 20	0
Harrisburg Junction, Utah			Feb. 11–May 19	75
Henderson, Nev	8	15		
Hiko, Nev	63	110	Feb. 11–May 16	150
Hoover Dam, Nev	11	20		
Hurricane, Utah	51	100	Feb. 11–May 19	20

See footnotes at end of table.

TABLE 2.—*Accumulated dosage in populated areas*—Continued

Location	Effective biological dose (mr.)	Infinite dose (mr.)	Film badge record	
			Time interval	Accumulated dosage (mr.)
Indian Springs, Nev	12	22	Feb. 11–May 18	70
Kanab, Utah			Feb. 11–May 19	10
Kanarraville, Utah	220	420	Feb. 11–May 20	50
Kimberley, Nev			do	110
Lake Mead Base, Nev	16	30		
Las Vegas, Nev	140	260	Feb. 11–May 17	30
Lathrop Wells, Nev	21	42	Feb. 11–May 16	0
Leeds, Utah	94	179	Feb. 11–May 20	10
Lincoln Mine, Nev	62	116	Feb. 14–May 16	100
Littlefield, Ariz	10	20	Feb. 11–June 10	160
Lockes Ranch, Nev	662	1,289	Feb. 11–May 21	77
Logandale, Nev	307	573	Feb. 11–May 10	[1] 230
Lund, Nev	513	1,012	Feb. 11–May 21	315
Lund, Utah			Feb. 11–May 20	545
Manhattan, Nev			Feb. 11–May 10	[1] 20
McGill, Nev	16	32	Feb. 12–May 20	80
Mercury, Nev	42	84		
Mesquite, Nev	120	229	Feb. 11–May 10	[1] 20
Milford, Utah	70	140	Feb. 12–May 23	120
Millet, Nev			Feb. 11–May 19	30
Minersville, Nev	170	330	Feb. 12–May 23	200
Moapa, Nev	890	1,653	Feb. 11–June 7	865
Modena, Utah	99	190	Feb. 14–May 20	40
Moon River, Nev			Feb. 11–May 21	0
Mount Carmel Junction, Utah			Feb. 11–May 19	50
Nellis, Nev	31	56		
North Las Vegas, Nev	192	357		
Newcastle, Utah	182	354		
New Harmony, Utah	59	110	Feb. 11–May 20	10
Nyala, Nev	500	930	Mar. 10–May 16	[1] 515
Orderville, Utah	19	37	Feb. 11–May 19	30
Overton, Nev	260	480	Feb. 11–June 10	160
Pahrump, Nev	3	6	Feb. 21–May 16	[1] 260
Panaca, Nev	149	283	Feb. 11–May 17	100
Paragonah, Utah	166	332	Feb. 11–May 21	120
Parowan, Utah	185	360	Feb. 11–May 12	[1] 20
Peavine, Nev			Feb. 11–May 19	40
Pintura, Utah	55	110	Mar. 4–May 20	[1] 20
Pioche, Nev	26	51	Feb. 11–May 16	20
Preston, Nev	80	172	Feb. 11–May 21	160
Quartz Mine, Nev	35	71		
Reed, Nev	2,580	4,590	May 5–16	[1] 2,990
Riverside, Nev	43	84	Feb. 11–June 10	390
Rockville, Utah	3	6	Feb. 11–May 19	20
Round Mountain, Nev	34	74	do	10
Ruth, Nev			Feb. 11–May 20	60
Santa Clara, Utah	197	388	do	0
St. George, Utah	179	350	Feb. 14–May 19	40
Shoshone, Calif			Apr. 12–May 16	[1] 20
Springdale, Nev			Feb. 11–May 17	20
Springdale, Utah			Feb. 11–May 19	60
Strawberry, Nev			do	20
Summit, Utah	70	140	Feb. 11–May 21	30
Sunnyside, Nev	5	10	do	20
Tenopah, Nev			Feb. 10–May 10	[1] 10
Toquerville, Utah	63	129	Feb. 11–May 19	30
Ursine, Nev	19	40	Feb. 11–May 16	10
Veyo, Utah	14	27	Feb. 11–May 20	10
Virgin, Utah			Feb. 11–May 10	[1] 10
Warm Springs, Nev	163	335	Feb. 11–May 19	190
Warm Springs Ranch, Nev	567	1,034	Feb. 11–June 7	880
Washington, Utah	197	385	Feb. 11–May 19	30
Whitney, Nev	8	15		
Zane, Utah	275	525		

[1] Interval covered by film badges did not include entire test period.
[2] A badge is missing, which may have added significantly to the total dose.

WATER AND MILK RESULTS

In tables 3 and 4 are presented selected results from radioactivity analysis of water and milk, respectively. The data are selective in that only results where the radioactivity concentration exceeded 10^{-5} μc/ml. at the time of collection are shown. During the series, a total of 149 water and 134 milk samples were processed.

The background concentration for water was 6.8×10^{-7} μc/ml. This average is based on the analysis of 30 samples. The background concentration for milk,

as determined from 21 samples was 1.8×10^{-6} μc/ml. These background samples were collected and analyzed prior to the start of the test series and the data presented are gross values.

Relative information can be obtained by comparing the respective background value with the data presented in the appropriate table. However, there are several factors which should be understood before making such a comparison. Such factors would include:

1. Background values are gross figures. In other words, there may be differences in background from one type source to another and from one area to another.
2. Extrapolation by the 1.2 decay law is not completely valid as radioactive materials are removed from water selectively and similar selectivity takes place when radioactive materials are metabolized by an animal. Therefore, the tabulated values are in all probability conservative.
3. Time of fallout in large lakes and running streams is difficult to determine.
4. Time of fallout as well as time and method of contamination for use in calculations involving milk samples is practically impossible to determine without exhaustive specialized study.

TABLE 3.—*Radioactivity concentration in water*

Location	Detonation which produced fallout	Activity at time of collection (uc/ml)	Activity at time of fallout (uc/ml)
Upper Pahranagat Lake	Met	2.4×10^{-4}	3.0×10^{-3}
Maynard Lake	do	1.0×10^{-3}	1.2×10^{-2}
Meadow Valley wash	do	5.7×10^{-4}	1.1×10^{-3}
Maynard Lake	do	1.4×10^{-4}	7.1×10^{-3}
Crystal Springs, Nev	do	6.0×10^{-5}	1.8×10^{-4}
Meadow Valley wash	Apple	4.6×10^{-5}	1.5×10^{-3}
Cedar City, Utah (melted snow)	do	1.3×10^{-4}	1.3×10^{-4}
Waterhole at Groom Rd. and Nevada Hwy. 25	do	1.3×10^{-4}	6.0×10^{-3}
Waterhole at north end Papoose Lake	do	1.4×10^{-5}	2.8×10^{-3}
Upper Pahranagat Lake	Apple Two	2.6×10^{-5}	5.7×10^{-5}
Crystal Springs, Nev	do	1.6×10^{-5}	3.7×10^{-5}
Upper Pahranagat Lake	do	2.8×10^{-5}	6.3×10^{-5}
Virgin River at Riverside, Nev	Ess	3.8×10^{-5}	2.0×10^{-4}
Lake at Beaver Dam, Ariz	do	9.1×10^{-5}	3.2×10^{-4}
Groom Lake	Tesla	6.2×10^{-5}	3.2×10^{-3}
Waterhole at north end Papoose Lake	do	2.8×10^{-5}	3.3×10^{-2}
St. George, Utah, tapwater	Post	1.3×10^{-5}	2.9×10^{-5}
Leeds, Utah, tapwater	do	1.0×10^{-5}	2.0×10^{-5}
Irrigation water near Warm Springs, Nev	Hornet	1.1×10^{-5}	3.1×10^{-5}
Cedar City, Utah, tapwater	Zucchini	2.0×10^{-4}	1.2×10^{-3}
Central, Utah, Reservoir	do	5.4×10^{-5}	5.4×10^{-4}
Leeds, Utah, tapwater	do	1.2×10^{-5}	8.8×10^{-4}
Waterhole at Groom Rd. and Nevada Hwy. 25	do	4.3×10^{-5}	2.3×10^{-3}
Beaver Dam, Ariz., Lake	do	1.7×10^{-5}	1.9×10^{-4}
Virgin River at Riverside, Nev	do	1.3×10^{-4}	8.3×10^{-4}
Upper Pahranagat Lake	do	6.0×10^{-5}	9.8×10^{-5}
Creek at Ursine, Nev	do	1.9×10^{-5}	2.2×10^{-5}
Cedar City, Utah (melted hail)	do	1.6×10^{-1}	1.6×10^{-1}
Lake Meat at Stewart's Point	do	1.0×10^{-5}	8.3×10^{-5}
Meadow Valley wash	do	1.5×10^{-5}	1.7×10^{-4}

TABLE 4.—*Radioactivity concentration in milk*

Location	Detonation which produced fall out (probable)	Activity at time of collection (uc/ml)
Alamo, Nev. (ranch)	Moth	1.3×10^{-5}
Hiko, Nev. (ranch)	do	1.2×10^{-5}
Alamo, Nev. (ranch)	Turk	1.3×10^{-5}
Do	Met	4.9×10^{-5}
Do	do	4.9×10^{-5}
Beaver, Utah (dairy)	Tesla	1.5×10^{-5}
Minersville, Utah (dairy)	Post	4.0×10^{-3}
Do	Met	1.0×10^{-5}
Beaver, Utah (dairy)	do	1.3×10^{-5}
Cedar City, Utah (dairy)	do	1.3×10^{-4}
Ely, Nev. (ranch)	Tesla	1.9×10^{-5}
Ely, Nev. (dairy)	Ess	1.2×10^{-5}
Do	Met	1.0×10^{-5}

AIR RESULTS

Table 5 is a summary of all the air analyses obtained during Teapot. The location of air samplers, the dates and times of sampler operation, and the radioactivity concentration expressed as $\mu c/m^3$. averaged over the sampling period are given for each detonation. Values of 10^{-6} $\mu c/m^3$. and less are in the range of normal air-radioactivity backgrounds measured before and during the operation. Therefore, results less than 10^{-6} $\mu c/m^3$. are not listed other than as 10^{-6} $\mu c/m^3$.

TABLE 5.—*Airborne radioactivity concentrations*

SHOT: WASP

Location	Sampling period		Activity uc/m³
	From—	To—	
Alamo, Nev	1125, Feb. 18	1510, Feb. 18	10^{-6}
Caliente, Nev	1218, Feb. 18	2220, Feb. 18	2.0×10^{-6}
Cedar City, Utah	1130, Feb. 18	1200, Feb. 19	5.0×10^{-6}
Currant, Nev	1130, Feb. 18	1500, Feb. 18	10^{-6}
Ely, Nev	1130, Feb. 18	1730, Feb. 18	10^{-6}
Eureka, Nev	1108, Feb. 18	1212, Feb. 19	10^{-6}
Glendale, Nev	1200, Feb. 18	2330, Feb. 18	10^{-6}
Indian Springs, Nev	1200, Feb. 18	2025, Feb. 18	6.6×10^{-5}
Las Vegas, Nev	1200, Feb. 18	2400, Feb. 18	10^{-6}
Lincoln Mine, Nev	1200, Feb. 18	1600, Feb. 18	10^{-6}
Mercury, Nev	1204, Feb. 18	2125, Feb. 18	1.2×10^{-6}
Mesquite, Nev	1300, Feb. 18	1700, Feb. 18	10^{-6}
Nellis, Nev	1200, Feb. 18	2400, Feb. 18	3.3×10^{-6}
Pioche, Nev	1215, Feb. 18	2315, Feb. 18	10^{-6}
St. George, Utah	1125, Feb. 18	1135, Feb. 19	1.6×10^{-6}
Tonopah, Nev	0800, Feb. 18	2000, Feb. 18	10^{-6}

SHOT: MOTH

Location	From—	To—	Activity uc/m³
Alamo, Nev	0620, Feb. 22	0850, Feb. 23	3.2×10^{-4}
Beaver, Utah	0545, Feb. 22	0845, Feb. 23	8.1×10^{-5}
Caliente, Nev	0540, Feb. 22	1745, Feb. 22	4.9×10^{-5}
Cedar City, Utah	1755, Feb. 22	0945, Feb. 23	4.4×10^{-2}
Currant, Nev	0620, Feb. 22	1004, Feb. 23	10^{-6}
Ely, Nev	0545, Feb. 22	0945, Feb. 23	1.9×10^{-6}
Glendale, Nev	0530, Feb. 22	0945, Feb. 23	1.2×10^{-3}
Indian Springs, Nev	0545, Feb. 22	0700, Feb. 23	1.0×10^{-5}
Las Vegas, Nev	0545, Feb. 22	1730, Feb. 22	7.9×10^{-6}
Lincoln Mine, Nev	0545, Feb. 22	2315, Feb. 22	8.8×10^{-6}
Mercury, Nev	0400, Feb. 22	1125, Feb. 22	10^{-6}
Mesquite, Nev	0600, Feb. 22	0945, Feb. 23	4.8×10^{-4}
Nellis, Nev	0545, Feb. 22	1700, Feb. 22	7.2×10^{-6}
Pioche, Nev	0600, Feb. 22	1000, Feb. 23	9.9×10^{-3}
St. George, Utah	0530, Feb. 22	0545, Feb. 23	2.8×10^{-4}
Tonopah, Nev	0630, Feb. 22	0750, Feb. 23	5.7×10^{-6}

SHOT: TESLA

Location	From—	To—	Activity uc/m³
Alamo, Nev	0530, Mar. 1	0520, Mar. 2	2.2×10^{-3}
Beaver, Utah	0530, Mar. 1	0530, Mar. 2	6.1×10^{-4}
Caliente, Nev	0530, Mar. 1	0930, Mar. 2	1.9×10^{-3}
Cedar City, Utah	0530, Mar. 1	0530, Mar. 2	6.7×10^{-4}
Ely, Nev	0525, Mar. 1	0930, Mar. 2	5.1×10^{-4}
Glendale, Nev	0515, Mar. 1	0515, Mar. 2	2.7×10^{-6}
Indian Springs, Nev	0530, Mar. 1	0530, Mar. 2	4.6×10^{-5}
Las Vegas, Nev	0520, Mar. 1	0730, Mar. 2	3.2×10^{-6}
Lincoln Mine, Nev	0530, Mar. 1	0930, Mar. 2	6.5×10^{-4}
Mercury, Nev	0400, Mar. 1	0405, Mar. 2	4.3×10^{-6}
Mesquite, Nev	0530, Mar. 1	0530, Mar. 2	7.8×10^{-5}
Nellis, Nev	0530, Mar. 1	0530, Mar. 2	1.3×10^{-5}
Pioche, Nev	0550, Mar. 1	0930, Mar. 2	1.8×10^{-3}
St. George, Utah	0540, Mar. 1	0930, Mar. 2	4.0×10^{-2}
Tonopah, Nev	0500, Mar. 1	0630, Mar. 2	3.4×10^{-6}

TABLE 5.—*Airborne radioactivity concentrations*—Continued

SHOT: TURK

Location	Sampling period		Activity uc/m³
	From—	To—	
Alamo, Nev	0530, Mar. 7	1320, Mar. 8	5.6×10^{-6}
Beaver, Utah	0520, Mar. 7	1620, Mar. 8	5.9×10^{-6}
Currant, Nev	0607, Mar. 7	1050, Mar. 8	1.7×10^{-3}
Ely, Nev	0525, Mar. 7	0900, Mar. 8	1.4×10^{-3}
Eureka, Nev	0645, Mar. 7	2000, Mar. 7	10^{-6}
Indian Springs, Nev	0545, Mar. 7	0700, Mar. 8	7.6×10^{-6}
Las Vegas, Nev	0520, Mar. 7	1000, Mar. 8	1.6×10^{-6}
Lincoln Mine, Nev	0520, Mar. 7	0920, Mar. 8	3.3×10^{-6}
Mercury, Nev	0400, Mar. 7	1020, Mar. 8	1.7×10^{-5}
Nellis, Nev	0520, Mar. 7	0920, Mar. 8	6.7×10^{-6}
Pioche, Nev	0545, Mar. 7	1000, Mar. 8	9.8×10^{-4}
St. George, Utah	0545, Mar. 7	1145, Mar. 8	1.5×10^{-4}
Beatty, Nev	0800, Mar. 7	0820, Mar. 8	3.7×10^{-3}
Barstow, Calif	0510, Mar. 7	1530, Mar. 7	10^{-6}
Lund, Nev	0545, Mar. 7	0950, Mar. 8	1.4×10^{-3}

SHOT: HORNET

Location	From—	To—	Activity uc/m³
Alamo, Nev	0600, Mar. 12	0910, Mar. 13	3.0×10^{-3}
Beaver, Utah	0520, Mar. 12	0920, Mar. 13	2.5×10^{-3}
Caliente, Nev	0525, Mar. 12	0920, Mar. 13	8.8×10^{-4}
Cedar City, Utah	0520, Mar. 12	1220, Mar. 13	2.7×10^{-6}
Ely, Nev	0520, Mar. 12	0920, Mar. 13	7.8×10^{-5}
Glendale, Nev	0520, Mar. 12	0920, Mar. 13	1.2×10^{-2}
Indian Springs, Nev	0530, Mar. 12	1007, Mar. 13	4.8×10^{-6}
Las Vegas, Nev	0520, Mar. 12	0920, Mar. 13	2.0×10^{-5}
Lincoln Mine, Nev	0520, Mar. 12	0920, Mar. 13	3.3×10^{-5}
Mercury, Nev	0520, Mar. 12	0800, Mar. 13	9.9×10^{-6}
Mesquite, Nev	0520, Mar. 12	0900, Mar. 13	1.4×10^{-3}
Nellis, Nev	0520, Mar. 12	0920, Mar. 13	1.7×10^{-6}
Pioche, Nev	0540, Mar. 12	0925, Mar. 13	7.5×10^{-3}
St. George, Utah	0545, Mar. 12	0930, Mar. 13	1.2×10^{-3}
Tonopah, Nev	0545, Mar. 12	0720, Mar. 13	1.2×10^{-5}
Lund, Nev	0520, Mar. 12	1620, Mar. 12	2.0×10^{-4}

SHOT: BEE

Location	From—	To—	Activity uc/m³
Alamo, Nev	0445, Mar. 22	0900, Mar. 23	5.6×10^{-4}
Beaver, Utah	0505, Mar. 22	0905, Mar. 23	1.7×10^{-4}
Caliente, Nev	0445, Mar. 22	0920, Mar. 23	4.5×10^{-4}
Cedar City, Utah	0505, Mar. 22	0905, Mar. 23	9.6×10^{-5}
Currant, Nev	0515, Mar. 22	1015, Mar. 23	2.0×10^{-6}
Ely, Nev	0505, Mar. 22	0900, Mar. 23	1.4×10^{-5}
Glendale, Nev	0515, Mar. 22	1000, Mar. 23	6.5×10^{-4}
Indian Springs, Nev	0515, Mar. 22	0700, Mar. 23	2.7×10^{-5}
Las Vegas, Nev	0505, Mar. 22	1440, Mar. 23	7.7×10^{-4}
Lincoln Mine, Nev	0505, Mar. 22	0905, Mar. 23	2.2×10^{-5}
Lund, Nev	0505 Mar. 22	1015 Mar. 23	4.2×10^{-6}
Mercury, Nev	0455 Mar. 22	0725, Mar. 23	3.4×10^{-6}
Mesquite, Nev	0505, Mar. 22	0820, Mar. 23	2.0×10^{-4}
Nellis, Nev	0505, Mar. 22	1705, Mar. 22	3.5×10^{-3}
Pioche, Nev	0520, Mar. 22	0905, Mar. 23	5.2×10^{-5}
St. George, Utah	0545, Mar. 22	1010, Mar. 23	4.1×10^{-4}
Tonopah, Nev	0855, Mar. 22	2200, Mar. 22	4.2×10^{-6}

TABLE 5.—*Airborne radioactivity concentrations*—Continued

SHOT: ESS

Location	Sampling period From—	Sampling period To—	Activity uc/m³
Alamo, Nev	1230, Mar. 23	0600, Mar. 25	1.3×10^{-6}
Beatty, Nev	0615, Mar. 23	1200, Mar. 28	5.8×10^{-6}
Beaver, Utah	1230, Mar. 23	2100, Mar. 24	1.2×10^{-6}
Caliente, Nev	1230, Mar. 23	0610, Mar. 25	4.3×10^{-6}
Cedar City, Utah	1000, Mar. 23	0800, Mar. 25	5.8×10^{-6}
Currant, Nev	1230, Mar. 23	0630, Mar. 25	1.9×10^{-6}
Ely, Nev	1000, Mar. 23	0545, Mar. 25	3.6×10^{-6}
Eureka, Nev	1230, Mar. 23	0610, Mar. 25	2.0×10^{-6}
Glendale, Nev	1300, Mar. 23	0630, Mar. 25	2.0×10^{-8}
Indian Springs, Nev	1330, Mar. 23	1430, Mar. 24	6.5×10^{-4}
Las Vegas, Nev	1440, Mar. 23	0600, Mar. 25	2.4×10^{-6}
Lincoln Mine, Nev	1230, Mar. 23	0600, Mar. 25	2.7×10^{-6}
Lund, Nev	1230, Mar. 23	0730, Mar. 25	2.0×10^{-6}
Mercury, Nev	1215, Mar. 23	0930, Mar. 25	4.0×10^{-4}
Mesquite, Nev	1400, Mar. 23	0700, Mar. 25	4.5×10^{-8}
Nellis, Nev	1800, Mar. 23	0600, Mar. 26	2.4×10^{-8}
Pioche, Nev	1255, Mar. 23	0715, Mar. 25	1.7×10^{-6}
St. George, Utah	1010, Mar. 23	0800, Mar. 25	2.2×10^{-8}
Tonopah, Nev	2015, Mar. 23	2015, Mar. 25	3.4×10^{-6}

SHOT: APPLE

Location	Sampling period From—	Sampling period To—	Activity uc/m³
Alamo, Nev	0455, Mar. 29	0855, Mar. 30	4.0×10^{-3}
Beaver, Utah	1600, Mar. 29	0900, Mar. 30	4.2×10^{-4}
Caliente, Nev	0430, Mar. 29	0300, Mar. 30	2.7×10^{-3}
Cedar City, Utah	0455, Mar. 29	0930, Mar. 30	9.9×10^{-8}
Currant, Nev	0455, Mar. 29	2300, Mar. 29	4.1×10^{-8}
Ely, Nev	0455, Mar. 29	0645, Mar. 30	4.1×10^{-8}
Eureka, Nev	0430, Mar. 29	0855, Mar. 30	5.9×10^{-6}
Glendale, Nev	0500, Mar. 29	0900, Mar. 30	2.9×10^{-4}
Indian Springs, Nev	450, Mar. 29	0700, Mar. 30	1.3×10^{-4}
Las Vegas, Nev	0455, Mar. 29	1500, Mar. 30	8.6×10^{-4}
Lincoln Mine, Nev	0455, Mar. 29	0855, Mar. 30	3.7×10^{-8}
Lund, Nev	0455, Mar. 29	1030, Mar. 30	1.6×10^{-3}
Mercury, Nev	0455, Mar. 29	0920, Mar. 30	1.1×10^{-6}
Mesquite, Nev	0555, Mar. 29	0855, Mar. 30	4.3×10^{-5}
Nellis, Nev	0500, Mar. 29	1300, Mar. 30	5.4×10^{-4}
Pioche, Nev	0515, Mar. 29	0650, Mar. 30	7.3×10^{-3}
St. George, Utah	0525, Mar. 29	0900, Mar. 30	2.1×10^{-6}
Tonopah, Nev	0500, Mar. 29	0830, Mar. 30	8.8×10^{-5}

SHOT: HA

Location	Sampling period From—	Sampling period To—	Activity uc/m³
Alamo, Nev	0950, Apr. 6	1015, Apr. 7	1.4×10^{-4}
Beaver, Utah	1000, Apr. 6	1200, Apr. 7	1.7×10^{-6}
Caliente, Nev	0955, Apr. 6	1355, Apr. 7	5.4×10^{-3}
Cedar City, Utah	1000, Apr. 6	1200, Apr. 7	7.7×10^{-6}
Ely, Nev	1000, Apr. 6	1340, Apr. 7	5.4×10^{-4}
Eureka, Nev	0945, Apr. 6	1150, Apr. 7	4.4×10^{-6}
Glendale, Nev	1000, Apr. 6	1700, Apr. 7	1.6×10^{-6}
Indian Springs, Nev	1000, Apr. 6	0700, Apr. 7	1.4×10^{-5}
Las Vegas, Nev	1200, Apr. 6	0900, Apr. 7	1.3×10^{-5}
Lincoln Mine, Nev	1000, Apr. 6	0600, Apr. 7	1.4×10^{-5}
Lund, Nev	1000, Apr. 6	1240, Apr. 7	8.0×10^{-6}
Mercury, Nev	0900, Apr. 6	1400, Apr. 7	3.9×10^{-6}
Mesquite, Nev	1000, Apr. 6	1200, Apr. 7	1.5×10^{-5}
Nellis, Nev	1000, Apr. 6	1800, Apr. 6	10^{-6}
Pioche, Nev	1024, Apr. 6	1415, Apr. 7	1.1×10^{-5}
St. George, Utah	1040, Apr. 6	1100, Apr. 7	1.1×10^{-6}
Tonopah, Nev	1200, Apr. 6	0900, Apr. 7	9.2×10^{-6}

TABLE 5.—*Airborne radioactivity concentrations*—Continued

SHOT: POST

Location	Sampling period From—	Sampling period To—	Activity uc/m³
Alamo, Nev	0430, Apr. 9	0830, Apr. 10	4.8×10^{-2}
Beaver, Utah	0430, Apr. 9	0800, Apr. 10	7.7×10^{-5}
Caliente, Nev	0430, Apr. 9	0845, Apr. 10	1.8×10^{-3}
Cedar City, Utah	0430, Apr. 9	0830, Apr. 10	3.9×10^{-3}
Ely, Nev	0430, Apr. 9	0800, Apr. 10	1.3×10^{-2}
Eureka, Nev	0420, Apr. 9	0840, Apr. 10	6.5×10^{-6}
Glendale, Nev	0430, Apr, 9	1430, Apr. 10	3.0×10^{-3}
Indian Springs, Nev	0430, Apr. 9	2200, Apr. 9	9.0×10^{-1}
Las Vegas, Nev	1500, Apr. 9	0800, Apr. 10	5.8×10^{-2}
Lincoln Mine, Nev	0530, Apr. 9	0830, Apr. 10	2.7×10^{-2}
Lund, Nev	0450, Apr. 9	0830, Apr. 10	1.8×10^{-2}
Mercury, Nev	0425, Apr. 9	1150, Apr. 10	2.5×10^{-1}
Mesquite, Nev	0430, Apr. 9	0830, Apr. 10	5.5×10^{-5}
Nellis, Nev	0430, Apr. 9	0830, Apr. 10	1.0×10^{-2}
Pioche, Nev	0500, Apr. 9	0837, Arp. 10	1.1×10^{-2}
St. George, Utah	0520, Apr. 9	0830, Apr. 10	8.5×10^{-6}
Tonopah, Nev	0800, Apr. 9	0900, Apr. 10	3.6×10^{-6}

SHOT: MET

Location	Sampling period From—	Sampling period To—	Activity uc/m³
Alamo, Nev	1105, Apr. 15	1115, Apr. 16	6.7×10^{-4}
Beaver, Utah	1115, Apr. 15	1205, Apr. 16	6.1×10^{-2}
Caliente, Nev	1200, Apr. 15	1103, Apr. 16	5.0×10^{-2}
Cedar City, Utah	1115, Apr. 15	1145, Apr. 16	7.6×10^{-4}
Ely, Nev	1115, Apr. 15	2315, Apr. 15	4.8×10^{-6}
Eureka, Nev	1115, Apr. 15	0730, Apr. 16	4.0×10^{-6}
Glendale, Nev	1120, Apr. 15	1215, Apr. 16	1.6×10^{-5}
Indian Springs, Nev	1615, Apr. 15	1315, Apr. 16	4.2×10^{-4}
Las Vegas, Nev	1130, Apr. 15	1100, Apr. 16	1.6×10^{-5}
Lincoln Mine, Nev	1115, Apr. 15	1115, Apr. 16	5.8×10^{-6}
Lund, Nev	1115, Apr. 15	1115, Apr. 16	8.3×10^{-6}
Mercury, Nev	1110, Apr. 15	1115, Apr. 16	2.6×10^{-5}
Mesquite, Nev	1115, Apr. 15	1515, Apr. 16	5.2×10^{-6}
Nellis, Nev	1115, Apr. 15	1115, Apr. 16	1.3×10^{-5}
Pioche, Nev	1135, Apr. 15	1115, Apr. 16	2.7×10^{-3}
St. George, Utah	1145, Apr. 15	1115, Apr. 16	4.2×10^{-6}
Tonopah, Nev	1320, Apr. 15	1000, Apr. 16	9.0×10^{-6}

SHOT: APPLE TWO

Location	Sampling period From—	Sampling period To—	Activity uc/m³
Alamo, Nev	0510, May 5	0710, May 6	1.2×10^{-5}
Caliente, Nev	0510, May 5	0930, May 6	8.6×10^{-6}
Cedar City, Utah	0510, May 5	0910, May 6	1.4×10^{-5}
Ely, Nev	0510, May 5	0910, May 6	5.9×10^{-2}
Eureka, Nev	0510, May 5	0900, May 6	2.4×10^{-3}
Glendale, Nev	0510, May 5	0910, May 6	4.1×10^{-6}
Indian Springs, Nev	0510, May 5	0700, May 6	5.7×10^{-4}
Lincoln Mine, Nev	0510, May 5	0910, May 6	1.8×10^{-5}
Lund, Nev	0510, May 5	1210, May 6	1.1×10^{-2}
Mercury, Nev	0540, May 5	1220, May 6	1.0×10^{-5}
Pioche, Nev	0535, May 5	1015, May 6	2.5×10^{-6}
St. George, Utah	1800, May 5	1045, May 6	8.7×10^{-6}
Tonopah, Nev	0540, May 5	0750, May 6	3.6×10^{-4}

SHOT: ZUCCHINI

Location	Sampling period From—	Sampling period To—	Activity uc/m³
Alamo, Nev	0500, May 15	0900, May 16	1.5×10^{-5}
Caliente, Nev	0530, May 15	0945, May 16	6.7×10^{-6}
Cedar City, Utah	0500, May 15	0910, May 16	7.8×10^{-3}
Indian Springs, Nev	0500, May 15	0700, May 16	6.6×10^{-4}
Las Vegas, Nev	0500, May 15	1000, May 16	1.1×10^{-3}
Lincoln Mine, Nev	0500, May 15	0715, May 15	8.5×10^{-6}
Mercury, Nev	0510, May 15	0900, May 16	1.4×10^{-5}
Mesquite, Nev	0555, May 15	0830, May 16	4.2×10^{-3}
Nellis, Nev	0500, May 15	0800, May 16	1.5×10^{-3}
Pioche, Nev	0510, May 15	1105, May 16	1.3×10^{-6}
St. George, Utah	0645, May 15	1030, May 16	4.3×10^{-3}

APPENDIX

PERSONNEL

A total of 66 Public Health Service men were assigned to this operation for various periods of time. PHS personnel were composed of two categories, regular PHS personnel, and Reserve personnel who were called to active duty for the purpose of assignment to this operation.

The regular PHS personnel and their normal affiliation are as follows:

Anderson, E. C., Assistant Chief, radiological health program
Bevis, Herbert A., radiological health program, Washington, D. C.
Brewer, Lial W., occupational health field station, Salt Lake City
Butrico, Frank A., DHEW, region 2, New York, N. Y.
Carter, Melvin W., PHS, offsite program, Nevada test site
Coleman, Richard D., radiological health program, Salt Lake City
Ernsberger, Edward L., PHS, Rockville, Md.
Fooks, Jack H., DHEW, region 8, Denver, Colo.
Hagee, G. Richard, Robert A. Taft Sanitary Engineering Center, Cincinnati
Henderson, Paul C., DHEW, region 3, Washington, D. C.
Holaday, Duncan A., occupational health field station, Salt Lake City
Ingraham, Samuel C., M. D., National Cancer Institute, NIH, Bethesda, Md.
Kusnetz, Howard L., occupational health field station, Salt Lake City
Longaker, Ralph K.,DHEW, DESE, Washington, D. C.
Macomber, Ronald G., DHEW, region 2, New York, N. Y.
Mills William A., radiological health program, Washington, D. C.
Minken, Joseph L., CDC, Mount Vernon, N. Y.
Placak, Oliver R., officer in charge, PHS offsite program, Nevada test site
Powell, Clinton C., M. D., National Cancer Institute, NIH, Bethesda, Md.
Rechen, Henry J. L., radiological health program, Washington, D. C.
Schreeder, William B. DHEW, region 9, San Francisco, Calif.
Seal, Morgan S., radiological health program, Washington, D. C.
Soneda, Shinji, Robert A. Taft Sanitary Engineering Center, Cincinnati
Stangler, Marlow, Robert A. Taft Sanitary Engineering Center, Cincinnati
Terrill, James G., Jr., Chief radiological health program, Washington, D. C.

The Reserve personnel who were called to active duty, with the designation of their normal affiliation, are listed below:

Alabama:
  Habel, John C., Jefferson County Health Department, Birmingham
  Thomas, Fred W., USTVA, Wilson Dam
Arkansas: Wilson, Edward F., Arkansas State Board of Health, Little Rock
California:
  Ausseresses, W. M., Southern Pacific Railroad, San Francisco
  Brewer, Robert, State department of health, San Bernardino
  Stone, Ralph N., consulting engineer (civil), Los Angeles
  Vaden, John D., Los Angeles County Health Department, Los Angeles
Colorado:
  Newman, Edison E., State Health Department, Denver
  VanNattan, W. R., State Health Department, Denver
Connecticut:
  Bertran, Albert E., State Health Department, Hartford
  Herlihy, James F., City Health Department, Hartford
  Holt, John A., City Health Department, Hartford
Florida: Greenley, John W., Dade County Health Department, Miami
Georgia: Fetz, Richard H., State Health Department, Atlanta
Hawaii: Woo, Francis H., Department of Health, Hilo
Idaho:
  Cotton, Charles E., State Health Department, Boise
  Despain, Carroll E., City-County Health Department, Boise
Illinois:
  Bullock, Harrison E., State Health Department, Carbondale
  Kaufmann, Oliver W., University of Illinois, Urbana
Indiana: Wraight, Frank D., State Health Department, Indianapolis
Kansas: Rucker, Vernon L., Santa Fe Railroad, Topeka
New Jersey:
  Baker, Walter C., New Jersey Neuropsychiatric Institute, Princeton
  Berry, Clyde M., Esso Standard Oil Co., Linden

New Mexico: Jensen, Carl R., State Health Department, Santa Fe
New York: Marchese, Anthony S., State Highway Department, Poughkeepsie
North Carolina:
- Ameen, Joseph S., State Health Department, Raleigh
- Long, William N., Gaston County Health Department, Gastonia
- Seagle, Edgar F., State Health Department, Raleigh
- Sharpe, Thomas J., District Health Department, Hickory
- Williams, Giles M., State Department of Agriculture, Raleigh

North Dakota: Olson, Otmar O., District Health Department, Williston
Oklahoma:
- Harris, Carroll F., Oklahoma A. and M., Stillwater
- Pummill, Lloyd F., State Health Department, Oklahoma City

Oregon: Bower, William F., State Health Department, Portland
Tennessee:
- Brockett, Thomas W., Oak Ridge National Laboratory
- Davidson, Charles M., USTVA, Chattanooga
- Harless, Bennett L., Oak Ridge National Laboratory

Texas: Ledbetter, Joe O., State Highway Department, Weatherford
Vermont: Gilbert, Wilfred C., State Highway Department, Montpelier
Washington:
- Gregg, George O., State Department of Health, Chehalis
- Ruppert, Edwin L., State Department of Health, Seattle

Abbreviations and Nomenclature

CP—Control point, located in Yucca Pass between Frenchman and Yucca Flats.
GZ—Ground zero, the point above which a device is detonated.
hr—Hour.
mr—milliroentgen.
r—roentgen.
D-day—The day of a particular detonation.
H—The hour of exact time of a particular detonation.
U. S.—Refers to a specific highway.
uc—microcuries.
m^3—Cubic meters.
N—North.
E—East.
S—South.
W—West.
Mi.—Miles.
NTS—Nevada test site.
Times—Pacific standard time through 2 a. m. April 24, 1955; thereafter, Pacific daylight standard time.
EBD—Effective biological dose as defined in the DBM criteria.
AEC—Atomic Energy Commission.
PHS—Public Health Service.
CAA—Civil Aeronautics Administration.
ml—Milliliter.
Co^{60}—An isotope of cobalt having the mass number 60.
MSA—Mine safety appliance.
Sr^{90}—An isotope of strontium having the mass number 90.
Y^{90}—An isotope of yttrium having the mass number 90.

The following system of terminology is used when presenting data concerning airway closure patterns, cloud tracking, and low-level terrain surveys:

1. All elevations are given in terms of mean sea level unless otherwise stated.
2. All bearings are given in terms of true bearings unless otherwise stated.
3. In giving locations, the Georef coördinate system has been used for simplicity. In this system, a group of 4 letters and 4 numbers locates any given point. In the area of concern to this report, the first two letters, EJ, are common to all locations and are omitted for the sake of brevity. Of the second group of two letters, the first letter denotes longitude and the second latitude, according to the following table:

Georef letter	Longitude	Georef letter	Latitude
A	120°00′W.		
B	119°00′		
C	118°00′	C	32°00′N.
D	117°00′	D	33°00′
E	116°00′	E	34°00′
F	115°00′	F	35°00′
G	114°00′	G	36°00′
H	113°00′	H	37°00′
J	112°00′	J	38°00′
K	111°00′	K	39°00′
L	110°00′	L	40°00′
M	109°00′	M	41°00′
N	108°00′	N	42°00′
P	107°00′	P	43°00′
Q	106°00′		

Similarly, the 2 groups of 2 members each denote, respectively, minutes of longitude and minutes of latitude within the 1° quadrangle specified by the letter group. To provide an example, the coordinates of Las Vegas are EG 5120.

The identification of the 15° quadrangle (EJ) is omitted for the reason previously stated.

EG identifies the 1° quadrangle.

51 identifies the Georef minute of longitude.

20 identifies the Georef minute of latitude.

Radiation Exposures Received on Populated Atolls as a Result of Operation Redwing

During Operation Redwing 4 gamma intensity readings daily were taken at populated off-site atolls utilizing a radiac meter AN/PDR-27F, calibrated against a standard consisting of 7 micrograms of radium. Following each test, hourly readings were taken for an interval of time dependent upon fallout forecasts, wind conditions at and following test time, cloud tracking, and readings obtained at the atolls. The attached tables and charts show the weighted daily averages of these readings for the atolls at which stations were maintained.

An estimated cumulative exposure of the populations of these atolls resulting from Operation Redwing has been computed based on these meter readings. Net readings (above preoperation background) have been utilized. Where a residual radiation remained at the time the stations were inactivated, the 70-year exposure due to this residual was computed based on the equation $I_2T_1^{-k}=I_1T_2^{-k}$. Based on available decay data, k=1.2 was utilized. It will be noted that the last day's reading at Ujelang was 1.5 mr/hr. This was due to the test of July 21. The complete record shows that fallout had stopped and radiation intensities were decreasing at the time the station was inactivated. A reduction factor to determine effective biological dose was not utilized as conditions under which the natives live are not believed to warrant the commonly accepted reduction factor. Computations involved are attached. On this basis, 70-year external gamma doses resulting from Operation Redwing are as follows:

Ujelang Atoll: 560 mr.
Utirik Atoll: 53 mr.
Wotho Atoll: 616 mr.
Rongerick Atoll: 853 mr.

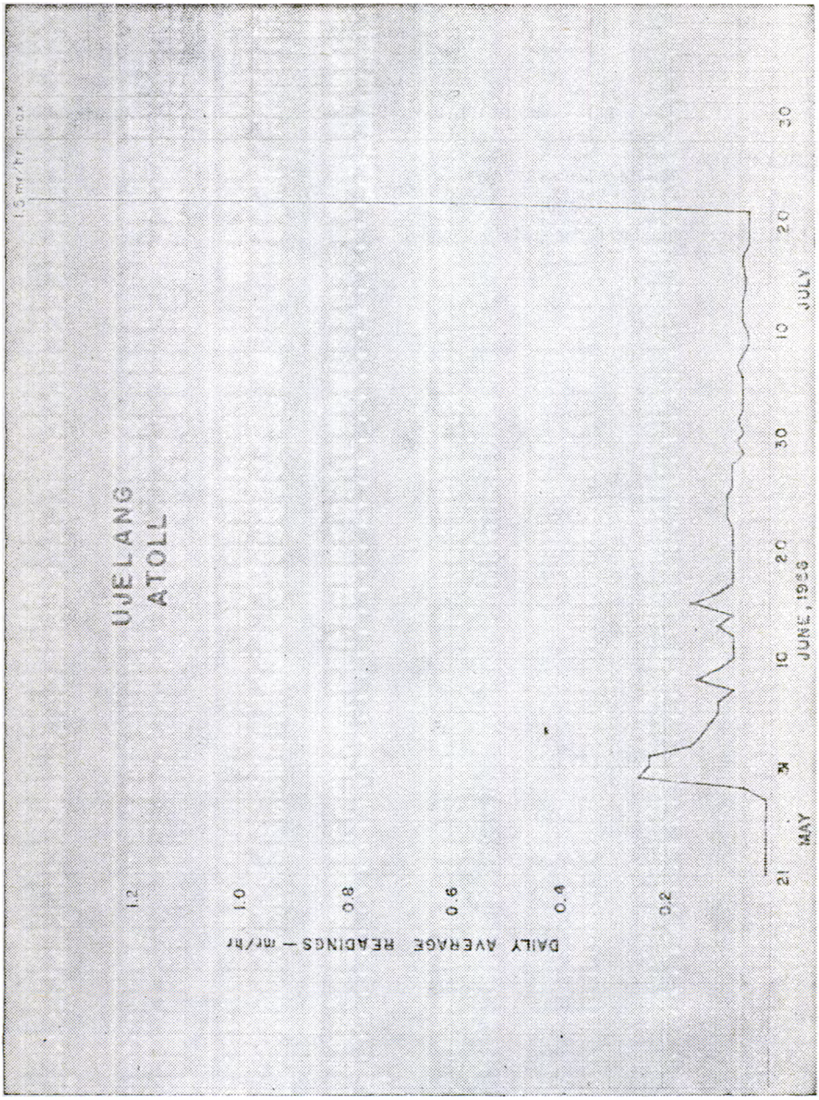
Also attached is a plot showing AN/PDR-27F readings at JTF-7 Headquarters, Parry Island, during the period July 21 to July 23, 1956. On the basis of these figures, effective external gamma doses to various periods of time have been computed as follows:

H+5 days: 3.45 R.
H+15 days: 5.7 R.
H+1 year: 7.95 R.
Infinity dose: 12.45 R.

Computations are attached.

Daily average readings

Date	Ujelang	Utirik	Wotho	Rongerik	Date	Ujelang	Utirik	Wotho	Rongerik
Apr. 26	0.01	0.02	0.01	0.1	June 26	.07	.02	.3	.5
May 29	.05	.03	.2	.15	June 27	.07	.02	.25	.1
May 30	.25	.03	4.5	.30	June 28	.07	.06	.25	.1
May 31	.26	.26	.26	.26	June 29	.05	.15	.23	.1
June 1	.23	.03	1.0	3.0	June 30	.06	.15	.23	.1
June 2	.15	.03	.85	3.0	July 1	.05	.15	.21	.1
June 3	.13	.02	.75	3.0	July 2	.06	.11	.21	.1
June 4	--------	.02	--------	2.0	July 3	.05	.11	.19	.1
June 5	.1	.02	.5	2.0	July 4	.05	.10	.18	.1
June 6	.1	.02	.4	2.0	July 5	.05	.10	.18	.1
June 7	.07	.02	.3	2.0	July 6	.05	.08	.18	.1
June 9	--------	--------	--------	1.5	July 7	.05	.08	.15	.1
June 10	--------	--------	--------	1.0	July 8	.05	.07	.14	.1
June 11	--------	--------	--------	1.0	July 9	.04	.07	.14	.1
June 12	.07	.02	.2	1.0	July 10	.04	.05	.11	.1
June 13	.1	.02	.18	1.0	July 11	.05	.06	.11	.1
June 14	.07	.02	.48	2.0	July 12	.05	.05	.12	.1
June 15	.15	.02	.90	1.5	July 13	.05	.05	.10	.1
June 16	.1	.04	.8	1.0	July 14	.045	.04	.10	.1
June 17	.07	.05	.7	1.0	July 15	.045	.045	.10	.1
June 18	.07	.04	.6	1.0	July 16	.05	.05	.09	.1
June 19	.07	.04	.7	1.0	July 17	.05	.04	.08	.1
June 20	.07	.03	.6	1.0	July 18	.04	.05	.08	.1
June 21	.07	.02	.5	1.0	July 19	.04	.04	.08	.1
June 22	.07	.02	.5	1.0	July 20	.04	.04	.08	.1
June 23	.08	.03	.4	1.0	July 21	.04	.04	.08	.1
June 24	.08	.02	.4	.75	July 22	.6	.04	.08	.1
June 25	.08	.03	.3	.5	July 23	1.5	--------	--------	.1


UJELANG
ATOLL
DAILY AVERAGE READINGS — mr/hr
1.2
1.0
0.8
0.6
0.4
0.2
1.5 mr/hr max
21
MAY
31
10
20
JUNE, 1958
30
10
JULY
20
30

Cumulative exposure computations—Ujelang

Rate mr./hr.	Days	Hours	Dose mr.	Rate mr./hr.	Days	Hours	Dose mr.
0.04	13	312	12.48	0.07	3	72	5.04
0.24	1	24	5.76	0.13	1	24	3.12
0.23	1	24	5.52	0.10	3	72	7.20
0.22	1	24	5.28	0.05	4	96	4.80
0.14	2	48	6.72	0.03	6	144	4.32
0.12	1	24	2.88	0.035	2	48	1.68
0.11	1	24	2.64	1.5	1	24	36.00
0.09	4	96	8.64				
0.06	12	288	17.28	Total			[2] 129.36

TEWA H=210600 M
240000 M=H+66 hours=2.75 days
70 year dose after this time=approx. 430 mr. assuming $I_2T_1^{-1.2}=I_1T_2^{-1.2}$.[2]
Total 70 year dose=130+430=560 mr.

[1] Rate above preoperation background of 0.01 mr./hr.
[2] Through July 23.

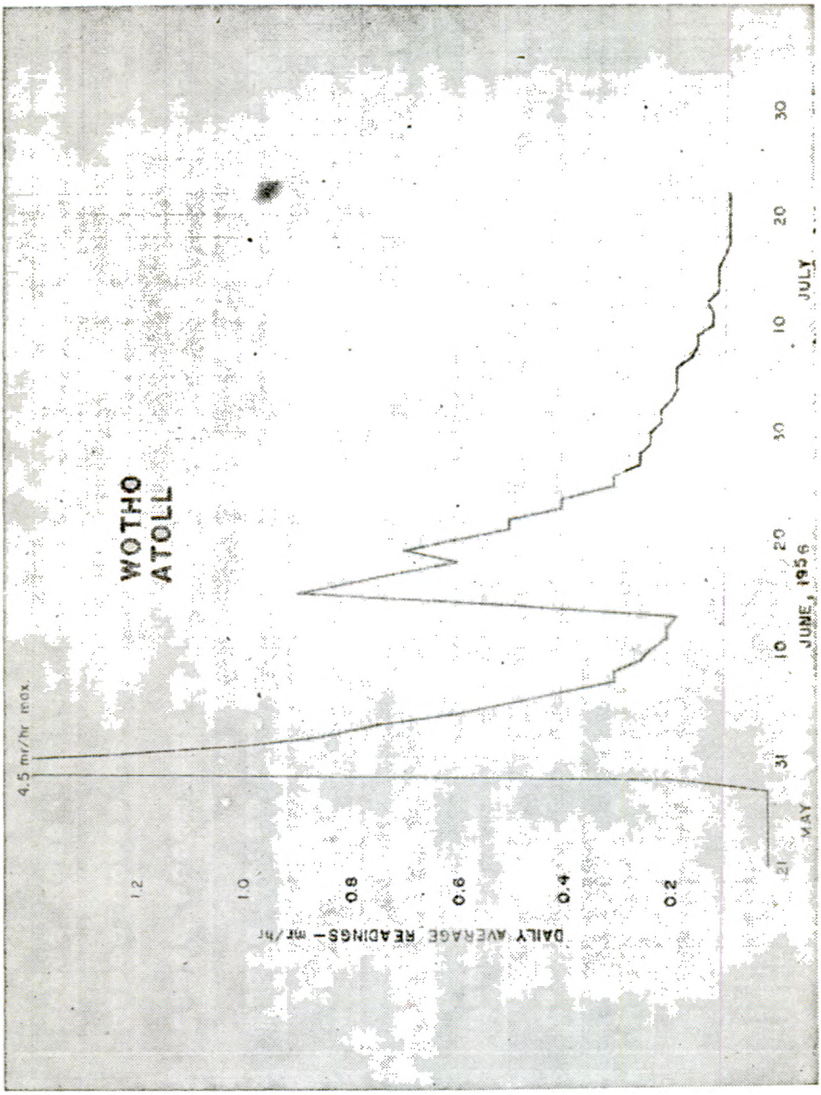

UTIRIK
ATOLL
DAILY AVERAGE READINGS – mr/hr
12
10
0.8
0.6
0.4
0.2
21 MAY
31
10 JUNE, 1956
20
30
10 JULY
20
30

Cumulative exposure computation—Utirik

Rate [1] mr./hr.	Days	Hours	Dose mr.	Rate [1] mr./hr.	Days	Hours	Dose mr.
0.01	8	192	1.92	0.09	2	48	4.32
0.02	9	216	4.32	0.08	2	48	3.84
0.025	1	24	0.60	0.06	2	48	3.28
0.03	6	144	4.32	0.05	2	48	2.40
0.04	2	48	1.92				
0.13	3	72	9.36	Total			36.28

[1] Rate above preoperation background of 00.2 mr./hr.

Assume D=June 27
70-year dose from July 23=approx. 17 mr.
Total 70-year dose=36+17=53 mr.

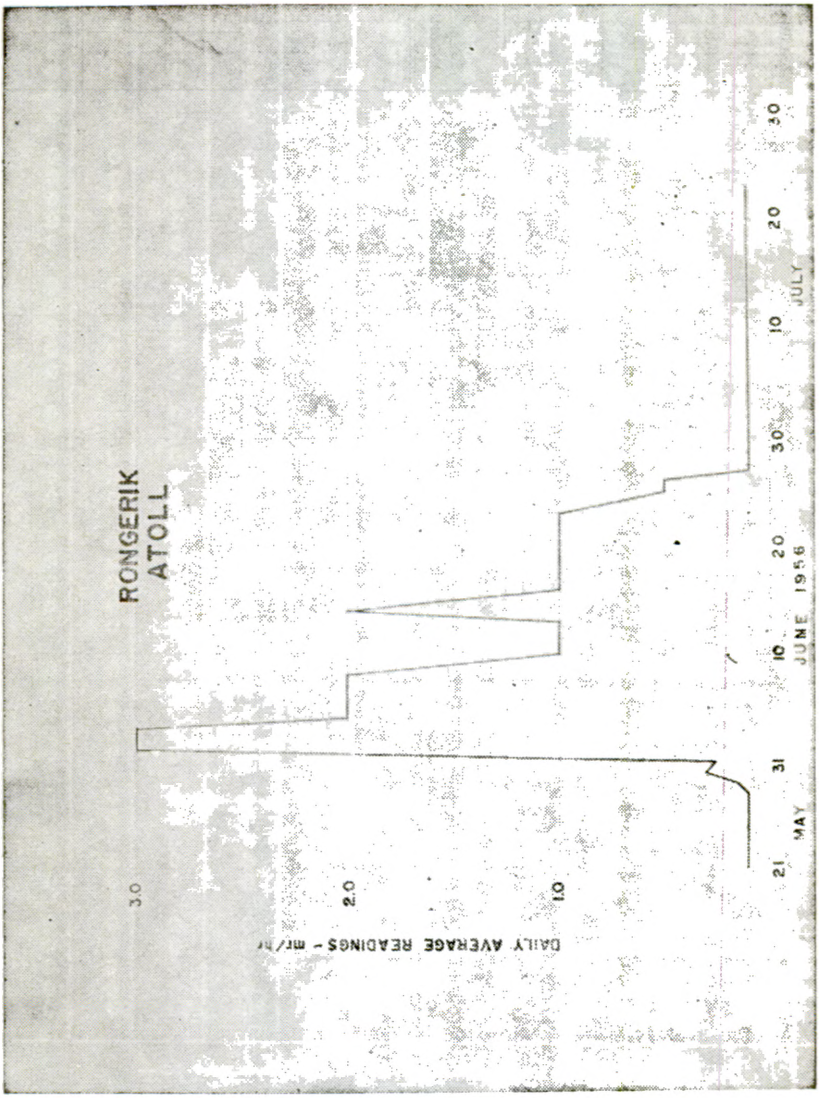

WOTHO
ATOLL
4.5 mr/hr max.
1.2
1.0
0.8
0.6
0.4
0.2
DAILY AVERAGE READINGS— mr/hr
21
MAY
31
10
JUNE, 1956
20
30
10
JULY
20
30

Cumulative exposure computation—Wotho

Rate [1] mr/hr.	Days	Hours	Dose mr.	Rate [1] mr/hr.	Days	Hours	Dose mr.
0.01	1	24	0.24	0.17	4	96	16.32
4.5	1	24	108.00	0.47	1	24	11.28
2.75	1	24	66.00	0.89	1	24	21.36
1.0	1	24	24.00	0.79	1	24	18.96
0.84	1	24	20.16	0.69	2	48	33.12
0.74	1	24	17.76	0.59	2	48	28.32
0.62	1	24	14.88	0.14	1	24	3.36
0.49	3	72	35.28	0.13	2	48	6.24
0.39	3	72	28.08	0.10	2	48	4.80
0.29	3	72	20.88	0.11	1	24	2.64
0.19	1	24	4.56	0.09	3	72	6.48
0.24	3	72	17.28	0.08	1	24	1.92
0.22	2	48	10.56	0.07	6	144	10.08
0.20	2	48	9.60				
0.18	1	24	4.32	Total			[2] 546.48

6/13=H
7/23=H+40 d
70 year dose=70 mr
Total 70 year dose=546+70=616 mr.

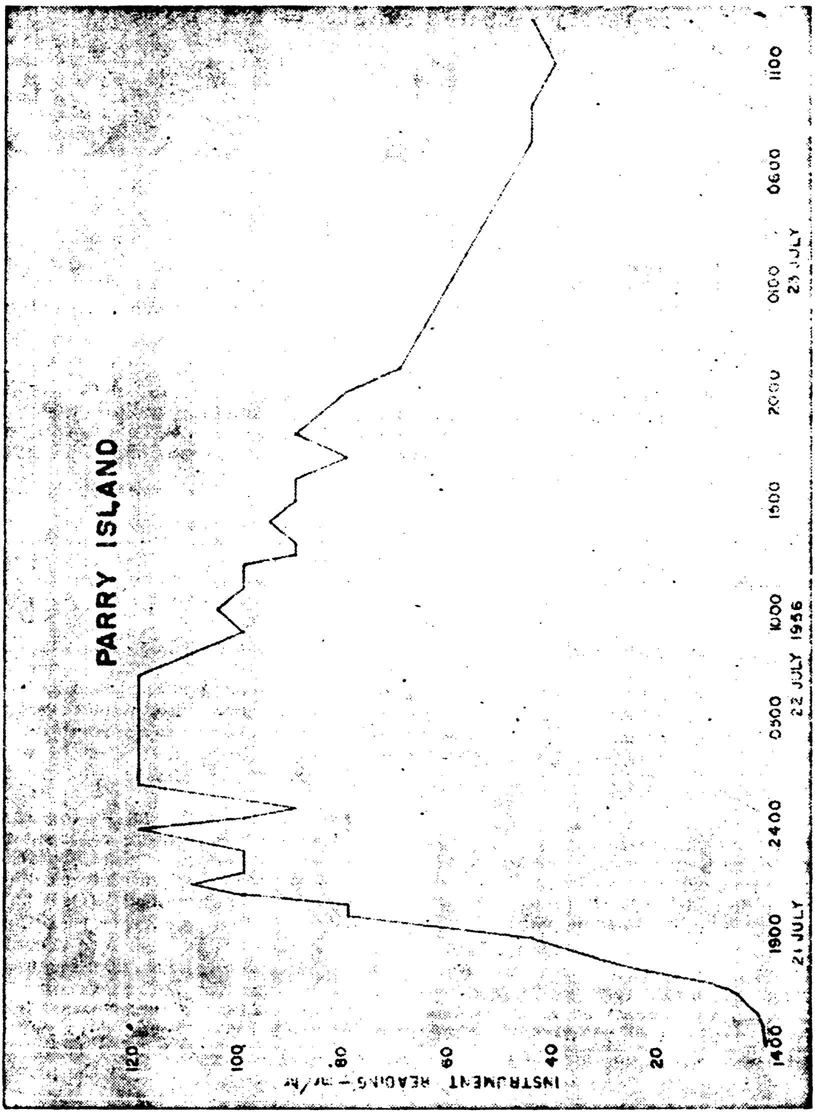
[1] Above preoperation level of 0.01 mr/hr.
[2] Through July 22, 1956.


RONGERIK
ATOLL
3.0
2.0
1.0
DAILY AVERAGE READINGS - mr/hr
21
MAY
31
10
JUNE 1956
20
30
10
JULY
20
30

Cumulative exposure computation—Rongerik

Rate [1] mr/hr	Days	Hours	Dose mr.	Rate [1] mr/hr.	Days	Hours	Dose mr.
0.05	1	24	1.20	0.9	12	288	295.20
0.2	1	24	4.80	0.65	1	24	15.60
0.16	1	24	3.84	0.4	2	48	19.20
2.9	3	72	208.80				
1.9	6	144	273.60	Total			853.44
1.4	2	48	67.20				

[1] Above preoperation level of 0.1 mr/hr.


PARRY ISLAND
INSTRUMENT READINGS
120
100
80
60
40
20
1400
1900
21 JULY
2400
0500
22 JULY 1956
1000
1500
2000
0100
23 JULY
0600
1100

DOSE CALCULATIONS FOR PARRY ISLAND

Accumulated dose to 220700 M was 0.6R
At that time decay started with the decay constant $K=-1.2$
Personnel leaving at H+5 days will receive:

$$16-12+0.6=4.6 \text{ R}$$

If this is reduced by 25 percent, the effective dose=3.45 R
Permanent personnel will receive:
For first 15 days:

$$16-9+0.6=7.6 \text{ R}$$

Effective 15 day dose=(0.75) (7.6) =5.7 R
For 15 days to 1 year:

$$9-4.5=4.5 \text{ R}$$

Effective dose=(0.5) (4.5) =2.25 R
Total effective dose to end of 1 year=7.95 R
Infinity dose for permanent personnel:

$$7.95+4.5=12.45 \text{ R}$$

REPORT ON EXPERIMENTAL FILM BADGE STUDY DURING OPERATION REDWING, OCTOBER 1, 1956

During Operation Redwing the Public Health Service group had planned, on an experimental basis, to utilize film badges on the off-site atolls of Utirik, Ujelang, and Wotho as a method of securing a figure for total radiation dosage on these islands. Arrangements were made for the procurement, transportation, and processing of film badges with task group 7.1 radsafe personnel and task group 7.4 nuclear research officer, Lt. W. J. Jameson.

The first group of film badges, enclosed in rigid, transparent plastic containers, were exposed for a period of approximately 50 days. A second group was sent out to the atolls and exposed for a period of approximately 15 days. When these films were developed and read, it was found that the results were much higher (up to 5 to 10 times) than would be expected on the basis of exposures computed from instrument readings taken at the atolls (AN/PDR27F geiger counters were used). On examination of the films, water marks could be clearly seen on most of them. It was theorized that heat or moisture, or both, was the cause of the high readings.

Capt. B. H. Purcell, task group 7.1, arranged to have some special film badges prepared. One lot was prepared by having the film packet dipped in Ceresine wax before sealing in the plastic case (referred to in this report as "film dipped badges"). The second lot was prepared by dipping the entire case in wax after the uncoated film packet was sealed in the pastic case (referred to in this report as "case dipped badges").

Preliminary work done on these badges by exposing them alternately to steam and then placing in a refrigerator indicated that they were more resistant to moisture than the regular badges. It was decided to place approximately 20 of each type on each off-site atoll, bringing in 3 sets from each atoll each week until the reliable life of each type of badge in the field could be determined. (Only half as many film dipped badges were available as the other two types.) Unfortunately, for the purposes of this test, the operation terminated before it could be completed. It is believed, however, that some tentative conclusions can be reached from the results obtained.

Films were collected after approximately 1 and 2 weeks' exposures. The balance of the films were collected after approximately 3 weeks' exposure. Readings taken on the films were compared with calculated doses based on instrument readings taken during the same periods. Tabulations of the results are attached.

It can be seen that the doses received during the first week are too low to give reliable results on the film badges used, while exposures received during the first 2 weeks are just at the borderline of sensitivity. All film badges appeared to be satisfactory after 1 week, but after 2 weeks the regular badges were showing signs of moisture penetration. After 3 weeks, almost all the regular badges, while none of the film-dipped or case-dipped badges, were watermarked.

Estimating the dose to which the films brought in after 3 weeks were exposed is complicated by the fact that Parry Island had a gamma radiation level of 20 to 30 mr/hr at the time the films reached there. Thus, it has been necessary to estimate exposure received at Eniwetok Atoll prior to development. How-

ever, a study of the results on these badges is believed to indicate a trend. Attached are curves showing the distribution of results for each type of film from each atoll and the results for each type of film from the three atolls combined.

Considering this latter curve, if one considers results within 50 percent of a "true" (in this case, calculated) dose to be satisfactory, it can be seen that 68 percent of the film-dipped badges and 60 percent of the case-dipped badges fall within this range, while only 19 percent of the regular badges meet this criterion. This is admittedly a limited sample, but the results are believed to warrant the following conclusions and recommendations:

1. Climatic conditions at the Pacific Proving Grounds have an adverse effect on ordinary film badges to the extent that they cannot be relied upon to give satisfactory results;

2. If possible, a study should be made to determine the reliable life of wax-coated film badges and possibly other types of dosimeters) under climatic conditions similar to those at Pacific Proving Grounds prior to the next test series. If this is not possible, such a study should be conducted at Pacific Proving Grounds during the next series.

3. For area monitoring of this type, a more sensitive film should be included in the film packet.

Experimental film badges—Ujelang Atoll

Station No.	Date out	Date in	Time exposed, in days	Indicated dose, milliroentgen			Calculated dose, milliroentgen
				Regular	C. D.	F. D.	
7	July 4, 1956	July 10, 1956	7	[1] 50	30		8
18	do	do	7	70	30	30	8
10	do	July 17, 1956	13	[1] 70	0	0	16
11	do	do	13	[1] 90	30		16
1	do	July 26, 1956	23	[1] 210	90	70	143
2	do	do	23	[1] 245	90		143
3	do	do	23	[1] 210	110	70	143
4	do	do	23	[1] 225	110	90	143
5	do	do	23	[1] 350	110		143
6	do	do	23	[1] 170	90	70	143
8	do	do	23	[1] 245	130		143
9	do	do	23	[1] 280	110		143
12	do	do	23	[1] 280	110	90	143
13	do	do	23	[1] 245	90		143
14	do	do	23	[1] 330	110		143
15	do	do	23	[1] 265	90	70	143
16	do	do	23	[1] 225	50	70	143
17	do	do	23	[1] 265	110	110	143

[1] Film watermarked.

Experimental film badges—Utirik Atoll

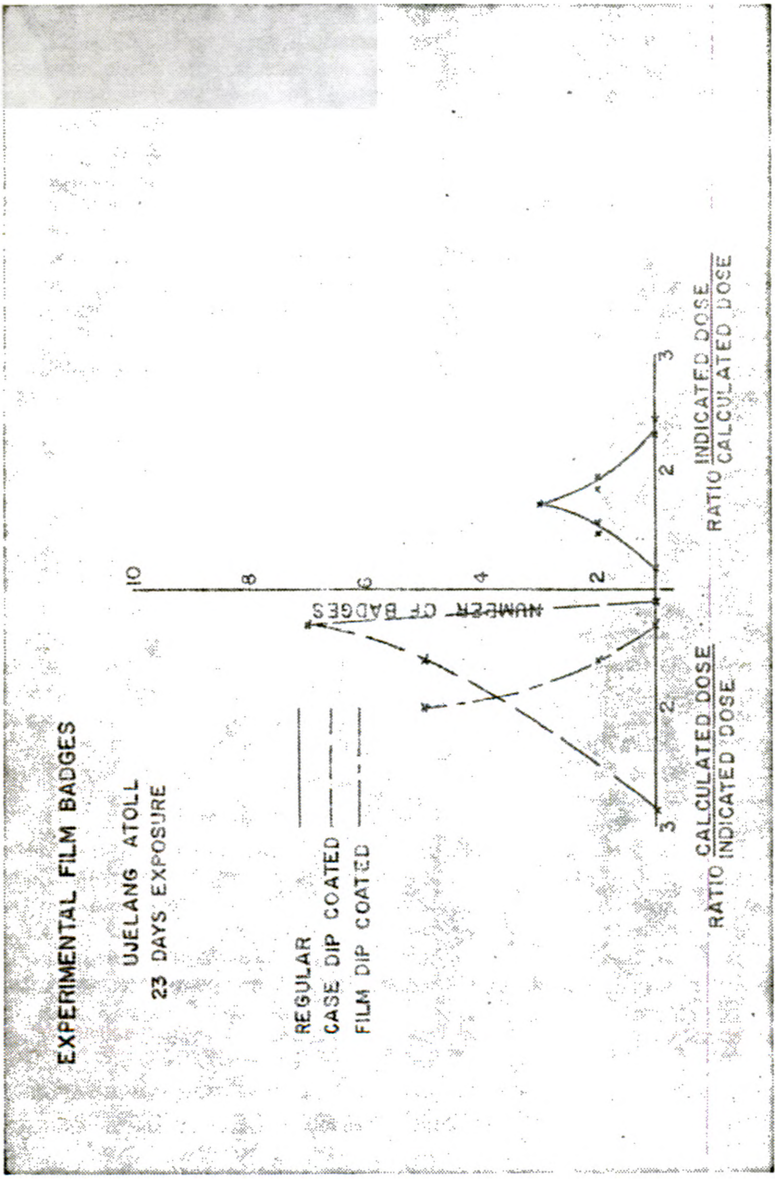
Station No.	Date out	Date in	Time exposed, in days	Indicated dose, milliroentgen			Calculated dose, milliroentgen
				Regular	C. D.	F. D.	
1	July 8, 1956	July 14, 1956	7	70	50	50	9.0
2	do	do	7	70	50		9.0
1	do	July 26, 1957	19	90	70		41.5
3 [2]	do	do	19	[1] 470	425	410	41.5
4 [2]	do	do	19	470	440		41.5
5	do	do	19	110	70	[3] 365	41.5
6	do	do	19	110	70		41.5
7	do	do	19	[1] 130	50	50	41.5
8	do	do	19	[1] 130	70		41.5
9	do	do	19	[1] 130	50	50	41.5
10	do	do	19	[1] 130	70		41.5
11	do	do	19	[1] 130	70	50	41.5
12	do	do	19	[1] 130	70		41.5
13 [2]	do	do	19	[1] 440	440	440	41.5
14	do	do	19	[1] 110	70		41.5
15	do	do	19	[1] 110	70	50	41.5
16	do	do	19	90	70		41.5
17	do	do	19	90	70		41.5

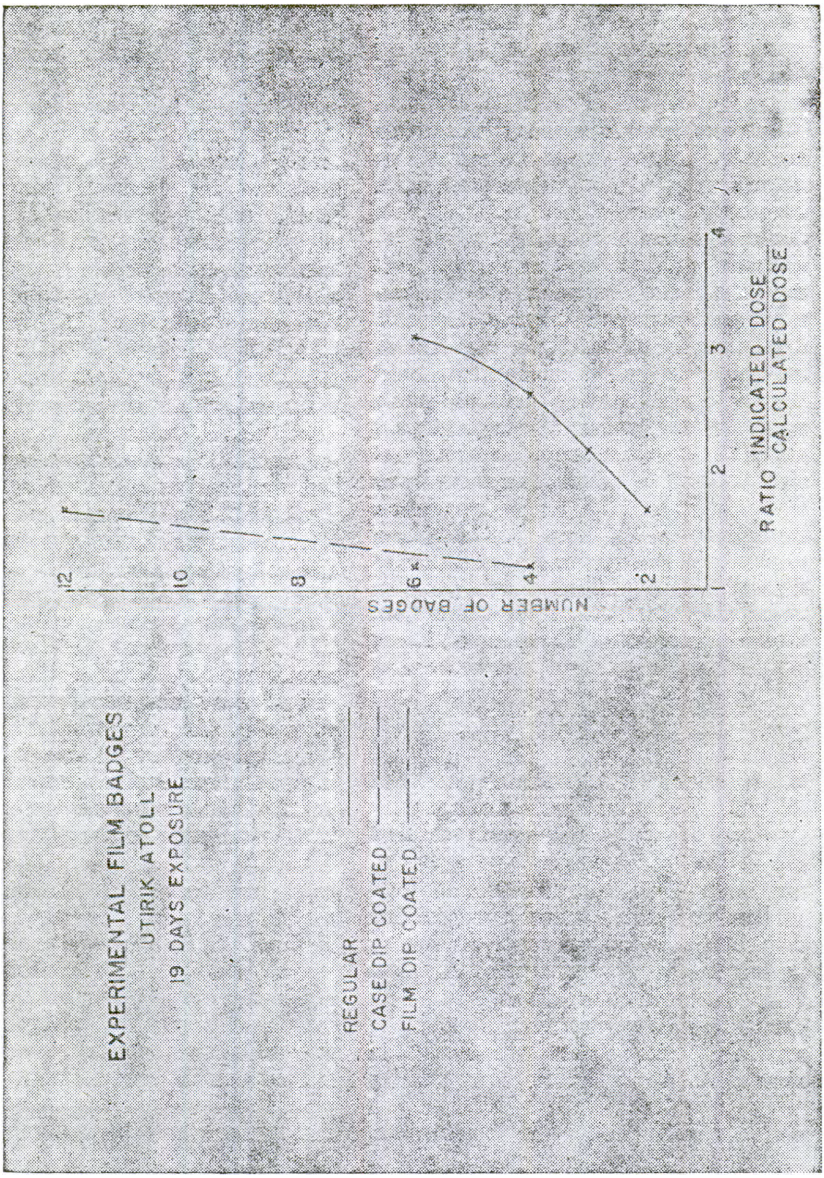
[1] Film watermarked.
[2] Films developed separately from other films exposed during this same period.
[3] Fogging due to unknown cause.

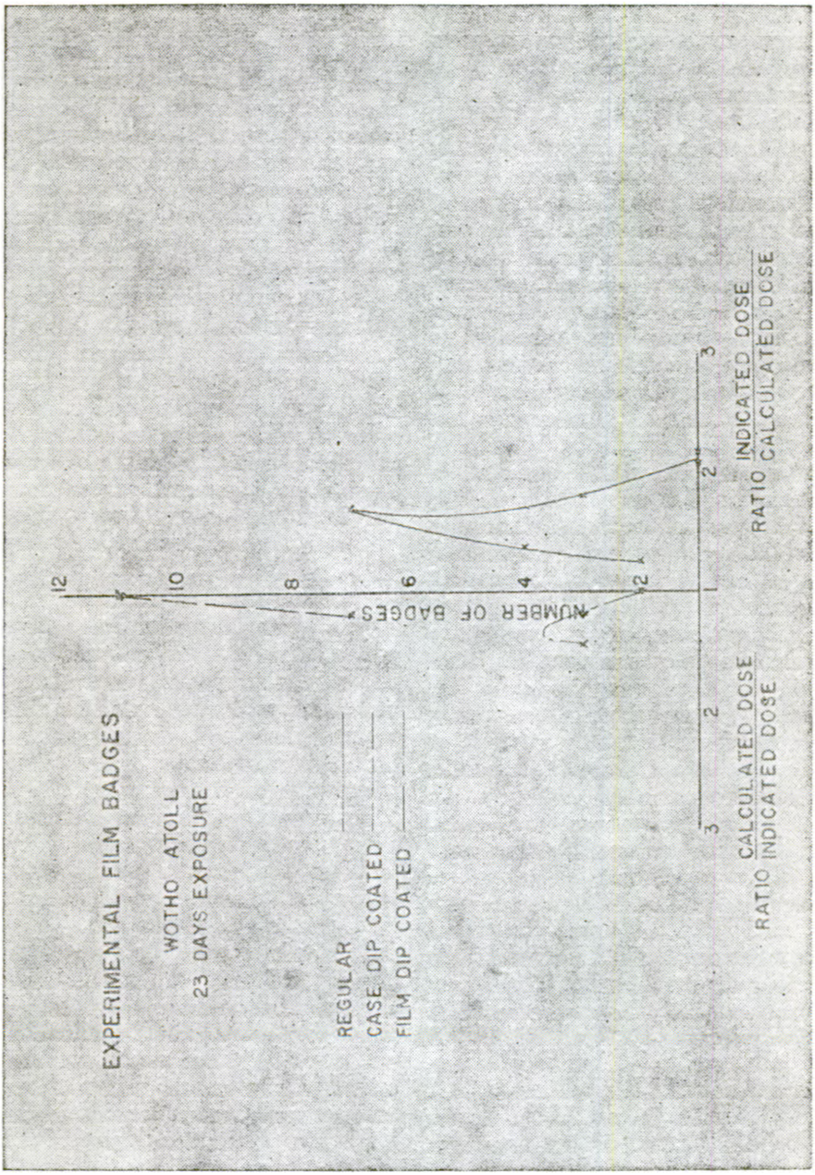
Experimental film badges—Wotho Atoll

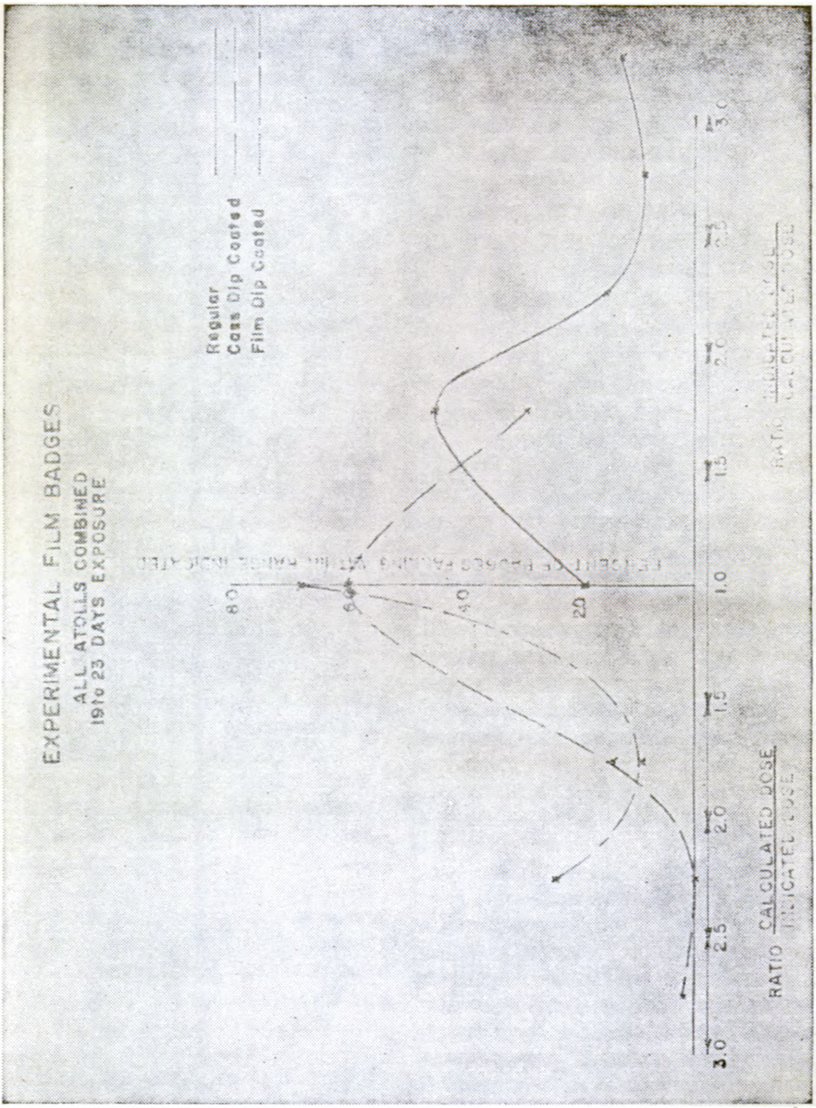
Station No.	Date out	Date in	Time exposed, in days	Indicated dose, mr.			Calculated dose, mr.
				Regular	C. D.	F. D.	
10	July 2, 1956	July 9, 1956	8	30	30	30	33
9	do	July 16, 1956	15	70	30	30	50
1	do	July 24, 1956	23	[1] 160	130	90	130
2	do	do	23	[1] 180	130	90	130
3	do	do	23	[1] 180	130	90	130
4	do	do	23	[1] 180	130	110	130
5	do	do	23	[1] 160	130	110	130
6	do	do	23	[1] 220	130	110	130
7	do	do	23	[1] 220	130	130	130
8	do	do	23	[1] 270	110	130	130
11	do	do	23	[1] 220	130		130
12	do	do	23	[1] 220	110		130
13	do	do	23	[1] 235	130		130
14	do	do	23	[1] 220	130		130
15	do	do	23	[1] 220	110		130
16	do	do	23	[1] 235	110		130
17	do	do	23	[1] 235	110		130
18	do	do	23	[1] 285	110		130
19	do	do	23	[1] 180	130		130
20	do	do	23	[1] 220	110		130

[1] Film watermarked.


EXPERIMENTAL FILM BADGES
UJELANG ATOLL
23 DAYS EXPOSURE
REGULAR
CASE DIP COATED
FILM DIP COATED
NUMBER OF BADGES
10
8
6
4
2
3
2
2
3
RATIO CALCULATED DOSE / INDICATED DOSE
RATIO INDICATED DOSE / CALCULATED DOSE


EXPERIMENTAL FILM BADGES
UTIRIK ATOLL
19 DAYS EXPOSURE
REGULAR
CASE DIP COATED
FILM DIP COATED
NUMBER OF BADGES
12
10
8
6
4
2
2
3
4
RATIO INDICATED DOSE / CALCULATED DOSE


EXPERIMENTAL FILM BADGES
WOTHO ATOLL
23 DAYS EXPOSURE
REGULAR
CASE DIP COATED
FILM DIP COATED
NUMBER OF BADGES
12
10
8
6
4
2
3
2
2
3
RATIO CALCULATED DOSE / INDICATED DOSE
RATIO INDICATED DOSE / CALCULATED DOSE


EXPERIMENTAL FILM BADGES
ALL ATOLLS COMBINED
19 to 23 DAYS EXPOSURE
Regular
Case Dip Coated
Film Dip Coated
80
60
40
20
3.0
2.5
2.0
1.5
1.0
1.5
2.0
2.5
3.0
RATIO CALCULATED DOSE / INDICATED DOSE

A BRIEF REVIEW OF THE PUBLIC HEALTH SERVICE RADIATION SURVEILLANCE NETWORK OPERATED BY THE PUBLIC HEALTH SERVICE UNDER AN AGREEMENT WITH THE DIVISION OF BIOLOGY AND MEDICINE, ATOMIC ENERGY COMMISSION, MAY 22, 1957

For the principal purpose of providing immediately useful environmental radiological health data, the Public Health Service, by an agreement with the Division of Biology and Medicine, AEC, dated April 15, 1956, established a nationwide radiation surveillance network, with a central laboratory service located in Washington, D. C.

Originally established to encompass the period of Operation Redwing conducted by the United States at the Pacific Proving Ground in 1956, the network stations commenced operation in April 1956 and ceased intensive operation on September 28, 1956. Sampling data were received from Hawaii, Alaska, and 28 States. The gratifying cooperation of the State and Territorial departments of health made possible the staffing and operation of 29 field stations on a cooperative basis.

During the period of September 28, 1956, to May 1, 1957, the Public Health Service encouraged and assisted in the continued operation of the field sampling stations. As a result, between 3 and 8 of the stations continued to submit samples, which were processed in the central laboratory.

An extension of the PHS-AEC agreement, signed on April 18, 1957, provides for resumption of intensive operations during the period of Operation Plumbbob conducted by the United States at the Nevada test site, commencing in May and continuing until November 1957. The number of field sampling stations has been increased to 38, as shown on the accompanying list. Thirty-six of these are operated by State, Territorial, and local health agencies, with the remaining two operated by the Public Health Service.

SAMPLING OPERATIONS

Sampling is performed on a 24 hour-per-day, 7 day-per-week basis wherever possible. Of the approximately 15,000 samples taken in 1956, 9 percent of the air samples were invalid because of equipment failures, and 10 reports failed to reach the laboratory.

Sampling operations at each station include (1) the daily radioassay of beta-emitting particulates with relatively long half lives, collected on a filter from approximately 2,000 cubic meters of air, (2) 2 (or more) daily determinations of external gamma radiation levels with a portable survey meter, (3) collection of radioactive fallout by means of the system developed by NYOO of the AEC, and (4) preparation of preliminary reports from which public information might be made available by State and Territorial departments of health.

During 1957 operations, precipitation samples are also being collected.

CENTRAL LABORATORY SERVICES

The radiological health program, Bureau of State Services, of the Public Health Service, maintains the field stations and provides accurate laboratory confirmation of preliminary field measurements.

Through a closely knit communications network, the radiological health program, PHS, and the Division of Biology and Medicine, AEC, cooperate to provide technical guidance to the State and Territorial health departments in the interpretation of day-to-day results and in replying to public inquiries.

PUBLIC INFORMATION AND CLASSIFICATION

No security classification is imposed on the nature of operations or findings of the network. Because of the approximate values derived at the field sampling stations, it has been requested that only the State and Territorial commissioners of health and the Washington headquarters staff be responsible for replying to public inquiry. No attempt was made to relate the network findings back to specific test operations at the Pacific Proving Ground; instead, interpretation of results has been confined simply to reporting the factual data. This apparently satisfied the numerous newspaper inquiries directed to the State and Territorial commissioners of health.

As far as can be determined, the network operations and the immediate availability of its data help explain, to those of the public who inquire, the dis-

semination of radioactive material from nuclear test areas outside of the continental United States, and the levels of activity which might occur in populated areas in the United States as a result. At many of the field sampling sites, there has been almost daily contact between the State health departments and the newspaper services.

PRELIMINARY RESULTS

During the entire 1956–57 sampling period, external gamma background radiation measurements have remained practically constant at all sampling stations. Depending upon the locality, the background varies from 0.01 to 0.035 milliroentgens per hour and, in general, is typical of that locality.

The beta activity of the particulates in air, having gross radioactive half lives longer than several days, showed minimum average concentrations varying from 0.5 to 1.0 uuc/M^3 at the time of measurement (3 to 5 days after collection). An exception was Alaska, where minimum concentrations were about one-fifth or one-tenth those in the United States and Hawaii.

Before, during, and well after the period announced as encompassing Operation Redwing conducted by the United States at the Pacific Proving Ground in 1956, maximums of air concentrations were noted at all sampling stations, each lasting from several days to more than a week. The highest value, 25.7 micromicrocuries per cubic meter of air, was measured in Honolulu, with equally high values being observed in Austin, Tex., Indianapolis, Ind., Springfield, Ill., and Gastonia, N. C. The latter 4 occurred about 55 days after the announced termination of the United States 1956 tests at the Pacific Proving Ground, and it is difficult to associate these maximums with our tests, because of the long time interval.

Table 1 and figure 1, accompanying this report, illustrate the shift to higher air radioactivity levels at areas east and west of the Mississippi River, at Honolulu, and in Alaska, with the passage of time. The most significant shift to higher air activities occurred after September 1, 1956, at least 30 days after the announced termination of our test operations.

It has been possible to analyze a number of the samples find the approximate date of formation. It should be realized that this method indicates, within limits, the formative age of the more recent fission products in each sample, and is not intended to assess more than the short-term significance of the gross beta radioactivity. Figure 2 illustrates the results of this procedure, and strikingly shows that the major portion of the intermediate half-lived fission products which were samples in the United States could not have resulted from announced test series conducted by the United States. During the 1957 operation all samples will be dated.

Since January 1957, and continuing until the present time, air activity levels measured in the United States have been substantially higher at all locations than for comparable periods in previous years by a factor of about 5 to 10. The radiation samples, when dated, show approximate formative ages coinciding to a degree with publicly announced foreign nuclear tests. The effect is most noticeable in precipitation sample, as described in The Distribution of Radioactivity From Rain, by Dr. Lloyd R. Setter and Dr. Conrad P. Straub (presented for publication proceedings, American Geophysical Union Meeting, Washington, D. C., April 29 to May 1, 1957).

The National Committee on Radiation Protection, in NBS Handbook 52, has suggested 10^{-9} uc/ml (1,000 uuc/M^3) as the provisional level of permissible concentrations of unkown mixtures of beta-emitting radioisotopes in air. When it is reduced to 10 percent of that value as suggested for large population groups, namely, 100 uuc/M^3, we realize that the measured levels of beta radioactivity in air, while generally below the recommended value, are more often approaching this level as time goes on.

Radiation surveillance network stations and operators

1–1	Hartford, Conn	Omer C. Sieverding, assistant director, Bureau of Laboratories, Connecticut State Department of Health, State Office Building, Hartford, Conn.
1–2	Lawrence, Mass	James L. Dallas, associate sanitary engineer, Massachusetts State Department of Health, Room 511, State House, Boston, Mass.

Radiation surveillance network stations and operators—Continued

2-1	Trenton, N. J	Byron Keene, radiation physicist, Bureau of Adult and Industrial Health, Division of Constructive Health, New Jersey State Department of Health, 17 West State Street, Trenton 7, N. J.
2-2	Albany, N. Y	Wallace W. Sanderson, assistant director, Division of Laboratories and Research, New York State Health Department, 84 Holland Avenue, Albany, N. Y.
2-3	Harrisburg, Pa	Ronald H. Boyer, assistant industrial hygienist, Bureau of Industrial hygiene, Pennsylvania Department of Health, 1680 South Cameron Street, Harrisburg, Pa.
3-1	Baltimore, Md	Kenneth M. Hallam, chemist, Division of Industrial Health and Air Pollution, State Department of Health, 2411 North Charles Street, Baltimore, Md.
3-2	Washington, D. C	Dr. Frederick H. Goldman, Chief, Public Health Engineering Division, District of Columbia Department of Public Health, Municipal Building, 300 Indiana Avenue NW., Washington, D. C.
3-3	Gastonia, N. C	William N. Long, Gastonia Health Department, Gastonia, N. C.
3-4	Richmond, Va	E. C. Meredith, director, Division of Engineering, State Department of Health, 12th and Bank Sts., Richmond, Va.
4-1	Jacksonville, Fla	Roe B. Hull, Division of Industrial Hygiene, Florida State Board of Health, 1217 Pearl St., Jacksonville 1, Fla.
4-2	Atlanta, Ga	Richard Fetz, Georgia State Department of Public Health, Industrial Hygiene Division, Atlanta 3, Ga.
5-1	Springfield, Ill	Robert R. French, sanitary engineer, Division of Sanitary Engineering, State Department of Health, State Office Building, 400 South Spring St., Springfield, Ill.
5-2	Indianapolis, Ind	Frank D. Wraight, Bureau of Environmental Sanitation, Indiana State Board of Health, 1330 West Michigan St., Indianapolis, Ind.
5-3	Lansing, Mich	Donald E. Van Farowe, chief, Standards and Analysis Section, Division of Occupational Health, Michigan Department of Health, Dewitt Rd., Lansing, Mich.
5-4	Cincinnati, Ohio	Dr. Lloyd R. Setter, Robert A. Taft Sanitary Engineering Center, 4676 Columbia Parkway, Cincinnati 26, Ohio
6-1	Iowa City, Iowa	Robert L. Morris, State Hygienic Laboratory, 272 Medical Laboratories Bldg., University of Iowa campus, Iowa City, Iowa

Radiation surveillance network stations and operators—Continued

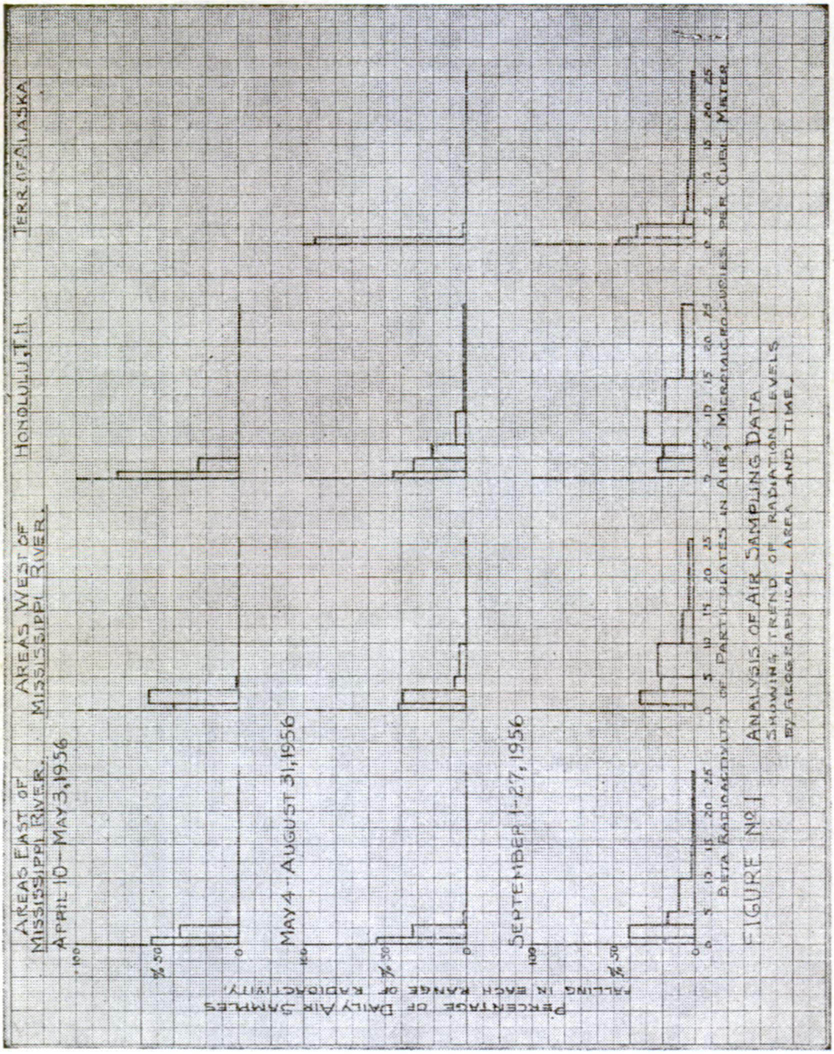
6-2	Minneapolis, Minn	George Raschka, Industrial Health Section, Division of Environmental Sanitation, Minnesota State Department of Health, campus, University of Minnesota, Minneapolis 14, Minn.
6-3	Jefferson City, Mo	Louis Garber, industrial hygiene consultant, Bureau of Public Health Engineering, Missouri Division of Health, State Office Bldg., Jefferson City, Mo.
6-4	Pierre, S. Dak	Donald G. Kurvink, Occupational and Radiological Health, State Department of Health, Pierre, S. Dak.
7-1	New Orleans, La	Warren H. Reinhart, chief, Section of Occupational Health and Safety, Louisiana State Department of Health, 1436 Dryades St., New Orleans, La.
7-2	Oklahoma City, Okla	Loyd F. Pummill, sanitary engineer, State Department of Health, 3400 North Eastern, Oklahoma City 5, Okla.
7-3	Austin, Tex	Martin C. Wukasch, Texas State Department of Health, Division of Sanitary Engineering, 410 East 5th St., Austin, Tex.
7-5	El Paso, Tex	James H. Tillman, City-County Health Department, 118 West Missouri St., El Paso, Tex.
7-6	Little Rock, Ark	Frank Edward Wilson, Bureau of Sanitary Engineering, State Health Department, State capitol grounds, Little Rock, Ark.
8-1	Denver, Colo	William R. Van Nattan, Colorado Department of Public Health, State Office Building, Denver 2, Colo.
8-2	Salt Lake City, Utah	Lynn M. Thatcher, State Department of Health, State Capitol Building, Salt Lake City, Utah
8-3	Cheyenne, Wyo	Robert E. Sundin, Industrial Hygiene Section, Wyoming Department of Public Health, State Office Building, Cheyenne, Wyo.
8-4	Boise, Idaho	Vaughn Anderson, Division of Engineering and Sanitation, Idaho State Board of Health, Post Office Box 640, Boise, Idaho
9-1	Anchorage, Alaska	William B. Page, chief, Environmental Sanitation Section, Arctic Health Research Center, Anchorage, Alaska
9-2	Phoenix, Ariz	George W. Marx, Bureau of Sanitation, State Department of Health, State Office Building, Phoenix, Ariz.
9-3	Berkeley, Calif	Dr. Harold L. Helwig, chief, Air Sanitation Laboratory, California State Department of Health, 2151 Berkeley Way, Berkeley, Calif.
9-4	Los Angeles, Calif	Remo Navone, chief, Branch Public Health Laboratory, California State Department of Health, 1930 Beverly Boulevard, Los Angeles 57, Calif.

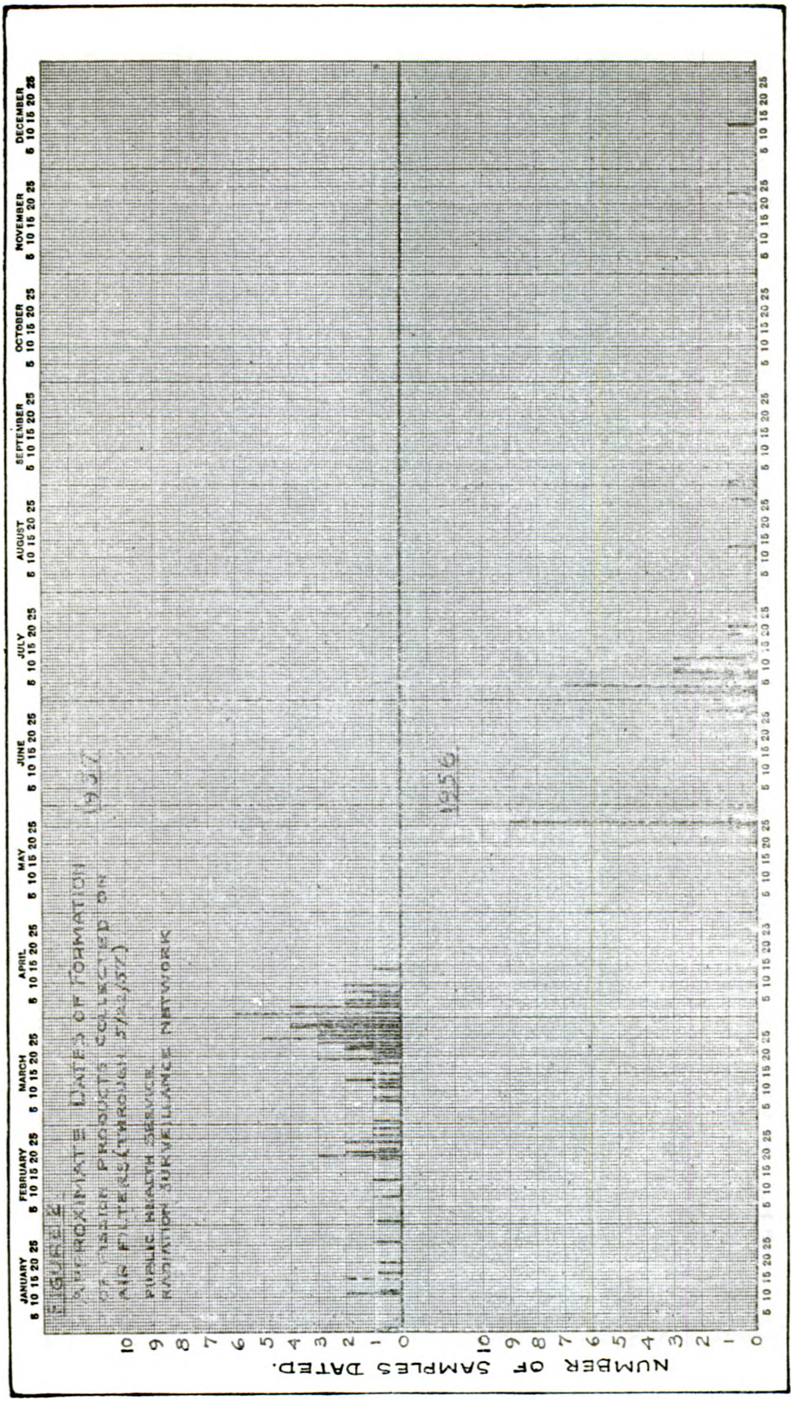
Radiation surveillance network stations and operators—Continued

9–5	Honolulu, T. H	Francis H. Woo, chief, Industrial Hygiene, Department of Health, Territory of Hawaii, Honolulu, T. H.
9–6	Mercury, Nev	Not operational in 1957 network.
9–7	Portland, Oreg	Richard E. Hatchard, director, air pollution control, Oregon State Board of Health, 1400 South West Fifth Avenue, Portland, Oreg.
9–8	Seattle, Wash	Edwin L. Ruppert, Washington State Department of Health, 2120 Smith Tower, Seattle 4, Wash.
9–9	Juneau, Alaska	Ralph B. Williams, director, public health laboratories, Alaska Department of Health, Alaska Office Building, Juneau, Alaska
9–10	Klamath Falls, Oreg	Max Braden, Klamath County Health Department, Klamath Falls, Oreg.

TABLE 1.—*Analysis of air sampling data, showing the trend of radiation levels in the United States, Hawaii, and Alaska for 3 time intervals: Before, during, and after Operation Redwing*

Geographical area	Time interval, 1956	Numbers of daily air samples, by beta activity level							
		0–1 uuc/M³	1–3 uuc/M³	3–5 uuc/M³	5–10 uuc/M³	5–15 uuc/M³	5–26 uuc/M³	Invalid	Total
Areas east of Mississippi River.	Apr. 10 through May 3.	90	60	0	0	0	0	16	166
	May 4 through August 31.	943	559	24	5	0	0	185	1,716
	Sept. 1 through Sept. 27.	78	170	67	39	7	4	41	406
Areas west of Mississippi River.	Apr. 10 through May 3.	71	99	2	0	0	0	7	179
	May 4 through Aug. 31.	704	663	114	65	7	4	133	1,690
	Sept. 1 through Sept. 27.	24	120	73	82	25	13	29	369
Honolulu, T. H	Apr. 10 through May 3.	12	4	0	0	0	0	0	16
	May 4 through Aug. 31.	51	86	12	7	2	2	3	113
	Sept. 1 through Sept. 27.	0	6	5	8	5	2	1	27
Territory of Alaska.	May 4 through Aug. 31.	198	4	0	0	0	0	10	212
	Sept. 1 through Sept. 27.	25	15	0	2	1	0	11	54


AREAS EAST OF MISSISSIPPI RIVER
AREAS WEST OF MISSISSIPPI RIVER
HONOLULU, T.H.
TERR. OF ALASKA
APRIL 10 – MAY 3, 1956
MAY 4 - AUGUST 31, 1956
SEPTEMBER 1-27, 1956
BETA RADIOACTIVITY OF PARTICULATES IN AIR, MICROMICROCURIES PER CUBIC METER
PERCENTAGE OF DAILY AIR SAMPLES FALLING IN EACH RANGE OF RADIOACTIVITY
FIGURE No 1
ANALYSIS OF AIR SAMPLING DATA
SHOWING TREND OF RADIATION LEVELS
BY GEOGRAPHICAL AREA AND TIME.


APPROXIMATE DATES OF FORMATION
PUBLIC HEALTH SERVICE
RADIATION SURVEILLANCE NETWORK
1957
1956
JANUARY
FEBRUARY
MARCH
APRIL
MAY
JUNE
JULY
AUGUST
SEPTEMBER
OCTOBER
NOVEMBER
DECEMBER
NUMBER OF SAMPLES DATED.

The Distribution of Radioactivity From Rain [1]

Lloyd R. Setter and Conrad P. Straub [2]

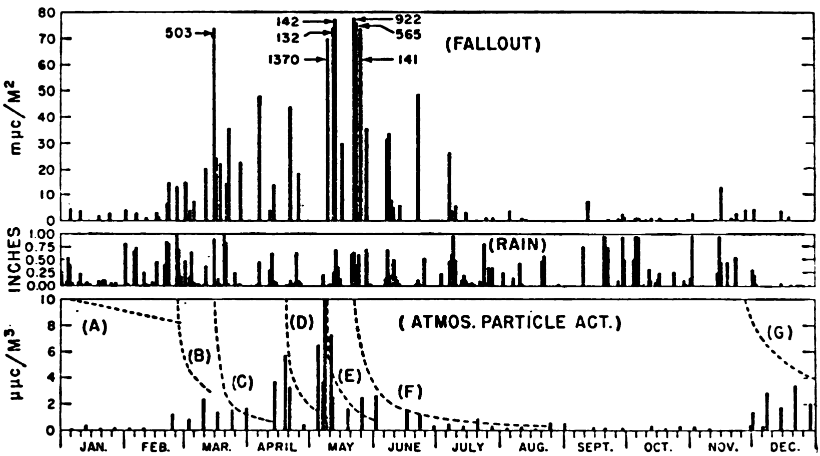
Fallout measurements, largely as precipitation, have been made on a continuous basis at Cincinnati, Ohio, since March 1953. The studies of 1953 [3] and 1954 [4] were implemented in October 1953 by weekly or daily tests of the atmospheric particulate activity and by the operation of an experimental cistern near the site of the rain collector. Correlation is now possible between air particle concentration and rainout (washout by precipitation) and the distribution of activity between the separated solids and the supernatant liquid in the cistern liquid can be determined. This report includes data relating to activity levels in air particulates, rainfall, and certain Ohio rivers.

METHODS

The methods of sample collection have been described.[4] The precipitation values reported in figures 1 and 2 are the means of values reported for six stations and were obtained from United States Weather Bureau climatological data. To compensate for differences in hours of collection some adjustment in these values was made.

Rainfall was collected in a pail-type sampler, processed to dryness, the gross beta-gamma activity was measured, and the results reported on an area basis.

FIGURE 1



RADIATION LEVELS, AIR AND RAINFALL 1955

Date bomb debris formed	Decay slope
a. June 4, 1954	1.0
b. February 18, 1955	1.2
c. Marsh 12, 1955	1.0
d. April 15, 1955	1.2
e. May 5, 1955	1.2
f. May 15, 1955	1.2
g. November 5, 1955	1.0

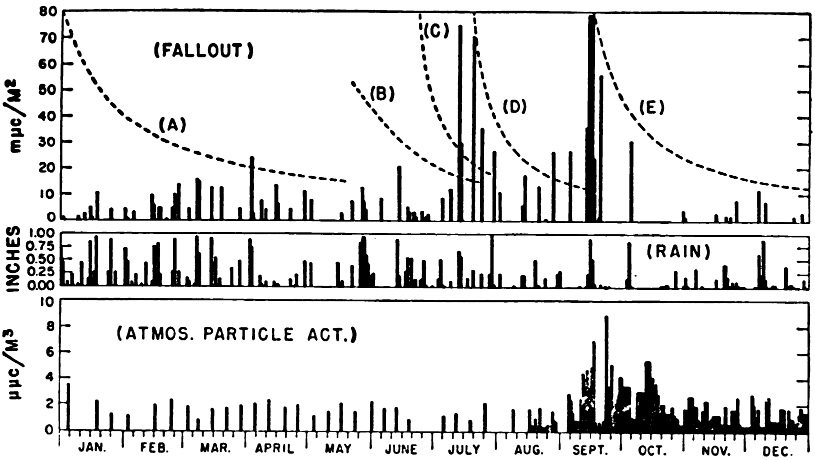
[1] For presentation at the American Geophysical Union Meeting, Washington, D. C., April 29 to May 1, 1957.

[2] Respectively, in charge, radiological investigations, and chief, radiological health program, Robert A. Taft Sanitary Engineering Center, Public Health Service, Department of Health, Education, and Welfare, Cincinnati, Ohio.

[3] Nader, J. S., A. S. Goldin, and L. R. Setter. Radioactive Fallout in Cincinnati Area. Jour. A. W. W. A., 46, 11, 1096 (November 1954).

[4] Setter, L. R., and A. S. Goldin. Radioactive Fallout in Surface Waters. Ind. Eng. Chem., 48, 2, 251 (February 1956).

FIGURE 2



RADIATION LEVELS, AIR AND RAINFALL 1956

Date bomb debris formed	*Decay slope*
a. December 1, 1955	1.0
b. May 3, 1956	1.0
c. June 13, 1956	1.0
d. July 10, 1956	1.0
e. August 27, 1956	1.0

Air particulate activity from January 1, 1955, to August 1956, was determined from swatch subsamples representing roughly 80 cubic meters of air filtered through glass-fiber filters over a 24-hour period, from the Lincoln Park area, Cincinnati.[5] Since August 1, 1956, the air activity reported was obtained by filtering 20 to 30 cubic meters of air through a membrane (cellulose acetate) filter. With the exception of weekend samples collections were made on a daily basis. Sampling was continuous and was interrupted only during the period required to change the filters. After collection, the membrane filter samples were placed in aluminum dishes, saturated with 95 percent alcohol, ignited, flash flamed with a Meeker burner, and counted for two 8-minute periods in an internal proportional counter. To permit decay of short-lived naturally occurring alpha activity[6] counting was delayed for 48 hours.

RESULTS

Precipitation, air particulate, and fallout activity

The precipitation in inches, air particulate activity in micromicrocuries per cubic meter ($\mu\mu c/m^3$), and the fallout activity in millimicrocuries ($m\mu c$) per liter results have been summarized in figures 1 (1955 data) and 2 (1956 data). From decay studies on rain samples it was also possible to determine the age of the samples. This age, shown with the air activity in figures 1 and 2, was obtained by calculating or estimating the slope of the decay rate. For time intervals between such decay curves, the decay rate of later date was used to correct samples collected during the interval indicated.

In general, the results show that increased air activity results in increased activity in the fallout samples collected. This is particularly true for the samples collected in the spring of 1955 during and following continental weapons tests and to a lesser extent for samples collected following foreign weapons tests activities during fall 1955 or Pacific and foreign tests in 1956.

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[5] Tabor, Elbert C. Protein Content of Air—Final Report of Research for the Chemical Corps Biological Warfare Research Laboratories on Contracts Nos. CD-3-3207 and CD-4-4432, USPHS 1956.

[6] Setter, L. R., G. R. Hagee, and C. P. Straub. The Analysis of Radioactivity in Surface Waters: Practical Laboratory Methods. Submitted for publication in the Am. Soc. for Testing Materials, Bull. 1957.

Considering particulate activity in air from stations in the Midwest (Cincinnati, Louisville, St. Louis, Chicago, Minneapolis, and Detroit) the average background level of approxmately 0.05 $\mu\mu c/M^3$ in the fall of 1953 has increased to an arithmetic average level of 0.6 $\mu\mu c/M^3$ in 1954, 1.55 $\mu\mu c/M^3$ in 1955, and 2.26 $\mu\mu c/M^3$ in 1956. During continental tests in 1955, the activity in Cincinnati air (fig. 1) fluctuated from a high of 10 $\mu\mu c/M^3$ of rapidly decaying radioactivity to a low of 0.08 $\mu\mu c/M^3$ of longer lived radioactivity. Less fluctuation in radioactivity levels (8.9 to 0.58 $\mu\mu c/M^3$) was observed during the Pacific and foreign tests of 1956. Presumably this was due to greater diffusion and decay of radioactivity from a more distant source. However, a higher sustaining level of air radioactivity was noted for the entire year.

As shown in figure 1, 15 rain collections made between March 15 and June 25, 1955, showed activity levels above 30 $m\mu c/M^2$, with the level in 7 of these varying from 141 to 1370 $m\mu c/M^2$. From the decay curves indicated, most of this activity was short lived. Relatively little radioactivity was observed in samples collected during the first 2 and last 6 months of 1955, but the activity collected was of longer half life.

A few peaks of high activity were noted (see fig. 2) during July and September 1956, but fallout levels in excess of 10 $m\mu c/M^2$ were common throughout the year. From the decay curves shown it will be seen that most of the activity collected in 1956 was longer lived than that collected during 1955.

Residual fallout

A somewhat better appraisal of the amount of long-lived radioactive fallout may be obtained by considering the cumulative residual deposition. At Cincinnati, and presumably at all nonarid areas several hundred miles removed from the weapons-test site,[7] increased fallout appears to be associated with periods of rainfall and snowfall.

The fallout, identified quite precisely as to age and rate of decay for the more radioactive samples and less accurately for the low-level radioactive samples, has been decayed to the first of each succeeding month. The cummulative residual activity in millimicrocuries per square meter ($m\mu c/M^2$) of earth's surface, assuming no loss, is plotted as shown in figure 3 for each of the 4 years of record from April 1953 to July 1, 1957. In each year there is a peak of activity during or immediately following test operations. High peaks of 1054 $m\mu c/M^2$ on June 1, 1953, and 970 $m\mu c/M^2$ on June 1, 1955, are shown for the 2 years of continental tests (1953 and 1955), whereas longer lived peaks of 110 and 390 $m\mu c/M^2$ were observed on July 1, 1954 and October 1, 1956, respectively. Fallout during the latter periods was from Pacific and foreign test operations. If a 6-month delay is assumed for observed fallout (that is correcting to the following July 1 after a year of accumulation), the residual activity for each year is as follows:

Year of deposition:	*Residual fallout on July 1 following year of deposition* $m\mu c/M^2$
1953	35
1954	35
1955	90
1956	110
Total 1953–566	270

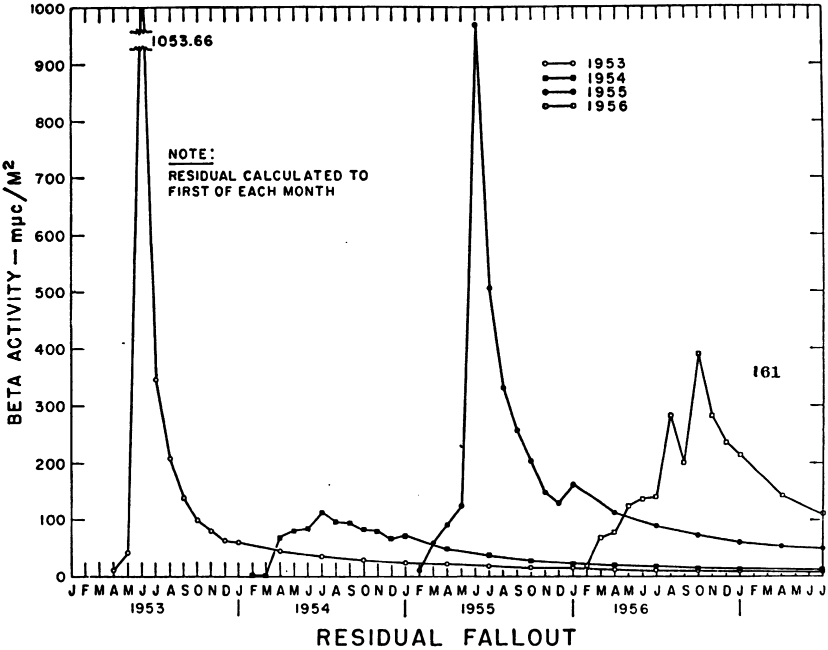
A steeper decay slope is noted on July 1, 1957, for the 1956 fallout, so that the 110 $m\mu c/M^2$ probably contains about as much long-lived activity as does the 1955 fallout. Nevertheless the table clearly shows approximately a threefold increase in fallout for 1955 and 1956 in comparison to 1953 and 1954.

Assuming that the residual fallout as of July 1, of the succeeding year has an average age of 1 year, one may estimate the strontium-yttrium activity which was deposited. According to Hunter and Ballou[8] the combined activity of Sr^{90}–Y^{90} 1 year after formation is 3.9 percent of the residual or 10.5 $m\mu c/M^2$ deposited over the 4 years. This amount, uniformly mixed with 30 in./yr x 4 yr.

[7] List, Robert. Radioactive Fallout in North America From Operation Teapot. USAEC Doc. No. NYO–4696 (February 1956).

[8] Hunter, H. F., and N. E. Ballou. Fission-Product Decay Rates. Nucleonics, 9, 5, C–2 (November 1951).

FIGURE 3



or 120 inches of rain over 4 years gives a Sr^{90}-Y^{90} concentration level of 3.46 $\mu\mu c/L$ as compared to the maximum permissible level of this parent-daughter combination of 800 $\mu\mu c/L$.[9]

Distribution of fallout in cistern water

Early in 1954, an experimental cistern, consisting of a hopper-bottomed tank of $2.62M^3$ capacity, was installed to collect precipitation from a portion of the roof of an adjacent building. The cistern was in operation from February 10, 1954, to March 7, 1956, and from July 5 to September 25, 1956. During this period, settled cistern water was withdrawn and tested and on 15 occasions the settled sludge was removed and tested. The more significant data from cistern operation are presented in table 1. The total activity found in the cistern water, sludge, and sludge supernatant are indicated in column 7: Sludge supernatant consisted of a known quantity of cistern water (up to 50 liters) required to wash down the cistern sides and bottom. The washed sludge was settled for 10 to 15 minutes and the supernatant removed by decantation. This supernatant was sampled and tested for dissolved and suspended activity in the same manner that the citern water itself was tested. The sludge and supernatant solids contained in the roof drainage amounted to an average of 30 p. p. m. of dry solids (ranging from 9 to 71 p. p. m.) of which about 30 percent were volatile.

[9] Maximum Permissible Amounts of Radioisotopes in the Human Body and Maximum Permissible Concentrations in Air and in Water. National Bureau of Standards Handbook No. 52, U. S. Department of Commerce. March 20, 1953.

TABLE 1.—*The distribution of fallout in cistern water*

Series No.	Date of accounting	Precipitation, inches	Ext. date [1]	Age of bomb-debris days	Total precipitation inches	Gross Beta Radioactivity					
						Recovered in cistern (μc)	In rain (μc)	Dissolved in cistern water (percent)	Suspended in cistern water (percent)	In cistern dissolved (μμc/L)	Water total (μμc/L)
1	Mar. 4, 1955	19	Mar. 8	19-300	6.34	2.86	1.40	14	77	69	445
2	Mar. 16, 1955	5	Mar. 19	7-30	1.37	10.00	9.71	53	22	4,160	5,890
3	Mar. 21, 1955	4	Mar. 22	10	1.96	1.02	.95	30	17	163	256
4	May 5, 1955	16	May 11	20-55	2.79	1.46	2.15	46	13	262	337
5	May 17, 1955	6	May 26	15	1.59	3.48	7.16	28	12	660	950
6	June 21, 1955	14	June 29	37	4.62	8.02	8.60	38	33	720	1,310
7	July 14, 1955	11	July 20	60	3.68	1.56	2.07	56	1	259	263
8	Aug. 24, 1955	16	Aug. 26	101	3.07	.36	.28	26	7	34	43
9	Sept. 28, 1955	6	Oct. 4	136	3.50	.15	.34	49	7	22	25
10	Oct. 25, 1955	11	Nov. 10	163	6.01	.17	.20	23	8	7	10
11	Nov. 18, 1955	6	Nov. 30	187	2.93	.11	.53	39	5	24	23
12	Feb. 9, 1956	36	Feb. 9	64-270	9.32	1.16	1.14	56	34	76	123
13	Mar. 7, 1956	11	Mar. 7	36-98	5.08	1.65	1.83	37	27	157	269
14	July 27, 1956	8	July 27	17-44	2.08	4.14	4.66	19	23	410	903
15	Sept. 25, 1956	24	Sept. 25	15-77	5.58	7.81	9.40	55	22	762	1,070
Total, 1 to 15		193			59.92	43.94	50.40	[2] 40	[2] 25	[2] 322	[2] 524

[1] All measurements in a series extrapolated for decay to this date.

[2] Obtained from activity (total or dissolved) ÷ total rainfall.

During the 16-month period of cistern operation, the rain collected contained a total of 50.4 μc of beta activity (col. 8) from bomb debris estimated to be 10 to 300 days old. Of this total, 36.72 μc or 73 percent was recovered from the cistern. The remaining 27 percent represents activity fixed to the asphalt roof shingles, and errors in measurement. It is interesting to note that estimations based on the activity found on washed shingle stone separated from the cistern sludge accounted for much more than the 27 percent unaccounted-for activity. However, longer contact was possible in the case of the shingle stone contained in the sludge in contrast to the contact time during roof runoff.

The soluble activity in the cistern water varied from 14 to 55 percent or amounted to 48 percent (weighted average). There seems to be no clear-cut relationship between the percentage of dissolved activity and the age of bomb debris. This is contrary to the observation made in 1953 [3] that the percentage of solubility in the rain was less with shorter lived radioactivity. Other factors, such as nonradioactive air pollutants, the time radioactive particles were exposed to moisture, the size and chemical structure of radioactive particles, and, possibly, the inflow of upper atmosphere contaminants to lower levels in the rainout zone, play a role in forming the soluble fraction. The concentration of dissolved activity in cistern water varied from 7 to 4,160 or an average of 322 μμc/L. Lower values (less than 76 μμc/L) were observed during periods when the bulk of the activity was estimated to be older than 100 days, whereas, for activity 7 to 98 days in age, the dissolved activity was 157 μμc/L or more.

The percentage of activity associated with the suspended solids ranged from 1 to 77 percent, with an average value of about 25 percent.

In summary, the cistern study reveals that slightly more than one-half of the activity found in the cistern water was soluble and that up to a third of the activity would be associated with the suspended solids. Over two-thirds of the insoluble activity settles as a sludge (1.5 kg. dry weight) having an activity of 10 μc/kg. (dry weight). Septicity resulted in further solution of activity, but diffusion into the supernatant was minimal if the sludge was undisturbed.

The distribution of fallout in surface waters

Nine grab and seven composite (7- to 10-day) samples from 13 stations on the Ohio River from Pittsburgh, Pa., to Louisville, Ky., were collected between December 27, 1955, to April 20, 1956, in a cooperative study with the Ohio River Valley Water Sanitation Commission. The maximum beta activity found was 100 μμc/L as compared to an arithmetic average value of 27.5 μμc/L of which 15 μμc/L was in solution.

Additional samples were collected on the tributaries of the Ohio River and tested. Through the courtesy of Mr. F. H. Waring, chief sanitary engineer. division of sanitary engineering, Ohio State Board of Health, some of these results are included in table 2 and figure 4. The samples were collected from the Little Miami, the Great Miami, and Scioto Rivers. Included in the table are maximum and average activity levels of the surface waters as well as Cincinnati rainout results for collections made between river samplings.

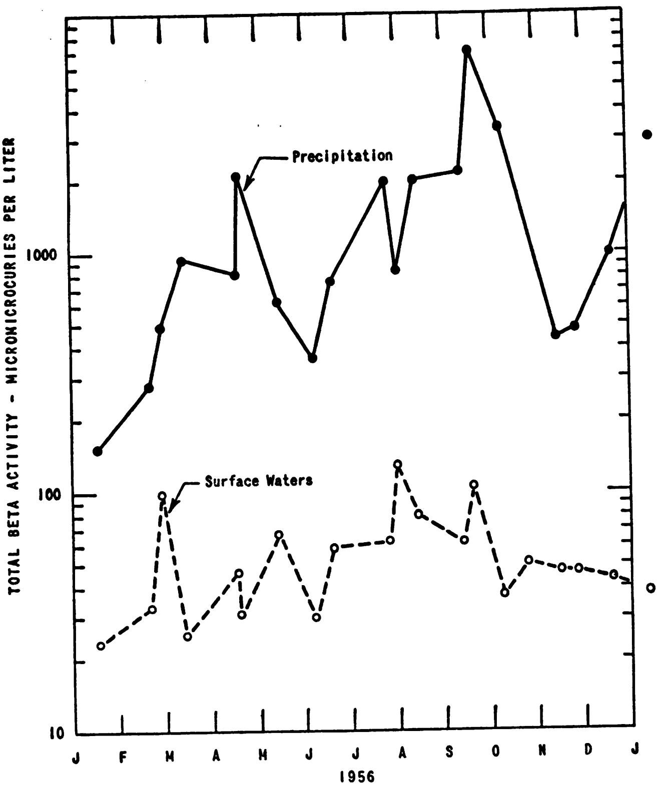
TABLE 2.—*The radioactivity of Ohio surface water, 1956, as compared to rainout*

Date of collection	Stream stations [1]	Stream-water activity, μμc/L				Rain, inches	Rainout μμc/L [2]	
		Maximum		Average			Dissolved	Total
		Dissolved	Total	Dissolved	Total			
Jan. 17	A	19	42	6	23	2.5	50	150
Feb. 20	A	----------	43	----------	33	6.8	160	280
Feb. 28	B	80	141	52	99	1.2	100	490
Mar. 13	A	----------	38	----------	25	2.0	360	950
Apr. 17	B	38	54	29	46	3.7	----------	830
Apr. 24	A	25	40	16	31	.2	----------	2,120
May 14	A	64	87	43	67	1.2	----------	620
June 6	B	29	50	10	30	4.6	140	360
June 18	A	30	66	23	58	1.1	420	760
July 25	B	45	79	44	62	5.0	----------	2,000
July 31	A	80	217	64	130	1.3	510	840
Aug. 15	A	51	140	32	80	.6	1,310	2,040
Sept. 11	A	84	116	35	62	1.2	1,420	2,180
Sept. 19	B	89	178	57	105	1.7	2,260	7,000
Oct. 8	B	27	44	22	37	1.1	1,320	3,300
Oct. 23	A	30	158	20	51	0	----------	----------
Nov. 14	B	36	46	31	47	.5	317	440
Nov. 26	A	60	68	39	47	.6	----------	478
Dec. 18	A	34	69	23	44	1.09	624	992
Jan. 15	B	26	42	20	38	1.42	193	2,941

[1] Ohio State Board of Health data on:
A—Little Miami River at Newtown and 5 stations on Great Miami River from river mile 24.8 to 96.6.
B—Four stations on Scioto River from river mile 15 to 140.

[2] Weighted average activity of rain at Cincinnati collected between dates of stream sampling.

FIGURE 4



A COMPARISON OF CINCINNATI FALLOUT IN RAIN WITH THE ACTIVITY OF OHIO STREAM WATER

The results show a maximum dissolved activity level of less than 90 $\mu\mu c$/L compared to a maximum total (suspended and dissolved) activity of 217 $\mu\mu c$/L for the year. The average dissolved activity appears to represent about one-half of the total activity indicated for a given date of sampling. The average total activity for each sampling date fluctuated from 25 to 130 $\mu\mu c$/L, depending to some degree on the activity of the more recent rains. The yearly geometric average values for the surface streams are 27 $\mu\mu c$/L for dissolved or a total beta activity of 50 $\mu\mu c$/L as compared to 389 and 974 $\mu\mu c$/L respectively, for Cincinnati rain. The average yearly removal by natural agents is, therefore, 93 to 95 percent for dissolved and total activity, respectively. Actually, the removals may be even greater inasmuch as some of these streams are known to receive radioactive materials from industrial, research, and medical facilities.

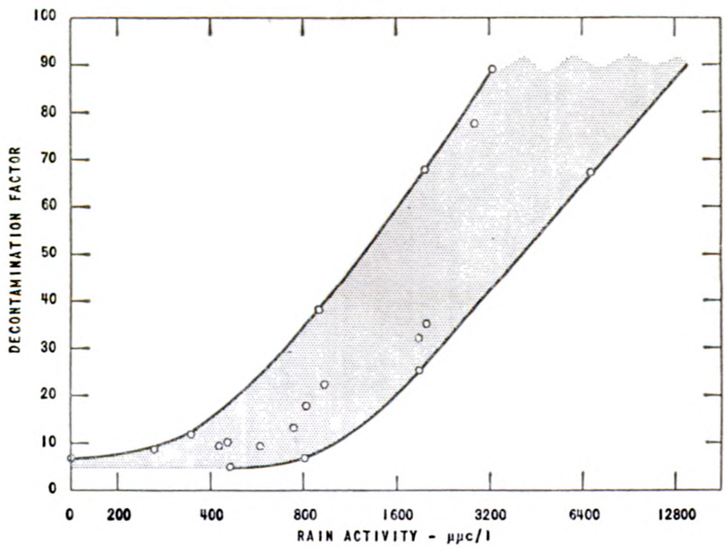
The plotted results (fig. 4) more clearly define the relationship of total rain activity (from Cincinnati fallout) with that found at stream stations up to several hundred miles distance. Obviously, Cincinnati rain and activity measurements can be expected to be at considerable variance to true values at these remote distances. Nevertheless, general trends may be observed and the desirability of establishing more rain monitoring stations as apparent. The rain data indicate a sharp increase (150 to 2,100 $\mu\mu c$/L) during the first of the year, then, following a drop in May and June, a second rise to levels of 2,000 to 7,000 by September. During the last 3 months of the year, the rain activity receded. Lower levels of activity correspond to longer lived bomb debris and high levels correspond to young (short-lived), rapidly decaying, fission products.

The ratio of total rain activity to total surface-water activity was determined for each month, and reported in terms of the decontamination factor (rainout or fallout activity over surface-water activity). These decontamination values were plotted against the rain activity as shown in figure 5. It will be seen that removal by natural agents increases with increased activity of the rain. Comparable in many respects with other pollutants, the plotted values indicate the application of the law of diminishing returns; i. e., high decontamination factors in the order of 65 to 90 (98.5 to 98.9 percent removal) for relatively young bomb debris and decontamination factors of 5 to 10 (80 to 90 percent removal) for lower level, longer lived activity.

For a direct comparison of the distribution of rainout in surface waters of these streams in the general area of Cincinnati, the rainout (corrected for decay to the end of a sampling series) and cistern-water activity of series 11 to 15 (table 1) have been summarized in table 3 with stream tests of appropriate date.

It is seen that subsidence in a cistern results in some purification (21 to 38 percent), adsorption on surfaces such as vegetation, soil and turbidity, percolation, and dilution play important roles in reducing the total activity of rain (91 to 92 percent).

FIGURE 5



THE DECONTAMINATION OF RAIN BY NATURAL AGENCIES

TABLE 3.—*Comparative beta activity of rain, cistern, and stream waters*

[Results in micromicrocuries per liter]

Series dates	Dissolved activity			Total activity		
	Rain	Cistern	Stream	Rain	Cistern	Stream
Nov. 18 to Feb. 9	54	76.0	6.0	130	123.0	0.23.0
Feb. 9 to Mar. 7	152	157.0	52.0	390	269.0	33.0–99.0
July 5–27	1,430	410.0	44.0	2,420	908.0	62.0
July 27 to Sept. 25	800	762.0	32.0–64.0	1,830	1,070.0	62.0–130.0
Geometric means	311	247.0	28.4	689	424.0	54.8
Percent removal compared to rainfall		20.6	91.0		38.5	92.0

RADSAFE EMERGENCY INSTRUCTIONS FOR POPULATED ISLANDS

1. The commander, JTF–7, has designated a representative for each off-site location outside the PPG. For the populated islands near the PPG, the representative is responsible for the radiological safety of the local population and the members of the task force.

2. The representative of the task force commander is provided guidance as follows:

(*a*) The Marshallese magistrate and irou if on hand and the Marshallese health aid and council on each atoll or island should be assured that every precaution has been taken to prevent exposure of the natives to radiation hazards resulting from fallout.

(*b*) The representative will consult with the local magistrate to insure that a method exists whereby all residents of an atoll may be summoned to a central location and evacuated by air or water transportation if a fallout emergency exists. A fallout emergency will be determined by the commander, JTF–7; however, the local representative will assume that a fallout emergency exists at such time as radiological survey instruments, when held at a position 3 feet above the ground, indicate a rate of 1r./hr.

(*c*) Should evacuation by air be necessary, baggage will be limited to that which each individual can carry or approximately 50 pounds. Whether evacuation is achieved by sea or air, no animals will be evacuated. A tabulation of animals left behind should be made as soon as possible to insure the accuracy of claims against the Government.

(*d*) The local magistrate should be informed that in event of an unforeseen emergency, doctors will be flown from the United States by special airlift to care for local inhabitants who will be evacuated to Kwajalein Atoll and that evacuation plans are in existence to permit the task force to cope with any emergency.

(*e*) Fallout of a dangerous nature can be suspected by the presence of a saltlike precipitate or unexpected mist. Should such an event take place, it should be confirmed by monitoring.

3. The representative will arrange through the local magistrate and native health aid to inform the Marshallese of the basic health measures that they may take to protect themselves from danger in case fallout is suspected or confirmed. These measures are:

(*a*) Remain indoors or under cover to protect themselves from the falling or settling radioactive particles.

(*b*) If particles settle on clothing, dust and shake off clothing.

(*c*) Bathe and keep clean. Particular attention should be given to washing under the arms, the groin, face, and hair.

(*d*) Keep food covered to prevent ingestion of fallout particles.

(*e*) Should the readings exceed 5 r./hr. it is recommended that the natives be advised to stand out in the water (ocean) and immerse themselves as often as practicable or keep themselves under water. This recommendation is based on the fact that water does extremely well in attenuating radiation.

(A report of the Radiological Health Branch, Bureau of State Services, Public Health Service, is inserted at this point in addition to the material submitted.)

RADIOLOGICAL HEALTH BRANCH,
BUREAU OF STATE SERVICES,
PUBLIC HEALTH SERVICE,
October 1952.

SUMMARY OF THE REPORTED RADIATION EXPOSURE IN THE UNITED STATES

I. INTRODUCTION

The program of the Radiological Health Branch, Public Health Service, is directed toward preventing the impairment of human well-being from accidental or unwise exposure to harmful amounts of ionizing radiations and toward improvement of health through judicious use of these radiations. This program can best be accomplished through cooperative efforts of the Radiological Health Branch and the State and local health agencies.

In order to pursue such an objective effectively, knowledge as to the sources of exposure of human beings to ionizing radiation in the United States, the relative importance of these exposures, and the total effect of them on the health of the Nation is a necessity.

This paper summarizes the available unclassified published data on current sources of radiation exposure in the United States and their relative quantitative importance. It is expected that the future will bring nuclear reactors for power purposes into widespread use. The quantities of radioactive materials involved will dwarf the presently produced sources of ionizing radiation. The competence of the health profession to cope with the increased problems arising from these new sources of radiation will be determined by the training and experience of its members in handling present radiological public health problems. Now is the time to prepare to meet the health problems of the nuclear-power industry.

II. SOURCES OF RADIATION EXPOSURE

The existing radiation exposure in the United States (exclusive of natural background radiation) originates from the use of radiation-producing machines and radioactive substances in many fields—broadly covered by the terms "the healing arts," "industrial usage," "commercial usage," and "investigative (research) usage."

The following table lists the sources of radiation exposure in the United States about which the Radiological Health Branch has been able to collect information.

Data currently pertinent to radiological health surveys

Radiation source	Cause of exposure	Where found	Persons exposed
X-ray	Healing arts:		
	Diagnosis (radiography and fluoroscopy) and therapy.	Hospitals, clinics, private practice, dentistry, dermatology, veterinary practice, chiropody, osteopathy.	Doctors, nurses, technicians, dentists, patients, persons holding or positioning patients, X-ray unit repairmen, bystanders and persons in adjoining offices or on floors above or below.
	Mobile and portable chest X-ray units; multiphasic testing units.	Trailer stations, community buildings, fairgrounds, etc.	Same as above with additional community participants, volunteer workers, local health department staff, etc.
	Industry:		
	(1) Radiography (permanent mobile and portable units); inspection of welds, castings, pressure vessels, and parts.	Nonferrous foundries, boiler-shop products, aircraft engines and parts, metal-working machines, steel foundries, gray-iron foundries, tires, V-belts, and inner tubes, propeller blades, electric welding apparatus, bronze babbitt, special industrial machinery, general industrial machinery, models and patterns, railroad cars and street cars, carbon and graphite products, motor vehicle, motor-vehicle parts, vacuum cleaners, motor, generator and motor generators, electric lamps, business machines.	Operators, observers, bystanders, persons in adjoining rooms, repairmen.
	(2) Fluoroscopy:		
	Inspection devices:		
	(a) Manufactured products	(1) Tires, V-belts and inner tubes, carbon and graphite products, vacuum cleaners, rubber industry, electrical equipment for industrial use.	Operator, observers, bystanders, repairmen.
		(2) Food-processing and packing plants	Operator.
	(b) personnel and packages	(1) Prisons, factories, defense plants, police, post office.	Operator, "patient," repairmen.
	(3) High-vacuum electronic tubes	Electronic tube manufacturers and users, testing and operation of the tubes.	Testers, observers, operators, bystanders.
	(4) Theater or projection type TV tubes	Theaters	Operator, repairmen, persons near tube face.
	Commercial usage: (1) Shoefitting	Shoestores	Shoe salesmen, customers, clerks, bystanders, repairmen.
	Research:		
	(1) Variations in electric potentials	Industrial oscillographs	Technicians, experimenters, and repairmen.
	(2) X-ray effects and diffraction studies	Research laboratories, educational institutions.	Do.
	(3) Microscopy-electron microscopes	do	Do.
	Accidents	In all phases	Potentially all users.

Data currently pertinent to radiological health surveys—Continued

Radiation source	Cause of exposure	Where found	Persons exposed
Cathode rays	Sterilization:		
	(1) Food	Food industry	Operator, repairmen.
	(2) Drugs	Drug industry	Do.
AEC distributed radioisotopes (does not include radium).	Transportation of isotopes	AEC to user	Carriers.
	Medical application: Diagnosis and therapy	Hospitals, clinics, private practice, pharmaceutical houses.	Laboratory personnel, doctors, nurses, technicians, patients, persons close to patients, persons handling clothing, bedding, etc., of patients.
	Industrial application (partially research):		
	(1) Radiography (cobalt 60) thickness gages, liquid level gages, density meters.	Automobile companies, oil industry, coating industry, steel companies.	Workmen, operators, bystanders, technicians.
	(2) Movable sources, liquid flow through pipes, location of "go-devil".	Rubber industry, pipeline industry, soap companies, laundry equipment manufacturers, coke industry, metallurgy.	Do.
	(3) Tracers, friction, wear, solid diffusion, detergency, mineral flotation, movement of preservatives, role of catalysts, Fischer-Tropsch synthesis, source of coke sulfur.	Industrial laboratories and test sites	Do.
	Research:		
	(1) Animal and plant physiology	Academic institutions, research groups	Laboratory personnel.
	(2) Bacteriology		Do.
	(3) Chemistry		Do.
	(4) Physics		Do.
	(5) Fundamental medical	U. S. Public Health Service (NIH)	Do.
	Isotope chemistry: (1) Preparations of radioisotopes for special uses.	Industrial laboratories	Do.
Non-AEC distributed radioisotopes (cyclotron produced).	Medical, industrial, research applications	Medical and industrial laboratories, colleges and universities.	Do.
All distributed radioisotopes (does not include radium).	Wastes	Air decontamination, soil contamination, water contamination.	Potentially all users, sewage-works operators, plumbers and persons downstream or downwind from user.
	Accidents	In all phases	Potentially all users.
Fission products	Industrial application	Industry	Few at present. (Field should expand.)
	(1) Activation of phosphors.		
	(2) Static eliminators.		
	(3) Fluorescent light tubes.		
	(4) Instruments containing radiation sources.		
	(5) Sterilization of food and drugs.		

Radium	Transportation of source	All sources are portable	Carriers.
	Medical application:		
	(1) Preparation of radon needles	Hospital, clinics	Laboratory personnel.
	(2) Therapy (radon and radium)	do	Laboratory and medical personnel, patients, persons near patients.
	Industrial application:		
	(1) Radiography	Inspection of welds, castings, pressure vessels.	Operation personnel.
	(2) Static eliminators	Coating industry, textile industry, all paper trades, printing, photographic processing, plastic manufacturing, telephone companies.	Personnel nearby machinery, maintenance and repairmen.
	(3) Activation of phosphors	Sign companies	Workers.
	(4) Luminous compounds	Radium dial industry	Workers, bystanders, users of product, personnel handling storage of products.
	Radium reprocessing	Radium industry	Workers.
	Accidents	In all phases	
Polonium	Industrial applications:		
	(1) Static eliminators	Printing shops	Manufacturers and users.
	(2) Film brushes	Film companies	Do.
	(3) Activation of phosphors	Sign companies	Do.
	(4) Spark plugs	Spark-plug manufacturers	Do.
Thorium	Industrial and research applications	Gas mantle manufacturers, research groups (neutron sources), thorium salt manufacturing and compounding.	Manufacturers (research personnel) and users.
Neutron sources (radium-beryllium)	Research application	Research institutions	Workers, handlers.
Radioactive scrap	Contaminated platinum and gold laboratory scrap and equipment.	Precious metals reclaiming companies	Process operators, sweepers, receiving clerks, stock handlers.
Nuclear reactors	Research, educational, industrial, medical, and military applications.	Educational institutions and industry	Persons working around reactors and handling the products.
	Wastes	Air contamination, soil contamination, water contamination.	Surrounding populace.
	Accidents:		
Raw materials for nuclear reactors	Uranium mining and milling, handling and shipping.	Transportation centers and Colorado Plateau.	Mine and mill workers, transporting workers, processing workers.
Particle accelerators	Industrial, medical, and military research.	Educational institutions	Research personnel.
Atomic weapons	Weapons tests	Air contamination, soil contamination, water contamination, food contamination.	Detectable in all United States.
AEC installations	Plant wastes	Air contamination, soil contamination, water contamination.	Environment surrounding various Atomic Energy Commission sites.

NOTE.—This chart represents a revised version of an earlier chart prepared by the Radiological Health Branch. Information furnished by the New Jersey State Health Department was utilized in the preparation of the revised chart.

III. RADIATION EXPOSURES

A. Background radiation

The human race has always been exposed to some ionizing radiation. It is continually being irradiated by radiation from natural sources in the general environment such as uranium, thorium, actinium, radium, radon, and their decay products, from cosmic radiation, and from cosmic-ray induced radioactive materials (1). The aggregate of these radiation sources is known as the natural background radiation. A person who lives to be 70 years of age is exposed to a total of about 9 roentgens background radiation during his lifetime (2).

B. X-ray in the "healing arts"

There are more than 125,000 X-ray units in the United States being used for diagnosis and therapy. Some 50,000 of these units are used by general practitioners, physician specialists, radiologists, and in hospitals and clinics (3). Approximately 65,000 of the units are used by dentists (4). It is estimated that some 11,000 are being used by doctors of osteopathy and doctors of chiropractic (5).

There are more than 215,000 operating personnel potentially exposed to radiation in the use of these X-ray units in the United States. These include some 3,000 radiologists devoting full time to their specialty, 500 physicians devoting most of their time to radiology, 600 physicians who are second- and third-year residents in radiology, 31,000 general practitioners and physician specialists owning their own equipment (3), 67,000 dentists utilizing X-ray equipment (4), and some 11,000 osteopaths and chiropractors using X-ray equipment (5). In addition, there are some 40,000 X-ray technicians (6) and probably close to 50,000 dental technicians and assistants (7) (8) currently potentially exposed to radiation. This listing undoubtedly omits many nurses, clerks, attendants, technicians, and others also exposed more or less often to radiation.

Many instances of excessive exposure of X-ray personnel in the United States are reported in the literature (9), (10), (11), (12). However, few specific data are available regarding average exposures for such workers. In one 9-month survey (13) of personnel in 4 X-ray departments, it was found that about 0.5 percent of the weekly exposures exceeded 0.3 roentgen. However, 97 percent were less than 0.05 roentgen. In a similar 3-week survey (13) of personnel in doctors' and dentists' offices and X-ray departments, 3 percent of the weekly exposures exceeded 0.3 roentgen with 81.5 percent being less than 0.05 roentgen. Surveys of United States Public Health Service hospitals by the Radiological Health Training Section, Cincinnati, Ohio (14), have revealed exposures ranging from 0 to 0.46 roentgen per 2-week periods for X-ray personnel. It has also been reported (15) that an appreciable fraction of radiologists experience exposures on the average of more than 0.1 roentgen per day and that 20 percent of the personnel operating photofluorographic equipment exceed 0.3 roentgen per week (16). For dental X-ray it has been reported (17) that the operator receives approximately 0.1 roentgen of general body exposure per 8 examinations of the mouth, each of which consists of a series of 103-second exposures. This assumes careful operation of the unit. Dental operators conducting mass dental X-ray surveys can easily receive present-day maximum allowable radiation dosages, even with some rotation of operators. Personnel in the immediate area of such dental X-ray units can receive appreciable percentages of daily and weekly maximum allowable limits (18).

In the United States, about 2,500,000 persons are seen each day by physicians. A large number of these people have some X-ray diagnostic procedure performed upon them by the physicians and, in addition, more than 82,000 of them are referred to radiologists (3).

Radiologists give approximately 25 million X-ray examinations annually (3). The following table summarizes the data relative to radiation exposures resulting from these examinations. These data have been obtained by the Radiological Health Branch from the published literature (2), (3), (15), (17), (19) through (28).

TABLE I.—*X-ray examinations*

Type	Average dosage (roentgens)	Relative distribution of each type	Roentgen dosage for average examination
		Percent	
Radiographic	2.7	51.88	
Photofluorographic	1.0	33.64	11
Fluoroscopic	65.0	14.48	

Probably the largest single contributing source to the total medical radiation exposure in the United States is the mass chest X-ray survey for tuberculosis. An estimate for 1950 as to the number of persons given chest X-rays in such surveys would be about 15 million (29). A portion of the 25 million X-ray examinations given annually to persons seen by physicians is probably included in the 16 million chest X-rays because of inherent overlap of the reporting procedures. Most of the X-rays given in the mass survey are of the photofluorographic type. This type of examination results in about 1.0 roentgen exposure to the chest of the patient (15) (21).

More than 4 million X-ray *treatments* are given annually to the 82,000 persons referred daily to radiologists (3). These treatments are confined as a rule to a very small portion of the patient's body. The average dosage received is of the order of 6,000 to 7,000 roentgens per treatment (30) through (34).

It has been estimated that in 1949 60 million persons (40 percent of the population) in the United States visited their dentists (8). Some 84 million roentgenograms are taken annually for dental X-ray purposes (35). The average exposure to the mouth of the patient is about 5 roentgens per film (36). Total body irradiation is a fraction of this amount.

C. X-ray in industry

Industrial X-ray devices include primarily (*a*) radiographic and fluoroscopic units used for the determination of defects in castings, fabricated structures, and welds, and (*b*) fluoroscopic units used for the detection of foreign material as in packaged foods.

It is estimated that there are at present approximately 800 active industrial *radiographic installations* in the United States (38). The total number of X-ray *units* (both radiographic and fluoroscopic) in use in industry is probably about 2,000 (40).

In a survey of 61 industrial radiographic X-ray units in Ohio (37) it was found that some 400 persons were potentially exposed. Applying this ratio to the 800 active industrial radiographic installations in the United States (assuming 1 unit per installation), it can be estimated that some 5,000 persons are potentially exposed to radiation from industrial radiographic installations in this country. Numbers of personnel exposed in the use of industrial fluoroscopic units are not known.

The hazards of X-radiation in industry are due to the high intensities employed, the frequency of operation (11), and the use of "homemade" or makeshift equipment or equipment originally designed for other purposes. It is not unusual for one radiographic installation to expose a thousand films in 1 day (39). Energy levels for radiographic installations range up to greater than 1,000 kilovolts. The use of makeshift equipment is especially hazardous in industrial fluoroscopy (39). However, this does not mean to imply, ipso facto, that all other fluoroscopy units are absolutely safe regardless of the manner of their operation.

Exposure levels for personnel in industry depend upon the installation. Most installations were designed and the personnel assignments planned so as to limit personnel exposures to the levels recommended in the National Bureau of Standards Handbook current at the time the equipment was built. However, the continual revision downward of maximum permissible exposure limits (currently 0.3 roentgen per week) calls for a reevaluation, by survey, of the older installations.

In the use of fluoroscopy for the scanning of personnel, such as at prisons, exposures of 0.045 to 0.09 roentgens per inspection may be received by the "patient." In addition, the unit operator may receive about 0.1 roentgen to the head and shoulders for each 50 persons inspected (11).

It has been reported (45) that during the manufacture, testing, and operation of high-voltage electronic tubes measurable amounts of potentially harmful X-rays were produced. In one industrial situation studied the exposure to the operator was found to be as high as 2.5 roentgens per day (45).

Theater- or projection-type television tubes have also been reported as sources of X-radiation.

D. Commercial use of X-ray

In the use of fluoroscopy in shoe-fitting, mean exposures from 7 to 14 roentgens per 20 second exposure (an average setting) have been reported (41 (42 (43). Although exposure is intended for the feet of the customer only, dosages of 0.03 to 0.17 roentgen per 20 second exposure have been reported as being received by the pelvis (44). The number of exposures received by shoe customers is not known. It is estimated that the 30,000 to 40,000 persons (shoe salesmen, clerks, and bystanders) are exposed chronically in the operation of the some 10,000 shoe-fitting fluoroscopes in the United States (42) (43).

E. X-ray in research

With the development of atomic and nuclear physics, high voltage X-ray machines have become familiar features of the average laboratory found at universities and similar institutions.

Few data are available as to the levels of exposure received by personnel in such radiation laboratories. Injuries have probably been held to a minimum by frequent turnover of personnel under university laboratory conditions. However, in such laboratories where cyclotrons, linear accelerators, and positive ion tubes, as well as high voltage X-ray machines, have been used, it is estimated there has been a frequency rate of 1 palpable injury per 20–30 man-years of active employment in radiation work (46).

There are about 1,500 X-ray diffraction units in the United States (40). Radiological Health Branch surveys of these units have recorded intensities of scattered radiation up to 1 roentgen per hour (14). There have been several reports of skin ulcers resulting from accidental overexposures in the use of this type equipment (39).

Many research laboratories today use the electron microscope. There are approximately 500 in use in the United States. Several authors (47) (48) (49) have presented papers on the exposure associated with radiation from these units. Intensities of scattered radiation ranging up to 1.5 roentgens per hour have been reported (47).

F. Radioisotopes (excluding radium)

More than 900 universities, hospitals, and research laboratories in the United States have or are using radioactive isotopes produced by the United States Atomic Energy Commission for medical, biological, industrial, agricultural, and scientific research and medical diagnosis and treatment (50) (53).

In a survey (51) of the available radiobiological research and training facilities throughout the United States and Canada, of 153 institutions covered, more than 1,300 staff members and 800 graduate students were engaged in work with isotopes. In 76 of the institutions, isotopes were being used in patients for research, diagnosis, and therapy. It has been estimated that a total of some 7,500 persons are currently engaged in work utilizing radioisotopes in the United States (52).

Of the users of radioisotopes, on the average, about 1 in 300 exceeds the present day maximum permissible radiation exposure (0.3 roentgen per week) and 50 to 75 percent receive less than 0.05 roentgen per week.

During 1950, an average of 45 curies of radioactive isotopes were distributed per month (52).

1. *Medical.*—Radioisotopes are used medically for diagnosis and therapy. Patients to whom these radioisotopes are internally administered may receive 10 or more roentgens whole body exposure from diagnostic doses and from 75 to 100 roentgens from therapeutic doses (54). Single organs such as the thyroid gland may receive from 10,00 to 300,000 roentgens total tissue dosage (55).

It has been recommended as a public health precaution that "patients who receive large doses of I^{131} or Au^{198} should be hospitalized until the total residual activity in the body is not over 30 millicuries" (56).

Radioisotopes are also used in medical therapy as external sources of radiation. Beta ray applicators are available for the treatment of certain eye conditions. Cobalt 60 sources are available in the form of large shielded concentrated

sources for deep therapy and in the form of small needle sources for intracavitary and interstitial therapy.

2. *Industry.*—Cobalt 60 is being used industrially for radiography. There are currently about 80 sources being used in the United States in such industries as railroads, steel, boiler, automotive, ceramics, pressure vessels, and castings (57). The intensity of radiation from 1 curie of unshielded cobalt 60 at 1 foot distance is 14.4 roentgens per hour (50). The quantities used in industry range from one or two hundred millicuries up to as high perhaps as 1 curie (58).

Thickness gages, using radioisotopes, are becoming more and more popular in industry. There are more than 50 utilizing strontium 90 and some 20 utilizing other radioisotopes presently in use in the United States (60). Surveys have shown that the external radiation to which personnel working around the units are exposed is well below permissible limits (61).

Strontium 90 is also being used as a source of ionizing radiation in luminous paint.

These and other radioisotopes are also being widely used in a variety of industrial research problems.

Wastes: Wastes from the use of radioisotopes in industry, the medical profession, and research laboratories, could cause radiation exposure to persons outside the installations using the radioisotopes. Safe disposition procedures have been well covered in official publications (62) (63). Estimates as to the adequacy of disposition methods presently in practice vary (64) (65).

G. Radium

The radiation from 1 curie of radium, inequilibrium with its decay products, and enclosed in 0.5 mm. of platinum, will produce a gamma ray exposure of about 9 roentgens per hour at a distance of 1 foot (66).

1. *Medical.*—In the medical use of radium, the technician, therapist, patient, and persons near the patient (other patients, nurses, and attendants) are exposed.

Since radium seldom can be applied to the patient remotely with accuracy, radium technicians and therapists often receive relatively large exposures. It has been reported in England that the local exposure to their hands in many cases exceeds 1 roentgen per day (67). Other exposure occurs in the preparation and handling of radon capillaries.

It has been reported that the average daily exposure in a typical teleradium installation in England ranged between 0.13 and 0.25 roentgen (68).

In radium therapy, patients receive radiation dosages comparable to those given in X-ray therapy.

2. *Industry.*—(*a*) Radiography: In 1948, approximately 50 grams of radium were being used for radiography in the United States (69). During World War II, this figure reached 100 grams (69), largely due to the fact that X-ray units were difficult to obtain. At present, much of the radium-radiographic work that was done during the war has been discontinued and X-ray machines and cobalt–60 are being used instead.

Radium sources are commercially available in 25, 50, 100, 200, 300, and 500 milligram units. The 100 and 200 milligram sources are most commonly used (69).

Average exposures received by industrial personnel handling radium are not known.

(*b*) Luminous compounds: Several hundred grams of radium were utilized in the luminous compound industry in the United States during World War II (73). During this period there were several thousand workers engaged in the use of self-luminous paints. After World War II, the number decreased and in 1948 there were only about 300 people engaged in this work in the United States (69).

Although individual workers handle only small quantities of radium daily, the hazards to health from this use of radium are significantly greater because it is not sealed in a container and, therefore, can be ingested or inhaled.

Under present conditions, it is recommended that no worker exceed a maximum permissible amount of 0.1 microgram radium fixed in the body (70). Under the best working conditions existing in 1943 in the radium dial painting industry, 15 percent of the workers accumulated more than the maximum permissible amount (71). It has been reported (72) recently that a survey in a military-aircraft-instrument shop, which followed the safety regulations of the National Bureau of Standards, found a degree of radium contamination greatly in excess of the maximum permitted.

It is generally accepted that the maximum allowable level for radon in the air is 10 micromicrocuries per liter (46). It has been reported that when workroom ventilation requirements are met, the radon concentration in the workroom air will not exceed 30 percent of the maximum permissible limit (71). However, ventilation requirements are not always met, especially in storage and packing rooms and offices.

The normal gamma radiation exposure received by dial painting workers appears to be about 0.02 roentgen per day (46).

Exposures may also occur in the use of the finished product to which a luminous compound has been added. A watch may have approximately 1 microgram of radium on it. Some clocks and aircraft instruments may contain from 10 to 100 micrograms of radium (74). It has been reported (74) in surveys in airplanes that the level of exposure at the instrument panel was 0.01 roentgen per hour and from 0.0002 to 0.001 roentgen per hour at the pilot's body position.

(*c*) Reprocessing and preparation of radium and radon sources: Radium preparations that have become obsolete in either design, or use, or the seal of which has broken or worn thin, should be reprocessed. This work involves hazards similar to those encountered in the subdivision of radium into capsules for medical or industrial use. The work is performed by relatively few companies. However, the work has not been governed by general regulations of the type applied to the luminous compound industry. A radon concentration of 2,200 micromicrocuries has been quoted as average in the general laboratory of one of the most reputable companies (46).

The operation of radon plants, commercially or in hospitals, can be very hazardous since the worker is exposed not only to radon but also to beta and gamma radiations of the radium and its decay products.

(*d*) Static eliminators: Static eliminators are widely used in textile and paper trades, in printing and photographic processing industries, and by telephone and telegraph companies.

One type, the ionotron, consists of a bar containing a strip of metal impregnated with radium. A thin layer of gold and nickel are plated over the radium-metal strip to protect the radium and act as a seal.

The main hazards from the ionotron are exposure to beta and gamma radiation (alpha constitutes little external hazard) and radon gas.

Actual exposures of 0.003 to 0.005 roentgen per hour for pressmen and 0.5 to 1.0 roentgen per hour for maintenance personnel have been reported in a survey of a printing plant (75). Fortunately the latter duties required only about 15 minutes per day of such exposure.

In other surveys, levels up to 0.085 roentgen per hour have been reported in the working area near such units (69).

The radon hazard from ionotrons is small, if they are given proper care. However, if the seal is broken, a radon hazard may result. Several surveys have pointed out that improper storage and handling of static eliminators is common (76).

A second type of static eliminator, the alphatron, contains polonium as the radioactive source. These units constitute little external radiation hazard since the alpha particles from polonium travel only a short distance in air. The hazards associated with their use result primarily from ingestion or inhalation of polonium liberated through breaking of the gold seal and flaking (77).

Polonium will volatilize at lower temperatures than radium and careful consideration must be given this point in certain specialized applications (77).

Static eliminators are also used with analytical balances and microtomes.

Polonium bars are also mounted on brushes for use as a static charge eliminator for phonograph records and photographic films.

Lost radium: Numerous instances of radium being lost have been reported in the newspapers. Taft (78) has reported on 107 losses with 59 complete recoveries, 11 partial recoveries, 36 total losses, and 1 unknown result. There is danger of unsuspected radiation exposure in all such instances.

Shipping of radioactive materials: Exposures can occur during the handling and shipping of radioactive materials. Interstate Commerce Commission and Post Office Department regulations governing the shipment of radioactive materials by domestic ground and water transport and the interim regulations for shipments by air attempt to accomplish three objectives:

(1) protection of people,
(2) protection of film,
(3) avoidance of excessive shielding weight (excessive carrying charges).

It has been reported that the maximum exposure which an airplane crew member or passenger would receive under the regulations would be 0.012 roentgens per hour. Maximum exposure for pilots (flying 85 hours per month) would then be 1.02 roentgens per month (74).

H. Nuclear reactors

A "water boiler" (79) type nuclear reactor is being built at the North Carolina State College. Data on this reactor can serve as a tentative guide in establishing the importance of such installations with regard to the radiation levels in the United States.

The North Carolina reactor is to operate at a maximum of 10 kw. Heavy shielding will limit the exposure, resulting from a maximum of some 105 curies of activity present, to a safe level.

The heat generated during operations will be removed by circulating water through sets of cooling coils inside the reactor cylinder. This water, from the Raleigh city system, will be fed into the reactor after passage through automatic pressure reducing-regulating valves; at 10 kw. power level a total of 3 gallons per minute will be required.

The intense neutron bombardment received by the coolant in passing through the reactor will induce radioactiivty in it (principally in the dissolved and suspended solids). The resultant activity will be near 1,000 disintegrations per second per cubic centimeter initially. Assuming no shielding and no internal absorption of radiation by the water, 10 gallons of freshly irradiated water would produce a radation dosage rate of approximately 0.08 rotentgen per 8 hours a distance of 5 feet. After 1 hour, the dosage rate would drop to about 0.0008 roentgen per 8 hours at a distance of 5 feet. Tanks for collecting and retaining this waste water for 10 hours are provided.

The reactor in operation produces a small volume of gaseous fission products, resulting from the fission of uranium into elements of gaseous nature, and larger volumes of other gases resulting from the decomposition of the water molecules in the fuel solution into hydrogen and oxygen. The activity of the fission product bases will be considerable, amounting initially to about 7,000 curies per kilowatt-minute. However, after 4 hours, the 7,000 curies will have decayed to 0.15 curies. The activity of the decomposition gases will be negligible. At 5 kilowatt operating level, the total volume of gases produced will be 40 liters per hour. Solid as well as other liquid and gaseous wastes will result if laboratory or experimental programs are conducted in conjunction with operation of the reactor.

It has been reported that at least five major universities have expressed interest in following the steps of North Carolina State College by building research reactors outside of Atomic Energy Commission sites (81). Undoubtedly, other reactors will soon be built at other colleges and universities and in industry.

A second type reactor is being built by North American Aviation, Inc., in California (80). It too should serve as a guide in establishing the importance of such installations in the total radiation exposure in the United States. The reactor is to be of the enriched-uranium, graphite-moderated, type and will be used for research. It will be operated at a designed capacity of 160 kilowatts and will be shielded by 6 inches of steel and about 3 feet of "heavy" concrete (total weight 450 tons). Data as to the wastes to be produced by this reactor and resulting radiation exposures have not been released.

I. Particle accelerators

In 1941, there were only some 16 cyclotron laboratories in the United States (12). It was reported that due to the tremendous cost of particle accelerator units (namely cyclotrons, synchrotrons, Van de Graaff generators, and betatrons), their number was not apt to grow to more than a few dozen (82). However, the impetus to nuclear research created by the atomic energy program has caused their number to increase to over 100.

For example, 20 machines came into operation in 1950 and 21 in 1951 under auspices of the Atomic Energy Commission (83). One company reported, in a recent advertisement, that it had built 15 Van de Graaff Electrostatic Accelerators at research installations in the United States (84).

Exact determination of the type and intensity of radiation encountered around particle accelerators is often difficult or impossible because of the mixture of radiations present (85). Beta radiation originates from the various accelerators but the possibility of direct exposure is slight (85). Neutrons probably constitute one of the main hazards as they are produced in profusion in the operation of cyclotrons and synchrotrons (82).

Impaired vision of several nuclear physicists as a result of work with cyclotrons was reported recently (86). The general injury rate for laboratory radiation workers was discussed under the portion of this paper covering "X-Ray in Research."

J. AEC activities

Activities of the Atomic Energy Commission in the fields of fissionable material production and weapons testing contribute to the level of radiation to which special groups and the general public are subjected. Specific items are:

1. *Uranium mining and milling.*—The uranium mining and milling activities in this country are predominantly restricted to the region of the Colorado Plateau. Some 2,000 miners and millers are engaged in this work (87). It has been reported that the mining and processing of the ores and metals yield dusts and fumes which are sources of radioactive air pollutants (88). The United States Public Health Service and several State agencies are active in studies of the specific hazards in this industry (89). They have reported finding radon exposures above the maximum permissible limit in several of the mines. In these instances, control measures are being applied as rapidly as possible.

2. *Operation of nuclear reactors.*—Operation of nuclear reactors within the Atomic Energy Commission contribute to the magnitude of the radiation exposure in the United States. It has been reported that the use of air for cooling purposes in nuclear reactors is a source of radioactive air pollutants (88). The use of water for cooling purposes at Hanford contributes radioactivity to the Columbia River.

3. *Other AEC plant wastes.*—Chemical dissolving and separation processes and the operation of "hot" laboratory hoods plus the incineration of contaminated solid materials (such as experimental animals) are all sources of radioactive air pollutants (88).

Since the operations producing these radioactive air pollutants are widespread, the potential hazards are by no means always confined to those directly employed in such activities. The discharge of radioactive gases, dusts, and other radioactive pollutants outside the immediate area of operation may greatly increase the radius of the possible health hazards (90).

The chemical dissolving and separation processes also contribute liquid wastes. It has been reported that the Oak Ridge installation of the AEC discharges up to 5 curies per day of liquid wastes (91).

4. *Weapons tests.*—In 1951 12 bombs were dentonated at the AEC Proving Ground in Nevada.

The fission product activity 1 hour after the dentonation of a nominal atomic bomb is in order of 10^9 curies. One week later, there are still about 10^7 curies remaining (92). Fortunately the majority of this activity probably remains in the upper atmosphere. However, some of it is distributed over most all of the United States. For example:

(*a*) Radioactive snow has been reported falling over wide portions of the United States following these tests (93).

(*b*) The background count in many radiation laboratories increases by a factor of 5 to 10 during these periods, necessitating in many instances the curtailment or complete stopping of counting activities.

(*c*) Film manufacturing companies must take precautions to prevent fogging of their film during these tests (94). In the 11th semiannual report of the AEC, it was stated that the photographic industry must specially treat water used during these periods due to contamination from fallout (83).

K. Other factors affecting exposure levels

1. *Availability of radioactive materials.*—Under the law, the Atomic Energy Commission carefully controls the distribution of radioactive materials produced in its operations. However, naturally radioactive materials are not subject to similar regulations. As a result, certain products containing small quantities of naturally radioactive materials may be purchased on the open market. For example, brushes for use as static charge eliminators (polonium), and instruments with luminous dials (radium). The high cost of large quantities of radium (such as are used for industrial radiography and medical therapy) and the care exercised by the radium companies in their distribution tend to eliminate them from the "open market" classification.

2. *Fission products.*—Only about 4 curies of fission products had been distributed by the Atomic Energy Commission through 1950; the rate of distri-

bution is expected to increase considerably within the next few years. Suggested uses for fission products include:

(*a*) Activation of phosphors.
(*b*) Static eliminators.
(*c*) Fluorescent light tubes.
(*d*) Instruments containing radiation sources.
(*e*) Industrial radiography.
(*f*) Cold sterilization of food and drugs.

L. Accidents

Accidents appear to be an implicit complication of all activities involving man. There is no reason to believe that his activities in the radiation field will differ in this respect. In fact, numerous instances of radiation injury have been recorded in the literature (39) (86) (97) (98) (99). In addition, based on private conversations, it is believed that only a small fraction of the accidents which do occur are ever recorded in the literature.

Any radiation exposure from accidents would be in addition to the exposures listed elsewhere in this article. Obviously, the potential amount of exposure and the probable severity of injury incurred in an accident would vary with the amount of radiation involved.

IV. COMMENTARY

A summary of the published literature available to the Radiological Health Branch and pertaining to the sources and levels of radiation exposure in the United States has been presented.

Definite conclusions concerning the relative public-health importance of the several categories of radiation sources in the United States should not be drawn from the data presented since the amount of the published literature in each field largely determined the data available.

The public-health importance of individual sources will vary for different localities. Furthermore, this relationship is constantly changing. For example, many radiation-minded health people feel that, in the future, use of nuclear reactors in research, medicine, and industry will be the biggest public-health problem.

This paper is intended to serve as a guide to the State and local health officer in seeking out, evaluating, and controlling the radiation affecting the public health in his area. If it is of assistance in initiating a radiological public-health program at the local level, it will have served its purpose.

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(*b*) Mr. B. Duane Moen, director, bureau of economic research and statistics, American Dental Association.

(5) Calculated from data supplied in private communications to:

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The committee will stand in recess.

(Whereupon, at 12:30 p. m., the committee recessed, to reconvene at 2 p. m., of the same day.)

AFTERNOON SESSION

Representative HOLIFIELD. The committee will be in order.

There will be a slight change in order of the witnesses by agreement. We will ask Dr. Western to be the first witness. Dr. Forrest Western of the Division of Biology and Medicine of the Atomic Energy Commission will be speaking on the subject of delayed fallout, the behavior in geological and physical processes and the mechanisms by which delayed fallout enters into the biological processes and reaches man.

Dr. Western, we are happy to have you with us.

STATEMENT OF DR. FORREST WESTERN, DIVISION OF BIOLOGY AND MEDICINE, ATOMIC ENERGY COMMISSION[1]

Dr. WESTERN. Thank you, Mr. Holifield. I believe at this point what I will say will mark a transition in the general nature of the discussions from a discussion of the largely physical and chemical processes which are involved in the production of the radioactive material and the factors which enter into the biological considerations. So this would start the item which on your outline is marked "Topic VIII." I believe this is the correct number. In topics VIII and IX we will plan to cover this entire subject, and my remarks perhaps should be considered in the light of introductory remarks to the papers which will follow.

Representative HOLIFIELD. Dr. Lyle Alexander, then, will take topic IX?

Dr. WESTERN. Topic VIII, I believe. I believe this is the correct numbering, according to the outline, with Dr. Eisenbud, Dr. Kulp, and Dr. Revelle on topic IX.

Before I start my remarks, I would like to do two things. First I would like to express my personal pleasure at the manner in which the hearings have been conducted until now. I have enjoyed very much the questions which you and others have asked the various witnesses. They have at times been very penetrating and very searching. At the same time they indicate a much greater understanding of the highly technical detail that is involved here than one normally finds on the part of people who are not routinely engaged in studies of this kind. I hope that what I have to say will add to the general understanding of the subject.

I have prepared a brief statement which does review a number of things which have already been discussed and which does mention a number of things which will be discussed.

Before I start my prepared statement, I would like to ask permission to submit to you for introduction into the record some statements by Dr. Norman S. McDonald, of the atomic energy project of the University of California at Los Angeles.

At the invitation of your staff, Dr. McDonald has submitted these statements, but I believe he submitted them through us or asked that we submit them for him rather than submitting them directly.

If you wish I shall be glad to identify the parts. If not, I will be glad to give you the copy.

Representative HOLIFIELD. Will you please identify the parts at this point.

Dr. WESTERN. Dr. McDonald has identified his statements in relationship to the outline for the hearings which had been provided.

His first statement has to do with the possibilities for water treatment for the removal of strontium. This is topic VIII.

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[1] Date and place of birth: August 25, 1902, Purdin, Mo.; education: bachelor of arts, Central College (Missouri), 1924; master of arts, University of Missouri, 1926; doctor of philosophy, University of Wisconsin; University of Pittsburgh, 1933; work history: Lincoln Memorial University, Harrogate, Tenn, professor of physics and head of physical sciences, 1935–42; University of Minnesota, lecturer (physics), 1942–44; Tennessee Eastman Corp., senior physicist, April 1944 to September 1944; Fercleve Corp., Director Laboratory No. 1, 1944–45; Oak Ridge National Laboratory, Assistant Division Director, 1945–51; AEC, Washington, D. C., health physicist, Division of Biology and Medicine, 1951—. (Submitted by the Atomic Energy Commission.)

His second statement has to do with the calcium model as a basis for predicting strontium 90 behavior. This is identified in the outline as topic IX (C).

His third statement is on the deposition in man, variations of strontium-90 level, which he identifies as topic IX (D).

His fourth statement, predicted occurrence from weapons test fired in 1957, topic IX (F).

In addition, there is a group of 11 references upon which some of the statements made are based.

Representative HOLIFIELD. Without objection Dr. McDonald's statement will be inserted in the record. (See p. 720.)

The Chair might state that similar treatment of our subject matter has been requested from a number of scientists who found it impossible because of their duties to attend these hearings. We felt that this would be a matter of supplementing the oral testimony and would give us a complete coverage of the subject as we can get at this time.

You may proceed, sir.

Dr. WESTERN. Thank you.

In these hearings on The Nature of Radioactive Fallout and Its Effects on Man, previous witnesses have discussed the nature of radioactivity and of radiation, and the production of radioactivity in large quantities in the fission process—both in the operation of nuclear reactors and in the detonation of nuclear weapons. The movement of the debris from nuclear weapons has been traced from the time of formation until it is deposited on the surface of the earth.

We are now entering a discussion of the processes by which fallout deposited on the surface of the earth enters the human food chain, either as the result of direct deposition on the surface of vegetation or in the normal uptake of the various chemical elements by vegetation from the soil. In these hearings, our principal interest in considering the movement of radioactivity from nuclear weapons through the human food chain is that of relating quantities of fallout to possible radiation effects in humans.

However, in the event of nuclear warfare, as evident from the discussion this morning, we will be interested also in the possible destruction of animal life, particularly of those animals which represent a major source of food to surviving members of the population.

In the case of local fallout, Dr. Dunning has discussed factors which relate the fallout to levels of radiation from fission products on the ground and some of the factors which determine the sizes of the doses of radiation which may be received by persons in areas of local fallout.

In this connection, he has observed that, during early periods of time after the fission products are produced; the principal hazard to man to be anticipated from exposure to fallout is from radiation originating in radioactive materials outside the body.

Both our knowledge of the extent to which fallout materials may be expected to enter the body by inhalation and ingestion, and observations of actual human uptake of radioactive materials under conditions of heavy local fallout, indicate that for weeks and, perhaps months, after the detonation of a nuclear weapon, the inhalation and ingestion of fallout material will be less important than radiation from fallout in the immediate vicinity outside the body. The length

of time for which this might continue to be true under conditions of actual warfare would depend upon such factors as the protection against external radiation afforded by shelter, upon precautions taken to avoid dust in air used for breathing, and upon sources of food and water.

At this point I would like to depart from my prepared text and discuss in a little more detail the considerations here.

In the inhalation of radioactive material we are interested in radiation exposure to the lungs from material which may be introduced into the lungs and remain there for some length of time. We are interested in radiation to the gastrointestinal system from materials which are brought up from the lungs by normal physiological actions, swallowed, and passed down through the gastrointestinal system.

In this connection we are particularly interested in the large intestine, since, as the material goes down, we have progressive concentration of radioactive material in the material in which it is contained. This results from removal of water from the feces as they pass through the colon.

A second interest which does not depend upon the gross activity of the material which is swallowed but upon the activity of specific radioisotopes contained in this, is the concentration of radioiodine in the thyroid and the consequent irradiation of the thyroid.

This is of particular interest because the thyroid is a very small organ. It is an organ of 20 grams, which is somewhat less than an ounce. While the amount of radioactive material which enters the thyroid is not large, the radiation dose due to the relatively high concentration is comparable to that which would be experienced by the lungs and that which would be experienced by the large intestine. These are all doses which at times for a week or so after the detonation are perhaps of the order of 10 percent of the dose which one would expect to receive from outside the body.

In this connection, if I may, Mr. Chairman, I would like to discuss a little bit farther the question which was raised in Dr. Dunning's discussion this morning about the comparative hazards due to radiation from radioactive materials within the body and radioactive materials outside of the body.

We have no reason to think that any tissue of the body recognizes whether or not the radiation which passes through it comes from within the body or from outside the body. Our interest in making these comparisons is simply in comparing radiological doses to the various tissues.

Perhaps I should go back and explain more precisely the two types of quantities in which we are interested.

In speaking of irradiation of the body, our primary interest is in the absorption of energy of radiation by the tissue itself. This, of course, is related to the amount of radioactive material from which the radiation is emitted. If we take radioactive material into the body, as I believe you pointed out this morning, it is concentrated in various tissues of the body. I have mentioned the case of concentration of radioiodine in the thyroid. We will discuss at considerable length during the hearings the next day and a half the concentration of strontium in the skeleton. These are all normal physiological processes.

When we speak of a dose of 1 roentgen, as in the case of X, or gamma radiation to a tissue of the body, we are not talking about the amount, the total amount, of energy that is absorbed by the body, but we are talking about an intensity of absorption—an absorption per unit of mass. So when a body is subject to a whole body dose of 1 roentgen, we mean that each portion of the body also receives 1 roentgen of radiation. If I expose 1 finger to 1 roentgen, that applies to the finger only. If I expose 2 fingers to 1 roentgen, each finger gets as much as before. We are not dividing the dose between the tissues but we are talking about the intensity of the dose to the tissue. I think this may have been a point of confusion this morning.

Representative HOLIFIELD. I am glad you cleared it up. Let me further try to clarify it in my own mind. Does the 1 roentgen have a different effect on different organs, tissues, or skeleton of the body?

Dr. WESTERN. I would like to say at this point that those of us who discuss these subjects come from many different disciplines of science. I personally am a physicist. I might say that up until this point practically all of the people who have spoken are from the physical sciences—physicists, meterologists, and so on—and while all of us feel we know as much about medical effects as the next man on the street and like to talk about them, we hesitate at times to answer questions of this sort.

I have, however, been spending the last 10 years in trying to understand the medical and biological points of view as well as the physical points of view in problems of this sort, and I will give you tentative answers to the biological problems which I am discussing with the reservation that as you go along you will check my answers against those of medical specialists who will appear later in particular connection with these questions. I give you the answer at this point only for your guidance in following the discussion.

It is true that various tissues have various degrees of radiosensitivity. In this respect, our biomedical people will tell you that the blood-forming organs of the body are among the more sensitive. Perhaps there is no point in enumerating them at this time. Of the ones which I mentioned, the lungs and thyroid are generally considered to be less sensitive than others. I might point out, and this is something which you can readily understand, that in talking about how serious a large radiation exposure is we are interested not only in the sensitivity of the organ but we are also interested in how vital the organ itself may be to the functions of the body.

For example, a man can get along very well without a thyroid. If his thyroid is completely ablated by radiation, he can still under normal conditions at least proceed to get along fairly normally with daily administration of dessicated thyroid or thyroxin. However, this may or may not be true after a nuclear war. Persons with inadequate thyroids would be in serious difficulty if they had no way of supplying the deficiency.

On the other hand, bone cancer has a high probability of being fatal. So when we are talking about the relative hazards due to radiation, we are interested not only in sensitivity of tissue, but we are also interested in the functions of the organ that is involved and the nature of the injury.

Let me say very briefly, then, as Dr. Dunning was saying this morning, in measuring the relative hazard to various tissues of the

body, if we neglect for the moment differences in sensitivity we do get a rough measure by considering the total radiation dose that a particular tissue might get.

Let me come back to the specific question that was asked this morning.

If the whole body is given a dose of 100 roentgens, this means that the skeleton as well as other portions of the body receive the same dose. You will say I am oversimplifying the question because the dose cannot be uniform throughout the body, even from penetrating gamma radiation, because there is some absorption and the surface gets more dose than the skeleton does, and so on, so when we talk about a uniform dose of 100 roentgens this is, in effect, an idealization. But nevertheless it conveys a concept.

If the skeleton only receives 100 roentgens from material within the skeleton, this is the same dose to the skeleton as before as far as the physical factors are concerned. From the biological point of view, one may say that the impact on the body is somewhat less than it would have been in the first case because it is perhaps better for only the skeleton to have received the dose than for the whole body to have received the dose.

There are other differences, however, which biologists may discuss. The question arises as to whether or not the biological effects of a certain amount of energy absorbed from beta radiation are exactly the same as those from the same amount of energy absorbed from gamma radiation outside of the body. This is the relationship to which Doctor Mills referred yesterday as the relative biological effectiveness of various amounts of radiation. We may say in this case we are talking about finer points. Let me say, for example, without making an authoritative statement, that the differences we are talking about are less than would be represented by a factor of 2. I would say considerably less, but I am just trying to give an order of magnitude here.

You asked also this morning about the relationship of a sunshine unit to dose to the skeleton. The statement which I am going to give you is taken from a study made by the National Academy of Science in their report, "The Biological Effects of Radiation," which they issued last year and with which you are familiar. There it was estimated that the radiation dose from 100 sunshine units in the body is equivalent to a lifetime dose of approximately 20 rep. The energy absorption from this dose is equivalent to that from 20 roentgens.

When we are talking about sunshine units in the body, we are talking about a ratio of strontium to calcium. If I may use the units, 1 sunshine unit equals 1 micro-microcurie of strontium 90 per gram of calcium in the skeleton. By 1 micro-microcurie I mean one-millionth of one-millionths of 1 curie. I think the curie was defined for you yesterday as 3.7 time 10 to the 10th disintegration per second of radioactivity. We are not talking about how much radioactivity is in the body. We are talking about how much strontium 90 is maintained in the skeleton at a uniform concentration for a lifetime when we make this comparison.

I should say further for clarification that we are not really interested in the fact that the material is in the body. As I will point out later, strontium itself is a natural constituent of the body. We have in the body something of the order of seven-tenth of a gram of strontium 90. When we are talking about a sunshine unit, for example, I am not in a

very good position to tell you how much we mean compared to seven-tenths of a gram. If I were to start dividing this up into smaller and smaller fractions I would have to take about one-hundredth of one-millionth of one-millionth of a gram of Sr 90 in order to describe a sunshine unit. This is a very small number. We are talking about quantities of material which in themselves are far to small to be of importance to the issue. We are interested only in the fact that the atom emits radiation which is absorbed in the tissues.

I would like to discuss also another portion of your question this morning. You asked, does not the fact that this is stretched out over a lifetime mean that it is more hazardous than if this all occurred at one time? This is a question to which we can't give an answer We are talking about a radiation dose which may be received over a lifetime. At the very low doses in which we are interested here we are not in a position to say whether or not it makes any difference to the individual whether the dose is received in a very short period of time by the tissue or whether it is spread out over a very long time. We do for acute effects, as was discussed this morning, observe tissue repair. At the levels we are discussing now we cannot say whether the dose is more hazardous or less hazardous if it is given in a short period of time.

Returning to the remarks I have prepared:

After longer periods of times, months or years, hazards which may be presented by the residual radioactivity contained in the fallout are primarily due to fission products which may be taken into the body by inhalation or by ingestion. This change in relative hazard of fallout inside and outside of the body follows from changes in the composition of the residual radioactivity with time. In the present discussion, I shall review briefly some of the physical, chemical, and biological factors which determine the relative imporatnce of the various radioactive materials produced by nuclear weapons and the various conditions under which one may be more important than another.

I apologize to you for the repetition contined in these next two pages, but I will go ahead and read them because they do summarize some of the things we have been discussing earlier.

If we examine the atoms of the chemical elements, as they exist in nature, we find that in some cases the atoms of a particular chemical element are all identical. In other cases, the atoms of particular elements can be classified into two or different groups on the basis of differences in the weight or mass of the atoms.

Most of the chemical elements have nonradioactive atoms of at least 2 different weights, and some have as many as 7 or 8. The atoms in a particular weight group are said to be an isotope of the chemical element. The literal meaning of the word "isotope" is "in the same place." It is used to express the idea that the atoms are different but occupy the same place in the periodic chart of chemical elements.

An example of some of the isotopic and chemical properties of interest in a study of fallout is afforded by the chemical element, strontium. Although strontium occurs throughout nature, and was discovered more than 150 years ago, until a few years ago it was perhaps best known as the chemical element used to produce a brilliant red color in fireworks. Aside from this, its principal use has been in the refining of beet sugar. The normal quantity of strontium in the skeleton is

about 0.7 grams, about 0.1 percent of the calcium content of the skeleton.

As strontium occurs in nature, it consists of four isotopes, strontium 84, strontium 86, strontium 87, and strontium 88. The numbers used to designate these isotopes indicate the approximate weights of the atoms in the various groups relative to the lightest atom in nature, that of the common isotope of hydrogen, H–1. In other words, the mass of an atom of strontium 88 is 88 times that of an atom of hydrogen 1. The atoms of all of these isotopes are stable; that is, not radioactive.

The half-dozen isotopes of strontium resulting from nuclear fission are all unstable or radioactive. All of them undergo radioactive transformation into isotopes of the chemical element, yttrium. This process of radioactive transformation is usually referred to as radioactive decay, and the atoms resulting from the transformation are called decay products. Three of the radioisotopes of strontium 90 resulting from fission decay so rapidly that, in a fallout situation, they will have become isotopes of yttrium before the fallout reaches the vicinity of personnel and are consequently of no interest here. Our interest in the isotopes of yttrium into which they decay depends upon whether or not the atoms of these isotopes are radioactive and upon other factors considered later in this discussion.

The atoms of a particular isotope decay at such a rate that a definite fraction of the total number of atoms will undergo transformation in a specified period of time. The time required for half of the atoms of a radioisotope to undergo radioactive transformation is called the radioactive half-life of that isotope. Radioactive half-lives may vary greatly from one radioisotope to another. Here I am speaking of radioisotopes in fallout or in the original fission product produced by the bomb—some are as short as a fraction of a second, and some are as long as millions of years.

Even though one cannot say just when the last atom of a particular quantity of a radioisotope will be transformed into an atom of another, the large number of atoms involved in any measurable quantity of a radioisotope does make it possible to state the average lifetime of the total number of these atoms. In each case, the average lifetime of the atoms is approximately 1.4 times their half life. This relationship has led to confusion from time to time because sometimes we speak of half lives and sometimes we speak of average lifetimes. Unless one distinguishes between the two there is an apparent discrepancy of this factor of 1.4.

Similar language is frequently used in discussing such matters as the length of time which the atoms of a particular chemical element may be retained in the human body, or the length of time which the debris from a high-yield nuclear weapon may be retained in the stratosphere. For example, it is sometimes said that the half life of strontium in the human skeleton is several years. Here I am talking of a number of the order of perhaps 7, 8, or 9 years. I prefer not to state it more precisely than that. This concept of a half time is valid only to the extent that the process to which it is applied involves a rate of decrease proportional to the quantity present. In the case of strontium and calcium in the skeleton, while it appears that this may not be strictly true, use of the term half life or half time gives a rough idea of the quantitative relationships involved.

Returning to a consideration of the relative hazards of the radioisotopes of strontium, we observe that strontium 91 has a half life of 10 hours, strontium 89 a half life of 53 days, and strontium 90 a half life of 28 years. About as many atoms of one as of another are produced in fission. For example, in a nuclear reactor, about 5 percent of the fissions give rise to strontium 89, 5 percent to strontium 90, and 5 percent to strontium 91.

In giving these numbers let me say I am using very roughly approximate numbers. These numbers may vary from about 5 to 6. In fission processes that involve different energies of neutrons and different fissionable materials, there is also some variation. So these numbers are sufficiently accurate for purposes of this illustration.

In a nuclear weapon the fractions are generally not greatly different. If we were to introduce large quantities of these radioisotopes into the human body in the relative proportions in which they are produced in fission, the strontium 90 would be the least hazardous of the three, and strontium 91 which is seldom mentioned in connection with fallout, would be the most hazardous. I will put it on this basis: I have as many atoms of strontium 91 as I have of strontium 89 or strontium 90. The atoms of strontium 91 would decay with a half life of 10 hours, or the average time they would spend in the body would be about 14 hours. These atoms would deliver to the skeleton after deposition—deposition would take place within a matter of an hour or two—all of their energy of disintegration in a relatively short time. Half of it would have been delivered after about 10 hours.

On the other hand, the atoms of strontium 90 would remain in the body until they either underwent radioactive decay or until they were eliminated by natural processes. These processes are both very slow but perhaps a third or more of the atoms would be eliminated before they decayed at all.

So the total dose delivered to the skeleton by the strontium 91 would be larger than the total dose delivered to the skeleton by strontium 90.

There are other differences which I will not discuss in detail. Strontium 91 has a somewhat more energetic beta ray than strontium 90, although it is comparable. Strontium 91 is also a gamma emitter, whereas strontium 90 is not. In addition to the dose delivered to the skeleton, strontium 91 would also deliver a sizable dose to the gonads, to the blood-forming organs, and other tissues of the body.

However, in an actual fallout situation, the strontium 91 is of interest only for a day or so, and then primarily because of the contribution which the gamma radiation it emits makes to the total radiation emitted by radioactive materials outside the body. By the end of 3 days, 99 percent of the atoms of strontium 91 will have decayed into yttrium 91. Yttrium 91 is also radioactive, but the chemical properties of yttrium are such that a much smaller fraction of its atoms enters the human body and a still smaller fraction is retained in the body. I say "enter" here in the sense of being absorbed into the body in the case of inhalation or in the case of ingestion. Yttrium, if ingested would pass readily through the gastrointestinal system with a much smaller uptake by the digestive processes than of strontium 90.

Similar considerations apply in comparing the relative hazards due to strontium 89 and strontium 90. In a local fallout situation, for several weeks after fallout the strontium 89 would constitute a hazard

equal to or greater than that of the strontium 90. This would be true either for the inhalation of fallout or for the ingestion of fallout retained on surfaces of vegetation or as an impurity in other sources of food and in water. By the end of about 6 months there is less than 10 percent as many atoms of strontium 89 as of strontium 90, and the relative hazard is correspondingly small.

In the case of long-range fallout, following the storage of bomb debris in the stratosphere, it is clear that our interest is in radioisotopes of which the half lives are measured in years rather than in days. Under these conditions, strontium 89 is, of course, of no concern.

Before entering into a more detailed discussion of relative physical and chemical characteristics, I would like to make it clear that our ultimate interest in the atoms of any particular radioisotope depends only on the radiation dose which they can give to the body. It is meaningless to say that one radioactive material is more hazardous than another except as, under a specific set of conditions, it can deliver a larger radiation dose to the same part of the body or a comparable dose to a more critical part of the body. For example, if we are considering the biological effects of beta emitting material uniformly dispersed through the mineral structure of the skeleton, we are not specifically interested in whether a given quantity of radiation is emitted by a radioisotope of barium, or of calcium, or of phosphorous, or of strontium, or of some other element of which the chemical properties are not inconsistent with the concept of uniform dispersal in the bone. We have every reason to believe that under such a condition, the biological effects produced by a given concentration of strontium 90 in the bone would be no greater or no less than those produced by that concentration of calcium 45 required to give the same dose of radiation to the bone.

After an oversimplified statement of this sort, I realize that I am subject to an argument which is not a primary argument in considerations of this sort. But because it illustrates a point which I would like to make, I will give the argument here.

We are talking here about radiation of the bone—beta radiation—from two different isotopes. When I speak of quantity of radiation I am speaking primarily of the amount of energy absorbed by the bone—the amount of energy which goes into ionization of the tissues of the bone—and consequently of the number of ions that may be formed in a tissue.

As a second order effect, it is true that one can detect very small differences under some circumstances between the biological effects of one beta particle which produces a certain density of ionization, and another which produces a slightly different density of ionization. The average density of ionization depends upon the average speed with which the beta particle moves through the tissue and that in turn depends on the initial energy of the particle.

I am making an oversimplified statement. I can be challenged on the statement.

This gives rise to the impression that I do not know what I am talking about. I had written a qualifying statement of this sort in the discussion. It was suggested that the idea is too complex to put into a discussion of this sort, and so I left it out. But in view of the nature of the things upon which people generally agree in a discussion of this sort, and the nature of differences of opinion, I do believe it is valuable

at this time to point out that in discussing a problem as complex as the one which is being discussed here in these next 2 or 3 weeks, that one must consider the importance of areas of agreement and areas of disagreement.

I am sorry that Senator Anderson is not present. Senator Anderson in the opening session asked about opinions of scientists which are diametrically opposite. So far as I know, in this field there are no opinions of scientists which are diametrically opposite. We disagree on many things. If we stop to analyze the areas of disagreement in general they are secondary issues. I think, if I might use a very homely illustration, I am reminded of a couple of men determining whether or not they would prefer to own a Cadillac or a Chrysler. The man who wants the Chrysler would say, "Look at those beautiful fins!" The man who owns the Cadillac has some beauty, too. But the men who owns the Chrysler can point out that the Chrysler Corp. has mentioned recently that at 90 miles an hour these fins give a certain amount of road stability. These people have diametrically opposite ideas about these automobiles, but I think both would agree that both the Chrysler and the Cadillac are good automobiles.

So when we talk about disagreement, I think we have to look at the manner in which the disagreement arises and consider whether or not the scientists who are disagreeing are disagreeing on their pet theories or whether they actually have fundamental disagreements as far as the overall picture is concerned.

Chairman DURHAM. What you are saying today, doctor, was true a thousand years ago in physics.

Dr. WESTERN. Yes. I am not speaking as a scientist here at all. This is commonsense.

Chairman DURHAM. You certainly talk like one.

What I was trying to point out is that the disagreement is no greater than it was a thousand years ago in theoretical physics.

Dr. WESTERN. No. One does not need to go very far in history to find disagreements of a similar nature. You will recall, Mr. Durham, I know, that it has not been very many hundreds of years ago since people felt it was extremely dangerous to sleep with the windows open. You may remember in much more recent history there were people who wanted to outlaw bathtubs for certain reasons. You can think of a great many instances in history in which we have had these very serious disagreements.

Chairman DURHAM. I was not taught strontium-emitting rays when I was dispensing drugs over the drug counter.

Dr. WESTERN. Yes; you undoubtedly dispensed uranium over the drug counter, too.

Chairman DURHAM. Yes.

Dr. WESTERN. We will pass on.

Chairman DURHAM. Yes.

Dr. WESTERN. Our interest in general fallout, as distinguished from local fallout, originated in an effort to evaluate hazards to general populations as a result of large-scale nuclear warfare.

I would like to say at this point that we were interested in this problem several years ago. My first connection with this problem was when I came with the Atomic Energy Commission in 1951. But there were already studies underway preceding that by at least 2 years which had indicated very generally the sort of thing which I am

discussing today. The studies had not gone nearly as far as we have carried them in the last 5 years, but they had outlined the general principles involved here.

With the important exception that vastly greater quantities of fallout would be involved and the hazards would consequenlty be vastly greater, conditions of fallout on noncombatant populations in warfare would be very similar to the general fallout from testing of nuclear weapons.

Under these conditions, we are interested in conditions in which the longer lived radioisotopes from fallout are present in the soil and on vegetation and, in varying concentrations, in food, water, and air.

Depending upon their respective chemical properties, quantities of the various radioactive isotopes in the soil will enter the normal metabolic processes of vegetation, and quantities of the various radioisotopes in or on vegetation will enter into the normal metabolic processes of animals and humans. In this respect, it should be observed that plants and animals do not distinguish between radioactive isotopes and nonradioactive isotopes of the same chemical element. Some of the biochemical factors involved will be illustrated by discussion of strontium as an example.

In this connection of not distinguishing between radioactive and nonradioactive isotopes I would like to mention some misconceptions which have occurred in popular statements during the past year or two. There is a feeling, for example, that some of the tissues of the body have the property of concentrating radioactive materials. These properties of which we are speaking are natural properties of the tissues.

Of course, the skeleton concentrates calcium, and because of the similarity between strontium and calcium, of course, the skeleton concentrates strontium. If the strontium and calcium contain some radioactive strontium, that of course would be concentrated to the same degree. At least the radioactive strontium will be concentrated to the same degree as the nonradioactive strontium in the skeleton.

Chairman DURHAM. Do you care, Doctor, to say what in your opinion creates that affinity?

Dr. WESTERN. It is not an affinity. The question is more fundamental than that. Why is calcium an essential constituent of the human body or the plant? It is one of the essential biological constituents. If we admit the human having a skeleton, it must be composed of certain minerals. The only way that the skeleton can obtain these minerals is to concentrate calcium and phosphorous from the various sources of food and water that the animal takes in.

In the case of strontium 90, the chemical properties of strontium, including strontium 90, are so nearly the same as those of calcium that one has difficulty in separating the two. The chemical properties are so nearly the same that the strontium atom can fit into the same position in the crystal structure of the bone as the calcium can. I believe this is also true of crystal structures in nature. If you look at a calcium compound in nature and in a natural crystal you will find here and there a strontium atom occupying a position which otherwise might have been occupied by a calcium atom. The apparent chemical affinity of the skeleton for strontium 90 is just a combination of two facts: The essential need for calcium in the process of

building the bone and the fact that strontium is so closely similar to calcium chemically.

There are other essential needs for calcium in the diet, as you know, but some of our biomedical people could discuss this problem at considerable length.

In addition to radioisotopes of strontium, we may be interested in one or more of the several other radioisotopes, including iodine 131, barium 140, cerium 144, plutonium 239, and cesium 137. Consideration of the physical and biochemical properties of other radioisotopes in fallout makes it very evident that these radioisotopes are of much less interest than strontium 90 except for periods of less than a few months after they are produced. From a given detonation, the relative number of atoms of a radioisotope entering the metabolic processes of the body will depend upon the following factors:

1. The relative number of atoms of the radioisotope formed in the fission process. The percentage of fissions giving rise, directly or indirectly to a particular radioisotope, is called the fission yield of that radioisotope. The fission yield of strontium 90 is about 3 percent or 4 percent. I used earlier a figure of 5 percent, but I did mention it varies from 1 process to another. It is somewhat less, I believe, in many of our nuclear weapons than it is in the reactor fission. This is due to several reasons.

Chairman DURHAM. The yield in the reactor process has a higher content of strontium 90?

Dr. WESTERN. Generally, yes, by a factor of less than 2.

The highest fission yields are about 6 percent.

That applies to, I believe, fairly well either the yields in a reactor or a nuclear weapon. This is an order of magnitude.

2. The principal method by which fallout enters the body is in the diet. Some fallout enters the diet as a result of retention on the surface of vegetation, without entering the soil. For example, a very little of the cesium 137 in the human diet is believed to come by way of the soil. I make this statement with some reservation. I believe this will be verified in the remarks which will be made either by Dr. Langham or Dr. Anderson tomorrow or Monday depending upon which day this is given.

Most of it comes from meat and milk, as the result of retention on the vegetation eaten by the animal with some possible contribution from surface water used by animals.

The extent to which any particular radioisotope will enter the diet, then, may depend upon several factors. Many radioisotopes, such as the rare earths and plutonium—the rare earths include yttrium, which we discussed earlier, and cerium 144 discussed in the first part of this paragraph—are taken up in comparatively small amounts from soil by plants; and also in small amounts by animals from the diet. As a result, quantities ingested by humans are very small, and the quantities assimilated by the human are still much smaller.

Take the extreme example of plutonium. In the case of inhalation, it is considered perhaps one of the most hazardous of the elements with which we work. Perhaps a considerable fraction, several percent, of the plutonium inhaled, if it is inhaled in sufficiently small particles, is retained in the lung for some time. It may be dissolved in the blood stream and a fairly good fraction deposited in the skeleton.

But when we are talking about plutonium on the ground, as orders of magnitude, perhaps the ratio of uptake of plutonium as compared to strontium by plants from the soil is something of the order of 1 to 1,000 or 1 to 10,000. When the human or animal digests the food we have a similar ratio of about the same order of 1 to 1,000 or 1 to 10,000. So if we had equal portions or equal numbers of atoms, let me say, of plutonium and strontium 90 in the soil, we would get roughly about a million times as much strontium in the skeleton as we would plutonium.

Chairman DURHAM. But suppose that cow had a dose of strontium 90—say 300 roentgens—and I drank the milk, then where do we go from there? Of course, it is a small amount we are talking about now that is assimilated into the digestion of the cow. Let us reverse it and give her a big dose.

Dr. WESTERN. What we are interested in is amounts which could be considered hazardous. I noted from the discussion this morning about fallout in milksheds and so on that there is a considerable amount of misunderstanding as to the importance of milk in determining the hazard of fallout to humans. As far as I am aware, people who get their calcium from milk are in at least as good a position as people who do not get their calcium from milk. This may be discussed in more detail by subsequent speakers.

But since you raised the question, let me mention that the human has to have calcium to exist. If the human has no milk, he gets his calcium from other sources. He gets it from vegetation. If there is strontium accompanying the calcium—strontium 90—in the vegetation, he gets the strontium. He may not have as much per gram as from milk, but he will eat enough vegetation to supply his calcium needs.

Following the process of uptake of strontium 90 through milk, the milk is in effect a barrier to the uptake of strontium 90.

To some extent, at least in the animal, and possibly in the plant, there is what we call discrimination between strontium and calcium. We have indicated that strontium and calcium are very alike chemically. We have also indicated that there are differences. I said it is difficult to separate calcium and strontium, but of course we can do it. We are talking here about two things which behave as they do because they are very much alike, but also have sufficient differences to behave differently.

In carrying calcium and strontium from plant to the milk, the cow appears to discriminate against calcium by a factor—let me just give an order of magnitude and it may be larger or smaller—of $\frac{1}{10}$. By this I mean to say that the ratio of strontium to calcium in milk is less—there is less strontium 90 or natural strontium, it doesn't make any difference—per gram of calcium in the milk than there is in the food which the cow eats.

Representative HOLIFIELD. Is that discrimination against the element or is it lack of availability of one element as compared to the other?

Dr. WESTERN. No. Suppose we consider the natural food of the cow. Let us take an American example. Roughly in the vegetation which the cow eats there is perhaps 1 atom of strontium for 1,000 atoms of calcium. Let us use larger numbers. Let us say 1,000 and 1 million. The cow, however, secretes into the milk a smaller ratio of strontium-calcium. For each million atoms in the milk there may

be only of the order of 100 atoms of strontium rather than 1,000 which one finds in the cow's diet. So one is better off by drinking the milk than by eating the forage which the cow eats. Of course, one does not eat forage, but what I mean to say is that if you get your calcium from vegetation you do not have this barrier in between the vegetation and your own diet.

Representative HOLIFIELD. But by the same token when the children and babies drink more milk than eat hay they get the strontium quicker, do they not?

Dr. WESTERN. I ought not to get into this type of discussion.

Representative HOLIFIELD. Let us leave the detailed discussion until later on.

Chairman DURHAM. Are you saying, Doctor, in the process of the cow assimulating through ingestion and the machinery of producing milk that if she had a dose of 300 roentgens that would not necessarily mean that there would be 300 in the milk.

Dr. WESTERN. We need to go back and talk about quantities of strontium 90 which in a cow would produce perhaps to the skeleton a radiation-dose equivalent to 300 roentgens. I am saying this. If one assumes, as a first order of magnitude, that the deposition of the strontium-calcium in the skeleton of the cow is the same as that of a human—this is only an approximation because I think one could expect, for example, that in a cow giving large quantities of milk one is going to get differences in the metabolism as compared to other animals—if one makes this assumption, then what I have said is equivalent to saying that if this amounts to a dose of about 300 roentgens in the skelton of a cow in a specified period of time it would amount to about one-tenth of that in the human who drinks the milk in the same length of time. This is because the cow is in effect shielding the person who drinks the milk from a certain amount of the strontium which he would have gotten if he obtained his calcium from vegetation instead of milk. This is a real effect.

I oversimplified the thing again because I promised myself I would not get into these biological discussions and I have friends back here in the audience who are having fits back here because there is another factor which I have not considered and that is that the actual deposition in the skeleton of strontium appears to depend upon whether you take it from milk or vegetation. This is something which I am neglecting altogether.

Let me disclaim any expertness in this field at the moment. I tried to give you a little bit of guidance as to what I mean.

At least let us say that as far as the elements are concerned, the fact that the child drinks the milk does not constitute any greater hazard than if he got his strontium and calcium from other sources, and to my mind it constitutes a lesser hazard. The cow is actually a protection.

Chairman DURHAM. I am assuming if the cow does not get a big dose.

Dr. WESTERN. Here we are talking about a matter of degree. Of course, when you start into a discussion of this sort you begin to say "if." Let us assume that what we are talking about is that the child is eating vegetation from the garden and that the cow is eating vegetation from a pasture across the fence. So we have comparable situations here. Then all we are talking about is what is the relative

hazard to the child of taking the calcium and strontium from the milk or from the vegetation.

Representative HOLIFIELD. The only thing I can draw from that, Doctor, to bring it down to a practical understanding of the layman is this. What you have said has amounted to this: The cow in eating the pasturage would retain a certain amount of strontium in her own skeleton, but that there would be a certain amount of it in the milk and it would be transferred into the diet of the infant. At the same time, if there was lettuce or radishes or things like that growing in the garden that had a direct deposit of strontium and the child ate that, it would get a bigger dose comparatively speaking by getting it direct without having it go through the process of the milk machine.

Dr. WESTERN. I think this is true.

Representative HOLIFIELD. But nevertheless we cannot draw any comfort from the fact that children do drink a lot of milk and eat very few vegetables directly and therefore they would get the maximum amount of this material probably from milk because that is the major part of their diet at early ages, at least.

Dr. WESTERN. May I make another remark?

Representative HOLIFIELD. Is that too oversimplified?

Dr. WESTERN. May I make this remark which I think bears on your statement. This may be a little bit abstruse but I believe it is worth stating.

When you say we cannot draw any comfort from this, let me say that the thing that we are interested in from the point of view of hazard to the individual is the amount of strontium 90 which we find in his skeleton. Let us consider a child—my own son for example, or my own daughter—who has a certain amount of strontium 90 in his skeleton. This is my primary consideration: How much does he have now? How much will he have? It makes very little difference to me, except as it enables me to predict in the future, how it got there. My fundamental interest is how much. I don't draw comfort from the fact that he got it one way or from another, or I don't say unfortunately this and this is true. We are relating a certain amount of strontium 90 in the skeleton to a certain amount that has been produced in weapons tests. It is this relationship which is fundamental regardless of the method.

This is another case of the sort of thing which I mentioned earlier, it doesn't make any difference whether two scientists disagree as to how it got there. The proof we are interested in the point of biological evaluation is that it is there.

Representative HOLIFIELD. I recognize that. The only reason I have tried to simplify it was that I have known times before when statements were made and immediately in the press or some public utterance a statement was made based on that, that we do not have to worry about milk because it has such a little amount of strontium in it compared to vegetables. I thought I was trying to bring it down to a practical matter of at least my understanding.

Dr. WESTERN. I agree with you completely in that. In any case we are interested in the amounts which are getting into the skeleton of the human.

Representative HOLIFIELD. You may proceed.

Dr. WESTERN. 3. Most of the radioisotopes resulting from fission are so unstable that by the time the fallout reaches the human they

have been largely transformed into stable isotopes, and are of little or no interest.

4. Finally, the biological effect of a particular isotope will depend not only upon how much enters the body but upon the distribution and retention in the body. If it is concentrated in a relatively small portion of the body, the radiation energy absorbed by that portion will be correspondingly higher. This is true, for example, in the case of radioiodine in the thyroid which consists of only some 20 grams of tissue. Because of the small size of the thyroid, the amount of radioiodine needed to destroy the thyroid by radiation is relatively small. If a large fraction of the atoms of a radioisotope are eliminated from the body before they undergo radioactive transformation, the radiation dose is correspondingly smaller. This is true, for example, of cesium 137, which has a half-life of 27 years but spends only about 200 days, on the average, in the body.

A survey of the many radioisotopes produced by fiission discloses that under current fallout conditions, because of some combination of the factors just enumerated, relatively small fractions, compared to strontium 90 and cesium 137, can reach the body. If the elapsed time is much smaller, as in the case of local fallout, some of the shorter half-lived radioisotopes become more important, especially strontium 89, with a half life of 52 or 53 days; iodine 131 with a half life of 8 days; and barium 140, with a half life of 13 days. At still earlier times, iodine 133, with a half life of 22 hours, may contribute an appreciable fraction of the dose to the thyroid. However, under these conditions, both experience and theoretical studies indicate that the critical consideration is the exposure of the body to gamma radiation from fallout outside the body.

The rare earths, such as cesium 144 and plutonium, are not only absorbed from the gastrointestinal tract of the animal and human to a much less extent than are iodine, strontium, and cesium, but their uptake by vegetation is very low.

The biochemical behavior of cesium in the body is very similar to that of the relatively abundant chemical element, potassium. Perhaps as much as 50 percent of the cesium 137 in the diet is assimilated and distributed through the soft tissues of the body. The average length of retention in the human body is about 200 days.

The evaluation of the relative importance of strontium 90 and cesium 137 is made difficult by the fact that the effects of exposure of the whole body to radiation, either from cesium within the body or from gamma-emitting materials outside the body, are different from the effects of exposure of the skeleton to the radiation from strontium 90. In the case of whole-body exposure of a large population group to radiation, one is interested in the increase in the rate of genetic mutation as well as the possibility of an increase in the incidence of leukemia, while in the case of a radioactive material in the skeleton the principal consideration is the possibility of an increase in the incidence of bone cancer. One cannot expect to find very much agreement between various individuals as to the relative importance of these very different effects. These questions are scheduled for discussion in these hearings at a later time.

Representative HOLIFIELD. Thank you very much, Dr. Western. You have given us quite a comprehensive comparison of the different radioactive isotopes which can be taken up by the body in this field,

at least, and I am sure this will add a great deal to our understanding of the importance of the different ones.

I believe we have one question here.

In the last paragraph in your discussion, in the comparison between radioactive particles being emitted within the body and outside of the body, there was no implication that strontium 90 is not associated with an increase of leukemia as well as bone cancer, was there?

Dr. WESTERN. That is a question which should properly be asked later. It was my intent to imply, as a matter of guidance, that the effect which I would expect from strontium 90 would be bone cancer. The probability of leukemia would be, relatively, sufficiently small to be of no concern. This is a question that should be asked of the biomedical experts.

Representative HOLIFIELD. Thank you very much, Dr. Western.

Our next witness will be Dr. Lyle Alexander from the Department of Agriculture. He will continue the discussion on delayed fallout and the behavior in geological and physical processes and the mechanisms by which delayed fallout enters into the biological processes and reaches man.

Dr. Alexander, we are happy to have you with us today. Will you proceed, please.

STATEMENT OF DR. LYLE ALEXANDER, UNITED STATES DEPARTMENT OF AGRICULTURE[8]

Dr. ALEXANDER. Thank you, sir.

Representative HOLIFIELD. Are you making this presentation on behalf of yourself, Mr. Reitemeier, and Mr. Seymour?

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[8] Born at Athens, Tex., in December 1903. Received the B. S. degree from the University of Arkansas in 1928. That same summer he joined the U. S. Department of Agriculture (Bureau of Chemistry and Soils) as a junior soil physicist. He completed the Ph. D. (in chemistry) at the University of Maryland in 1935 while continuing laboratory research in the Department. Researches in soil science became increasingly broad and significant in both the laboratory and in the field. He was steadily advanced to research positions of greater responsibility. In 1938 he represented the Department at an important international conference on soil chemistry in Finland. After 1945 he had responsibility both for the basic soil research in the Division of Soils, Fertilizers, and Irrigation, and for the service and research laboratories of the Division of Soil Survey of the Bureau of Plant Industry, Soils, and Agricultural Engineering. He continued, however, highly significant personal research in soil science, including pioneer work on the use of radioactive isotopes. In 1951 he headed a successful international mission of scientists, under the auspices of the OEEC of the ECA, for the detailed study of laterite formation in the soils of Africa. His responsibilities for cooperative work with the Atomic Energy Commission increased. In 1950 he received the Superior Service Award from the Department.

Near the end of 1952, the Soil Survey was transferred to the Soil Conservation Service and he continued as Chief of the Soil Survey laboratories and as the Department's principal scientific liaison with the AEC. Except for broad project planning, others took over the details of the work on radioactive isotopes in what is now the Soil and Water Conservation Research Branch of ARS, while he gave increased attention to highly classified research for the Atomic Energy Commission. An important phase of this included researches dealing with the effects of radioactive fallout following bomb explosions, which have required many overseas study missions and field researches.

Besides these researches, he strengthened and reorganized the Soil Survey laboratories to serve the expanded soil survey program in the Soil Conservation Service. He has greatly improved the methods for measuring soil permeability and especially for interpreting the results. He is giving unusually valuable counsel to the leaders within the Service on all phases of soil chemistry and plant nutrition that relate to soil use and conservation. In 1954 he was advanced to his present position as soil scientist, GS-14.

Member and leader in many scientific societies, including the Sigma Xi, the American Chemical Society, the Soil Science Society of America, the American Society of Agronomy, of which he is a fellow, and the International Society of Soil Science, of which he is an officer. Vigorously applied his unusual gifts for sustained, arduous study—in the laboratory, in the field, and in the library—to highly difficult and complex scientific problems. He cooperates easily and effectively with other scientists in his own and related fields, and with nonscientists as well. He has an exceptionally deep sense of his responsibilities as a scientist, as a research administrator, and as a public servant. As a result, he not only carries along and finishes an enormous amount of work of the highest quality person-

Dr. ALEXANDER. Yes, sir. We will cover the rest of your No. VIII in your outline, except for that portion to be covered by Dr. Revelle.

On behalf of Dr. Reitemeier and Dr. Seymour, I wish to express my appreciation for the opportunity of discussing some of these questions involved in radioactive fallout with your committee.

DEPOSITION AND MIGRATION IN SOIL AND TRANSPORT BY SURFACE WATERS

The chemical and physical properties of strontium are closely related to, but systematically different, from those of the essential plant growth element calcium. Hence, a consideration of the fate of the fission product strontium 90 in the earth's geochemical and physical processes becomes a study of the behavior of calcium and the degree to which the heavier strontium ion may lag behind its lighter counterpart.

In a similar manner, the behavior of cesium 137 is related to that of the quite common essential plant growth element potassium, which has a naturally radioactive isotope, potassium 40. The rare earths, on the other hand, have no vital role to play in the nutrition of plants or animals, so far as we know. Since strontium 90, cesium 137, and the rare earths comprise nearly all of the long half-life fission products that might be of concern to man, a consideration of their behavior and that of the fissionable element plutonium will give the picture of the longtime fallout problem.

Calcium in the soil occurs in two forms. The important one to plants and to animal and human nutrition is the fraction that is attached to the surfaces of soil organic matter or humus and of the clay particles of the soil. This is the so-called exchangeable calcium or fraction that can be replaced by other positively charged ions. It is this fraction that gets into man's foodstuffs through plant and animal systems. The other fraction of the soil calcium is found in relatively insoluble minerals and is not of immediate importance to us.

Strontium 90 that has fallen at considerable distances from bomb bursts has been mostly water soluble or has become soluble on contact with most soils. The strontium 90 ions in solution immediately react with the exchange complex of the soil. As a result the strontium 90 is attached to the soil in such fashion that it cannot be readily leached by water but can be taken up by the plant. The newly at-

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ally, but also stimulates and helps others to work more effectively throughout the Federal Government and in other research institutes. He is in great demand from many agencies as a counselor in several scientific areas. Outstanding contributions have been so many that only a few of the principal ones can be outlined.

A so-called radioactive growth stimulant being offered to farmers was thoroughly tested by Dr. Alexander in a large group of experiments having unusually precise control on several kinds of soil. By proving the material valueless and by promulgating the results (against great pressure) he protected American farmers from a highly active promotional scheme.

Dr. Alexander organized a very fruitful basic research program on the use of radioactive materials in research dealing with soils and plant nutrition, conducted jointly by the Department and the Atomic Energy Commission. He gave immediate leadership to the work for about 5 years. During this time elaborate scientific laboratories, greenhouses, and equipment were developed and installed. Ways were found for using and for measuring precisely levels of phosphorus, potassium, and several other nutrient elements in soils and plants. These methods are now widely used in many research institutes throughout the world and have been critically important to the solution of many heretofore unsolved problems of great significance in soil fertility.

Recently Dr. Alexander has been studying the distribution of radioactive fallout from nuclear explosions. This has involved an unusual combination of detailed knowledge of the chemistry and radioactivity of a whole group of elements, including especially strontium, calcium, and cobalt; of the morphology, chemistry, and distribution of soils; and of the chemical relations between soils and growing plants under different conditions. (Submitted by U. S. Department of Agriculture.)

tached strontium 90 ions become a part of a reservoir of calcium and strontium ions that are almost alike to the plant—not necessarily identical but certainly similar.

If the strontium 90 becomes a part of a large pool of exchangeable calcium and strontium ions, the fraction of it taken up by a plant root is less than if it becomes a part of a small number of ions from which the plant may draw its nutrients. We know of no mechanism by which these exchangeable calcium and strontium ions of the soil can become fixed in a form unavailable to plants. The addition of calcium ions to acid soil increases the size of the pool of calcium and strontium and hence decreases the number of the strontium 90 ions taken up by the plants. This is the basis for methods that have been suggested for decreasing strontium 90 uptake by liberal use of lime on acid soils. At best, these methods may reduce the uptake of strontium 90 by a fewfold.

Calcium and strontium do not readily move downward in the soil by leaching with rainwater. When the water carries appreciable quantities of calcium or hydrogen ions, strontium 90 does indeed move downward but the rate of movement is slow. It is impractical to move strontium 90 from the root zone of our crop plants by a leaching process.

Representative HOLIFIELD. The point you are making there is that once strontium 90 is deposited on our soil where plants and vegetables are grown, that it is very hard to leach it and get rid of it?

Dr. ALEXANDER. Yes, sir. I will mention later some of the tillage practices that will minimize the effect. But it is there and it is difficult to get it out. You can't expect rainwater to leach it out.

Cesium and potassium differ materially from strontium and calcium in their chemical behavior in the soil. In addition to the categories of available and mineral latice ions attributed to strontium and calcium in the soil, there is a third condition or state that falls between these two so far as the plants are concerned. In this condition the cesium and potassium ions are neither readily available to plants nor are they completely inaccessible to them. Depending on the level of potassium in the exchangeable form, some of these ions may become available to the plant. In soils containing certain types of silt or clay particles, the potassium may become a part of a mineral in which it is unavailable to plants and cannot be leached from the soil. While we do not know as much as we would like to know about the behavior of cesium in soils, it seems reasonable that it would be more tightly held than the potassium. And this does indeed seem to be the case. Plants take up but little cesium from the soil and it is difficult to remove it from the soil by replacement with another ion.

It would appear, then, that cesium 137 falling on the soil in amounts equal to the strontium 90 will be a lesser constituent in the plants that are used by man and animals for food and feed.

The rare earths and plutonium are so tightly held by soil and so little taken up by plants that they will be of slight or no concern to man or animals by entry through roots. It has been shown that some plants—particularly trees—can take up and differentiate between some of the rare earths. However, the total amounts involved are so small that they are of no importance in the fallout problem.

If we accept rainfall as the principal agent for depositing fission products on the earth's surface, one conclusion is that the deposition

in a given area is uniform—it falls on the just and the unjust alike. Since soil movement is one of the main processes by which the land is formed, and man accelerates this movement by cultivating and overgrazing the soil, it is inevitable that part of the fallout deposited in many areas will be moved into lower lying positions. These soils formed at the foot of slopes and in stream bottoms may accumulate larger amounts of fission products than fell on them directly. In the case of thin sheet erosion and subsequent deposition in a lower lying position, the concentration could be many times as high as the area has received on the average through direct fallout. As pointed out above, the elements with which we are concerned are rather tightly held by the soil, and where the soil goes, they go. For the same reason, water that has moved through the soil will have had most of its long-lived radioactive contamination removed.

On the other hand, fission products in water might move through relatively large underground channels in rocks for considerable distances before they were absorbed in the channel walls. For example, as at Arco. Inland lakes that have no exits must accumulate fission products to the extent that these are carried in the water and sediments of the entering streams. Because of the low content of fission products in the irrigation water the amounts entering the Salton Sea at the present time are very small.

While the downward movement of long half-life fission products that we are discussing is slow, there are differences in rate of movement that reflect the capacity of the soil to hold such elements in the ionic state. Sandy soils, for example, will generally have deeper penetration of strontium 90 than will the finer textured silt loams and clays. Mechanisms for penetration of fallout into soil below the surface few inches are the earth mixing due to earthworms and the development of large cracks in some soils during dry weather. When the rains come, surface-soil material flows into the cracks and carries the surface-deposited strontium or cesium downward. In most of our soils that have not been cultivated since 1953, the bulk of the strontium 90—say 75 percent—is found in the upper 2 inches of soil.

The foregoing statements give a basis for conducting a program of evaluation of the total amount of fallout that occurs in a given region. The sampling sites are so situated that they receive no runoff water from higher ground and lose no rainwater by runoff. This requires a permeable soil that is nearly level, and that has enough clay or organic matter to hold on to the strontium ions that reach the soil surface. It is desirable that the soil be in a grass cover, although farm soils that are in rotations involving corn, small grains, and hay crops have proved quite satisfactory.

THE EFFECT OF AGRICULTURAL PRACTICES

As mentioned earlier, farm practices such as leaving sloping ground bare to erosion by water or by wind can move the fallout from where it fell to some place of accumulation. This, of course, makes for nonuniform distribution in an area. The extent of this depletion and concentration has not been determined.

The vertical distribution of fallout in a soil may be altered by agricultural practices such as deep plowing. This changes the position of the fallout with respect to the root zone of the crops. In case

of shallow-rooted crops such as some grasses and many vegetables, the deep plowing may materially reduce the uptake of strontium 90. At best, one can expect only a fewfold change in uptake by the plant from these mechanical soil treatments. Plowing, harrowing, and mixing of soils on which strontium 90 has fallen are generally expected to reduce the uptake of this element due to moving it from areas of higher to lower concentration. Experiments are underway to test this supposition.

The practice of liming has been suggested as a remedial measure for areas having high concentrations of strontium 90. This can be reasonably effective if the amount of available calcium in the soil is very low, such as in some of our unimproved pastures. By increasing the amount of ions similar to strontium in the soil its uptake is decreased. While this is feasible on some soils—and in fact it would benefit many of them aside from considerations of strontium 90 uptake—there are limitations on the heavy application of lime to most agricultural soils. Their productivity for many crops would be jeopardized by large amounts of lime. Such additions or heavy applications of gypsum would lead to imbalance of both major and minor essential elements.

As with mechanical practices, one cannot expect liming to change the strontium uptake by an order of magnitude. Only a fewfold reduction can be anticipated at most. Even less can be said for large additions of fertilizers containing calcium, because the calcium content in them is lower than in lime.

THE EFFECT OF FALLOUT ON WATER SUPPLIES

The fallout on lakes, rivers, and oceans is mixed with relatively large volumes of water and hence is not so concentrated as the fallout that lodges in the top few inches of soil, where plant roots are usually concentrated. These waters, other than the salty ones, have flora and fauna that are in need of calcium. Hence, strontium 90 also is taken up and eventually settles out on the bottom of the lake or in the mud of the river. The moving waters also contain soil or rock powder, and these absorb the strontium 90 so that open waters are not hazards from fallout from atomic tests.

Water that comes from wells or springs has been filtered through soils. The strontium 90 has been largely removed in the process of filtration. For industrial processes that require large volumes of water, containing negligible amounts of radioactivity, open lakes, and rivers might pose a problem. From the standpoint of direct effect on man, waters do not constitute a major source of concern. In case of contamination due to warfare, the strontium 90 could be readily removed from drinking water by water softeners or ion exchange resins.

FOOD COLLECTION AND DISTRIBUTION

In case of atomic warfare, food supplies could be seriously contaminated by strontium 90 and other fisssion products. Material that falls out directly on vegetables consumed by man, could, in some cases, be removed by washing with water or by discarding portions of the foodstuff such as the outer leaves of cabbages or the peelings of

potatoes. Strontium 90 that becomes a part of the plant either by uptake through the plant roots or by assimilation through the leaves from direct fallout cannot be removed from the foodstuff without removal of the calcium also. Milk containing strontium 90 probably could not be made free from this contaminant without simultaneous removal of the calcium and consequently changing the basic character of the milk. Food treatment and processing during wartime conditions could be expected to alleviate only that contamination due to direct fallout.

Alterations of food distribution patterns could be used to lower strontium 90 intake. This might involve bringing in vegetables and milk, which serve as a considerable source of calcium in the diet, from areas that have had little or no contamination. The contaminated areas could be used for growing the foodstuffs that contribute little to the calcium of the body, such as grains and meat. Obviously, this might involve difficult problems, or even be impractical, depending on the size and location of the contaminated and uncontaminated areas. Remedial measures could be evaluated from our knowledge of the occurrence and chemical behavior of calcium and strontium in soils and in food and feedstuffs.

ENTRY INTO BIOLOGICAL PROCESSES

When fallout lodges directly on plants that are eaten by man and beast, all of the radioactive elements which are present in it may be of concern. Strontium 90 and cesium 137 can be metabolized by the plant and become a part of it even though not coming through the roots. The extent to which the rare earths and plutonium would remain on the plant material and be ingested by man and animals is not well known at the present time. It is believed that they are of less concern than strontium 90.

It is possible that most of the cesium found in milk comes from direct uptake through deposition on forage that the cows eat, although it is also possible that surface drinking water could be a major source. A few years ago, when the soil contained less fallout and atmospheric fallout was increasing, a major share of the strontium 90 in forage consumed by livestock was from direct fallout on the vegetation. Since the level has increased in the soil, the indications are that most of the strontium 90 gets into the plant by way of the soil and root uptake at the present time.

Alfalfa grown on two very sandy soils in Illinois derives its calcium from high-calcium subsurface horizons rather than from the plowed layer. In these cases, the uptake of strontium 90 has been very small in comparison to vegetation that obtains its calcium largely from the plow depth, where the strontium 90 occurs. Experiments with black-eyed peas, lima beans, and snap beans at Beltsville, Md., during 1956 indicated that only a small part of the strontium 90 in the vegetation came from direct deposition on the plant surfaces.

Representative HOLIFIELD. May I stop you there, Dr. Alexander, and ask you why this statement could be possible in view of the fact that the number of tests have increased within the last 2 or 3 years and therefore there would logically be a greater deposit from the atmosphere on the surface of the ground and less underneath. Why

is it that the plant is taking up more from below the ground than it is through its leaf?

Dr. ALEXANDER. The amount in the soil has increased. I believe the figures were mentioned this morning from 5 to something like 20 or 25 in this general area.

Representative HOLIFIELD. In other words, the spring plowing takes the deposit that occurs in the nongrowing season and puts it underneath the surface.

Dr. ALEXANDER. Yes, sir. You can plant snap beans and harvest them in 42 days. So the time exposed is relatively short and the amount of fallout during that short period of time is relatively small compared to the total in the ground at the present time.

Representative HOLIFIELD. The amount in the ground has increased by a factor of 4.

Dr. ALEXANDER. Four or five since 1953.

Representative HOLIFIELD. If the number of bomb tests increase and we go on for a series of another 10 years, could we now extrapolate an approximation as to the amount that would be taken up by the vegetables through the soil? Would there be a geometric increase or what type of increase could we estimate that would occur?

Dr. ALEXANDER. It seems to me that if we keep the amount coming out in the air constant and increase the amount of soil, then with another 10 years the amount from direct fallout on the leaf would be insignificant compared to the amount taken from the soil. It must go in that direction.

Representative HOLIFIELD. What would be a reasonable extrapolation for the next 10 years, let us say, in view of the history of the last 3 or 4 years?

Dr. ALEXANDER. Sir, there are some uncertainties. These estimates are based on strontium 89 and strontium 90. There has been uncertainty in the determination of strontium 89. So I do not really know exactly.

Representative HOLIFIELD. That is true about strontium 89, but the facts are pretty well known about strontium 90 with its 28-year life.

Dr. ALEXANDER. Yes.

Representative HOLIFIELD. You could well say that for 28 years from 1954, if we use that as the base year to start—if we want to go back further we could to some of the earlier tests—you would have an accumulative increase, would you not, in the soil?

Dr. ALEXANDER. Yes, sir. In the Chicago milkshed area from 1953 to 1955 approximately the increase in strontium 90 in vegetation was proportional to the increase in the soil.

Representative HOLIFIELD. Then the next thing that would concern us would be: Would that amount of increase be to the point where it would be detrimental, and if so, how much? Or would it be so infinitesimal that it would not be of any concern?

Dr. ALEXANDER. All I could do would be to make a prediction as to how much would be there if so much more falls out. You could double the amount in the soil and perhaps approximately double in amount in vegetation. As to what would be harmful to man I would rather someone who is a biologist or medical man pronounce on that.

Representative HOLIFIELD. We will have testimony on that factor later.

Dr. ALEXANDER. Yes, sir.

Representative HOLIFIELD. When we talk about increasing 4 or 5 times and then as the years go by increasing 10, 15, 20 times the amount that was in the soil in the year 1954, this is the thing that alarms people, because they have no way of evaluating what that increase amounts to in terms of detrimental effects. We hope that there wil be some evaluation from the scientists if we are to the point where we can evaluate that in the later hearings.

Chairman DURHAM. Doctor, is there not a limit to plant life as to the amount of any type of chemical substance they will absorb and take into the plant?

Dr. ALEXANDER. Mr. Durham, in this case I believe the amount in terms of weight is so small that I don't believe one would approach any kind of saturating point.

Chairman DURHAM. Having had a little experience with farming, especially in vegetables, I think we always felt even the use of fertilizer—which is a different thing—you go through the same process of assimilating it into the plant life. I was always taught that a plant would assimilate so much chemical substance of whatever kind.

Dr. ALEXANDER. That is right.

Chairman DURHAM. It has a food limit just like if I do not want 2 pieces of pie I will eat 1.

Dr. ALEXANDER. If you have 4 pieces of pie sitting in front of you you will take only 1 instead of all of them.

Chairman DURHAM. Of course, the danger period would be as to the amount absorbed by the plant. When you reached that point you could be concerned.

Dr. ALEXANDER. The amount absorbed by the plant will depend upon the amount of other similar ions that are present. So it depends on whether or not the quantity added of fission product is a measureable amount compared to what is already there.

I believe Dr. Western or somebody spoke of million—millionths of a gram.

Chairman DURHAM. It is not going to run up the beanstalk unless there is something there that attracts it.

Dr. ALEXANDER. The cow thinks it is calcium. It can hardly tell the difference between calcium and strontium. So it needs calcium and takes up the strontium.

Representative HOLIFIELD. To carry the illustration further, it is true that one plant in its lifetime will be limited to the amount that it takes up. But if people continue to eat plants, let us say, from contaminated ground, it becomes accumulated in the human being in the period of 28 years, does it not?

Dr. ALEXANDER. Yes, sir.

Representative HOLIFIELD. So the hazard of the one lifetime of a plant is not the thing that we can stop at. We have to carry it on to its final resting place as far as the human being is concerned.

Dr. ALEXANDER. We will take up a little later the matter of discrimination factors and the ratio between the strontium calcium in the vegetation compared to what will come out in the human bone.

Representative HOLIFIELD. You may proceed.

Dr. ALEXANDER. Since 1953, the strontium 90 found in vegetation has increased in proportion to the increase in the total amount of strontium 90 that has fallen out on the soil on which the vegetation grew. The uptake of strontium 90 by shallow-rooted plants is not so erratic as for the deep-rooted plants such as alfalfa and sweet clover. These deep-rooted plants may be getting the bulk of their calcium near the surface, if growing on an acid soil that has been limed. On the other hand, as mentioned above, the alfalfa may be drawing its calcium from a deep horizon and consequently getting little strontium 90 from the deposition of fallout on the surface. Soils having abundant calcium in the soil zone containing the fallout will produce vegetation of lower strontium 90 content than comparable soils with low calcium levels.

Cesium 137 seems to be taken up more readily by plants from solution cultures than from soils. Apparently, the cesium ion is so firmly held by the soil surfaces that it is not readily available to plants. Likewise, the rare earths and plutonium are little taken up by plants from soils; hence, these elements become of interest only to the extent that they are deposited directly on foodstuffs or in water supplies.

Soil-to-plant discrimination factors have been of considerable interest to those working with fission products that get into the food chain. Evidence for a discrimination against the uptake of strontium relative to calcium is conflicting. Some data based on tracer experiments have indicated that there might be a 2-to-1 factor against strontium uptake.

Menzel and Heald made 2 studies designed to measure discrimination between stable strontium and calcium, 1 in the greenhouse and 1 in the field. In the greenhouse experiment with 10 crops on 4 soils, the average discrimination factor for stable strontium and calcium between soil and plant was 0.7. Under field conditions at 93 sites in 11 States no discrimination, on the average, was found between the ratio of calcium and strontium in alfalfa and wheat and the ratio in the exchangeable form in the soils on which they were grown. There may be no single answer to the problem, but it seems that one should not count on a large discrimination factor for strontium.

All evidence available points to a rather large discrimination factor for the uptake of cesium from soil. Menzel found a factor of 50 for the reduction of uptake of cesium relative to acid-soluble potassium.

It should be emphasized that discrimination factors, where they exist, are strictly applicable only to equilibrium conditions. Probably none of our soil root zones has been brought into equilibrium with the recently added fission products. Thus, it is difficult at this time to make calculations based on uptake found under field conditions. At the present time in the United States, we can find forage that has strontium 90 to calcium ratios that are lower than, higher than, or equal to the ratio of these elements in an exchangeable form in the surface horizons of the soils from which the forage came. These variations are due to unequal distribution of the fission products and exchangeable calcium in the soil, and to uncertainties as to what constitutes the root zone of these particular plants.

UPTAKE AND RETENTION IN ANIMALS AND MAN

Food and water comprise the main mode of entry of radioactive constituents of delayed fallout into the bodies of animals and humans. In the period immediately following the deposition of fallout from the atmosphere, a substantial fraction of the ingested contaminants may originate in fallout deposited directly on edible vegetation. In the case of animals this would involve primarily forage grasses and legumes, and in the case of man, vegetables. With the passage of time, and with the discarding of directly contaminated food, however, the relative importance of the fraction which is absorbed from the soil through plant roots becomes preponderant. Accompanying this change with time is a shift in the composition of the radioactive atoms contaminating feed and food, from a mixture of a number of isotopes to one eventually predominated by strontium 90, and, to a lesser extent, by cesium 137.

For a period of days following a heavy deposition of fresh fallout, iodine 131, which has a half life of 8 days, may be of importance in direct contamination of vegetation. Radioiodine is selectively concentrated in the thyroid gland, where excessive accumulations cause cancer and cell destruction. Injury to the gland may not be detected until long after the iodine has decayed.

Plutonium and the rare earths are absorbed only very slightly from the gastrointestinal tract, so that these elements are not relatively important biologically from the standpoint of ingestion of fallout The fraction of these atoms that does enter the blood system becomes selectively concentrated in the organic matrix of forming bones. After they are deposited there, they undergo only a slight decrease in concentration with time. Since the half life of plutonium 239 is 24,000 years, this means that no substantial reduction of radiation from this isotope will occur during the life of the affected individual.

Cesium 137 becomes distributed in the body similarly to the essential nutrient element potassium. It occurs in muscle tissues, other soft tissues, and the blood. Because it emits gamma rays also, it subjects the entire body to radiation. The rate of clearance of cesium from the body is relatively so high that the continuous ingestion of substantial amounts from food and water is required to maintain a high body burden.

Strontium is readily absorbed from the gastrointestinal tract, somewhat less readily than is calcium, and a large fraction of that absorbed is accumulated in the crystalline mineral portion of bones, similarly to calicum. This deposition in the bone occurs partly by an exchange replacement of calcium ions located in the surfaces of the mineral crystals and partly by bone growth. The concentration of strontium 90 in the bone relative to calcium is not uniform unless the ratio of strontium 90 to calcium in the food has remained reasonably constant during the entire period of development of the skeleton. At any one time, the skeletal deposition of currently ingested strontium occurs preponderantly at sites of active bone growth and bone tissue reforming. The radiation at these sites from strontium 90 from its daughter by decay, yttrium 90, is a potential cause of bone cancer and damage to the blood-forming tissues. The likelihood of this injury in general would increase with the skeletal concentration of strontium 90, and with the length of its retention in the bones. It would be

greater for children than for adults whose skeletons were completely developed. The average period of retention in the human body is several years. Because the radiological half life is 28 years, the main cause of reduction in radioactivity from strontium 90 during the period of residence in the body is the process of elimination of the strontium atoms from the body.

Although strontium is closely similar to calcium in behavior, it moves somewhat more slowly through metabolic processes and membranes in animals and man than does calcium. The magnitude of this discrimination may be small in any one metabolic process, but by a succession of such processes, each one magnifying the preceding discrimination by a small factor, substantial discrimination between the two elements may ultimately be effected. The discrimination factor for a specific process is defined as the ratio of strontium to calcium in the product divided by the ratio of strontium to calcium in the reactants. The observed ratio for a sequence of processes, for which the individual discrimination factors may or may not be known, is the ratio of strontium to calcium in the final product divided by the ratio of strontium to calcium in the reactants of the initial process.

Representative HOLIFIELD. Doctor, going back to the previous paragraph, you plainly state that the degree of absorption of the body depends upon the growth of the bone tissues. You point out that in the case of children whose bones are growing there would be a greater percentage of strontium 90 absorbed than in adults. Is that correct?

Dr. ALEXANDER. Yes. As I understand from discussions that have been had in a grown person there is some bone turnover. There are points of bone that are regrowing and others that are decomposing. To the extent that the regrowth of bone is taking place, either from an injury or from a natural repair, you would be taking in some from your diet. But the child is laying down all of its skeleton.

In other words, it is growing more constantly. The bone in a child is getting so many more pounds every year, whereas with a grown person the weight may stay the same but he has a certain amount of turnover.

Representative HOLIFIELD. It is building the size of its bone skeleton as well as repairing the part that dissolves?

Dr. ALEXANDER. That is right.

Representative HOLIFIELD. There is no question about this fact, is there? I mean the scientists are agreed upon this point?

Dr. ALEXANDER. I believe so; yes, sir.

Representative HOLIFIELD. I ask that for a very definite purpose. We realize in all testimony that most scientists take into consideration systematic error or even personal error in making their conclusions. The reason I wanted to ask that question was to find out if there was any appreciable controversy on that point.

Dr. ALEXANDER. I don't believe so.

Representative HOLIFIELD. You may proceed.

Dr. ALEXANDER. Recently experimental studies with humans and various animals, based on different approaches in many instances, have established a number of the important discrimination factors and observed ratios, with commendable precision. The techniques used in these experiments have included:

(*a*) Radioactive strontium tracer versus stable natural calcium;
(*b*) Radioactive strontium tracer versus radioactive calcium tracer; and
(*c*) Stable natural strontium versus stable natural calcium.

The field sampling program for strontium 90 also has provided valuable information. Drs. Cyril L. Comar, Daniel Laszlo, and Norman S. MacDonald, and their colleagues, are among those who have made important experimental contributions to the attack on this problem.

The discrimination values mentioned here apply strictly only when the ratio of strontium to calcium in the diet remains constant during the entire period of development of the skeleton. On a diet not containing milk or milk products, the strontium to calcium ratio in the bones of rats, goats, and humans has been found to be about 0.25 to 0.30 of that in the diet. When milk is the source of the calcium and strontium, the ratio in the bones of rats and humans will be about 0.55 of that in the diet. Some components of milk increase the relative absorption of strontium from the gastrointestinal tract. The milk produced by cows and goats from their normal diet will have a strontium-to-calcium ratio of about one-tenth to one-seventh of that in the diet.

Representative HOLIFIELD. May I ask you to stop there to clarify this statement above:

> When milk is the source of the calcium and strontium, the ratio in the bones of rats and humans will about about 0.55 of that in the diet.

Let us assume that there is a hundred units of strontium in the diet. Your point there is that the body would only absorb 0.55. In other words, just a little over half of 1 percent of the gross amount of intake?

Dr. ALEXANDER. That is relative to laying down an equal amount of calcium. These units are in terms of the strontium relative to calcium.

Representative HOLIFIELD. Then I am wrong in my interpretation of what you save said there. That refers to the percentage it would take up in relation to the calcium that would be taken up.

Dr. ALEXANDER. Yes, sir.

Representative HOLIFIELD. How much of the calcium would be taken up?

Dr. ALEXANDER. That would depend on the stress. If it were a calf on a low-calcium diet, he would use maybe 90 percent. If it had an excess of calcium it would use a smaller percent. It would make that discrimination.

For a gram of bone growth, there would be 0.55 as much strontium as was in the gram of calcium that went into the food. So it is a reduction of half as far as the strontium uptake is concerned.

Other things being equal, sir, your question would be right. If the animal is using all the calcium in its diet, then your statement would be essentially right, that for a gross intake of so much it would only mean half as much was taken up.

Representative HOLIFIELD. So we can at least draw this comfort from the fact that the amount that was ingested by the animal would not be transferred either to the meat or the milk, and then in turn transferred by human ingestion to the human body. Am I right in that interpretation?

Dr. ALEXANDER. Perhaps I was thinking of a growing animal rather than one that was lactating. I believe the system is a little more complicated in a cow depending on the calcium. If she is very low in calcium she may be drawing calcium from the bones in order to get the amount she needs to make the milk every day.

As far as the meat is concerned, strontium and calcium are not a factor. They are a factor in the bone and the milk.

I believe in general lactating animals will give preference to the production of milk. Nature in general tends to make the milk all of the same composition. If you give a cow a low- or high-calcium diet, she will produce milk of the same composition. But the utilization of what goes through her would be quite different percentagewise.

Representative HOLIFIELD. How would that differ in the case of a hen and an egg?

Dr. ALEXANDER. I am afraid you have me over a barrel because I do not know too much about the calcium to make hen eggs. The only thing I know is that if a chicken is too low in calcium they produce an egg which has very little calcium and it is a soft egg without a shell. I personally don't know very much about the nutrition of chickens.

Representative HOLIFIELD. We will go on then, if you do not know that.

Dr. ALEXANDER. Studies with rats and rabbits indicate that the strontium-to-calcium ratio in the fetus would be about one-half of that in the body of the mother. Experiments with lactating goats suggest that the ratio in the milk produced would be about 0.38 of that in the blood plasma. Absorption from the intestines and urinary excretion appear to be the processes causing the major discrimination against strontium in nonpregnant nonalactating mammals.

The appropriate discrimination factors and observed ratios can be combined mathematically to estimate the overall discrimination between strontium and calcium in the vegetation and in human bone. To be valid, these calculations must be based on reasonable assumptions concerning the fractions of the calcium intake derived from various sources—for example, cow's milk, vegetables, and the mother's body. For newborn infants, the predicted overall discrimination has been estimated to range from a minimum value of 0.10 to a maximum value of 0.045. For 6-month-old children, the estimated range is from a minimum discrimination of 0.13 to a maximum of 0.041. For children over 6 months of age and adults, the estimated minimum discrimination value is 0.20 and the maximum 0.09. It is apparent, therefore, that the metabolic processes in humans and animals are favorable to a substantial reduction in the hazard of strontium 90 to man, as compared to the level of contamination of the vegetation in the food chain and of the soil on which it is grown.

Uptake by marine organisms: In the consideration of the uptake of fallout by marine organisms no recognition is made of the difference between early and delayed fallout. In fact, most quantitative observations have been on the distribution of local fallout. One important difference might be expected, that delayed fallout would be small particles, which would remain in the surface waters.

The distribution of fallout in the ocean is dependent upon the location and time where fallout occurs and the dispersion following fall-

out. Dispersion is mainly by water movement, but also can be by transport in plankton, fish, or other organisms.

The depth in the ocean where the fallout particles occur has an important bearing on distribution. The water from the surface to the thermocline is often called the stirred layer, as it is assumed that this water is being mixed constantly. Below the thermocline, which occurs from 100 to 200 meters below the surface, the water is stratified and slow moving. Therefore, it may be assumed that small fallout particles will remain in the surface and may be transported great distances, whereas larger fallout particles will be moved horizontally while passing through the stirred layer, but relatively little once they are below the thermocline. Some support is given to this assumption by the fact that deep water samples from the vicinity of the Eniwetok test site just previous to the Redwing tests in 1956 were radioactive from previous fallout, which suggests that some radioactivity from earlier tests had not removed far horizontally.

Ultimately, the fallout in the surface water becomes a part of the major current system of the ocean and moves in a gyro around the entire basin. In the Northern Hemisphere the circulation is clockwise, and in the Southern Hemisphere counterclockwise. In the vicinity of the Eniwetok test site, the North Equatorial Current moves westward to the Philippines, where the current splits, with most of the water flowing northward to become the Japanese Current. Off the coast of Japan the current turns eastward to flow across the Pacific Ocean and arrives off the coast of North America at about 50° north latitude; there it flows southerly, and later westwardly, to complete the cycle.

In 1955, 1 year after Operation Castle, a survey was made to determine the distribution of radioactivity in the plankton and water of the North Equatorial Current in the western Pacific. Starting near the test site, the survey moved westward to the Philippines and thence northward to Japan. Activity, or other than from naturally occurring isotopes, was widespread and of low level, with the highest values found off the Philippines, a distance of 2,500 miles from the test site. Values for water ranged from zero to 537 disintegrations per minute per liter and for plankton from 3 to 140 disintegrations per minute per gram wet weight. As a comparison, the radioactivity in sea water from the naturally occurring isotope, potassium 40, is about 540 disintegrations per minute per liter. Although this fallout probably remained in the North Pacific circulation system, it would become increasingly difficult to detect because of the continuing processes of dilution and radioactive decay.

The radioactivity of water and plankton samples from an area contaminated by fallout, shortly after bomb tests, was determined on two series of Redwing samples. One series was collected during the operation and the other 6 weeks after its conclusion. During this period of time the maximum water value decreased to 16 percent and the plankton value to 2 percent of the earlier values.

The transport of radioactive isotopes away from a contaminated area by marine organisms is possible. One way this could happen would be for migrating fish to prey upon radioactive organisms while moving through a contaminated area. Another, and probably a more important way, is associated with the daily vertical migration of

plankton. The diurnal migration of plankton to near the surface during hours of darkness and to deeper waters during hours of light is well known. There was evidence of this situation in samples collected near Bikini, just previous to Redwing. At the surface plankton was radioactive but the water was not, while in deep water both were radioactive. It is thought that this was a result of plankton becoming radioactive in the deep water and then moving toward the surface.

Marine organisms can acquire radioctive isotopes directly from the water in which they are living or indirectly by ingestion of other radioactive organisms. The acquisition of radioisotopes directly from the water may be either by absorption or adsorption, but this is of little importance from the point of view of the food chain. An example of a food chain leading to man starts with the one-celled plants, then leads to the one-celled animals, to the small many-celled animals, to small fish, to large fish, such as a tuna, and finally to man. There may be a few, or there may be many links in the food chain, and radiosotopes may enter the chain at any point.

Marine organisms, similar to terrestrial plants and animals, differentially select and retain minerals in their bodies, and can concentrate elements from sea water. The selection of an element depends upon the physiological demand of the organism for the element and the availability of the element, and to some extent, of chemically similar elements. The greatest concentration will occur when the demand and availability are greatest, and vice versa. For example, because zinc is relatively scarce in sea water and there is a physiological demand for this element by fish and shellfish, zinc in an available form that is added to sea water is rapidly removed and concentrated by fish and shellfish. A similar example is that of the alga, *Asparagopsis*, for iodine. On the other hand, when strontium, which is naturally more abundant in sea water, is added to sea water, its concentration by most marine organisms is not great.

The plankton samples collected during and after the Redwing operation were analyzed for the radiosotope composition. The isotopes found include strontium 89 and 90; barium 137 and 140; cerium 144; praseodymium 144; ruthenium 103 and 106; zirconium 95; cobalt 57, 58, and 60; zinc 65; iron 59; and trivalent rare earths. There were differences in the isotopes found in the plankton samples collected in different areas. One reason for this difference is that plankton is composed of many groups of organisms, each with an affinity for specific isotopes.

For this same reason, the concentration factor of radioactivity by plankton in relation to the water of its environment varies greatly. A concentration factor of a thousand or greater is often observed. Because plankton does concentrate fission products and other radioisotopes, measurement of the radioactivity in a plankton sample is a convenient method of determining the presence of radioactive isotopes in the water.

In fish most of the elements expected from fission are found and also the nonfission products formed by neutron irradiation of bomb and tower metals, manganese 54, iron 59, cobalt 58 and 60, and zinc 65. Often the nonfission products predominate. It has been mentioned that little strontium is found in most marine organisms. A land crab does accumulate strontium 90 in the liver and shell, whereas marine crabs contain little strontium 90.

A program of radiobiological monitoring has been a part of all test series in the Pacific Ocean. Samples of fish, invertebrates, birds, plankton, water, soil, algae, and terrestrial plants and animals have been collected from the ocean, lagoon, reef, and islands of many places in the Pacific Ocean. Some have been visited only once, while others many times. Except for the immediate vicinity of the test site, biological contamination by fallout in the Pacific Ocean has been very slight.

Representative HOLIFIELD. Thank you very much, Dr. Alexander. That was quite a task for you to go through that, particularly at the speed that you went through it. But I think you have given us a lot of very valuable information for our record, and we appreciate it.

Our next wtiness is Dr. Roger Revelle, Scripps Institute of Oceanography, University of California.

As a fellow Californian, sir, I welcome you to the witness stand.

STATEMENT OF DR. ROGER REVELLE, DIRECTOR, SCRIPPS INSTITUTE OF OCEANOGRAPHY [3]

Dr. REVELLE. Thank you, Mr. Holifield. I have some charts that we might put up.

Representative HOLIFIELD. Very well.

Dr. REVELLE. I have a prepared statement which I would like to submit for the record.

Representative HOLIFIELD. Without objection, your prepared statement will be received for the record, and you may proceed to summarize in any way you desire.

(The statement referred to follows:)

STATEMENT BY PROF. ROGER REVELLE, SCRIPPS INSTITUTION OF OCEANOGRAPHY, UNIVERSITY OF CALIFORNIA

1. At present the measurable radioactivity in the oceans consists of the natural radioactive elements and artificial radioisotopes from tests of atomic weapons.

2. In the future, additional radioactivity may become introduced into the ocean from the use of atomic weapons, accidents to nuclear powered vessels, accidents to nearshore reactors, from the oceanic disposal of atomic waste, and in coastal areas, from land runoff. Some evidence that this latter effect now exists in the La Jolla region is shown on table 1.

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[3] Scripps Institution of Oceanography. Date and place of birth: March 7, 1909, Seattle, Wash. Education: A. B., Pomona College in 1929; Ph. D. in oceanography, University of California in 1936. With the exception of service in the Navy during and immediately after World War II, his entire career has been spent at the University of California's Scripps Institution of Oceanography, of which he became director July 1, 1951. On staff of Adm. W. H. P. Blandy in Operations Crossroads in charge of oceanographic measurements. Organized resurvey of Bikini in 1947. Fellow of American Association for Advancement of Science, San Diego Society of Natural History, Geological Society of America; member, American Meteorological Society, Sigma Xi, Society of Limnology and Oceanography, Geological Society of Washington, Western Society of Naturalists, Cosmos Club, Men's Faculty Club, Berkeley, American Association of Petroleum Geologists, and American Geophysical Union. President for 1956–59 of the section on oceanography of the American Geophysical Union. President of special committee on oceanographic research of International Council of Scientific Unions; chairman, National Academy of Sciences Committee on Biological Effects of Atomic Radiation in Oceanography and Fisheries; member for North America of International Advisory Committee on Marine Sciences of UNESCO; member, Committee for the International Geophysical Year of the International Association of Physical Oceanography; the Panel on Oceanography of the National Committee for the International Geophysical Year; and Divisional Committee for Mathematical, Physical, and Engineering Sciences of National Science Foundation. In 1954, Dr. Revelle received the Albatross Medal of Swedish Royal Society of Science and Letters for outstanding achievements on oceanography, especially in deep-sea research. Honorary doctor of science from Pomona College. Elected to National Academy of Sciences April 28, 1957. (Submitted by witness.)

3. Natural radioisotopes exist everywhere, but they are much lower in the sea than in any other region of the earth where life exists. Therefore, artificial radioactivity can modify the marine environment to a somewhat greater *relative* extent than it can the land environment. Due to the great volume of the sea, however, the *absolute* change in radioactivity will be small. Table 2 shows the natural radioactive nuclides in the sea, and it can be seen that the natural radioactive background of the ocean is almost wholly due to radiopotassium and is very low. In consequence, marine organisms and sailors normally experience very low exposure to external radioactivity.

4. In addition, because man is a land animal, the oceanic radioactivity usually has less *direct* effect on him than does radioactivity on land. Table 3 shows the total natural exposure of man and other organisms in different environmental situations.

5. The principal mechanisms by which artificial radioactivity in the ocean may represent hazards to man are through the marine products—the animals and plants of the sea that are eaten by man and his domestic animals or employed as fertilizers.

6. At present, fallout from weapons tests is the only significant source of artificial radioactivity in the oceans. Table 4 shows activities of some oceanic organisms collected off California in April 1957.

Most of the fallout from United States weapons tests falls on the oceans. In the first place the large weapons tests occur there and the close-in fallout comes down directly on the sea. In addition, the oceans cover 71 percent of the earth's surface and of the widely distributed stratospheric fallout from both United States and Russian tests, up to 71 percent falls into the sea.

The proportion of fallout occurring in the close-in region varies from zero for a high airburst to perhaps one-half for a ground surface burst of a large weapon, and 90 percent for an underwater burst of a nominal atomic bomb.

7. In the sea this fallout radioactivity mixes rapidly to an appreciable depth. Since land fallout ends up on a surface, the concentration of fallout per volume of material tends to be greater on land than on the ocean.

It is known that the very close-in fallout from ground explosions penetrates the ocean very rapidly, part of it penetrates through 100 meters in less than 20 minutes. For water and air bursts and outside the very close regions for surface land bursts, the fallout is mixed throughout the upper 50 to 150 meters within a few days after arrival. In the trade wind region this mixing is complete in 30 hours.

The rate at which fallout becomes distributed below the surface depends on two factors: the size of individual particles containing the radioactivity and the rate of vertical mixing in the sea. The rate of settling is of primary importance for particles larger than eight hundredths of a millimeter in diameter while the rate of mixing of the water itself dominates, at least during the first few days, for particles with a diameter of less than 0.04 millimeter. Within a few miles of a big surface burst over shallow water or land, the particles average about 0.2 of a millimeter in size and settle at a rate of more than 200 meters per hour. The concentration of radioactivity not far above the "front" of settling particles is fairly uniform. At greater distances, where vertical mixing of the water predominates, the lower boundary of the radioactivity layer deepens at first at a rate of 5 meters per hour, and below 20 or 30 meters at a rate of 2 or 3 meters per hour, until the bottom of the mixed surface layer at 50 to 100 meters is reached. At this depth the rate of mixing greatly diminishes and the radioactive layer may not become much deeper for periods of several weeks.

8. Because of this rather rapid mixing, the fallout, once it gets in the ocean, presents a much smaller *direct* hazard than fallout on land owing to the shielding by the immense mass of water with which it has mixed.

It can easily be shown that because of this vertical dispersion of the fallout, and the absorption of radiation coming from below the surface by the water, the radioactive hazard to a human being at or above the surface is less than one hundredth of that which would exist over land after the first half hour from the cessation of fallout. It is only about 20 percent of the land hazard during the first half hour.

9. Both close-in and early (or tropospheric distant) fallout fall in a relatively small part of the ocean. The radioactive substances are moved by currents and by mixing to other areas in concentrations that are high in comparison with a final distributed level. Thus the ocean currents represent a mechanism of transport of fallout materials. Subsequent concentrated fallout of radioactivity from the same site commonly is not added to the same water mass and hence, unlike

the case of land detonations, concentrations are not increased. During the process of transport the waters are mixed laterally and to some degree vertically and the concentration of radioactive materials decreases. As an example of the concentrations to be expected at moderately great distances from the Pacific Proving Ground in a few months after a weapons test, we may cite the results obtained by the Japanese Shukotsu Maru expedition shown in the accompanying chart.

10. Over a long term of perhaps 200 to 400 years the material falling to the surface waters of the sea is diffused throughout the whole volume of the ocean, which is more than 40 times the volume of the surface layers.

11. The surface layers, into which fallout rapidly mixes, are of the greatest importance to man as they contain most of the living plants and animals that he harvests, and which may take up radioactivity from the water.

12. There is communication between the deep and surface layers along shores and in certain other areas where the phenonmenon known as upwelling occurs. In these regions, several factors are worthy of note. The upwelling waters are rich in nutrients and a subsequent heavy growth of plankton results. This plankton growth is the primary foodstuff upon which the major fisheries of the world depend. Hence in a region where the surface is contaminated by radioactivity, these primary food substances, derived in part from deeply submerged waters, may contain a somewhat lower level of activity than plankton in a region where upwelling does not occur. On the other hand, disposal of radioactive materials, accident to nuclear-powered vessels, or the use of deep ASW weapons, which may introduce radioactivity below the mixed layers will, through the process of upwelling introduce radioactivity directly into the surface food chain.

13. In addition, marine organisms migrate vertically more than 1,000 feet between day and night and from this migration there may be appreciable transport of radioactive material that is in the food chain, particularly because of the capacity of the organisms to concentrate certain isotopes. In the cruise of the *Taney* (see reference, United States Atomic Energy Commission, 1956), it was apparent that the activity of the organisms is not only influenced by the immediate water in which they are found but also by the underlying and adjacent water masses.

14. The ocean is a concentrated salt solution, which contains nearly all of the chemical elements either in solution or suspension, just as ordinary rock and soil contain all of these elements. However, the concentrations and state of these chemical elements in the sea differ in several significant respects from those in the soil. For example, radium and its daughter products are almost completely absent from ocean water whereas they are more common in rock, and almost all elements exist in a form wholly available to organisms, unlike the soil and rock where many elements are tied up in unavailable silicate complexes. The concentration of various elements in the sea is shown in table 5.

15. Organisms tend to concentrate certain of the more dilute elements and many chemically identical or similar artificially radioactive substances are highly concentrated by marine plants and marine animals. An example of such concentration is the case of Co–60 and Zn–65.

Two long-lived radioactive isotopes that are present in fallout, but are not fission products, are cobalt 60 and zinc 65. Cobalt 60 has a half life of 5.3 years, zinc 65 has a half life of 250 days. Both are beta and gamma emitters. Although the total quantities of these substances produced by the explosions is small compared to the fission products they are very highly concentrated in marine food animals. Amounts as high as one-third of a microcurie of cobalt 60 were found in the liver of an 8-inch clam from a location in the Marshall Islands about 80 miles from the location of an explosion 2 years before. The average cobalt 60 in three samples of clam livers was about 1.5 microcuries per pound (0.003 microcurie per gram). The factor of concentration for cobalt in these animals may be several hundred thousand over the amount in the water because radiocobalt could not be detected in the surrounding water or in the sand of the reef. Traces of cobalt, of course, are essential for nutrition of all animals and are a constituent of vitamin B_{12}. Zinc 65 is concentrated by food fishes by a factor of several thousand over the amount in sea water. The highest concentration is found in the liver, spleen, and skin. The average amount found in these organs of a parrot fish and a snapper from the same lagoon as the high-cobalt clam was about 0.7 microcurie per pound (0.0014 microcurie per gram). A tuna from the open sea in the same general area had about twice as much zinc per unit weight

as these lagoon fishes. The amounts in the fish flesh were lower by a factor of 10 than those in the internal organs and the skin. Considerably more than half of the activity was radiocobalt in the clams and radiozinc in the fish. The fish contained about 10 times as much zinc as cobalt whereas the mollusks contained considerably more cobalt than zinc.

From the standpoint of the possible hazard to human beings who might eat these heavily contaminated clams or fish it should be remembered that the biological half life in human bodies is only about 10 days for cobalt and zinc, while half of the strontium 90 ingested remains for nearly 10 years. As a result, the maximum permissible concentrations for radiozinc and cobalt are about 100,000 times that for radiostrontium (for children, 6 x 10 μc Zn–65/gram and 1.8 x 10^{-3} μc Co–60/gram).

Although strontium 90 makes up about 5 percent of the activity of fission products at the end of 2 years, it was found in fairly low amounts in the organisms of the Marshall Islands lagoons. Principal fission products in these organisms, other than cobalt and zinc, were cerium, praesodynium, and ruthenium. This relatively low level of strontium 90 probably results from the rich calcium supply. The biological concentrations of certain chemical elements are shown in table 6. The effect of a rich calcium supply on strontium uptake is shown in tables 7 and 7a, where only the highly calcareous soils of the coral atolls compare with the condition in the sea.

16. Table 8 shows several of the important effects discussed in the foregoing. In a region of intense close-in fallout over the sea the Sr–90 is relatively unimportant in fish because of mixing and dilution with calcium and because of discrimination against strontium by fish. Zinc 65, on the other hand, can be far more important because of the low level of zinc in sea water and the concentration of zinc by marine organisms.

17. The marked differences in the concentration of various radioactive substances in different parts of marine organisms should be considered, for in the case of some marine organisms not only the flesh but the skin, viscera, and bones are eaten. Table 9 shows added information on the great variation of activity between tissues of marine organisms.

18. Insofar as marine plants and animals are concerned we must distinguish between pelagic organisms, which, although many of them are vertically migratory, can absorb fission products only from water in which they immediately move, and fixed animals and plants such as oysters, mussels, other shellfish, and kelp, which filter great quantities of water as it moves by them. Some insoluble or precipitated radioactivity tends to be concentrated on the bottom just as it is concentrated on the land surface. Thus the levels of radioactivity to which bottom-dwelling organisms are subjected may be somewhat higher than the pelagic organisms experience. In the lagoons of the Marshall Islands the fish living freely in the waters of the lagoons show beta activity less than that of the bottom-dwelling gastropods. The organisms living in the adjacent sea areas show a level similar to those of free-swimming fish in the lagoon.

Thus different problems may be associated with commercial fisheries that take benthic or bottom-dwelling organisms and the fisheries for pelagic creatures. In addition, many of the benthic fisheries are below the thermocline and subjected to the effect of deep rather than shallow contamination.

19. In considering the distribution of weapon-produced radioactivity between the upper and lower levels of the ocean, attention must be paid to the size and location of the weapons; for example, a deep underwater burst of a nominal weapon will leave approximately two-thirds of the radioactivity below the surface layers and only one-third in the surface layers.

20. In addition to radioactive fallout from the explosion of nuclear weapons, we must also consider the disposal of radioactive wastes at sea particularly in coastal shallow waters and the possible hazard arising from accidents; for example, the sinking of nuclear-powered ships and the accidents to reactors located near the sea and using ocean cooling waters.

If and when an appreciable part of the world's merchant fleet is powered by nuclear reactors, serious hazards may arise in confined waters from collisions in which the reactor is damaged and the fuel elements with their contained fission products are lost in the water. Suppose for example that a 50,000-kilowatt reactor (probably fairly typical for a large fast freighter) has been in service without refueling for 1 year on a ship that has spent half its time underway. Approximately 10 kilograms of fissionable material will have been used up and the total amount of fission products will be approximately 10^7

curies. If, owing to a collision, the reactor is lost in a harbor, say 8 miles long by 3 miles wide by 50 feet depth, and the fission products become uniformly distributed, the water in the harbor would contain 10^{-2} curies per cubic meter giving an almost constant radiation dose of about .5 r per day on the surface. Dock pilings, ship bottoms, and other structures covered with fouling organisms would accumulate a much higher level of radioactivity, and, of course, local concentration in the water may be extremely high.

21. Food habits of different nations must be taken into account in evaluating the hazards to human beings from fallout over the oceans. This is particularly true in the case of the Japanese, who obtain about one-third of their calcium from marine fisheries. Table 10 shows the sources of calcium in the Japanese diet.

To accomodate these differences we must investigate the differences between the terrestrial and marine uptake problems for specific isotopes. For example, coming specifically to the strontium 90 problem we have made a comparison between the strontium 90 that will be obtained from land-derived and ocean-derived foods in table 7.

Three factors enter into this: (1) The much higher concentration of calcium in soils than in sea water, (2) a dissemination of strontium 90 from fallout in a much greater volume of water in the ocean than soil on land, and (3) the different amount of discrimination against strontium in fish than, for example, in milk cows.

22. Although maximum permissible concentration has been established for most radioactive substances, recent evidence indicates that much smaller amounts of radioactivities do produce physiological effects, for example leukemia.

23. Introduction of radioactive substances in the ocean has beneficial as well as harmful effects in so far as it enables us to use tracer techniques in the study of the movement of the water and the life cycles and metabolism of marine organisms. As an example figure 1 indicates the intrusion of a clean water mass along the level of high stability and the persistence of deep activity around Bikini Atoll 2 years after an event.

TABLE 1.—*Apparent effect of runoff on activity of nearshore ocean water—Samples of suspended sediment*[1]

Date of collection 1957	Preceding weather	Activity[2]		
		Zr, Nb	Ru, Rh	K^{40}
April 12-22	Calm, dry	5,000	1,200	900
April 22-May 5	do	2,000	900	900
May 5-16	Calm, rain	11,000	4,000	900
May 16-20	Heavy swell, intense rain	22,000	2,300	900

[1] Collected by filtering water from about 300 meters offshore at La Jolla where the sediment concentration is 10-30 p. p. m. of the seawater (from unpublished data T. R. Folsom).
[2] Gammas/minute/kilogram of sediment determined by gamma spectrometer.

TABLE 2.—*Radioactivity of sea water*

Nuclide	Concentration (g cm.$^{-3}$)	Specific activity—Number of of disintegrations (cm.$^{-3}$ sec.$^{-1}$)	Total amount in ocean (megatons)	Total activity in ocean (megacuries)	Energy of γ radiation (MEV.)
K^{40}	4.5×10^{-8}	1.2×10^{-2}	63,000	460,000	[a] 1.5
Rb^{87}	8.4×10^{-8}	2.2×10^{-4}	118,000	8,400	No. γ
U^{238}	2.0×10^{-9}	[b] 1×10^{-4}	2,800	3,800	.05-.82
U^{235}	1.5×10^{-11}	[b] 3×10^{-6}	21	110	.06-.18
Th^{232}	10^{-11}	[b] 2×10^{-7}	14	8	.03-.08
Ra^{226}	3.0×10^{-16}	[b] 3×10^{-5}	4.2×10^{-4}	1,100	.18-.60
C^{14}	4×10^{-17}	7×10^{-6}	5.6×10^{-5}	270	No γ
H^{3} [c]	8×10^{-20}	2.5×10^{-8}	1.5×10^{-9}	12	No γ

[a] $\gamma/\beta=0.1$.
[b] Activity of nuclide+daughter products.
[c] Only in top 50-100 meters of the ocean.

TABLE 3.—*Radiations in 12 radiological domains*

	Total mrads/year
Man over granite:	
1. At 10,000′ elevation: Cosmic rays 100+granite 90+internal 17	=207
2. At sea surface: Cosmic rays 35+granite 90+internal 17	=142
Man over sedimentary rock: 3. At sea level: Cosmic rays 35+rock 23+internal 17	=75
Man over sea: 4. Cosmic rays 35+sea 0.5+(1) internal 17	=52
Man in Swedish concrete house (measured values):	
Minimum=125	
Mean=220	
Maximum=520	
Large fish in sea:	
5. Near surface: Cosmic rays 35+sea 0.9+(1) internal 28	=64
6. 100 meters deep: Cosmic rays ½+sea 0.9+(1) internal 28	=30
Microorganism (mean radius 0.01 mm or less) in water:	
7. Near sea surface: Cosmic rays 35+sea 3.6+(1) internal (3)	=39
8. 100 meters deep in sea or more: Cosmic rays 0.5 sea 3.6+(1) internal—(3)	=<5
9. Buried in deep sea sediments: Cosmic rays 0.000+clay 40–620+internal—	=40–620
10. Near fresh water surface: Cosmic rays 35+water activity—(2) internal	(2)=35
11. 100 meters deep in a fresh lake: Cosmic rays 0.5+water activity 0+(2) internal	(2)=<0.5

TABLE 4.—*Net gamma spectra of organisms—Assumed radionuclides* [1]

Organisms	Ru^{106}, Rh^{106}	Zr^{95}, Nb^{95}	1.1 Mev [2]	1.3 Mev [3]	K^{40}
Oceanic: [4]					
Bathypelagic krill	0	0	43	0	660
Planktonic barnacle	200	480	93	0	210
Benthic:					
Commercial shrimp [6]	120	200	0	0	640
Abalone viscera	130	100	400	140	560
Sessile:					
Kelp [7]	300	750	ND	ND	480
Kelp [8]	1,300	600	ND	ND	960
Kelp [9]	260	560	ND	ND	960

[1] Activity in gammas/min/Kg wet weight based on lines occurring at proper energy, but without chemical identification. (T. R. Folsom, in work.)
[2] Possibly $Co^{60}+Zn^{65}$.
[3] Possibly Co^{60}.
[4] Deep water off California.
[5] From midwater at 1,000 meters.
[6] From Gulf of California.
[7] From 10-foot depth off La Jolla.
[8] From surface fronds off La Jolla.
[9] From tidal flats Gulf of California.

TABLE 5.—*Concentrations of chemical elements in atoms per million atoms of chlorine in the marine hydrosphere*

Element	mg/l	Atoms/10^6 atoms Cl	Element	mg/l	Atoms/10^6 atoms Cl
H	108,000	202,000,000	Mn	.002	.07
He	.00001	.0004	Fe	.01	.3
Li	.2	50	Co	.0005	.02
Be	----	----	Ni	.0005	.02
B	4.8	830	Cu	.003	.09
C	28	4,300	Zn	.01	.3
N	.5	70	Ga	.0005	.01
O	857,000	100,000,000	Ge	.0001	.003
F	1.3	130	As	.003	.07
Ne	.0003	.03	Se	.004	.1
Na	10,500	850,000	Br	65	1,500
Mg	1,300	100,000	Kr	----	----
Al	.01	.7	Rb	.3	7
Si	3	200	Sr	10	200
P	.07	4	Y	.0003	.006
S	900	52,000	Zr	----	----
Cl	19,000	1,000,000	Nb	----	----
A	----	----	Mo	.01	.2
K	380	18,000	Tc	----	----
Ca	400	19,000	Ru	----	----
Sc	.00004	.002	Rh	----	----
Ti	.001	.04	Pd	----	----
Ag	.003	.0005	Tm	----	----
Cd	.000055	.0009	Yb	----	----
In	.02	.3	Lu	----	----
Sn	.003	.05	Hg	----	----
Sb	.0005	.008	Ta	----	----
Te	----	----	W	.0001	.001
I	.05	.7	Re	----	----
Xe	----	----	Os	----	----
Cs	----	----	Ir	----	----
Ba	.09	1.2	Pt	----	----
La	.0003	.004	Au	.000004	.00004
Ce	.0004	.005	Hg	.00003	.0003
Pr	----	----	Tl	.00001	.00009
Nd	----	----	Pb	.003	.03
Pm	----	----	Bi	.0002	.002
Sm	----	----	Po	----	----
Eu	----	----	At	----	----
Gd	----	----	Rn	9.0×10^{-15}	8.0×10^{-14}
Tb	----	----	Fr	----	----
Dy	----	----	Ra	3.0×10^{-11}	2.0×10^{-10}
Ho	----	----	Ac	----	----
Er	----	----	Th	.0007	.006
V	.001	.04	Pa	----	----
Cr	.00005	.002	U	.002	.02

TABLE 6.—*Approximate concentration factors of different elements in members of the marine biosphere—The concentration factors are based on a live weight basis*

Element	Form in seawater	Concentration in seawater (micrograms/l.)	Algae	Concentration factors			
				Invertebrates		Vertebrates	
				Soft	Skeletal	Soft	Skeletal
Na	Ionic	10^7	1	0.5	0	0.07	1
K	do	380,000	25	10.0	0	5	20
Ca	do	1	1	10.0	----	10	----
Ca	do	400,000	1,000	10.0	1,000	1	200
Sr	do	7,000	20	10.0	1,000	1	200
Zn	do	10	100	5,000.0	1,000	1,000	30,000
Cu	do	3	100	5,000.0	5,000	1,000	1,000
Fe	Particulate	10	20,000	10,000.0	100,000	1,000	5,000
Ni	Ionic	2	500	200.0	200	100	0
Mo	Ionic-particulate	10	10	100.0	----	20	----
V	(?)	1	1,000	100.0	----	20	----
Ti	(?)	1	1,000	1,000.0	----	40	----
Cr	(?)	.05	300	----	----	----	----
P	Ionic	70	10,000	10,000.0	10,000	40,000	2,000,000
S	do	900,000	10	5.0	1	2	----
I	----	50	10,000	100.0	50	10	----

NOTE.—The data in this table were principally gathered from Vinogradov (1953), unpublished results of E. Goldberg and Laevastu and Thompson (1956).

TABLE 7.—*Factors of Sr-90/Ca concentration for conditions of equal fallout*

Material	(Sr-90/Ca, soil=1)	Present concentration [1] curies/grm Ca
Unplowed soil (Wisconsin)	1	3×10^{-11}
Alfalfa	0.75	
Animal bone	0.25	
Milk	0.10	3×10^{-12}
Seawater	0.005	1.5×10^{-13}
Fish flesh (pelagic)	0.0015	
Whole fish (pelagic)	0.0005	1.5×10^{-14}
Plowed soil (2 feet)		
All Ca from fertilizer (.02 gm/cm²)	1	
All Ca natural	0.1	
Highly calcareous soil (Marshalls)	0.003	

[1] For 25 MT worldwide fallout.

TABLE 7-A.—*Radioactivity levels in nearby fallout area as of February 1955*

Item	Median total B- μc/gm	Median Sr90 μc/gm	Item	Median total B- μc/gm	Median Sr90 μc/gm
Lagoon:			Islands—Continued		
Large fish	4×10^{-5}	9×10^{-8}	Ferns	1×10^{-4}	
Small fish	8×10^{-5}		Coconuts	2×10^{-5}	1×10^{-7}
Crabs and clams	5×10^{-4}		Arrowroot	6×10^{-5}	1×10^{-7}
Snails	2×10^{-3}	1×10^{-6}	Pandanus	7×10^{-5}	6×10^{-7}
Coral	8×10^{-5}	4×10^{-7}	Papaya	6×10^{-5}	8×10^{-8}
Water	2.3×10^{-6}	9×10^{-8}	Breadfruit	6×10^{-6}	
Plankton	9×10^{-5}	9×10^{-8}	Morinda fruit	7×10^{-5}	1×10^{-6}
Islands:			Ocean:		
Rats	4.5×10^{-4}		Jellyfish	4×10^{-4}	ND
Fresh water algae	7×10^{-3}	4×10^{-6}	Fish	3×10^{-5}	ND
Fresh water	8×10^{-7}	8×10^{-8}	Plankton	6×10^{-5}	ND
1st inch soil	5×10^{-3}	2×10^{-5}	Water	9×10^{-7}	ND
Rooster	6×10^{-4}				

TABLE 8.—*Sr-90 in nearby fallout (assuming no fractionation from fission products)*

	curies/cm.²	Sea water, curies/gram Ca	Whole fish, μ curies/gram flesh	Proportion MPD [1]
Maximum	1.1×10^{-7}	2.7×10^{-8}	6.6×10^{-7}	8.0
Mean	6.0×10^{-9}	1.5×10^{-9}	3.8×10^{-8}	0.5
Minimum	2.0×10^{-12}	5.0×10^{-13}	1.2×10^{-11}	0.00015

	curies/cm.²	Land, Wisconsin, curies/gram Ca	Milk, μ curies/gram	Proportion MPD [1]
Maximum	1.1×10^{-7}	5.4×10^{-6}	6.5×10^{-4}	8,000
Mean	6.0×10^{-9}	3.0×10^{-7}	3.6×10^{-5}	500
Minimum	2.0×10^{-12}	1.0×10^{-10}	1.2×10^{-8}	0.15

Zn-65 in same fallout (assuming Zn-65 induced in equal atom yield to Sr-90)

	curies/cm²	curies/gram Zn	Fish flesh		Fish liver, proportion MPD [1]
			μ c/gram	Proportion MPD [1]	
Maximum	5×10^{-6}	1.0×10^{-1}	1.0	800	8,000
Mean	2.7×10^{-7}	5.4×10^{-3}	5.4×10^{-2}	18	180
Minimum	9.0×10^{-11}	1.8×10^{-6}	1.8×10^{-5}	0.006	0.06

[1] For children.

TABLE 9.—*Variation of radioactivity in different parts of a single fish*[1]

	Gama activity
Body part	([2])
Liver	1,060
Stomach and contents	320
Vertebrae	60
Flesh	40
Spleen	2,200
Gall bladder	440
Kidney	100
Tail fin	200
Scales	60

[1] Snapper collected in Marshall Island lagoon April 1956, and counted immediately.
[2] d/m/gm of wet weight.

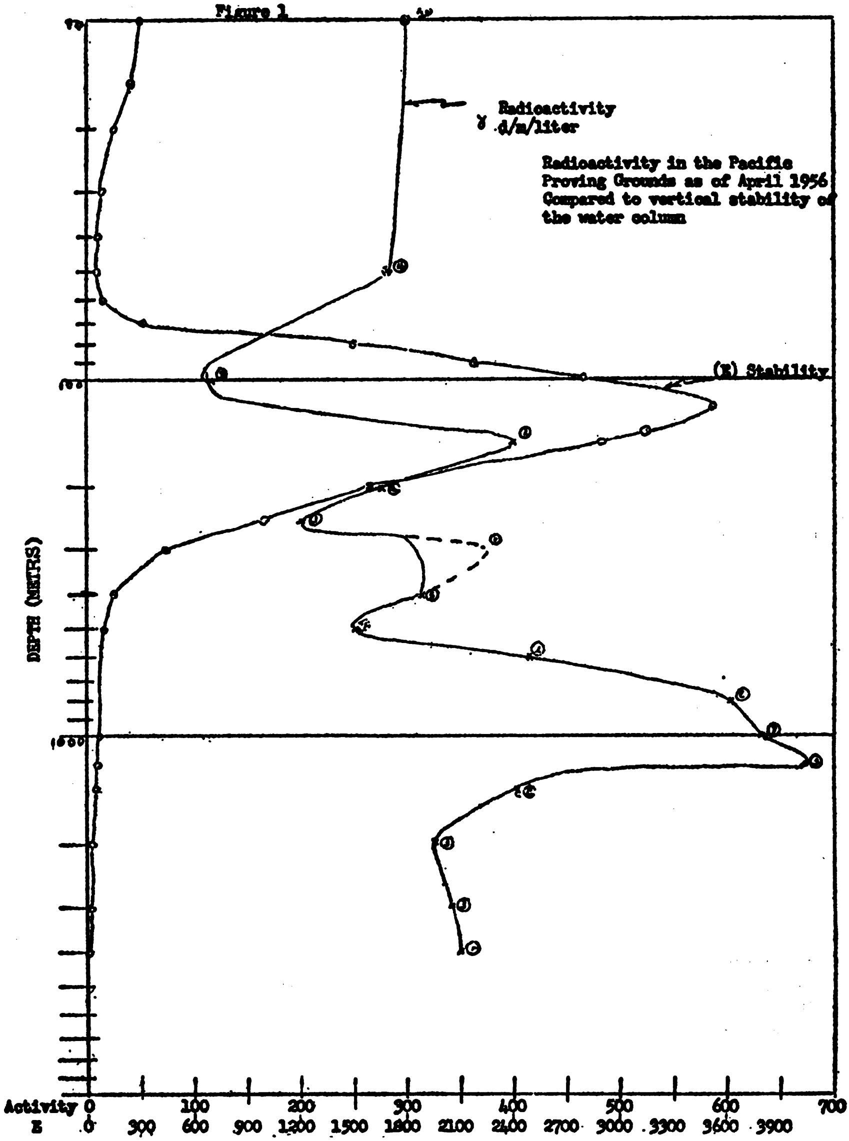
TABLE 10.—*Average per capita daily intake of calcium in Japan from Hiyama, Y; maximum permissible concentration of Sr-90 in food and its environment, Records of Oceanographic Work in Japan, vol. 3, 1956*

Kind of food	Ca (mg.)	Sr/Ca by weight
Marine fish	120	1/100
Freshwater fish	5	1/100
Rice and cereals	150	1/1,000
Vegetable, milk, and others	100	1/1,000
Total	375	

Principal isotopes from slow neutron fission of 1 kilogram U-235

Isotope	Percent	Activity in curies	Isotope	Percent	Activity in curies
1 day, 3.22×10^8 curies:			20 days, 9.8×10^6 curies—Continued		
2Xe-135	12.5	40.3×10^6	Nd-147	5.0	4.9×10^5
Cb-97	9.7	31.2×10^6	Ru-103	4.4	4.3×10^5
Zr-97	8.9	28.7×10^6	Rh-103	4.2	4.1×10^5
Y-93	7.5	24.2×10^6	Cb-95	2.3	2.3×10^5
I-133	7.3	23.5×10^6	Ce-144-Pr-144	2.64	2.6×10^5
Sr-91	6.7	21.6×10^6	Mo-99	1.3	1.3×10^5
Ce-143	6.7	21.6×10^6	I-132	1.05	1.0×10^5
I-135	5.7	18.3×10^6	Total	96.89	9.51×10^6C
Mo-99	5.6	18.0×10^6	1 year, 3.1×10^5 curies:		
Y-92	4.1	13.2×10^6	Ce-144-Pr-144	52.8	164×10^3
Pr-145	3.0	9.7×10^6	Cb-95	14.7	45.6×10^3
Y-91	3.0	9.7×10^6	Zr-95	7.2	22.4×10^3
I-132	2.7	8.7×10^6	Pm-147	5.7	17.7×10^3
Te-132	2.6	8.4×10^6	Y-91	3.8	11.8×10^3
Pm-149	1.45	4.7×10^6	Sr-89	2.7	8.4×10^3
Rh-105	1.35	4.4×10^6	Ru-106-Rh-106	4.9	15.2×10^3
La-141	1.35	4.4×10^6	Sr-90-Y-90	3.7	11.5×10^3
Ba-140	1.25	4.0×10^6	Ca-137-Ba-137	2.9	9.0×10^3
Xe-133	1.22	3.9×10^6	Ru-103	0.8	2.5×10^3
I-131	0.90	2.9×10^6	Rh-103	0.8	2.5×10^3
Total	95.52	3.01×10^8C	Total	100.0	3.1×10^5C
20 days, 9.8×10^6 curies:			20 years, 9.8×10^4 curies:		
La-140	13.9	13.6×10^5	Sr-90, Y-90	48	47×10^3
Ba-140	12.0	11.8×10^5	Cs-137, Ba-137	45	44×10^3
Pr-143	12.0	11.8×10^5	Pm-147	3.4	3.3×10^3
Ce-141	9.7	9.5×10^5	Sm-151	2.6	2.5×10^3
Xe-133	6.3	6.2×10^5	2-Kr-85	1.23	1.2×10^3
Zr-95	5.9	5.8×10^5	Total	100.23	9.8×10^4C
Y-91	5.6	5.5×10^5			
I-131	5.6	5.5×10^5			
Sr-89	5.0	4.9×10^5			

Figure 1



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Research in the Effects and Influences of the Nuclear Bomb Test Explosions

Compiled by Committee for Compilation of Report on Research in the Effects of Radioactivity

JAPAN SOCIETY FOR THE PROMOTION OF SCIENCE, PP. 1–1824, 1956—SEE PARTICULARLY THE FOLLOWING PAPERS

Y. Miyake, Effects of Atomic Explosions on the Atmosphere and the Sea, p. 1.

Y. Miyake, Y. Sugiura, The Radiochemical Analysis of Radio-Nuclides in Sea Water Collected Near Bikini Atoll, p. 409.

Y. Miyoke, Y. Sugiura, K. Kameda, On the Artificial Radioactivity in the Sea Near Japan, p. 415.

T. Kiba, S. Ohashi, M. Shibata, T. Minabe, Radioactive Substances Found on the Contaminated Fish, p. 537.

T. Shokowa, T. Kambara, S. Kokudai, F. Yomo, K. Kojima, Radiochemical Research of Fishes Contaminated with Radioactivity, p. 561.

M. Saiki, S. Yoshino, R. Ichikawa, Y. Hiyama, T. Mori, Studies on the Radioactivity of Fishes Caught from the Pacific Ocean in 1954, p. 825.

K. Amano, K. Yamada, M. Bito, A. Takase, Studies on the Radioactivity in Certain Pelagic Fishes. 1. Distribution of Radioactivity in Various Tissues of Fishes, p. 839.

K. Yamada, H. Tozawa, K. Amano, A. Takase, Studies on the Radioactivity in Certain Pelagic Fish. II. Group Separation of Radioactive Element in Fish Tissues, p. 849.

K. Yamada, H. Tozawa, K. Amano, T. Takase, Studies on the Radioactivity in Certain Pelagic Fish. III. Separation and Confirmation of ^{65}Zn in the Muscle Tissues of a Skipjack, p. 855.

T. Kawabata, Radiochemical Studies of Contaminated Fish, p. 861.

K. Saito, M. Sameshima, Studies on the Radiologically Contaminated Fish Caught at Kagoshima Sea Region, p. 875.

R. Ichikawa, Y. Hiyama, Some Notes on the Radioassay of the Contaminated Fishes, p. 883.

T. Mori, M. Saiki, Studies on the Radioactive Material in the Radiologically Contaminated Fishes, p. 889.

K. Nagasawa, I. Kawashiro, Results of Investigations and Examinations on the Influences upon the Food Sanitation in Japan Caused by the Hydrogen Bomb Experiment at Bikini Atoll, March 1954, p. 895.

G. Yoshii, Studies on the Radioactive Samples (especially "Katsuwonus Vagans") Collected by the "Shunkotsu Maru" in the Pacific Ocean in 1954, p. 917.

S. Watari, Summary of Investigation by the "Shunkotsu Maru" into the Effects of Radioactivity in the Bikini Water, p. 939.

T. Kawabata, State of Radiological Contamination in Fishes and its Possible Routes on the Basis of the Findings of "Bikini Expedition", p. 943.

Y. Miyake, Y. Sugiura, K. Kameda, On the Distribution of the Radioactivity in the Sea around Bikini Atoll in June 1954, p. 953.

H. Miyoshi, The Atoll and Radiologically Contaminated Water, p. 965.

M. Uda, S. Hori, S. Yoshida, Hydrographic Researches in the Equatorial Pacific waters adacent to Bikini Atoll in Relation to the Radioactive Pollution of the Fishing Grounds, p. 983.

H. Nakamura, Survey by the "Daifuji Maru" traversing the Pacific especially on the Radioactivity of Sea Water contaminated by H-Bomb Tests at Bikini, 1954, p. 997.

T. Kuroki, T. Tanoue, On the Radioactivity of Fishes, Plankton and Seawater in the Sea off Kagoshima, p. 1011.

T. Morita, K. Saitō, Hydrographical Distribution of Radioactivity in the Coral Sea and the Equatorial Sea Fronts of the Pacific Ocean, p. 1021.

Y. Hiyama, R. Ichikawa, T. Isogai, Y. Oshima, H. Koyama, S. Arasaki, K. Nozawa, State and Behavior of Fission Products in Fresh-water, Brackish-water, and Marine Coastal System in Japan, 1954–55, p. 1027.

H. Nakamura, H. Yabe, T. Kamimura, Y. Yabuta, A. Suda, S. Uyeyanagi, H. Yamanaka, p. 1067.

Y. Hiyama, R. Ichikawa, Movement of Fishing Grounds where Contaminated Tuna were Caught, p. 1079.

T. Kawabata, Radiological Survey of Fishes at the Landing Ports in Japan, p. 1085.

Y. Matsue, Transmission and Metabolism of Fission Products in Aquatic Organisms (Summary), p. 1095.

Y. Matsue, R. Hirano, Accumulation of Radioactivity in Plankton Cultured or Kept in Water Infusions of the "Bikini Ash", p. 1099.

Y. Hiyama, R. Ichikawa, F. Yasuda, Studies on the Processes of Contamination of Fish by the Fission Products, p. 1119.

Y. Suyehiro, S. Yoshino, Y. Tsukamoto, M. Akamatsu, K. Takahashi, T. Mori, Transmission and Metabolism of Strontium 90 in Aquatic Animals, p. 1135.

R. Ichikawa, Y. Hiyama, Deposition of Radiostrontium to the Vairous Tissues of Jack Mackerel, Mackerel, and Tuna, p. 1143.

T. Tomiyama, S. Ishio, K. Kobayashi, Absorption of Dissolved ^{45}Ca by Carassius Auratus, p. 1151.

T. Tomiyama, S. Ishio, K. Kobayashi, Absorption in Carassius Auratus of ^{45}Ca Contained in Rhizodrilus Limasus, p. 1157.

T. Tomiyama, S. Ishio, K. Kobayashi, Absorption of Dissolved ^{45}Ca by Marine Fishes, p. 1163.

S. Ishio, Excretion of Absorbed ^{45}Ca by Gold Fish, p. 1169.

T. Tomiyama, K. Kobayashi, S. Ishio, Absorption of Dissolved ^{45}Ca by Rhizodrilus Limasus, p. 1173.

T. Tomiyama, K. Kobayashi, S. Ishio, Excretion of Absorbed ^{45}Ca by Rhizodrilus Limasus, p. 1177.

T. Tomiyama, K. Kobayashi, S. Ishio, Absorption of ^{90}Sr (^{90}Y) by Carp, p. 1181.

T. Tomiyama, K. Kobayashi, S. Ishio, Distribution and Excretion of Intramuscularly Administered ^{90}Sr (^{90}Y) in Carp, p. 1189.

T. Tomiyama, K. Kobayashi, S. Ishio, Absorption of $^{32}PO_4$ Ion by Carp, p. 1195.

T. Tomiyama, K. Kobayashi, S. Ishio, Distribution and Excretion of Intramuscularly Administered $^{32}PO_4$ by Carp, p. 1201.

T. Mori, M. Saiki, Studies on the Distribution of Administered Radioactive Zinc in the Tissues of Fishes, p. 1205.

COMMITTEE ON EFFECTS OF ATOMIC RADIATION IN OCEANOGRAPHY AND FISHERIES, 1956, SUMMARY REPORT, THE BIOLOGICAL EFFECTS OF ATOMIC RADIATION, NATIONAL ACADEMY OF SCIENCES—NATIONAL RESEARCH COUNCIL

(Individual chapters in press)

1. Craig, Harmon, Disposal of Radioactive Wastes in the Ocean: The Fission Product Spectrum in the Sea as a Function of Time and Mixing Characteristics.

2. Renn, Charles E., Physical and Chemical Property of Wastes Produced by Atomic Power Industry.

3. Folsom, Theodore R., and Harley, John H., Comparison of Some Natural Radiations Received by Selected Organisms.

4. Wooster, Warren S., and Ketchum, Bostwick H., Transport and Dispersal of Radioactive Elements in the Sea.
5. Carritt, Dayton E., and Harley, John H., Precipitation of Fission Product Elements on the Ocean Bottom by Physical, Chemical, and Biological Processes.
6. Ketchum, Bostwick H., The Effects of the Ecological System on the Transport of Elements in the Sea.
7. Boroughs, Howard, Chipman, Walter A., Rice, Theodore R., Laboratory Experiments on the Uptake, Accumulation and Loss of Radionuclides by Marine Organisms.
8. Krumholz, Louis A., Goldberg, Edward D., Boroughs, Howard, Ecologic Factors Involved in the Uptake, Accumulation, and Loss of Radionuclides by Aquatic Organisms.
9. Krumholz, Louis A., and Foster, Richard F., Accumulation and Retention of Radioactivity from Fission Products and other Radiomaterials by Fresh-water Organisms.
10. Donaldson, Lauren, and Foster, Richard F., Effects of Radiation on Aquatic Organisms.
11. Craig, Harmon, Isotopic Tracer Techniques for Measurement of Physical Processes in the Sea and the Atmosphere.
12. Folsom, Theodore R., and Vine, Allyn C., On the Tagging of Water Masses for the Study of Physical Processes.
13. Schaefer, Milner B., Large Scale Biological Experiments using Radioactive Tracers.

Dr. REVELLE. I would like to begin, Mr. Chairman, by making some generalities about the ocean. These are all platitudes, but we have to keep them in mind in considering the problem of radioactivity in the ocean. In the first place, the ocean is very large and very deep. It covers about 71 percent of the earth's surface, and its average depth is 4,200 meters or about 14,000 feet. Second, it is a highly concentrated salt solution. It contains almost all of the known chemical elements, many of them, however, in very small quantities.

Third, the ocean is stratified, that is, there is subsurface mixed layer on the average somewhat less than 300 feet thick, and below this is a series of layers of increasing density which have relatively little communication with each other, except in high latitudes and along the shores. The surface mixed layer, the top 300 feet, is of principal interest to human beings because it is in this layer that most of the organisms, most of the marine plants and animals that constitute the ocean harvest, live at least most of the time. There is very rapid mixing within the top layer, and very slow mixing between the upper layer and the depth.

Fourth, there is a very low level of natural radioactivity in the ocean. The natural radioactive substances such as potassium 40 are present only to the extent of one one-hundredth—the total of all natural radioactive substances in the ocean is only about one one-hundredth of that on land in terms of their radioactivity.

As a result both sailors and fish have a much lower level of exposure to natural radiation than any other creatures living on the earth.

Representative HOLIFIELD. I assume that is true except on the *Lucky Dragon.*

Dr. REVELLE. That is right. I said natural radioactivity.

Fifth, most fallout ends up in the ocean. This is true for 3 reasons. In the first place, a large part of the tests of nuclear weapons have been conducted over the ocean—those tests conducted by the United States and the British. The close-in fallout nearly all falls over the sea. Because the ocean covers 71 percent of the earth's surface, a very

large part, up to 71 percent but probably somewhat less, of the stratospheric fallout falls in the ocean. As far as the distant so-called tropospheric fallout is concerned, the portion is less than 71 percent, because of the higher percentage of land in the Northern Hemisphere, but still probably about 50 percent.

The rapid mixing to somewhere between 50 and 100 meters of the ocean means that fallout falling on the sea surface rapidly is diluted down to this depth. In the trade-wind region it takes about 30 hours for very small particles to be thoroughly mixed in the top hundred meters. This gives great protection to ships and to anybody on the surface of the sea, provided the fallout does not actually end up on him or his ship. The fallout ending up on the surface of the ocean represents a hazard from external radiation of a fifth to a hundredth of the corresponding fallout on the land. During the first half hour after fallout the factor of protection is about five; that is, the amount of the radiation is about one-fifth or about 20 percent of that from a land surface. After the first half hour the amount of radiation received is only about one hundredth.

The waters of the ocean move, but they move very sluggishly. The average speed of the surface currents is about 0.5 of a mile per hour to 1 mile per hour. That is somewhere between 12 and 24 miles per day. The mixing between the upper and lower layers probably takes something of the order of 200 to 500 years to occur. This is true except in areas of upwelling along the shores of the ocean where water of intermediate depths comes up to the surface in considerably greater speeds but still quite slowly—only 1 or 2 meters a day.

Representative HOLIFIELD. Are there areas in the deepest part of the ocean where the water remains permanently there and does not come up to the surface as far as you know?

Dr. REVELLE. Not as far as we know. But the best chance for finding deep water that has been there for perhaps as much as 1,000 years is in some parts of the Pacific Ocean. So far we have not really carried out enough investigations to know how long it does stay down.

Representative HOLIFIELD. No doubt you realize the reason for my question is the problem of disposal of radioactive wastes. Of course, that will be testified to later. We have some testimony on the disposal of radioactive wastes, but I thought I would take this opportunity in view of the fact that you are speaking on the ocean phenomena to ask you that question.

Dr. REVELLE. As a matter of fact, I have given a great deal of thought to this. I am the chairman of the National Academy of Sciences' Committee on the Effects of Radiation in Oceanography and Fisheries, and this is one of the questions that we have been very much concerned with. I believe that certainly some protection can be gained even from fairly long-lived fission products by putting them into the deep sea. But the question of how much protection depends on a whole series of unknowns, and one of these certainly is the rate at which the deep water moves up to the surface. We think that in the Pacific it may be perhaps more than 500 years, although there is a very large factor of uncertainty.

More important probably than the rate at which the deep and surface waters mix is the fact that the deep waters themselves mix, and become fairly well homogenized over very large volumes so that the concentration of radioactivity might be quite low. The thing

which bothers us, as I will point out in a minute, is that the marine organisms concentrate radioactivity by a very large factor.

I might also point out that the marine organisms, unlike the waters, move fairly rapidly. Many of them conduct a vertical migration between day and night over a depth of 1,000 or 2,000 feet. They move up to the surface in the night and down to a depth of several hundred fathoms during the day. Many of the fish we are interested in, such as tuna, marlin, sailfish, bonito, skipjack, and salmon, migrate over very large horizontal distances. This is also true of the marine mammals such as the whales.

Representative HOLIFIELD. Do they feed on any of the marine life that moves vertically up and down?

Dr. REVELLE. Yes, sir, they do indeed; particularly the whalebone whales, and the tunas feed almost exclusively on these smaller organisms which do migrate over depths that I have mentioned.

Representative HOLIFIELD. What are the natures of some of the small organisms? You are not referring to plankton?

Dr. REVELLE. I am referring to zooplankton and other invertebrates which swim, such as the squid. In tropical regions of the Pacific very large tuna are obtained by the Japanese at considerable depths below the surface during the daytime and presumably the big tunas are down there, because that is where their food supply is, although this is not very well known, either. I do not want you to get me wrong. We really know very little about the ocean. Everything I say is subject to a good deal of uncertainty.

Representaitve HOLIFIELD. That is spoken like a true scientist. I think any scienist who testifies before us disclaims any knowledge whatever of the subject upon which he is testifying. We take that as modesty and we would rather have you that way than the other way.

Dr. REVELLE. As a matter of fact, in my case it is not modesty. It is just a simple statement of fact. Oceanographers are masters of not knowing very much about what they are doing. One of the things that is very often said, and I think with complete justification, is that we know less about the bottom of the ocean than we do about the surface of the moon.

Although the ocean has almost every substance in it, many of these substances, as I pointed out a minute ago, are present in very low concentration. Hence in order to live marine organisms have to concentrate the substances they need for their growth from sea water. Often they concentrate such substances—these trace substances—by factors of many thousand times. This necessity for concentrating trace substances from sea water means that the marine organisms are especially adapted for doing this job, and it means they will concentrate other substances present in small concentrations, such as artifically radioactive substances originating from fallout, or in other ways also by factors of many hundreds to many thousand of times.

What is the significance of these generalities? I have stated nearly everything I can in my prepared statement, but perhaps we might summarize some aspects of this problem of the significance. Coming first to the close-in fallout, even several months after a major weapons test there is a relatively high level of fallout within 500 to 1,000 miles of the test site for a period of at least a few months. This depends, in the case of the Marshall Islands area, on the sluggish

nature of the motion of the water. Here I have shown on this chart the surface currents in the neighborhood of Bikini Atoll. Here is Bikini. Here is Eniwetok. Kwajalein gives you some idea of the scale of the chart. The green lines show the surface current and the red lines currents at a depth of about 1,000 feet.

We see that the surface currents are somewhat disturbed by the existence of the atolls. But in general we have a motion from east to west of the order of half a mile per hour, or about 12 miles per day. At a depth of about a thousand feet, there is a very much more obvious effect of the existence of the islands. We see a great eddy in the neighborhood of Bikini Atoll. The water, instead of moving past the atoll, tends to stay in this area right around the atoll for a very considerable length of time.

As a result of this difference in the circulation near the surface and at depth, a graph in my statement shows that in fact after 2 years the concentration of radioactive substances in the neighborhood of Bikini is much higher at depth than at the surface by a factor of 2 or 3. It is quite low, however, at all depths. That is 2 years after a test.

Representative HOLIFIELD. Is that strong enough to affect the edibility of the fish in that area?

Dr. REVELLE. Yes, sir, it certainly is, as I will show you in just a few minutes. I simply want to point out here the relatively sluggish motion of the currents, which means that the fallout tends to stay in this general area, but is diffused laterally and as I pointed out, vertically in the top 100 meters.

I am sorry the next chart is on such a small scale, but these will be left with you. This small scale has the great advantage that it shows how big the ocean is. This is the water hemisphere covering just about half the earth—the Pacific Ocean. In this area in here this is Bikini, here is Japan, the Philippines, New Guinea, Australia, United States, and South America. This area of 1,000 miles around Bikini was carefully investigated by Japanese oceanographers and biologists 4 months after the Castle test. They got figures like this in the water: 23,000, 90,000, 79,000, 26,000 disintegrations per minute per liter of sea water. The values that we are talking about here are values of the order of one one-hundredth to three one-hundredths of a microcurie per liter of water 4 months after a test. The distribution was quite spotty as we go along this time. (See folding chart, p. 551.)

I will read some numbers—5,100, 90,000, 14,000, 16,000, 23,800, and 16,500. This is at a distance of about 300 miles.

Representative HOLIFIELD. Is that measurement of quantities of sea water?

Dr. REVELLE. Those are disintegrations per minute of radioactive material.

Representative HOLIFIELD. In a certain amount of sea water.

Dr. REVELLE. In a quart of sea water. This is about 300 miles west of Bikini Atoll. When we go a thousand miles west of Bikini, we get much smaller values. The biggest one here is about 4,500 disintegrations per minute per liter.

Representative HOLIFIELD. What is the natural disintegration per minute per liter?

Dr. REVELLE. In the water itself, due to the natural radioactivity, if we are talking about gamma radiation, it is about 50 disintegrations

per minute per liter. For gamma plus beta about 10 times this amount, or about 500; 70, I guess, is a better figure than 50. You can see over a distance of several hundred miles, the amount of radioactivity in the water was as much as a hundred times greater than the natural radioactivity. The radioactivity in fish caught in this area was very widely variable. One fish caught here about 100 miles west of Bikini had between three and four hundredths of a microcurie per gram in its liver. In other words, about 10 microcuries per pound of fish liver.

Chairman DURHAM. That is a much more rapid disintegration in sea water than it is on land.

Dr. REVELLE. It is a more rapid disperson because of the mixing, as I pointed out, down to a hundred meters, and because of the horizontal spreading of the water.

Chairman DURHAM. It does not have any effect on the half life.

Dr. REVELLE. It does not have any effect on the half life, except that you do get some separation due primarily to the fact that some materials are larger than others, and settle to great depths.

Thirteen months after the test, the situation is shown by the next chart. This shows the result of a cruise conducted by the Atomic Energy Commission and the Office of Naval Research in which oceanographic work was done by Dr. Wooster and others of my institution, and the radioactivity measurements were made by the New York operations office of the AEC under the direction of Dr. Harley. By this time, 13 months later, the radioactivity had moved all the way to Japan, but it was present in very much smaller concentrations. The maximum was off the Philippines, where about 5 times background was obtained. Mostly the values were only a couple of times background. (See p. 551.)

Representative HOLIFIELD. Would that be sufficient strength to affect the edibility of the fish in that area?

Dr. REVELLE. I am not sure it would affect the edibility, but it certainly affects the radioactivity of the fish.

Representative HOLIFIELD. Would the measurement in the areas where fishing is done for food be detrimental to the human body?

Dr. REVELLE. It is my opinion that they probably were not, this far away. I will come to some other considerations on this subject in just a minute. Actually I would like, if possible, to refer you to one of the tables here, if I may. I am not sure I have this figure quite straight there. I guess we have not got any table from that far away in this particular operation. I should refer you to the report which is called Operation Troll. It is given among my references on page 5, United States Atomic Energy Commission, 1956. That is page 5 of the references. That publication gives the values of the radioactivity. I am sorry I don't remember them. I simply wanted to use this chart to point out how the radioactivity is spread over the course of 13 months after the test, and how spotty it is, the maximum being over here [indicating on the chart].

Representative HOLIFIELD. What are some of those figures?

Dr. REVELLE. These are disintegrations per minute per liter of water in excess of the natural background. The highest value was 210 off the Philippines. Here, south of Japan, it was 192 distinegrations per minute per liter of water. In the plankton the disintegrations are per minute per gram. The highest value was again off the

Philippines: 118 disintegrations per minute per gram. The highest value, I am sorry, was between the Philippines and Japan. That is 137.

Representative HOLIFIELD. That is in the plankton, you say?

Dr. REVELLE. Two thousand disintegrations per minute would be a thousandth of a microcurie. So this is about one ten-thousandth of a microcurie per gram of plankton.

Representative HOLIFIELD. Is it not true that as you go up from the plankton to the fish that you get an unusual multiplication of that?

Dr. REVELLE. It depends entirely on the substance we are talking about.

Representative HOLIFIELD. If we are talking about plankton that is consumed by the lower forms of marine life, and there is a very marked increase in concentration—in other words, they collect it and maintain that out of the plankton—until its cumulative effect in the higher form of marine life is much more marked. Is that not true?

Dr. REVELLE. I don't think that is so, sir. The actual factors of concentration are shown in table 6 of my statement. You will see that the vertebrates, which in the sea are primarily fish, do concentrate some substances to a much higher degree than the plankton which they eat, and this is particularly true of zinc. In the case of strontium, the fish discriminate against strontium by about a factor of three. In the case of iron again there is a very considerable discrimination in the vertebrates as opposed to the invertebrates. On the other hand, the fish do concentrate phosphorous by a factor of 2 million whereas the invertebrates never go above 10,000. So it is not possible to make any generalization. We have to investigate every element—not every isotope—independently. This depends on how the organism absorbs the material, whether it comes to it by simply physical absorption or in its food supply and whether he needs the substance for growth or for his vital processes.

These, I should emphasize, are the best estimates that a group of us on the National Academy Committee could make. I still would not be willing to place a bet at any odds on any one of those figures.

The next chart shows the distribution of fish catches that had more than 100 counts per minute when assayed by the Japanese within the first 6 or 7 months after the 1954 test. You can see that unlike the water and the plankton, which behaves just like water, the migratory fish extend over a very much wider area. On the other hand, most of these fish had a relatively small amount of radioactivity in them. As I remember, the Japanese monitored 78,000 tons of fish. Only 385 tons were found to have more than 100 counts per minute with the type of equipment they used. (Portion of chart, p. 552.)

Chairman DURHAM. What do you mean by counts?

Dr. REVELLE. Just the number of counts on the Geiger tube.

Representative HOLIFIELD. That would not make the fish inedible?

Dr. REVELLE. This is a matter of opinion. The Japanese discarded all these fish.

Representative HOLIFIELD. Then it was inedible as far as they were concerned.

Dr. REVELLE. If we look at the distribution of radioactivity in the fish, remember, only one-half of 1 percent of the fish were found to be radioactive, to give you figures—something like 385 tons were discarded out of 78,000 tons. Only about 3 or 4 tons, about 1 percent of

the discarded fish—0.8 of a percent to be exact—had more than 5,000 counts per minute; 65 percent had a count of less than 100 counts per minute.

Representative HOLIFIELD. Could you tell us if the high counts occurred at specific times after the test? That is, did it show a pattern in relation to any specific test or was it fish that were obtained without direct relation to time.

Dr. REVELLE. These were fish that were obtained, I think, within the first 7 or 8 months after the first Castle series of tests. In this time period there did not seem to be any relationship between the count and the time. This is partly due to the fact that we have a rather inadequate sample.

Representative HOLIFIELD. You say a rather inadequate or an adequate sample?

Dr. REVELLE. A rather inadequate sample. I just do not know. I will point out, however, that nobody could sample the ocean better than the Japanese. Their fishing is conducted over almost the entire ocean and very intensively, in contrast to the United States, where about 11 pounds of fish per person are eaten per year, and the Japanese eat about 80 pounds of fish per person per year.

Representative HOLIFIELD. This certainly explains to some extent part of their concern, because they are dependent upon the sea for a substantial part of their diet.

Dr. REVELLE. Yes. As I will try to show in a few minutes, this has itself both good and bad aspects from the point of view of radioactivity.

So much for the charts. A great deal has been said about strontium 90 and the dangers from it because of its being a long-lived fission product which is a bone seeker. One thing that can be said from this point of view about the ocean is that strontium 90 in the ocean represents a considerably smaller hazard than strontium 90 on most soils.

Chairman DURHAM. Has there been the same general increase since 1952 as has been on the land?

Dr. REVELLE. Remember what I said to begin with.

Chairman DURHAM. That is why I asked the question.

Dr. REVELLE. At least three-quarters of the strontium falls into the ocean. However, it is present in quite low concentrations in the ocean compared to the concentration on the land.

Chairman DURHAM. But there has been an increase.

Dr. REVELLE. There certainly has been an increase in principle. There are not enough analyses for us to tell you the figures of what this increase is. It should be fairly clear cut from an a priori point of view. We know it will mostly stay in the surface layers and mostly spread throughout the surface layers.

Representative HOLIFIELD. Because there is more depth and area in the ocean there is more chance for diffusion of fallout than the land, which must necessarily concentrate it in the first few inches of soil.

Dr. REVELLE. That is absolutely correct. Table 7 of my statement shows this quantitatively. If we compare the soil that Dr. Libby is always talking about, the Wisconsin soil—simply because his group on the Sunshine project has made a lot of measurements there—with other substances, you will see that the strontium-to-calcium ratio, if

we take it as one in the Wisconsin soil, and one-tenth in milk, in sea water will be only 0.005. In fish flesh, because of the fact that there is some discrimination against strontium in fish, it will be only 0.0015. In whole fish, including the bones, we have a strontium-calcium ratio only five ten-thousandths of that which we would find in the Wisconsin soil, and only five one-thousandths of what we would find in milk. It is only the very highly calcareous soils of the Marshall Islands region which result in little strontium in the food grown on them. It is for this reason that the Japanese eating a lot of fish and obtaining about a third of their calcium from fish, means that they are subjected in terms of food intake to very much less strontium 90 hazard than the people of Wisconsin.

Representative HOLIFIELD. I think that is a very important statement, Doctor, because if that statement can be sustained, it disposes of what is, I believe, a common belief that there is a great buildup in the Japanese people because of their diet of fish.

Dr. REVELLE. I think the facts are, sir, as I attempted to point out, that the reverse is probably true from the standpoint of strontium alone. The fact that their calcium intake is in large part from fish means that they will suffer less hazard from strontium.

Chairman DURHAM. What part of the fish does the strontium assimilate itself in mostly?

Dr. REVELLE. Most of it goes to the bone. However, the strontium-calcium ratio in the flesh is higher than in the bone. You often hear it said that strontium in the fish bone does not represent a hazard. I don't think this is true. The Japanese, and in fact all of us, eat fish bones all the time, not by accident, but by design. Whenever we eat a sardine or anchovy or small herring or many little fish, we eat them bones and all. We eat the skin which also very often tends to concentrate these radioactive substances.

Representative HOLIFIELD. Let me ask you a question which I certainly do not understand thoroughly myself. It has been called to my attention that the Japanese people are concerned about radioactive iodine 131. Could you shed any light upon that particular point?

Dr. REVELLE. No, I cannot. I would think that this is not a problem they would face with their fish catch, although I may be wrong. It has such a short life I would think they are primarily concerned there with fallout—the tropospheric fallout—reaching them either by the winds from the Marshall Islands or from Russia.

Representative HOLIFIELD. This element would not be collected by either the fish or by any vegetation which is grown in the sea. If I remember, they do use some of the kelp, do they not, in the sea as an element of diet in Japan?

Dr. REVELLE. That is correct. It is certainly concentrated by marine organisms. If you look at table 6, you will see that algae concentrate iodine by a factor of 10,000 and vertebrates by a factor of about 1,500.

Representative HOLIFIELD. Then it would be in some sort of marine vegetation rather than fish if it is in existence?

Dr. REVELLE. That is right. However, the Japanese eat a lot of marine plants and algae. Everything you eat in Japan is wrapped in seaweed practically. I don't have any opinions on this subject.

Representative HOLIFIELD. We will explore that later when we get to the medical part of our testimony.

Dr. REVELLE. We have not considered that because it has such a short half life.

Continuing with the strontium for just a minute, it is clear that only that in such calcareous soil as the Marshalls is the hazard from strontium 90 less than it is in marine animal foods. This is demonstrated in table 7–A, where we have listed the concentrations of radioactivity found in marine animals and in the water, in land animals and in the soil, and in the open ocean as well as in the lagoons from the Marshall Islands area as of February 1955. You will see that although the lagoon water had 2.3 times 10 to the minus 6 microcuries per gram, while the soil had 5 times 10 to the minus 3, a factor of 2,000 greater for the same amount of material the amount of strontium——

Representative HOLIFIELD. Which one are you referring to now?

Dr. REVELLE. Just look in general terms there. Just take a quick look at all of the figures on table 7–A. First look at the water. Notice how much more radioactivity there is in the soil than in the lagoon water, and more in the lagoon water than in the open ocean.

Second, notice that total radioactivity in the marine animals is just about the same as the total radioactivity in the land animals and the land plants. These numbers obviously cover quite a range. One gets the impression that the radioactivity in the marine animals is about the same as it is in the land animals, not only in the lagoon, but also in the open ocean. The marine animals have concentrated the radioactivity by a factor of as much as a thousand over that in the water, whereas the land animals in general have not concentrated the radioactivity, but have less radioactivity than the soil in which they grow. So we have just the reverse effect between the marine animals and the land animals as opposed to the water and the soil.

On the other hand, notice the strontium values for the most part in the marine organisms are no greater—they are about the same—as the strontium values in the land animals. The marine and land animals have about the same strontium even though the marine animals have concentrated radioactivity relative to the water, whereas the land animals have lost radioactivity relative to the soil. In a soil with less calcium than the Marshall Islands soils, the land would have relatively much more radiostrontium. This is because of this protection that marine animals have from strontium because of the fact that the strontium-calcium ratio is different between the ocean and ordinary soils.

Representative HOLIFIELD. There is more calcium available to them?

Dr. REVELLE. That is right.

Chairman DURHAM. Looking at the water, Doctor, is the water on the island used for consumption? I notice your table runs about the same with fresh water.

Dr. REVELLE. I don't know, sir.

Chairman DURHAM. The ocean water runs 9 to 10.

Dr. REVELLE. I am sorry I did not understand your question.

Chairman DURHAM. I asked the question did they use the water on the island because of the fact that it carries just about as much strontium as the salt water does.

Dr. REVELLE. Yes, that is right. I don't know whether this water was used.

Chairman DURHAM. I should imagine they have to drink water.

Dr. REVELLE. Yes. These were not inhabited islands at the time of the measurements. I am afraid I may have confused this, but perhaps we can straighten it out in the record. The fact is that the strontium is about the same with these very, very calcareous soils, and with soils that are less calcareous we have more concentration in the land plants and animals than in the marine animals.

Even with the maximum amount of fallout observed in the Marshall Islands area, as shown in table 8—remember that this is very spotty—nearby fallout very shortly after the test, the fish living in that water and keeping the concentration at the maximum would still have only eight times the maximum permissible dose in terms of microcuries per gram or microcuries per milliliter. Whereas with similar fallout on land, milk would contain 8,000 times the maximum permissible dose.

This is just another way of emphasizing the same thing, the great advantage that the people who eat fish have over the people who drink milk and get their calcium that way.

Representative HOLIFIELD. In order to clear up this in order that no one may say that this ratio of contamination exists in Wisconsin, this would be assuming that we had the same kind of test in Wisconsin that we had in the Marshall Islands.

Dr. REVELLE. Exactly; yes.

Representative HOLIFIELD. That is a completely hypothetical assumption and therefore nothing for anyone to take alarm at.

Dr. REVELLE. I should hope not, except that they ought to eat more fish.

In contrast to the strontium problem, there are two substances produced by the large weapons tests which are not fission products. One of them is cobalt 60, and the other is zinc 65. Zinc and cobalt are a necessity for growth. Surprisingly enough, they are very highly concentrated by marine organisms, but not by the same organisms. You will notice in my statement on page 6, amounts as high as one-third of a microcurie of cobalt 60 were found in the liver of an 8-inch clam in a location off the Marshall Islands, about 80 miles from the location of an explosion 2 years before. The average cobalt 60 in 3 samples of clam livers was about 1½ microcuries per pound. The factor of concentration of cobalt in these animals may be several hundred thousand over the amount in water because radiocobalt could not be detected in the surrounding water or in the sand of the reef. In other words, these clams and mollusks in general are very much better chemists than people are. They are able to make separations very much more effectively than we are able to do by any of the presently available techniques.

It should be remembered that traces of cobalt are an essential constituent of human life, and are a part of vitamin B-12. It is one of the constituents. This concentration of cobalt took place in the clams.

Zinc 65 on the other hand was concentrated by the fish and just about to the same extent that the cobalt was in the clams. The highest concentration was found in the internal organs and in the skin. The average amounts in these organs of a parrot fish and a snapper were about 0.7 of a microcurie per pound, or about 0.0014 microcurie per gram.

Tuna from the open sea had about twice as much zinc per unit weight as these lagoon fish. The amounts in the fish flesh were lower by a factor of 10 than those in the internal organs and the skin. Considerably more than half of the entire radioactivity in the clams was radiocobalt and in the fish was radiozinc. The concentration of radiocobalt was 10 times higher in the clam and of radiozinc 10 times higher in the fish. Before we get alarmed about these figures, because of the biological half life of both cobalt and zinc being relatively short compared to the biological half life of strontium 90, the maximum permissible concentrations for radiozinc and radiocabalt are about 100,000 times those for strontium. For example, these average values about 80 miles from the test site 2 years later of about 0.0015 microcurie per gram are about a fourth of the average permissible dose for zinc. The average concentration of 0.003 microcurie of cobalt was about twice the maximum permissible dose for cobalt.

I would like to say two other things. The first one is that marine organisms live in different radiological domains, even with respect to artificial radioactivity. Some of the radioactivity settles through the water onto the bottom or can settle through the water onto the bottom, and bottom living organisms just like organisms on land therefore tend to be subject to a higher dose than organisms swimming freely in the water. There are such things as clams and other marine mollusks which are eaten by many people, including, I hope, by most people in this room. Also, kelp and seaweed. Kelp is very widely harvested and used in this country. Every quart of ice cream you eat has some algae and kelp in it. These bottom living organisms are attached to the bottom through which and across which the water flows, and tend to live in a different radioactive regime than the organisms that swim freely in the water.

Finally, I should point out that there are many ways both actual and potential in which artificial radioactivity can get into the ocean besides the fallout from nuclear tests. One of the ones that must certainly be kept in mind is the possibility of an accident to a nuclear powered ship. Suppose, for example, that a reactor is damaged and its fuel elements with their contained fission products are lost in the waters of an average size or somewhat small size harbor in the United States, if this is a 50,000 kilowatt reactor, which does not seem to be exceptionally large, and has been going for a year, there will be approximately 10 kilograms of fission products, making up about 10 to the seventh curies. In a harbor 8 miles long by 3 miles wide, 50 feet in depth, if the fission products become uniformly distributed, the water would contain about 10 to the minus 2 curies per cubic meter, giving almost a constant radiation dose of about 0.5 of a roentgen per day on the surface. So there is a real hazard from a nuclear reactor which is damaged—a shipborne nuclear reactor in a harbor.

Representative HOLIFIELD. Of course, the Congress has authorized, as you know, a merchant marine ship for a large reactor. There has been 1 carrier and I believe there are 2 or 3 others. There are several submarines already in existence. As you point out, if by the very nature of war if these were destroyed or sunk or the reactors were disrupted by torpedoes, you would have this element of contamination, wouldn't you?

Dr. REVELLE. I am thinking of peacetime uses of atomic energy here. Just the very attractive prospect of powering a merchant fleet

with nuclear power. The number of collisons between merchant ships is surprisingly large. I think some attention should certainly be paid to this problem, and I believe the Atomic Energy Commission and the Maritime Commission are thinking quite seriously about it.

Representative HOLIFIELD. I hope the care is being given to it that should be given to it.

Dr. REVELLE. The other ways in which fission products and other artifically radioactive substances can get into the ocean are by the disposal of atomic waste. This question is a very complicated one, as you undoubtedly know, who have been ont his committee for several years. My own opinion is that although in this country it may be possible to dispose of the waste from the nuclear power industry in several ways, that this may not be so easy in countries like England, Italy, and Japan, which have a very dense population, have not got many holes in the ground, and are completely surrounded by the ocean. Of course, as we know, most countries which are now operating reactors are in fact disposing of low level wastes into the coastal waters. I urge at every opportunity in public that this matter should be very carefully controlled. I am glad to see the United Nations Committee on Biological Effects of Radiation has now taken the same point of view.

Representative HOLIFIELD. I think that it accents the importance of these hearings, too, to bring a realization to the people that we are dealing with a very serious element here, and the disturbance which it can make to nature if it is not controlled properly.

Dr. REVELLE. On the other hand, a very valuable one if it is controlled. The potentialities are tremendous.

Chairman DURHAM. I assume from your statement, then, Doctor, that you would not recommend it as a burial ground?

Dr. REVELLE. What was that, sir?

Chairman DURHAM. That you would not recommend the ocean as a burial ground for fission products?

Dr. REVELLE. I think that I would not either recommend it or not recommend it. I think the probabilities are that the ocean can be used for this purpose, but we really need to know a good deal more than we know now about the concentration of different substances by different organisms and in different parts of organisms. We need to know a lot more than we now know about the circulation and particularly the vertical movement of the ocean waters. We need to know a lot about the potentialities of transport of radioactive substances by organisms moving from one layer to the other. We also need to know something even more fundamental and much more difficult to understand, and that is the nature of the mixing process in the ocean, and particularly the exchange between the bottom muds and the waters as well as the rates and character of dispersion in the water itself. The problem here is that this seems to be quite spotty. You get high concentrations and low concentrations.

Representative HOLIFIELD. Would you say that this is an area where a great deal of research is needed?

Dr. REVELLE. I think that research is needed, but I naturally would, being an oceanographer.

Representative HOLIFIELD. Could you tell me how much research is actually being done? Let us put it that way.

Dr. REVELLE. It is very hard to separate one kind of marine research from another.

Representative HOLIFIELD. How much is directed toward the development of information on this so-called vertical movement from the lower muds up to the surface?

Dr. REVELLE. Actually very little indeed on this problem. The Woods Hole Oceanographic Institution, the Lamont Geological Observatory, the University of Washington, Texas A. and M., the Chesapeake Bay Institute, the Scripps Institution, the Japanese are all concerned with the problem, but not as yet with the level of effort that seems to be highly desirable.

Representative HOLIFIELD. Could you give us an estimate as to the number of scientists that are working on these projects and whether these projects are being financed by the grants from AEC or whether they are operating on other types of grants?

Dr. REVELLE. I am not sure that I can give you the exact figures, but my guess would be that the AEC is spending in these institutions that I listed something of the order of $150,000 a year. The Office of Naval Research and other parts of the Department of Defense are spending in related projects perhaps twice or three times this amount. The relationships are often not very close. This work that I have been talking about today, for example, has been supported by the Department of Defense and the AEC partly. As far as the Scripps Institution is concerned, it is supported more continuously by the Department of Defense.

Representative HOLIFIELD. Would you say that in your best opinion if there is going to be considerable disposal in the low ocean depths of radioactive wastes there should be, in advance of any policy such as that, a thorough exploration of this subject?

Dr. REVELLE. Yes, indeed I would, sir. I would urge it very strongly indeed for the welfare of mankind.

Chairman DURHAM. How many people are with the Scripps Institution?

Dr. REVELLE. What is the total staff of the Scripps Institution?

Chairman DURHAM. Yes.

Dr. REVELLE. About 550 people.

Chairman DURHAM. Are all of them primarily concerned with this research?

Dr. REVELLE. At the present time, at least, not more than 15 out of that number are involved with this kind of problem. It is a very small percentage, and I think the percentage should be increased. I think it could be increased.

Chairman DURHAM. You do not have any contract with AEC?

Dr. REVELLE. We have a small contract with the AEC, yes.

Representative HOLIFIELD. In this field, or a different field?

Dr. REVELLE. In this field, yes. We get a lot of support from the Federal Government. The total budget of the institution is about $3½ million a year, of which two-thirds comes from the Federal Government.

There is one other very minor point I would like to make. It looks as if some of the radioactivity in the ocean—artificial radioactivity—comes from the land. The ocean is a great hole in the ground, and at least in our part of the ocean, off the California coast, there is a

very considerable increase in the radioactivity of the sea water after a rainfall. This increase seems to be due to fission products almost entirely. So we have here an example of the complicated cycle of these things.

The surprising thing about it is that we have not yet been able to find any cesium or strontium in the water off our coast. Particularly the cesium is a curious deficiency because we have been looking for it with a device that ought to detect it. It looks as if cesium is trapped in the soil and does not run off with the other fission products.

I have kept you gentlemen too long. Thank you very much.

Chairman DURHAM. What is the opinion for that increase in the sea water off the west coast?

Dr. REVELLE. It come from the fallout which has settled on the land and washed into the ocean. At least that is our present opinion. But this opinion needs to have a lot more substantiation. This is simply a hypothesis. It is kind of interesting because it closes the cycle from the ocean explosion to the air to the land and back to the ocean.

Representative HOLIFIELD. Dr. Revelle, you have given us a most interesting presentation here today, and I am only sorry that the full membership of the committee could not be here to hear this. You understand that we have important legislation in both bodies today. Mr. Durham and I have been staying with this and missing a lot of votes on amendments which we hope to pick up tomorrow when we have our record votes. Thank you very much.

Dr. REVELLE. I certainly appreciate the chance of appearing here.

(The charts referred to by Dr. Revelle follow:)

n Pacific: Disintegrations per minute/liter
ute/gram of plankton. Course of research
① surface currents, May–June 1954; ②
wet liver of marlin tuna. Y. Miyake,
Research Institute, Tokyo.) (Uda, Hori,
Fisheries, the Japanese Hydrographic De-
tional Institute of Health.)

Radioactiv
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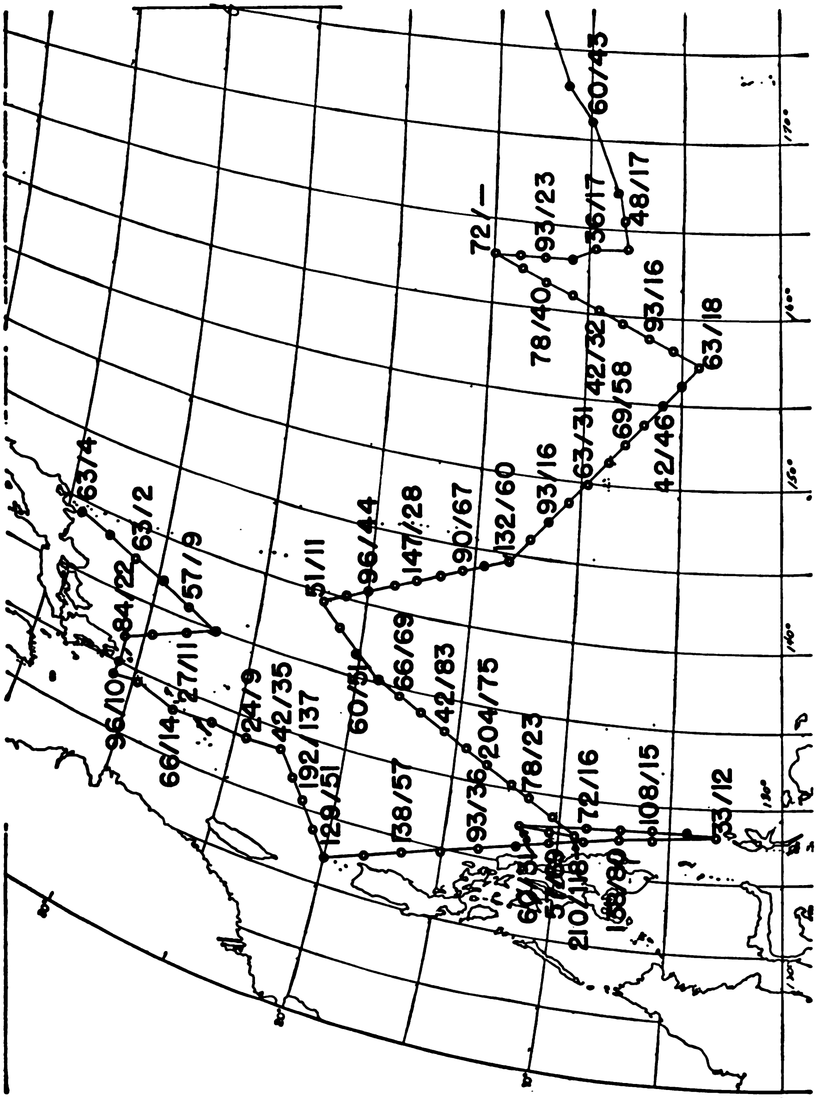

Troll cruise—Disintegrations per minute per liter of seawater/disintegrations per minute per gram of plankton.

Distribution of contaminated fish hauls

CPM:	*Percent of fishes*
5,000	0.8
3,000	1.8
1,000	14.3
500	18.8
100	64.3

NOTE.—Radioactivity of fishes measured at 10 cm. from surface of fish (T. Kawabata, the National Institute of Health).

Representative HOLIFIELD. The meeting will reconvene in this room tomorrow morning at 10 o'clock, and we will have Dr. Merril Eisenbud of the New York Operations Office of the Atomic Energy Commission, Dr. Kulp of Columbia University, and Dr. Neuman of the University of Rochester School of Medicine and Dentistry. The subject will be a detailed discussion of the occurrence of strontium 90 and cesium 137 in the atmosphere, and biosphere, and its uptake and behavior in man.

Until tomorrow at 10 we stand adjourned.

(Thereupon at 5:20 p. m., Tuesday May 28, 1957, a recess was taken until Wednesday, May 29, 1957, at 10 a. m.)

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

WEDNESDAY, MAY 29, 1957

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION
OF THE JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The special subcommittee met, pursuant to recess, at 10:10 a. m., in the caucus room, Senate Office Building, Hon. Chet Holifield, chairman of the subcommittee, presiding.

Present Representatives Holifield, Durham (chairman of the joint committee), Price, Van Zandt; Senators Anderson, Knowland, Hickenlooper, and Bricker.

Also present: Professional staff members: James T. Ramey, executive director; George E. Brown, Jr., Hal Hollister, staff technical adviser, and Paul C. Tompkins, consultant.

Representative HOLIFIELD. The subcommittee will be in order.

This morning we have a notable array of witnesses. We have several witnesses, and we plan to add on additional witnesses from the Walter Reed Institute of Research to our list of this morning, provided the time allows. But before we call the first witness to the chair, I intend to make a short statement.

This committee and its chairman, under the direction of the committee, is trying to hold an objective and fair hearing. We are trying to bring to this record scientific opinion on the different facets of nuclear weapon fallout, and of the scientific problems that are pertinent thereto.

There will, of necessity, be controversial opinions stated; and this is as it should be in the scientific world, because different scientists on their own responsibility and integrity certainly have a right to be heard.

We do not want to get into the field of controversy with anyone as a committee, but there was called to my attention this morning a release by the Atomic Energy Commission in regard to this question of a clean or dirty bomb.

In the first place, the Atomic Energy Commission violated its own rules by not preparing and submitting to the committee its release 24 hours in advance of its release. We do not intend to get into this question at this time, and into a controversy with the Commission, but the record will stand as it has been given; the remarks of Dr. Graves will stand as those of the man most qualified to give an opinion on the cleanness or the dirtiness of nuclear weapons, and at the proper time there will be other evidence brought forward on this subject.

93299°—57—pt. 1——36

Our first witness this morning is Mr. Merril Eisenbud of the New York Operations Office of the Atomic Energy Commission. The subject this morning is "A Detailed Discussion of the Occurrence of Strontium 90 and Cesium 137 in the Atmosphere, Biosphere and Its Uptake and Behavior in Man."

Mr. Eisenbud has a notable record in the scientific world.

Mr. Eisenbud, proceed please.

STATEMENT OF MERRIL EISENBUD, MANAGER, NEW YORK OPERATIONS OFFICE, ATOMIC ENERGY COMMISSION [1]

Mr. EISENBUD. Thank you, Mr. Chairman.

If it pleases the committee, I would like to submit the prepared text of my statement, which is entitled "Measurement of Strontium 90 in Geophysical and Biological Materials," for the record of the proceedings.

Representative HOLIFIELD. Without objection, it shall be received.

(The statement referred to follows:)

UNITED STATES ATOMIC ENERGY COMMISSION,
NEW YORK OPERATIONS OFFICE,
New York, N. Y.

Summary of remarks by Merril Eisenbud, Manager, New York Operations Office and Director, Health and Safety Laboratory before hearings conducted by the Joint Congressional Committee on Atomic Energy on the Nature of Radioactive Fallout and Its Effects on Man, May 1957, Washington, D. C.

MEASUREMENT OF STRONTIUM 90 IN GEOPHYSICAL AND BIOLOGICAL MATERIALS

INTRODUCTION

The studies being reported on are directed toward a full understanding of the physical and biological behavior of the strontium 90 produced in nuclear detonations. The fundamental questions to be answered by consideration of the data are:

1. How much strontium 90 has been deposited on the earth's surface?
2. How much strontium 90 from detonations to date remains suspended in the upper atmosphere and how long will it take to precipitate?
3. How much strontium 90 will human skeletons contain when they are in equilibrium with the expected levels of strontium 90 in soil?

The properties that make strontium 90 the most hazardous of the nuclides formed in the fission process are its long half-life (28 years) and its chemical similarity to calcium. Because of its resemblance to calcium, strontium 90 may be assimilated by biological processes. If strontium is ingested by human beings in food or water, it will deposit, like calcium, in the skeleton.

Samples are collected throughout the world for strontium 90 analyses. Many of these are sent to the United States for analyses, but others, in increasing numbers, are analyzed in the laboratories of other lands by the many scientists whose data, like our own, are routinely submitted to the United Nations Committee on the Effects of Radiation.

Monitoring for radiostrontium can be divided conveniently into studies of its geophysical and biological distribution. Under the former classification are collected those samples which give us an understanding of the behavior of radiostrontium from its formation in the fireball to its deposition on the surface of the earth and incorporation into soils. The biological studies trace the movements of strontium 90 from the soils and waters through the flora and fauna of the oceans, pastures, and farms, to the skeleton of man.

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[1] Date and place of birth, March 18, 1915, New York, N. Y. Education: electrical engineering, New York University, 1936. Work history: Liberty Mutual Insurance Co., industrial hygienist. 1936–47: Atomic Energy Commission, industrial hygienist. Director, Health and Safety Laboratory, 1947–49; New York University, associate professor of industrial medicine, 1945–51; Atomic Energy Commission, Director, Health and Safety Laboratory, New York Operations Office, 1949–54; Atomic Energy Commission, Manager, New York Operations Office, 1954—. (Submitted by the Atomic Energy Commission.)

TYPES OF SAMPLES

Soils.—A soil sample, expertly selected, can represent the accumulated fallout at a given location. An ideal sampling site is considered to be an open, level area, undisturbed by cultivation and covered by grass or simple vegetation covering to immobilize the surface. The most common division of sampling is to collect the top 2 inches and the 2-to-6-inch layer separately. The known area and the measured weight of sample allow the strontium 90 measurements to be converted to terms of millicuries per square mile.

Experimental work has shown that the movement of strontium 90 in the soil is slow enough that adequate samples can be obtained. For example, from 75 to 80 percent of the strontium 90 found in the soil is in the top 2 inches. The disadvantage of soil samples is in the complex and time-consuming nature of the analysis.

Collections in open pots.—Stainless steel pots, with a face area of about 1 square foot, are finding increasing application in fallout sampling both here and abroad. The collected precipitation and dust are analyzed for strontium 90 at intervals, usually on a monthly basis.

Gummed film collections.—The need for a simple collecting technique suitable for network operations, where large numbers of collecting stations employ untrained personnel, led to the development of the gummed film collector. This collector has an adhesive surface of 1 square foot which is exposed for 24 hours. A network of up to 200 stations collecting daily samples has been operated by the Health and Safety Laboratory since 1951. Samples are mailed to the laboratory where the analyses are performed.

Atmospheric sampling.—The fission products, including strontium 90, are in particulate form in the atmosphere and the most common air sampling procedure is to draw a large volume of air through a filter. This type of sampling has been carried out at the earth's surface, in the lower atmosphere by jet planes, and in the stratosphere by balloons.

BIOLOGICAL DISTRIBUTION

The chemical similarity of strontium and calcium makes it desirable to use the strontium 90: Ca ratio in tracing strontium 90 from soils to man. A useful unit of strontium 90 contamination, first proposed by Libby, is the micromicrocurie of strontium 90 per gram of calcium ($\mu\mu c$/gmCa).

The three principal types of samples taken for analysis are (1) vegetation, including both animal and human food, (2) milk, and (3) human bone.

Vegetation.—Strontium 90 may occur in vegetation in either of two routes. Strontium 90 from the soil may be taken up by the root system and incorporated into the plant. In addition, fallout may be directly deposited on the plant.

Milk.—The major source of body calcium in the United States is milk, hence its analysis has received considerably more attention than that of any other foodstuff although other foods may have a higher strontium 90 to Ca ratio. Actually, cow's milk contains less strontium per gram of calcium than do the vegetables which comprise the balance of our calcium intake. This is because biological processes in general tend to discriminate against strontium. Thus the strontium 90 has been selectively eliminated by 2 stages—the vegetable and the cow—in the case of milk and only 1 stage in the case of a vegetable. Milk seems to be the most practical index of human exposure to strontium 90 because it is the source of most of the calcium in the skeletons of American adults, and it is a material which is relatively easy to sample.

Human bone.—Analyses of human bone afford a direct measure of the strontium 90 level at a given time. These samples show the lowest levels of strontium 90 of any biological material being analyzed. Since human bone is not yet in equilibrium with the strontium 90 of the environment, however, measurements made at the present time are only of value when viewed in relation to other materials, particularly human food and the present and predicted levels of strontium 90 on the earth's surface.

DISCUSSION OF FINDINGS

Estimates of fallout obtained by analyzing soil samples are lower than those obtained by gummed film collection but the reason why this is so is not clear. One megacurie, being intermediate between the estimates for gummed paper and for soil, is perhaps the most reasonable approximation of the total amount of strontium 90 deposition on the earth by delayed fallout in mid-1956.

It is also noted that the soil data indicate a greater degree of latitudinal variation than the gummed film data. In the latter case the mean deposition in the North Temperate Zone is slightly high in relation to the somewhat uniform deposition elsewhere in the world. The data derived from soil samples likewise indicate a higher deposition in the North Temperate Zone but there is less uniformity elsewhere in the world and a north-south gradient is quite evident.

STRATOSPHERIC RESERVOIR

An estimate of the future distribution of strontium 90 may be obtained from the sampling data plus knowledge of the amount of strontium 90 suspended in the upper atmosphere, and the rate at which it precipitates to earth's surface.

It is apparent that the rate of precipitation of the strontium 90 must be considered in any estimation of future hazard. If the time of descent was infinitely great, the strontium 90 would decay before it reached the earth's surface and it would not constitute a potential hazard. Actually, we have learned that the time of descent is relatively short in relation to the half-life of strontium 90. It is estimated that the average residence time is approximately 10 years (equivalent to a half-life of 7 years). It is possible this estimate is too long and the average life is as little as 6 to 7 years (half-life about 4½ years).

This discussion of the foreseeable levels of strontium 90 will thus be simplified by the assumption that the material now contained in the stratospheric reservoir will be completely deposited on the earth's surface before any radioactive decay has occurred. Moreover, it will be assumed that the geographical distribution in the future will follow approximately the same distribution as the deposition of stratospheric debris in the past. This will tend to introduce an error on the side of safety since it would be expected that future fallout would be more uniform than in the past.

FUTURE ESTIMATE OF STRONTIUM 90 IN MAN

It will be assumed that essentially all of the 2.4 megacuries of strontium 90 stored in the stratosphere in mid-1956 will be deposited on earth's surface by about 1970 and that stratospheric fallout will be distributed in approximately the same pattern as the past.

This discussion of future levels of strontium 90 in man will be based on data for a region in Northeastern United States where the deposition of strontium 90 in the fall of 1956 averaged 20 millicuries per square mile. Of this, about 6 mc/mi^2 is estimated to be tropospheric fallout from tests prior to mid-1956. The stratospheric fallout may thus be estimated as 14 millicuries per square mile. This was the level which existed when the worldwide deposition of strontium 90 was about 1 megacurie. It is estimated that when the 2.4 megacuries then in the stratosphere has deposited, the deposition on this milkshed will be 45 millicuries per square mile, about 2.3 times the level in mid-1956.

To define the potential risk from a given distribution of strontium 90 on the surface of the earth requires that the distribution be quantitatively related to the skeletal burden of strontium 90 of a human population in dietary equilibrium with the soil from which its nourishment is derived. This equilibrium is already established for a variety of radioactive elements normally present in the earth's crust. For example, the average upper foot of soil in the United States contains about 1,000 millicuries of radium per square mile. The average adult skeleton in this country contains about 10 microcuries of radium, resulting from assimilation of this trace element from foods and water. Thus, the value of 10^{-4} microcuries of radium represents the amount deposited in the skeletons of the populations whose mineral metabolism is in equilibrium with the soil minerals.

Freshly deposited strontium 90 takes a relatively long time to complete the biological route to bone since bone being formed at the present time utilizes calcium which left the soil in months gone by. In addition, storage of cattle fodder and hold-up of human foods in commercial distribution lead one to expect the human strontium 90 burden to lag in time behind a given soil concentration. Equilibrium can be expected to be achieved over a period of years but not over a period of months.

During periods of actual fallout the concentration of strontium 90 in milk originates from the soil by normal root uptake or it may short circuit the soil through deposition directly on the leaf and ingestion by the cow.

At present, the extent to which strontium 90 occurs in milk as a result of direct deposition on leaves is not known. This fraction presumably diminishes with time as the accumulation in soil increases and the rate of fallout remains approximately constant. In contrast, the strontium 90 of the soil constitutes a relatively long lived reservoir for future uptake. Diminution will result only from radioactive decay or if the strontium 90 is leached beyond the root zone. For the purpose of this discussion it will be assumed that all of the strontium 90 in milk is metabolized by way of the roots. This is a conservative assumption which tends to exaggerate the forecast of future levels.

Milk from the milkshed previously discussed has been analyzed routinely since early 1954. The strontium 90 level of this milk averaged 3.5 $\mu\mu c/g$ Ca in the period just preceding the sampling of the soils in October 1956. If we neglect the effect of fresh fallout, and further assume that the strontium 90 in milk is proportional to the amount in soil, the future level may be estimated as $35 \times 2.3 = 8 \mu\mu c$ Sr-90/g Ca. This prediction is in good agreement with the value of 8.3 $\mu\mu c/g$ Ca which was similarly estimated by the data available from this milkshed in the summer of 1955.

Based on the data now available a child being nourished on milk containing $8 \mu\mu c$ Sr-90/g Ca would be expected to develop a skeleton containing strontium 90 in somewhat lower concentrations than this value (because the human metabolism descriminates in some measure against strontium,) possibly higher than 4 $\mu\mu c$ Sr-90/g Ca. Thus 4-6 $\mu\mu$/g Ca may be said to be the highest foreseeable value to be attained from a diet containing 8 $\mu\mu c$ Sr-90/g Ca.

Assuming 5 $\mu\mu c$ Sr-90/g Ca to be the maximum value to be attained, one can calculate this amount of strontium 90 will deliver a dose of 0.5 rads to the skeleton over a lifetime of 70 years. This compares with a normal skeletal irradiation of 7 to 30 rads resulting from potassium 40, carbon 14, cosmic rays, terrestrial gamma radiation and radium. The maximum foreseeable value of 5 $\mu\mu c$ Sr-90/g Ca is thus equivalent to 1.5 to 6 percent of the dose from natural sources of skeletal irradiation.

It should be noted that this estimate includes a number of assumptions which are deliberately conservative. No allowance has been made for radioactive decay before the strontium 90 descends from the stratosphere. This may diminish the amount of available strontium 90 by about 25 percent. The assumption that all of the strontium 90 in milk originated by root uptake, is another conservative assumption. The combined effect of these and other safety factors is apt to be appreciable.

The question has been raised as to whether a discussion of average fallout values is adequate to define the upper limit of hazzard to people exposed to unusually heavy fallout. In this connection it is worth noting that as the data continue to accumulate from every corner of the globe the large deviations from average are in the safe direction. Whereas fallout values greater than twice the mean are rarely reported in any given region, it is not uncommon to observe values which are of the order of 10 percent of the average.

This forecast has been made on the basis of data from Northeastern United States. How applicable are these data to other parts of the United States or of the world? Summaries of the concentration of strontium 90 in milk as observed in 6 domestic and 2 foreign sources of supply as well as the data from the fallout measuring systems described earlier, indicate that the strontium 90 content of dietary calcium might be higher in certain areas. A factor of three applied to the estimates for northeastern United States should be adequate, however, to bracket the highest foreseeable value, from tests to date, in any region of the United States.

I. Measurement of Strontium 90 in Geophysical and Biological Materials

By Merril Eisenbud for presentation at hearing on the Nature of Radioactive Fallout and its Effects on Man

INTRODUCTION

The properties that make strontium 90 the most hazardous of the nuclides formed in the fission process are its long half-life (28 years) and its chemical similarly to calcium. Because of its resemblance to calcium, Sr-90 may be

assimilated by biological processes. If strontium is ingested by human beings in food or water, it will deposit, like calcium, in the skeleton.

Investigation of the potential hazard from contamination of soils and the biological food chains by Sr–90 began very early in the United States atomic energy program. The first studies, associated with the wartime weapons-development program, were theoretical and were designed to identify the principal parameters which influence the long-range effects of nuclear detonations. It was clear, from the start, that studies of radioactive fallout, and of the ultimate fate of Sr–90 in particular, would require the application of knowledge from a wide assortment of the physical and biological sciences. The initial theoretical studies provided a valuable basis for the experimental approach to the problem that became possible with the programs of weapons testing that began in 1948 and have continued intermittently to the present time.

The Sr–90 studies have increased in scope and complexity, and the overall program has, for some years, been global in extent, involving physical, chemical, and biological investigations on land, in the oceans, and in the air. Known as Project Sunshine and directed at a full understanding of the physical and biological behavior of the Sr–90 produced in nuclear detonations, these studies are concerned with an unprecedented variety of scientific questions. From the standpoint of its vast geographic dimensions and the variety of scientific mechanisms involved in the investigation, Project Sunshine rivals the most comprehensive scientific studies ever undertaken.

The factors that influence the behavior of Sr–90 begin in the complex physics and chemistry of the fireball and the mushroom-shaped cloud which forms after a nuclear detonation. The height of the burst aboveground, the nature of the terrain, and the particle size of the soil and debris sucked into the fireball all influence the fallout pattern.

When the particles descend to the earth's surface, they leave the domain of the meteorologist and become involved in the physics, chemistry, and biology of the soil. How soluble is the Sr–90 in fallout? Does it leach from the soil? At what rate is it incorporated into plants, and how can this rate be expressed quantitatively as a function of type of soil and type of plant? These are a few of the questions that have been studied in tracing the Sr–90 into the first of the biological links in the food chain between soil and man. The answers to these and many more questions have been obtained by many investigators working in many laboratories throughout the country.

From its formation in a nuclear detonation until it is metabolized by man, the path of a strontium 90 atom is long and tortuous. Understanding of its route has come from studies which know none of the bounds of any one of the conventional scientific disciplines. The phenomenology of strontium 90 in fallout can be described only in the combined languages of all the principal combined sciences—geophysics, physical chemistry, biophysics, and biological chemistry. Samples are collected throughout the world for strontium 90 analyses. Many of these are sent to the United States for analyses, but others, in increasing numbers, are analyzed in the laboratories of other lands by the many scientists whose data, like our own, are routinely submitted to the United Nations Committee on the Effects of Radiation.

Monitoring for radiostrontium can be divided conveniently into studies of its geophysical and biological distribution. Under the former classification are collected those samples which give us an understanding of the behavior of radiostrontium from its formation in the fireball to its deposition on the surface of the earth and incorporation into soils. The biological studies trace the movements of strontium 90 from the soils and waters through the flora and fauna of the oceans, pastures, and farms, to the skeleton of man. A summary of the AEC sampling program is given in table I.

SAMPLING FOR GEOPHYSICAL DISTRIBUTION

Except for immediate fallout in the area of detonation, the original fission products are injected into the troposphere or stratosphere and are geographically distributed by the winds. Radioactive materials are brought down to the surface by precipitation, settling, and, to a lesser extent, by air turbulence.

It is desirable to measure the rate of fallout, its accumulation, and the atmospheric reservoir of material yet to be deposited on earth's surface. The samples currently taken for this program include soils, sea water, collections of fallout in open pots and on gummed film, and collections of atmospheric dust on filters.

SOILS

A soil sample can represent the accumulated fallout at any given location. There are many criteria to be met to insure that a soil sample is representative. An ideal sampling site is considered to be an open, level area, undisturbed by cultivation and covered by grass or simple vegetation covering to immobilize the surface. Drainage slopes, silted areas or other unusual drainage conditions should be avoided. Samples are collected with soil augers to give definite areas and depths. The most common division of sampling is to collect the top 2 inches and the 2- to 6-inch layer separately. The known area and the measured weight of sample allow the strontium 90 measurements to be converted to terms of millicuries per square mile.

Removal of strontium 90 from a soil sample is a difficult problem because the relatively low levels of activity currently found require use of very large samples. At present 2 to 4 pounds of soil are taken for analysis and this makes the usual procedure of completely dissolving the soil by fusion a practical impossibility. Thus although the fusion technique is the only method certain to remove all strontium 90 from the soil it has been necessary to study other methods for strontium 90 extraction. After considerable experimentation, procedures that agree satisfactorily with the fusion technique have been developed. These involve the extraction of the strontium 90 by electrodialysis or by hydrochloric acid leaching without completely dissolving the soil. Present evidence leads us to believe that the acid leach removes 85 percent or more of the strontium 90 and the electrodialysis at least 75 percent. Some early analyses were obtained by ammonium acetate leaching of the soil, but this method is now felt to be inadequate, and none of the data presented here were obtained in this way.

The chemical analysis following extraction requires separation of the strontium 90 from the inactive bulk constituents of the extract and from other radioactive isotopes, which include the natural constituents of the soil and other fission products, including strontium 89.

The soil sample offers a practical means of measuring the accumulation of strontium 90 at a particular location. Experimental work has shown that the movement of strontium 90 in the soil is slow enough that adequate samples can be obtained. For example, from 75 to 80 percent of the strontium 90 found in the soil is in the top 2 inches. The disadvantage of soil samples is in the complex and time-consuming nature of the analysis.

SEA-WATER SAMPLES

Since approximately two-thirds of the earth's surface is covered by the oceans, a complete accounting for strontium 90 deposition requires sampling of the oceans. However, in contrast to soils, the strontium 90 in the oceans is distributed through a depth of several hundred feet, at least. The analytical requirements for this type of sample have been beyond the capabilities of the participating laboratories except for samples taken near the Pacific Proving Grounds shortly after a major weapons test. Therefore, studies of distribution in the ocean have been limited to measurements of total fission products and their transport by ocean currents as a function of time. Such measurements of mixed fission products do not give reliable estimates of strontium 90 in sea water since the fission products may be fractionated by solubility, precipitation, and sedimentation.

Mixed fission products in sea water are concentrated by coprecipitation of the majority of the isotopes with various carriers such as iron hydroxide or calcium carbonate. The activity levels of interest are high enough so that conventional counting equipment can be used for measurement. Extension of the sensitivity by measurement with low background counters would probably not be justified in terms of the overall accuracy of the determination.

FALLOUT COLLECTIONS IN OPEN POTS

Stainless-steel pots, with a face area of about 1 square foot, are finding increasing application in fallout sampling. Other types of vessels have been tried and are currently used in some parts of the monitoring program in this country and abroad. The collected precipitation and dust can be analyzed for strontium 90 at intervals, usually on a monthly basis. In sharp contrast to the soil samples, the residues submitted to analysis are essentially free of nonradioactive material. The monthly increment in fallout is readily determined from

these samples, while it is only feasible to measure annual increments in soil samples.

The analysis of pot collections requires the separation of strontium 90 from the small amounts of inert material and from other isotopes of mixed fission products. The deposition of naturally occurring radioisotopes during a month is negligible. The levels currently found are such that low background counting equipment is required for satisfactory analysis.

Collection in pots is open to the criticism that more fallout may be collected than would be deposited in the open air. This is caused by inpaction on the inner sides of the vessel. Studies are underway to determine the best shape and size of vessel for collections. However, it should be noted that comparison of annual pot increments with soil increments have shown good agreement.

GUMMED FILM COLLECTIONS

The need for a simple collecting technique suitable for network operations, where large numbers of collecting stations employ untrained personnel, led to the development of the gummed film collector. This collector has an adhesive surface of 1 square foot which is exposed for 24 hours. A network of up to 200 stations collecting daily samples has been operated by the Health and Safety laboratory since 1951. Samples are mailed to the laboratory where the analyses are performed. It is not possible to determine Sr–90 directly in gummed film—first, because the amount of Sr–90 deposited in 24 hours is to a small, and second, because during rainfall a variable fraction of the Sr–90 is lost by preferential solubility. The Sr–90 component has in the past been estimated by calculation from the mixed product values. In addition, an estimate has been made of the gamma dose resulting from this fallout. The increased weapons test activity has made these calculations extremely difficult and the methods originally followed have been found inadequate. At the present time a new calculation procedure has been devised and is being tested.

The gummed film samples are ashed to reduce their area and the total mixed fission product activity is measured with automatic beta geiger counters. The activity measurement and the weather data for each sample are entered on IBM cards for calculation and tabulation. The results of this work have been presented in several summary papers.

The gummed film has two major disadvantages. First, it does not allow direct measurement of Sr–90 fallout, and second, from two standpoints it is not a perfect collector of fallout. There are definite indications that there is some wash off of radioactive material during precipitation. When there is no precipitation the gummed film may collect more activity than would be found on a square foot of a perfect collector since some airborne dust, which would not settle ordinarily, may be scavenged by the gummed surface. Experiments in several locations, however, have shown an average collection efficiency of about 60 percent and this value is used to correct the measured values obtained in the laboratory. This will be discussed further in a later section.

The advantage of gummed film is its extreme simplicity as a collector and its adaptability to network operation with a central processing facility. In addition, the fact that the gummed film can be measured on a daily basis offers some advantage in detecting the time of arrival of radioactive material. This is particularly useful in the estimation of the low levels of total gamma dose associated with fallout, since the gamma dose at any given location is sensitive to the time of arrival of the fission products.

ATMOSPHERIC SAMPLING

The fission products, including Sr–90, are in particulate form in the atmosphere and the most common air sampling procedure is to draw a large volume of air through a filter. The dust on the filter can then be analyzed for Sr–90 and the atmospheric concentration can be obtained from a knowledge of the volume of the air sampled. This type of sampling has been carried out at the earth's surface, in the lower atmosphere by jetplanes, and in the stratosphere by balloons. The major difference between these samples lies in the amount of air that can be sampled and thus in the activity level which can be collected on a filter.

The analysis consists of the separation of Sr–90 from the inactive constituents of the dust on the filter and the separation from natural radioactive materials and other fission products. Activity levels at the surface or in the lower atmosphere are sufficient that simple beta geiger counters can be used. The

volume of air that can be sampled in the stratosphere, when converted by calculation to normal temperature and pressure, is small. Consequently a small amount of particulate material is collected and low background counters are an absolute necessity.

Estimation of the atmospheric reservoir of Sr-90 is needed to predict the level which will be finally deposited on the ground when all the material is deposited. Because of nonuniformity in the distribution of the radioactive debris in the atmosphere, both laterally and vertically, the sampling program must be very extensive to develop a reliable picture.

Air samples at the surface of the earth are the simplest to obtain but are the most heavily influenced by local meteorological conditions. A better estimate can be obtained from stratospheric samples which are the most costly in terms of both sampling and analysis.

At the present time monthly samples at four stratospheric altitudes are being taken at Minneapolis, Houston, Panama Canal Zone, and at one station in the Southern Hemisphere. This pilot program is designed only to determine the degree of uniformity and thus the extent of sampling required for complete evaluation of the atmosphere.

BIOLOGICAL DISTRIBUTION

The chemical similarity of strontium and calcium makes it desirable to use the Sr-90:Ca ratio in tracing Sr-90 from soils to man. A useful unit of Sr-90 contamination, first proposed by Libby, is the micromicrocurie of Sr-90 per gram of calcium ($\mu\mu$c/gmCa). For convenience this unit is frequently referred to as the "Sunshine unit."

The biological distribution of Sr-90 is being studied in its entirety and these investigations have produced a wide variety of data. For this discussion, which is concerned primarily with an evaluation of the potential human hazard from Sr-90, the presentation will be simplified greatly by reviewing only the principal sources of human calcium. The presently observed and predicted future levels of Sr-90 in these foods offer a convenient means of estimating the amounts of Sr-90 that the human skeletons of the future are expected to contain.

In the United States, and many western countries, the main calcium source is milk, with leafy vegetables supplying the majority of the balance. In some areas, such as southeastern United States, this ratio may be reversed. The sources of calcium in countries of the world is under study by the Food and Agricultural Organization and data on the non-milk-drinking populations is slowly becoming available.

The three principal types of samples taken for analysis are (1) vegetation, including both animal and human food, (2) milk, and (3) human bone.

VEGETATION

Sr-90 may occur in vegetation in either of two routes. Sr-90 from the soil may be taken up by the root system and incorporated into the plant. In addition, fallout may be directly deposited on the plant. Part of this latter material may be washed off by rain but a considerable portion apparently is retained. The relative distribution of Sr-90 in the plant and on the outer surfaces may be quite variable, depending on the amount of fallout and the type of soil.

The type of vegetation selected depends on whether it represents food for animals or for humans. For human foods the samples can be taken from the consumer market. The analysis of vegetation is simpler than for soils or for fallout collections since the amount of inactive material is small related to the amount of Sr-90, and because the plant discriminates against many of the radioactive isotopes. Analysis still requires, however, separation of Sr-90 from the inactive constituents of the plant ash and from other radioisotopes which are present.

MILK

The major source of body calcium in the United States is milk, hence its analysis has received considerable more attention than that of any other foodstuff although other foods may have a higher Sr-90-to-Ca ratio. As a matter of fact, cow's milk contains less strontium per gram of calcium than do the vegetables which comprise the balance of our calcium intake. This is because, as noted by other speakers, biological processes in general tend to discriminate against strontium. Milk contains less strontium per gram of calcium because the calcium has passed through two biological processes. The strontium has

passed through the vegetable and then through the cow. Thus the Sr-90 has been selectively eliminated by 2 stages in the case of milk and only 1 stage in the case of a vegetable. Nevertheless, milk seems to be the most practical index to human exposure to Sr-90 because it is the major source of calcium in the skeletons of American adults. Moreover, it is a material which is relatively easy to sample, and the samples represent the pooling of milk from large geographical areas.

Monthly analyses of milk samples are available from 6 major milksheds in the United States, for periods ranging from 1 to 4 years. The sampling procedures have been designed to follow the exposure level of the human population at the location rather than to follow an individual farm or even an individual processing plant.

The determination of Sr-90 in milk is a relatively simple procedure, since the cow discriminates sharply against other fission products. The main analytical problem is the separation of Sr-90 from the large amount of calcium present.

The chief advantage of following milk as a monitoring device is that it yields an immediate estimate of the amount of Sr-90 which man will ingest. This is more direct and more reliable than attempting to predict the movement of Sr-90 to man from analyses of fallout or of soil.

HUMAN BONE

Analyses of human bone afford a direct measure of the Sr-90 level at a given time. However, as previously mentioned, human bone is not yet in equilibrium with the Sr-90 of the environment. Thus, measurements made at the present time are only of value when viewed in relation to other materials, particularly human food and the present and predicted levels of Sr-90 on the earth's surface.

Human bone samples show the lowest levels of Sr-90 of any biological material being analyzed. This means that a large sample of bone is required for analysis and such specimens are not always readily obtainable. The extensive program at the Lamont Geological Observatory has yielded several hundred autopsy specimens from all over the world. Dr. Kulp, who is in charge of this work at Lamont, will report on it in detail.

The analysis of bone requires the separation of Sr-90 from large quantities of calcium but only negligible amounts of other radioisotopes are present. The current levels require not only low background counting equipment but the most extreme care in the prevention of contamination of samples with other radioactive materials.

DISCUSSION OF FINDINGS

We have seen that many kinds of samples are being collected routinely from a large number of locations, and that the Sr-90 in these samples serves as a tracer for the study of many physical, chemical, and biological processes. In this discussion, an effort will be made to simplify the analyses of the data and to deal only with those portions of the overall Sunshine program that are concerned with an estimate of the human hazard from Sr-90.

The fundamental questions which must be answered are these:

1. How much Sr-90 has been deposited on the earth's surface?
2. How much Sr-90 from detonations to date remains suspended in the upper atmosphere and how long will it take to precipitate?
3. How much Sr-90 will human skeletons contain when they are in equilibrium with the expected levels of Sr-90 in soil?

DEPOSITION OF SR-90 IN EARTH

As noted earlier there are two basic procedures by which the widespread deposition of Sr-90 can be documented. The first, and the most direct, is by the collection of soil samples and their analysis for Sr-90.

The soil analyses for the year 1956 are plotted in figures 1 and 2, which give the estimates of Sr-90 deposition in millicuries per square mile. Similar data have been obtained on the basis of samples collected from the gummed film network. These data are summarized in figures 3 and 4 which give the estimated cumulative Sr-90 distributions as of July 1, 1956. In recent months a third method has been adopted, utilizing pots which have been located at 21 stations throughout the world but too few data are available to justify presentation at this time.

It will be noted that, for most of the world, the estimates of fallout obtained by analyzing soil samples are lower than those obtained by gummed film collection. The differences can be seen in table 4 which summarizes the observation of both the soil sampling and the gummed film network by zones. The estimated worldwide fallout of Sr–90, based on the gummed film samples, is 1.5 megacuries compared to 0.88 megacurie as estimated by soil samples. It is also noted that the soil data indicate a greater degree of latitudinal variation than the gummed film data. In the latter case the mean deposition in the North Temperate Zone is slightly high in relation to the somewhat uniform deposition elsewhere in the world. The data derived from soil samples likewise indicate a higher deposition in the North Temperate Zone but there is less uniformity elsewhere in the world and a north-south gradient is quite evident.

The reason why the soil samples yield lower values in most locations is not clear. The integrated fallout as estimated by soil analyses is 59 percent of the value obtained from the gummed film observation. As noted earlier it is possible that the Sr–90 is incompletely removed from the soil by chemical analysis and this may account for some, but certainly not all, of the difference. The gummed film tends to yield results which are comparable to the Sr–90 measurements of pot samples but it is possible that both the pot and the gummed film tend to concentrate fallout, particularly on dry, windy days. It is also possible that some of the Sr–90 has been leached beyond the sampling depth. At the present time, there is insufficient knowledge to explain the difference between the two methods. One megacurie, being intermediate between the two estimates, is perhaps the most reasonable approximation of the total amount of Sr–90 deposition on the earth by delayed fallout in mid-1956.

The test firings in Nevada only partly account for the relatively elevated deposition in the North Temperate Zone. There is evidence that much of the differentiation between the North Temperate Zone and the rest of the world can be attributed to the preferential fallout in this region from 1954 series in the Pacific (Operation Castle).

STRATOSPHERIC RESERVOIR

An estimate of the future distribution of Sr–90 may be obtained from the above data plus knowledge of the amount of Sr–90 suspended in the upper atmosphere, and the rate at which it precipitates to earth's surface. The direct method of measuring the stratospheric reservoir is to obtain samples, using balloons or high-flying aircraft. This has been done intermittently since 1953 but the data are too few to permit one to estimate by this method, with any degree of confidence, the total inventory of stratospheric Sr–90.

Using a more indirect procedure, Libby suggested a value of 2.4 megacuries of Sr–90 in the fall of 1956. This estimate is based not on direct measurements in the stratophere, but rather on what might be described as material balance studies. The amount of Sr–90 produced in detonations, to date, can be estimated. Estimates of the amounts of Sr–90 that are deposited in the intense fallout in the vicinity of a detonation are available from extensive investigations conducted during the test programs in Nevada and in the Pacific. The total strontium produced in a detonation, less the Sr–90 which falls out in the immediate vicinity of the detonation, gives the total inventory of Sr–90 that is available for subsequent deposition at places remote from the site of detonation.

It is apparent that the rate of precipitation of the Sr–90 must be considered in any estimation of future hazard. If the time of descent was infinitely great, the Sr–90 would decay before it reached the earth's surface and it would not constitute a potential hazard. Actually, we have learned that the time of descent is relatively short in relation to the half-life of Sr–90. Libby has estimated that the average residence time is approximately 10 years. In mathematical terms this would be equivalent to a half-life of 7 years. It is possible this estimate is too long and the average life is as little as 6–7 years (half-life about 4½ years).

Our discussion of the foreseeable levels of Sr–90 will thus be simplified by the assumption that the material now contained in the stratospheric reservoir will be completely deposited on the earth's surface before any radioactive decay has occurred. Moreover, it will be assumed that the geographical distribution in the future will follow approximately the same distribution as has been true of the deposition of stratospheric debris in the past. This will tend to introduce in error on the side of safety since it would be expected that future fallout will be more uniform than in the past.

FUTURE ESTIMATE OF SR–90 IN MAN

As noted earlier, it will be assumed that essentially all of the 2.4 megacuries of Sr–90 stored in the stratosphere in mid-1956 will be deposited on earth's surface. This will have occurred by about 1970. It will be further assumed that stratospheric fallout in the future will be distributed in approximately the same pattern as the past.

This discussion of future levels of Sr–90 in man will be based on data for Northeastern United States. The fallout levels, as estimated by soil analyses in late 1956, averaged 20 millicuries per square mile in 1 large milkshed which has been sampled since 1954. Of this, about 6 mc/mi^2 is estimated to be tropospheric fallout from tests prior to mid-1956. The stratospheric fallout may thus be estimated as 14 millicuries per square mile. This was the level which existed when the worldwide deposition of Sr–90 was about 1 megacurie. It is estimated that when the 2.4 megacuries then in the stratosphere has deposited, the deposition on this milkshed will be 45 millicuries per square mile, about 2.3 times the level in mid-1956.

To define the potential risk from a given distribution of Sr–90 on the surface of the earth requires that the distribution be quantitatively related to the skeletal burden of Sr–90 of a human population in dietary equilibrium with the soil from which its nourishment is derived. This equilibrium is already established for a variety of trace elements normally present in the earth's crust. Some of these, like potassium and radium, are radioactive, and this is reflected by the presence of these substances in the human body. For example, the upper foot of soil in the United States contains, on the average, about 1,000 millicuries of radium per square mile. The average adult skeleton in this country contains about 10^{-4} microcuries of radium, which is derived from assimilation of this trace element from foods and water. Thus, the value of 10^{-4} microcuries of radium represents the amount deposited in the skeletons of the populations whose mineral metabolism is in equilibrium with the soil minerals.

The freshly deposited Sr–90 takes a relatively long time to complete the biological route to bone. At the present time the skeletons of all but very young children were formed prior to the introduction of Sr–90 to the soil. Moreover, bone being formed at the present time utilizes calcium which left the soil in months gone by. The fact that cattle may be fed on hay many months old and the holdup of human foods in the commercial distribution system are but two of many factors which would lead one to expect the human Sr–90 burden to lag in time behind the potential value which might ultimately be expected from a given soil concentration. The human skeleton cannot be expected to respond quickly to the gradual accretion of Sr–90 by soil. Equilibrium can be expected to be achieved over a period of years but not over a period of months.

In the United States, as in a number of other parts of the world where the population derives much of its calcium from dairy products, analyses of milk for Sr–90 provide a convenient method of estimating the levels of human absorption which may be expected in the future.

During periods of actual fallout the concentration of Sr–90 in milk originates from two sources: The Sr–90 level may have been metabolized from the soil by normal root uptake or it may have short circuited the soil by having been deposited directly on the leaf with which it is ingested by the cow. The presence of the latter fraction is dependent on current fallout. If all detonations ceased, the fraction due to direct deposition on the leaf surfaces would diminish with the reduction in the rate of fallout. With the cessation of fallout this fraction would be eliminated altogether. In contrast, the Sr–90 of the soil constitutes a relatively long lived reservoir for future uptake. Diminution will result only from radioactive decay or if the Sr–90 is leached beyond the root zone.

At the present time, it is not known to what extent the Sr–90 which occurs in milk may be due to direct deposition on leaves. This fraction presumably diminishes with time as the accumulation in soil increases and the rate of fallout remains approximately constant. For our purposes it will be assumed that all of the Sr–90 in milk is metabolized by way of the roots. This is a conservative assumption which tends to exaggerate the forecast of future levels.

Milk from the milkshed previously discussed has been analyzed routinely since early 1954 (fig. 5). The Sr–90 level of this milk averaged 3.5 $\mu\mu c/g$ Ca in the period just preceding the sampling of the soils in October 1956. If we neglect the effect of fresh fallout, and further assume that the Sr–90 in milk is proportional to the amount in soil, the future level may be estimated as $3.5 \times 2.3 = 8$ $\mu\mu c$ Sr–90/g

Ca. This prediction is in good agreement with the value of 8.3 μμc/g Ca which was similarly estimated by the data available from this milkshed in the summer of 1955.

Our final problem is to estimate the burden of Sr–90 which will be attained by a population whose principal dietary source of Ca contains 8 μμc/g. As has been noted by previous speakers, it is known that human metabolism involves some measure of discrimination against strontium and in favor of calcium. Based on the data now available a child being nourished on milk containing 8 μμc Sr–90/g Ca would be expected to develop a skeleton containing Sr–90 in somewhat lower concentrations than this value but probably higher than 4 μμc Sr–90/g Ca. Thus 4–6 μμc/g Ca may be said to be the highest foreseeable value that will be attained from a diet containing 8 μμc Sr–90/g Ca. A value of 5 may be taken as the basis for discussion.

Assuming 5 μμc Sr–90/g Ca to be the maximum value to be attained, one can calculate that this amount of Sr–90 will deliver a dose of 0.5 rads to the skeleton over a lifetime of 70 years. This compares with a normal skeletal irradiation of 7 to 30 rads resulting from potassium 40, carbon 14, cosmic rays, terrestrial gamma radiation, and radium. The maximum foreseeable value of 5 μμc Sr–90/g Ca is thus equivalent to 1.5 to 6 percent of the dose from natural sources of skeletal irradiation.

It should be noted that this estimate includes a number of assumptions which are deliberately conservative. No allowance has been made for the radioactive decay which will take place before the Sr–90 descends from the stratosphere to the earth. This may diminish the amount of available Sr–90 by about 25 percent. The assumption that all of the Sr–90 in milk originated by root uptake, is another conservative assumption. It has been estimated that 30 percent of the Sr–90 in milk in 1956 originated by direct foliar deposition. The combined effect of these and other safety factors is apt to be appreciable.

A number of individuals have raised the question as to whether a discussion of average fallout values is adequate to define the upper limit of hazard to people exposed to unusually heavy fallout. In this connection it is worth noting that as the data continue to accumulate from every corner of the globe the large deviations from average are in the safe direction. Whereas fallout values greater than twice the mean are rarely reported in any given region, it is not uncommon to observe values which are of the order of 10 percent of the average.

This forecast has been made on the basis of data from Northeastern United States. How applicable are these data to other parts of the United States or of the world? Table 5 summarizes the concentration of Sr–90 in milk as observed in 6 domestic and 2 foreign sources of supply. These data, and the fallout measurements of figures 1 to 4 indicate that the Sr–90 content of dietary calcium might be higher in certain areas but a factor of 3 applied to the estimates for Northeastern United States should be adequate to bracket the highest foreseeable value, from tests to date, in any region of the United States.

TABLE 1.—*Scope of Sr–90 sampling program*

Type sample	Frequency	Number of stations	Total number per year
Soil:			
United States	Annually	17	17
Gummed film:			
United States	Daily	39	14,235
Foreign	do	66	24,090
Pots:			
United States	Monthly	7	84
Foreign	do	14	168
Milk:			
United States	do	5	60
Foreign	do	2	24
Tap water (New York)	do	1	12
Canned fish	do	5	60
Pasture program (United States):			
Soil	Annually	7	168
Vegetation	do	7	168
Animal bone	do	7	168
Sea water (Pacific)	Monthly	50	600
Stratosphere	do	5	240
Human bone		20	[1] 1,200

[1] About.

TABLE 2.—*Cumulative fallout data from gummed film through June 1956—world*

Station	Sr-90 mc/mi²
1. Anchorage, Alaska	8.7
2. Edmonton, Alberta	12.2
3. Regina, Saskatchewan	9.5
4. Winnepeg, Manitoba	11.4
5. Churchill, Manitoba	3.9
6. Moosoonee, Ontario	9.0
7. North Bay, Ontario	10.8
8. Ottawa, Ontario	8.7
9. Montreal, Quebec	11.0
10. Seven Islands, Quebec	7.8
11. Moncton, New Brunswick	9.8
12. Goose Bay, Labrador	8.6
13. Stephenville, Newfoundland	13.5
14. Thule, Greenland	5.8
15. Keflavik, Iceland	9.3
16. San Juan, Puerto Rico	12.1
17. Bermuda	13.9
18. Mexico City, Mexico	11.6
19. San Jose, Costa Rica	4.8
20. Panama Canal Zone	6.4
21. Bogota, Colombia	6.3
22. Quito, Ecuador	3.6
23. Lima, Peru	3.6
24. La Paz, Bolivia	6.2
25. Belem, Brazil	5.8
26. Sao Paulo, Brazil	5.0
27. Buenos Aires, Argentina	5.6
28. Prestwick, Scotland	11.2
29. Oslo, Norway	7.9
30. Rhein Main, Germany	9.4
31. Sidi Slimane, Morocco	14.5
32. Tripoli, Libya	15.9
33. Dakar, French West Africa	6.2
34. Lagos, Nigeria	4.1
35. Leopoldville, Belgian Congo	5.5
36. Addis Ababa, Ethiopia	7.1
37. Pretoria, Union of South Africa	4.2
38. Durban, Union of South Africa	2.4
39. Colombia, Ceylon	6.5
40. Singapore, Malaya	6.1
41. Misawa, Japan	13.9
42. Tokyo, Japan	12.7
43. Hiroshima, Japan	13.1
44. Nagasaki, Japan	14.8
45. Kadena, Okinawa	—
46. T'ai-pei, Taiwan	18.3
47. Manila, Philippine Islands	11.1
48. Iwo Jima	30.5
49. Yap, Caroline Islands	14.6
50. Guam, Caroline Islands	15.8
51. Truk, Caroline Islands	14.0
52. Ponape, Caroline Islands	18.2
53. Wake Island	10.1
54. Noumea, New Caledonia	6.8
55. Sydney, Australia	5.2
56. Melbourne, Australia	6.0
57. Wellington, New Zealand	3.6
58. Honolulu, T. H	13.0
59. Johnston Island	16.1
60. Canton Island	6.0
61. Dhahran, Saudi Arabia	7.3
62. Beirut, Lebanon	18.5

Table 2.—*Cumulative fallout data from gummed film through June 1956—world*—Continued

Station	*Sr–90 mc/mi²*
63. Bangkok, Thailand	8.3
64. Nairobi, Kenya	4.2
65. Monrovia, Liberia	7.1
66. Lagens, Azores	15.6
67. Nome, Alaska	5.7
68. Fairbanks, Alaska	11.8
69. Juneau, Alaska	8.4
70. French Frigate Shoals	13.6
71. Midway	12.1
72. Koror	11.1
73. Lihue	10.0
74. Hilo	19.7

Table 3.—*Cumulative fallout data from gummed film through June 1956—United States*

101. Detroit, Mich	16.0
102. Louisville, Ky	14.1
103. Knoxville, Tenn	10.5
105. Memphis, Tenn	15.7
108. Atlanta, Ga	11.0
115. Philadelphia, Pa	12.7
116. Pittsburgh, Pa	18.0
117. New York (LaGuardia)	16.7
118. Binghamton, N. Y	8.9
122. Rochester, N. Y	12.9
127. New Haven, Conn	12.0
132. Jacksonville, Fla	7.9
133. Miami, Fla	12.1
134. Washington, D. C	12.0
137. Cleveland, Ohio	15.9
138. Cape Hatteras, N. C	9.4
139. Concord, N. H	8.0
141. Boston, Mass	13.8
204. Corpus Christi, Tex	6.3
206. Dallas, Tex	12.9
209. Wichita, Kans	14.7
211. Scottsbluff, Nebr	12.7
212. Rapid City, S. Dak	11.6
216. Minneapolis, Minn	16.4
219. Des Moines, Iowa	15.5
221. St. Louis, Mo	18.9
222. Chicago, Ill	14.5
225. New Orleans, La	13.6
804. Boise, Idaho	18.5
809. Billings, Mont	14.9
810. Salt Lake City, Utah	34.6
814. Tucson, Ariz	15.2
821. Grand Junction, Colo	27.7
823. Albuquerque, N. Mex	34.9
826. Las Vegas, Nev	17.8
401. Seattle, Wash	13.4
404. Medford, Oreg	8.9
407. San Francisco, Calif	8.9
410. Los Angeles, Calif	6.8
Mean	14.2

TABLE 4.—*Regional Sr-90 fallout cumulative to June 1956*

Region	Soil			Gummed film		
	Number of locations	Average mc/mi^2	Total megacuries	Number of locations	Average mc/mi^2	Total megacuries
Arctic	3	2.2	0.02	1	5.8	0.04
North Temperate	33	9.4	.49	70	12.8	.67
North Tropic	9	3.9	.15	20	9.1	.35
South Tropic	7	2.0	.08	8	5.2	.20
South Temperate	16	2.6	.14	6	4.6	.24
Total	----	----	.88	----	----	1.50
United States	17	20.5	.06	39	14.2	.04

TABLE 5.—*Dried milk analyses*

	Sr-90 micromicrocuries per gram calcium							
	1	2	3	4	5	6	7	8
1954—April	0.47	----	----	----	----	----	----	----
May	1.2	----	----	----	----	----	----	----
June	1.3	----	----	----	----	----	----	----
July	1.5	----	----	----	----	----	----	----
August	1.2	----	----	----	----	----	----	----
September	1.5	----	----	----	----	----	----	----
October	1.4	----	----	----	----	----	----	----
November	1.1	----	----	----	----	----	----	----
December	0.64	----	----	----	----	----	----	----
1955—January	2.5	----	----	----	----	----	3.0	----
February	0.77	----	----	----	----	----	1.0	----
March	0.75	----	----	----	----	----	2.0	1.8
April	0.31	----	----	----	----	----	1.8	2.9
May	1.9	2.6	4.1	1.0	7.3	1.7	1.9	----
June	2.5	4.7	4.6	4.6	9.2	2.6	0.8	5.5
July	1.9	4.4	3.9	0.8	6.3	----	----	2.6
August	2.0	4.1	----	1.2	5.8	----	0.8	----
September	1.5	3.2	----	3.3	4.7	----	2.0	----
October	2.8	----	----	4.4	6.9	----	7.5	----
November	2.5	----	----	3.7	7.4	----	2.5	----
December	3.3	----	----	3.0	10.	----	3.5	----
1956—January	2.3	----	----	3.0	3.5	----	2.7	4.0
February	2.0	----	----	3.5	8.1	----	----	----
March	2.0	6.3	----	3.4	11.	----	3.5	----
April	2.9	6.7	----	3.4	9.6	5.2	3.0	4.6
May	2.8	4.9	----	2.8	17.	6.4	----	4.5
June	3.0	4.4	----	3.4	8.7	5.0	----	5.0
July	2.7	6.1	----	4.2	6.6	----	2.3	----
August	3.1	3.8	----	4.7	8.6	----	----	----
September	4.9	4.8	----	4.3	10.7	----	2.7	----
October	5.4	----	----	4.72	8.9	----	----	----
November	5.6	----	----	----	3.6	----	----	----
December	----	----	----	----	----	----	----	----

Locations:

1. Perry, N. Y.
2. State College, Miss.
3. St. Louis, Mo.
4. Columbus, Wis.
5. Mandan, N. Dak.
6. Portland, Oreg.
7. Japan
8. United Kingdom

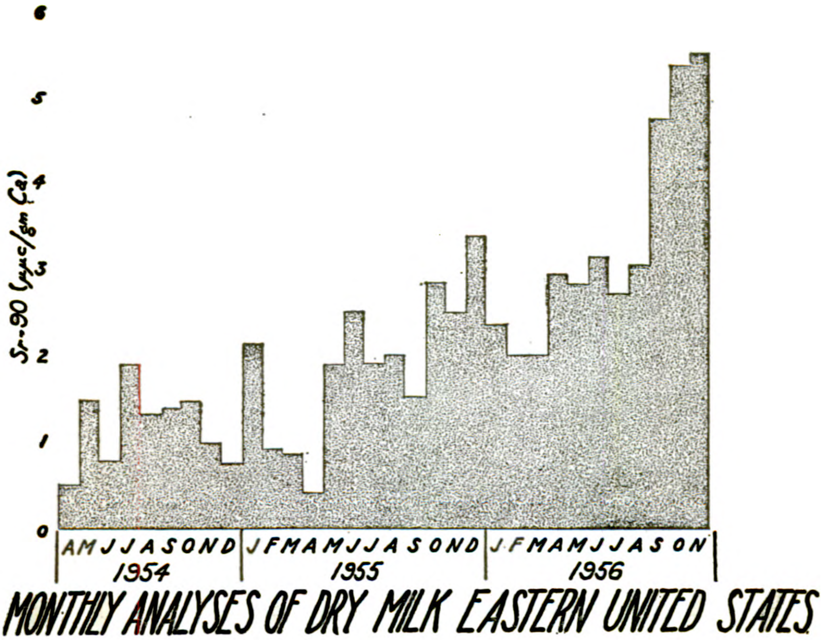
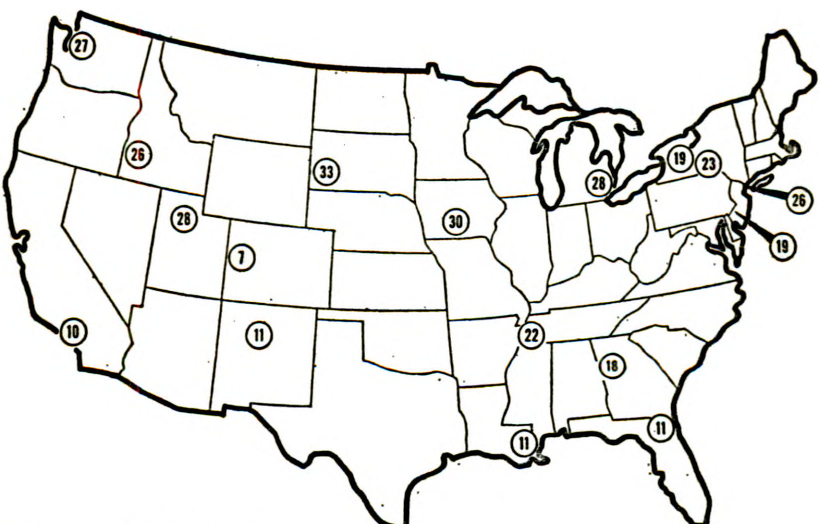


FIGURE 1

Sr[90] IN U. S. SOIL (HASL - OCT. 8, 1956) (HCl EXTRACTION METHOD)



Numbers are in mc/mi² at individual site.

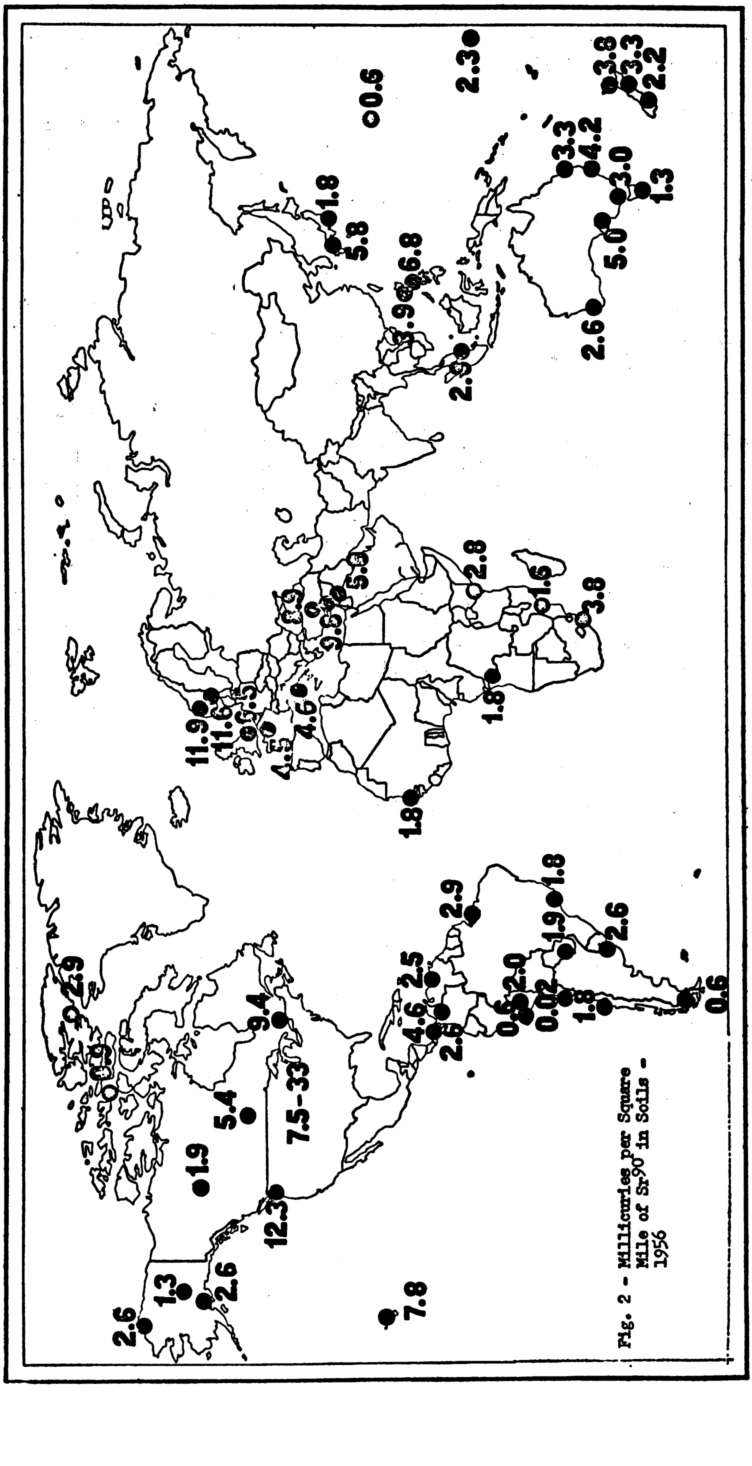


Fig. 2 - Millicuries per Square Mile of Sr^{90} in Soils - 1956

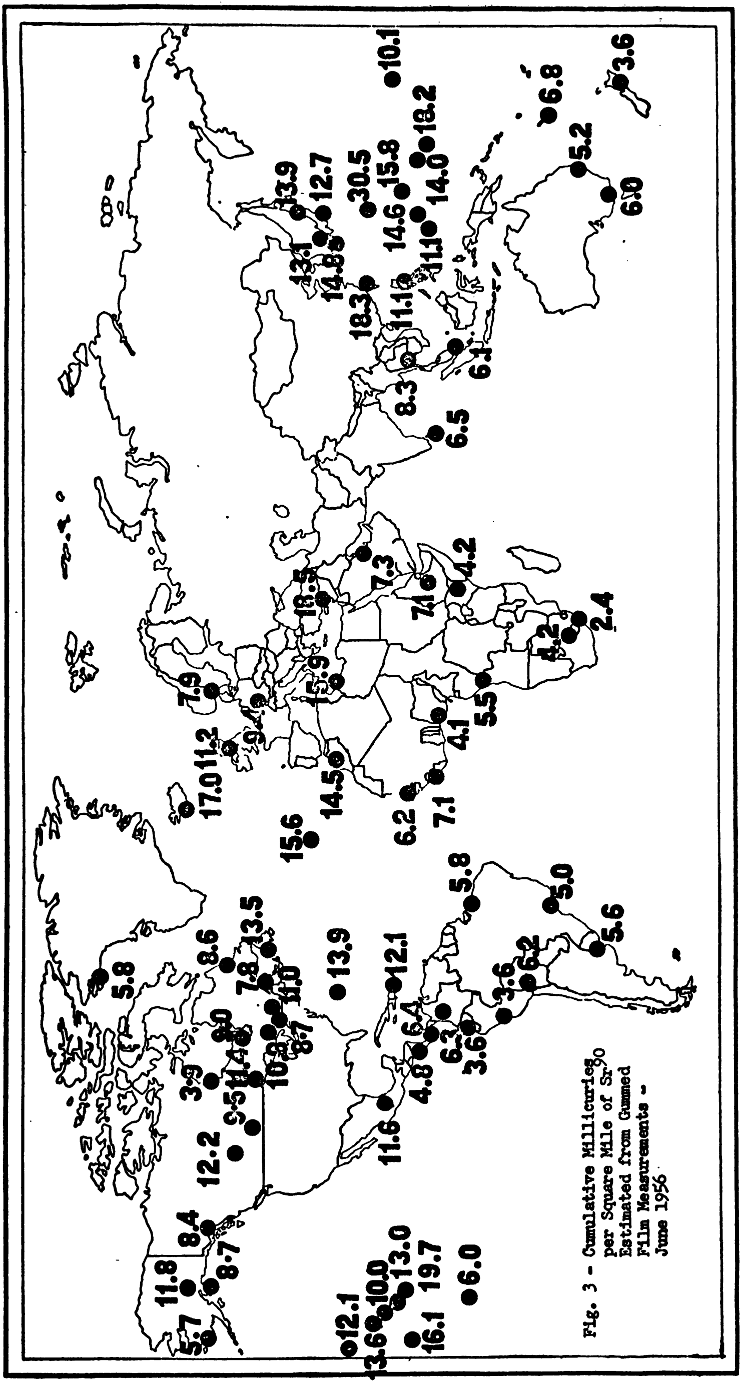


Fig. 3 - Cumulative Millicuries per Square Mile of Sr^{90} Estimated from Gummed Film Measurements - June 1956

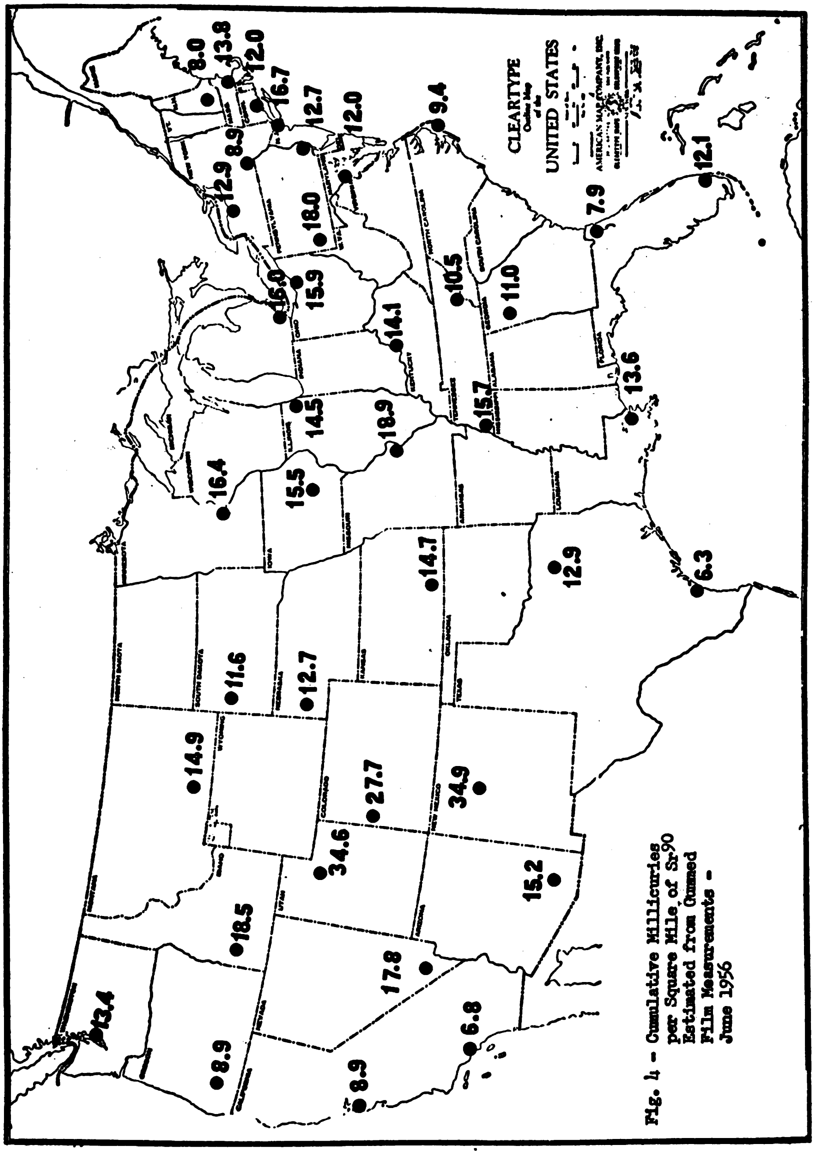


Fig. 4 - Cumulative Millicuries per Square Mile of Sr90 Estimated from Gummed Film Measurements - June 1956

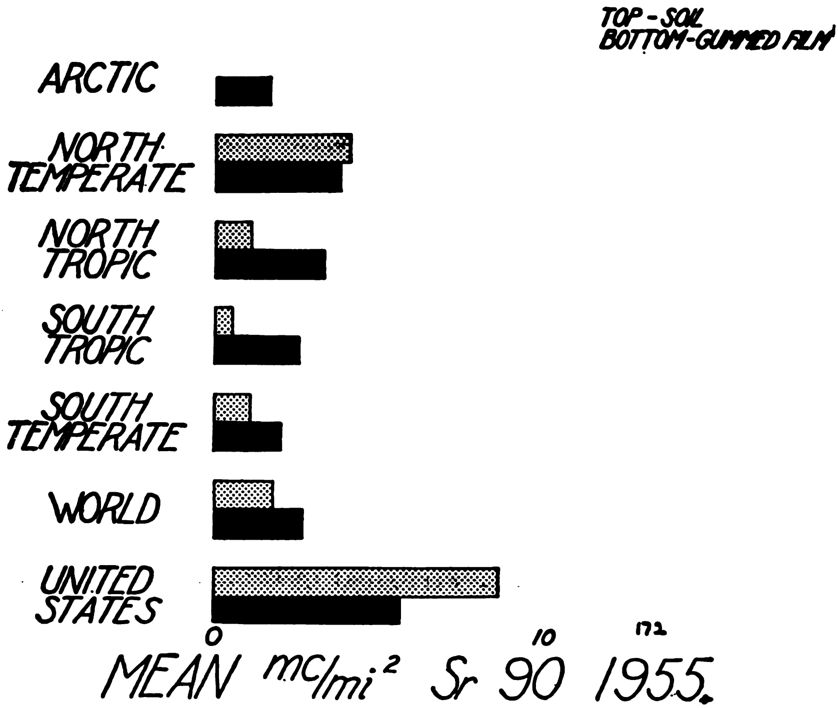


FIGURE 5.

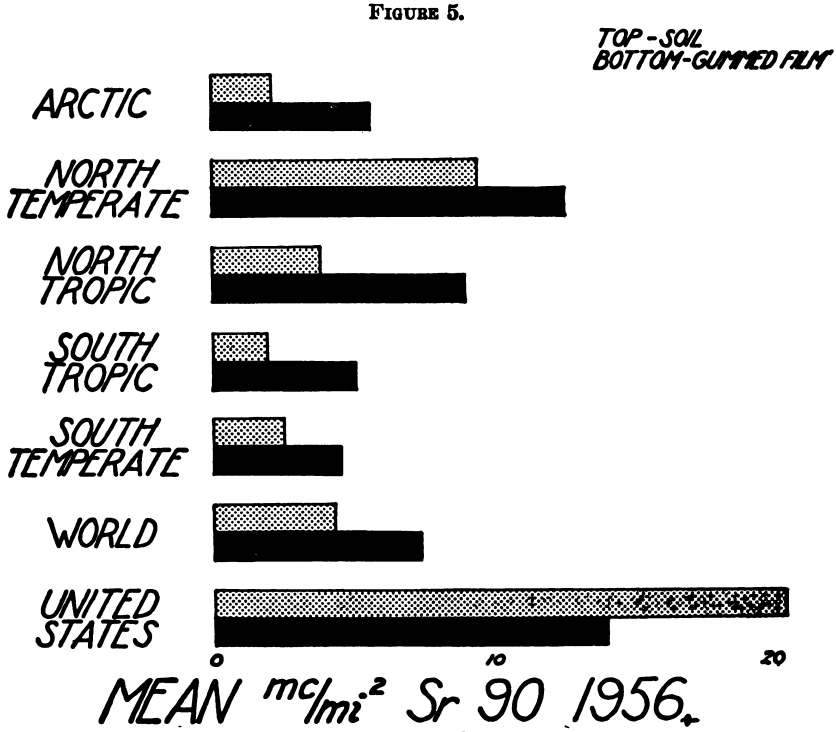


FIGURE 6.

II. Cumulative Gamma Dose Through June 1956

I would like to submit the attached table, "Cumulative gamma dose as estimated by gummed film measurements through June 1956." These data supplement the last report[1] from the Health and Safety Laboratory, which covered the period through September 1955.

It should be noted that these exposures are the theoretical exposures which would be delivered on the assumption that the measured fallout persisted indefinitely on the surface of an infinitely smooth plane. In actual practice the exposures would be very much less than the reported values due to various shielding factors as well as weathering. It is of course not possible to be quantitative about these factors in a general statement but most investigators would agree that the overall effect of these factors would be to limit the whole body exposure to less than 10 percent of the reported values.

Cumulative gamma dose as estimated by gummed film measurements through June 1956

Station	*γ dose millirads*
Albuquerque, N. Mex	126
Atlanta, Ga	24
Billings, Mont	35
Binghamton, N. Y	15
Boise, Idaho	26
Boston, Mass	56
Cape Hatteras, N. C	20
Chicago, Ill	34
Cleveland, Ohio	46
Concord, N. H	18
Corpus Christi, Tex	15
Dallas, Tex	37
Des Moines, Iowa	38
Detroit, Mich	34
Grand Junction, Colo	134
Jacksonville, Fla	19
Knoxville, Tenn	31
Las Vegas, Nev	56
Los Angeles, Calif	12
Louisville, Ky	34
Medford, Oreg	13
Memphis, Tenn	58
Miami, Fla	28
Minneapolis, Minn	31
New Haven, Conn	27
New Orleans, La	36
New York (La Guardia)	31
Philadelphia, Pa	25
Pittsburgh, Pa	29
Rapid City, S. Dak	31
Rochester, N. Y	24
St. Louis, Mo	42
Salt Lake City, Utah	127
San Francisco, Calif	13
Scottsbluff, Nebr	26
Seattle, Wash	22
Tucson, Ariz	29
Washington, D. C	22
Wichita, Kans	40
Alaska:	
Anchorage	13
Fairbanks	18
Juneau	16
Nome	12
Argentina: Buenos Aires	13
Australia:	
Melbourne	11
Sydney	15
Azores: Lagens	25
Belgian Congo: Leopoldville	10
Bermuda	29
Bolivia: La Paz	17
Brazil:	
Belem	15
Sao Paulo	12
Canada:	
Churchill, Manitoba	6
Edmonton, Alberta	18
Goose Bay, Labrador	18
Moncton, New Brunswick	17
Montreal, Quebec	21
Moosonee, Ontario	19
North Bay, Ontario	20
Ottawa, Ontario	18
Regina, Saskatchewan	17
Seven Islands, Quebec	17
Stephenville, Newfoundland	29
Winnipeg, Manitoba	23
Ceylon: Columbia	25
Colombia: Bogotá	15
Costa Rica: San Jose	13
Ecuador: Quito	11
Ethiopia: Addis Ababa	14
French West Africa: Dakar	11
Germany: Rhein Main	15
Greenland: Thule	8
Hawaii:	
French Frigate Shoals	27
Lihue	21
Hilo	36
Honolulu	25
Iceland: Keflavik	24
Japan:	
Hiroshima	22
Misawa	22
Nagasaki	27
Tokyo	23
Kenya: Nairobi	9
Lebanon: Beirut	26

[1] Radioactive Fallout Through September 1955, by Merril Eisenbud and John H. Harley, Science 124, 251-255 (1956).

Cumulative gamma dose as estimated by gummed film measurements through June 1956—Continued

Station	*γ dose millirads*	*Station*	*γ dose millirads*
Liberia: Monrovia	13		
Malaya: Singapore	20	Manila, Philippine Islands	36
Mexico: Mexico City	28	Midway	22
Morocco: Sidi Slimane	20	Noumea, New Caledonia	16
New Zealand: Wellington	8	T'ai-pei, Taiwan	39
Nigeria: Lagos	7	Wake Island	21
Norway: Oslo	12	Panama Canal Zone	17
Pacific islands:		Peru: Lima	7
Yap, Caroline Islands	46	Puerto Rico: San Juan	23
Guam, Caroline Islands	149	Saudi Arabia: Dhahran	13
Truk, Caroline Islands	73	Scotland: Prestwick	16
Ponape, Caroline Islands	104	Thailand: Bangkok	35
Canton Island	21	Tripoli: Libya	25
Iwo Jima	159	Union of South Africa:	
Johnston Island	40	Durban	6
Kadena, Okinawa	—	Pretoria	7
Koror, Palau Island	37		

Mr. EISENBUD. In this discussion I will describe the sampling program of the Atomic Energy Commission, and summarize for you the many useful conclusions that have been derived from analyses of the data.

The fundamental purposes of these studies are threefold:

1. How much strontium 90 has been deposited on the earth's surface?
2. How much strontium 90 from detonations to date remains suspended in the upper atmosphere, and how long will it take to precipitate?

How much strontium 90 will human skeletons contain when they are in equilibrium with the expected levels of strontium 90 in soil?

All of these subjects were discussed for you by specialists in meteorology, oceanography, and soil chemistry. Now that the theoretical background has been presented, I will present the data that have been obtained as a result of several years of study.

Samples are collected throughout the world for strontium 90 analyses. Most of these are sent to the United States for analyses, but others, in increasing numbers, are analyzed in the laboratories of other lands by many scientists whose data, like our own, are routinely submitted to the United Nations Committee on the Effects of Radiation.

Monitoring for radiostrontium can be divided conveniently into studies of its geophysical and biological distribution. Under the former classification are collected those samples which give us an understanding of the behavior of radiostrontium from its formation in the fireball to its deposition on the surface of the earth and incorporation into soils. The biological studies trace the movements of strontium 90 from the soils and waters through the flora and fauna of the oceans, pastures, and farms, to the skeleton of man.

Types of samples: For studies of the geophysical distribution of strontium 90, various kinds of samples are sought. A soil sample, expertly selected, can represent the accumulated fallout at a given location. An ideal sampling site is considered to be an open, level area, undisturbed by cultivation and covered by grass or simple vegetation to immobilize the surface. The most common division of sampling is to collect the top 2 inches and the 2- to 6-inch layer separately. The known area and the measured weight of sample allow the strontium 90

measurements to be converted to terms of millicuries per square mile.

Stainless-steel pots, with a face area of about 1 square foot, are finding increasing application in fallout sampling both here and abroad. The collected precipitation and dust are analyzed for strontium 90 at intervals, usually on a monthly basis.

The gummed-film collector was developed for a simple collecting technique suitable for network operations, where large numbers of collecting stations employ untrained personnel. This collector has an adhesive surface of 1 square foot which is exposed for 24 hours. A network of up to 200 stations collecting daily samples has been operated by the Health and Safety Laboratory of the Commission since 1951. Samples are mailed to the Laboratory where the analyses are performed.

Radioactive particulates suspended in the atmosphere can be studied by drawing a large volume of air through a filter. This type of sampling has been carried out at the earth's surface, in the lower atmosphere by aircraft, and in the stratosphere by balloons.

For studies of the biological distribution, the chemical similarity of strontium and calcium makes it desirable to use the strontium 90 to calcium ratio in tracing strontium 90 from soils to man. A useful unit, described to you yesterday, is the micromicrocurie of strontium 90 per gram of calcium.

The three principal types of samples taken for analysis are (1) vegetation, including both animal and human food, (2) milk, and (3) human bone.

The major source of body calcium in the United States is milk; hence its analysis has received considerably more attention than that of any other foodstuff, although other foods may have a higher strontium 90 to calcium ratio. Actually, cow's milk contains less strontium per gram of calcium than do the vegetables which comprise the balance of our calcium intake. This is because biological processes in general tend to discriminate against strontium. Thus the strontium 90 has been selectively eliminated by 2 stages—the vegetable and the cow—in the case of milk, and only 1 stage in the case of vegetable. Milk seems to be the most practical index of human exposure to strontium 90 because it is the source of most of the calcium in the skeletons of American adults, and it is a material which is relatively easy to sample.

Analyses of human bone afford a direct measure of the strontium 90 level at a given time. These samples show the lowest levels of strontium 90 of any biological material being analyzed. Since human bone is not yet in equilibrium with the strontium 90 of the environment, however, measurements made at the present time are only of value when viewed in relation to other materials, particularly human food and the present and predicted levels of strontium 90 on the earth's surface.

The subject of strontium 90 in human bones will be discussed in very great detail by Dr. Kulp, under whose capable direction these studies have been undertaken.

Estimates of fallout obtained by analyzing soil samples and gummed film are given in these charts. (See pp. 570, 571.)

I will explain for you, so you can compare the two, the data that have been collected throughout the world by two basic procedures that we use—gummed film and soil analysis.

You will note that in general the gummed film yields higher estimates of fallout than are actually observed by soil analysis. The reason for this is not clear, and remains to be investigated. But if we integrate the total amount of fallout on the surface of the earth, as prepared by the estimates from gummed film data, we come out with a total of about 1½ megacuries. That is, 1.5 million curies of strontium 90 are estimated from gummed films to be deposited on the surface of the earth; whereas in the case of soils we estimate about 0.8 million curies, 800,000 curies of strontium 90.

Thus, for some reason or other, which remains to be explained, the soil analyses yield about 59 or 60 percent of the values obtained from the gummed film, and for the purposes of discussion, I will use an intermediate value of 1 megacurie, which is in within 25 or 50 percent of either extreme.

Now, these charts are worth a considerable amount of study. One sees here the evidence of the latitudinal variation in results that was discussed by Dr. Machta yesterday and which has been noted in the publication of Dr. Libby. I want to caution you, though, that the difference is not a very large one, as we will see when I present the summaries of these data.

Particularly in the case of the soil samples one notices a path of higher fallout through the northern latitudes, with a corresponding diminution as one proceeds in a southerly direction. The same effect is also apparent in the case of gummed film, although the differences do not seem to be as great.

Mr. Chairman, you inquired yesterday as to whether we had any information about Japan, and I can submit data on the basis of both soil analyses and gummed films. We operate four gummed-film stations in Japan. These data indicate that the fallout in Japan is not higher than it is in other places in the northern latitudes, and, in fact, is considerably lower than in most parts of the United States.

Represent Holifield. In order to make the answer very clear—I do not believe your answer was as clear as I would like to have it on the record—have we used the same methods in testing the soil of Japan, and the same scientific methods in testing it there as we have in various parts of the world, including the United States? Have you used the gummed paper and chemical analysis of the soil?

Mr. Eisenbud. The analysis by means of gummed paper is the same as it is elsewhere in the world. We use one method for all samples.

Representative Holifield. You have not answered my question, and this is an important question because we do not want to convey to the people anything but the exact truth on this matter. We want to know if the same type of sampling was used on Japanese soil that is used on the American soil.

Mr. Eisenbud. Yes, sir.

Representative Holifield. So that the comparison is a valid comparison?

Mr. Eisenbud. Yes, sir.

Representative Holifield. Thank you.

Mr. Eisenbud. I want to add, along those lines, sir, that in recent months we have had confirmatory evidence from other governments. The Swedish Government, the British, the Canadians, the Japanese, perhaps 1 or 2 others, have submitted their own estimates derived

by methods which are similar but from analytical procedures in their own laboratories which indicate that, in general, we are in agreement.

Now, this chart (figs. 5 and 6) shows the latitudinal deposition. It is identified by the upper horizontal bars, which are the result of the soil samples, and the lower horizontal bars, which are the result of gummed-film data. (See p. 573.)

One notes, first of all, that the fallout in the United States is in fact very much higher than elsewhere in the world; secondly, that the North Temperate Zone does, in fact, show the stratification that Dr. Machta was talking about yesterday. But I would like to emphasize for the record, sir, that these differences are not large differences. They are differences that are going to require the resolution of fine shades of scientific opinions, but in terms of their practical significance I do not believe there is any difference between the interpretation that we have placed on these data in the past, and the way they are interpreted by some at the present time.

We have always known that the North Temperate Zone was higher than elsewhere in the world. In the past we attributed it to excessive tropospheric deposition. It appears now that perhaps the difference may be due to collective stratospheric deposition, but the fact is we knew it was there. So the evidence has come from these measurements, and not from theory.

Senator BRICKER. Doctor, how do you account for the fact there is a reversal of the testing in the United States, the soil testing showing a greater deposit than the gummed testing, and the reverse is true in the other testing?

Mr. EISENBUD. I do not know. The reason I was hesitating when Chairman Holifield asked his question before is there was a difference in the chemical procedures for these samples as compared with some of these [indicating], but this difference is at most 30 percent, and would not explain the complete reversal you see here.

Senator BRICKER. It is not as marked as that graph shows?

Mr. EISENBUD. I suppose this is the point in my presentation where we have to emphasize, as others have, that we have much to learn, and there are a great many things about these data that remain to be explained.

This, then, will summarize. How much is on the ground as of mid-1956?

I am going to use, for purposes of discussion, the round figure of 1 million megacuries of strontium 90, which is the intermediate of 0.88 estimated from soil analysis and 1.5 estimated from gummed film.

Senator BRICKER. If there were no more radiation activity put into the atmosphere at the present time, would the decay on the face of the earth offset the fallout that will continue for the years ahead?

Mr. EISENBUD. No, sir. I am going to come to that in some detail. That is a very important point, Senator. It is estimated in mid-1956 approximately 2.4 megacuries of strontium 90 were suspended in the stratosphere. The rate of precipitation of strontium 90 must also be considered in any estimation of future hazard. If the time of descent was infinitely great, the strontium 90 would decay before it reached the earth's surface, and would not constitute a potential hazard.

Actually, we have learned that the time of descent is relatively short in relation to the half life of strontium 90. It is estimated that the average residence time is approximatly 10 years—equivalent to a half

life of 7 years. It is possible this estimate is too long, and the average life may be as little as 6 to 7 years.

In any case, the bulk of the radioactive dust now stored in the stratosphere will have deposited by 1970. Thus essentially all of the 2.4 megacuries of strontium 90 stored in the atmosphere as of mid-1956 will be deposited on the earth's surface by about 1970.

This discussion will assume that the stratospheric fallout will be distributed in approximately the same pattern as in the past.

You see we do not have to hypothesize about the distribution in the stratosphere, because the pattern has been developed, and we will just assume that this, as Dr. Machta pointed out, will ultimately disclose what the stratospheric pattern is.

Now what, then, does the 1 megacurie on the ground plus the 2.4 that will be on the ground mean to man?

Our discussion of future levels of strontium 90 in man will be based on data for a region in Northeastern United States where the deposition of strontium 90 averaged 20 millicuries per square mile. Of this, about 6 millicuries per square mile is estimated to be tropospheric fallout from tests prior to mid-1956.

Another thing I want to point out, in fairness to all of you—is that Dr. Machta might disagree with this figure. I think instead of being 6, he might estimate as high as 8 or perhaps 10. But this would not affect the conclusion. My own estimate is 6 millicuries per square mile. In other words, 20 minus 6 equals 14 millicuries per square mile. This was the level which it is estimated that then existed.

It is estimated that when the 2.4 megacuries then in the stratosphere has deposited—I am talking about mid-1956—the deposition on this milkshed will be 45 millicuries per square mile, about 2.3 times the level in mid-1956.

To define the potential risk from a given distribution of strontium 90 on the surface of the earth requires that the distribution be quantitatively related to the skeletal burden of strontium 90 of a human population in dietary equilibrium with the soil from which its nourishment is derived. This equilibrium is already established for a variety of raidoactive elements normally present in the earth's crust.

For example, the average upper foot of soil in the United States contains about 1,000 millicuries of radium per square mile. The average adult skeleton in this country contains 10^{-4} microcuries of radium, resulting from assimilation of this trace element from foods and water. Thus the value of 10^{-4} microcuries of radium represents the amount deposited in the skeletons of the populations whose mineral metabolism is in equilibrium with the soil minerals.

Freshly deposited strontium 90 takes a relatively long time to complete the biological route to bone, since bone being formed at the present time utilizes calcium which left the soil in months gone by. In addition, storage of cattle fodder and holdup of human foods in commercial distribution lead one to expect the human strontium 90 burden to lag in time behind a given soil concentration. Equilibrium can be expected to be achieved over a period of years, but not over a period of months, or even a period of a few years.

During periods of actual fallout the concentration of strontium 90 in milk originates from the soil by normal root uptake as was discussed yesterday, or it may short-circuit the soil through deposition directly on the leaf and ingestion by the cow.

At present, the extent to which strontium 90 occurs in milk as a result of direct deposition on leaves is not known. This fraction presumably diminishes with time as the accumulation in soil increases and the rate of fallout remains approximately constant. In contrast, the strontium 90 of the soil constitutes a long-lived reservoir for the future uptake. Diminution will result only from radioactive decay or if the strontium 90 is leached beyond the root zone. For the purpose of this discussion it will be assumed that all of the strontium 90 in milk is metabolized by way of the roots. This is a conservative assumption which tends to exaggerate the forecast of future levels. Milk from the milkshed previously discussed has been sampled routinely since early 1954.

This pictogram gives the variation and the gradual increase of strontium 90 in micro-microcuries per gram of calcium in the milk of this large milkshed in the eastern United States.

There are a considerable number of details about which many interesting scientific discussions can take place, but the thing that is important is that the milk up until the end of December 1956 was gradually increasing, with some variations, which are presumably soil in origin, and which are becoming less important as time goes on.

Representative HOLIFIELD. Will you give us the percentage increase in that period? Was it 2 years or 3 years?

Mr. EISENBUD. It has gone from half a mirco-microcurie per gram of calcium to about 5. That is about a tenfold increase.

Senator ANDERSON. Do you expect that trend to continue?

Mr. EISENBUD. I am going to deal with this. It is a very important question. May I continue?

Representative HOLIFIELD. Yes.

Senator ANDERSON. I mean, if you do not expect it to continue, you give us the scientific basis why.

Mr. EISENBUD. I expect it to continue in a certain value. The next question I must answer, or attempt to answer, is what must this value be.

Representative HOLIFIELD. Do you believe you have an actual computation of the amount of fissionable material that has been released into the atmosphere from the three nations capable of testing these weapons?

Mr. EISENBUD. Yes, sir. It depends on how you define accuracy. There are areas in normal life, as well as in scientific work that where sometimes a factor of 10 is considered to be good enough. So you say a hundred thousand or a million, and you do not make any differentiation between them.

Obviously in this case, we want to do better than that. My own opinion is that we can estimate this within a factor of two, which I think, considering the levels that we are at at the present time, is sufficiently accurate to enable one to make a forecast of human hazards.

Representative HOLIFIELD. This would indicate, then, that you have confidence in your testing mechanism, whatever they may be, from the standpoint of the global testing?

Mr. EISENBUD. I have considerable degree of confidence, and personally have been very pleased by the amount of confirmatory evidence that has come in from investigators in this country and in other parts of the world that have tended to support the conclusions that we drew in a very preliminary way a year or 2 years ago.

Representative Holifield. Is it possible for you to extrapolate from air samples tested, let us say, over the United States, the power of the weapons which we have tested in the Pacific within any reasonable degree of accuracy?

Mr. Eisenbud. Well, sir, I am concerned in the analysis of the samples with the implication of health, and I am not attempting to diagnose weapons or estimate yields on the basis of the chemical work that is done. I think this is beyond my competence.

Representative Holifield. So you measure the amount of radioactivity in the air without any reference at all to the number or yield of weapons detonations which have taken place?

Mr. Eisenbud. That is right, sir.

Representative Holifield. You start with that fact?

Mr. Eisenbud. Yes.

Representative Holifield. I see.

Senator Anderson. Did you say something about these data being based on studies 2 years ago, or rather some conclusions being based on studies of a couple of years ago? You mentioned that just a moment ago.

Mr. Eisenbud. Yes, sir. I think in general, the model that has been developed has grown firmer with time, and has developed a considerably finer detail and greater internal consistency, if you will.

I think, unfortunately, many of the investigators, being very close to their particular specialty, have tended to emphasize differences in this fine detail, which do not really affect the conclusions one draws from the data.

Senator Anderson. I do not have your final statement. Have you referred to the National Academy of Sciences, or somebody, as having dealt with this matter?

Mr. Eisenbud. Yes, sir. That is a very fundamental part of the National Academy of Sciences study which was published last June and is still——

Senator Anderson. The study of the National Academy of Sciences was published last June?

Mr. Eisenbud. Yes, sir.

Senator Anderson. What data was that based on? What year? What period of time did it cover?

Mr. Eisenbud. They have access to data up through September 1955.

Senator Anderson. Did they have these figures showing a 10-percent increase?

Mr. Eisenbud. No, sir; these are about to be published now. But there is nothing surprising about these data. This is exactly in line with the forecast made about a year ago, and which I will cover later on in my prepared statement.

Senator Anderson. I am just reminded that I meant 10 times.

Mr. Eisenbud. Tenfold.

Senator Anderson. Tenfold, yes. The National Academy of Sciences is constantly quoted as saying there is no danger from radioactive fallout, and so forth. Did they look at this tenfold increase when they said that?

Mr. Eisenbud. When they saw the data it was not tenfold, but perhaps sevenfold, and the increase has been by the expected amount.

Senator ANDERSON. Has it been a curve that is moving upward, or is it a steady line?

Mr. EISENBUD. I think you are as good a judge of this as I am, sir.

Senator ANDERSON. It looks to me that if you plotted a curve, it might be moving upward very rapidly.

Mr. EISENBUD. It has got to reach a limit.

Let me get to this, if I may.

Senator ANDERSON. All right. Why does it have to reach a limit?

Mr. EISENBUD. Well, because there is just so much strontium, and when it is all accounted for there just cannot be any more from detonations to date.

Senator ANDERSON. But if it is accounted for by being deposited on the skin of the earth, it is quite a bit different from being buried somewhere, is it not? Never mind. I should know better than to start asking that type of question so soon.

Representative HOLIFIELD. Doctor, in making this statement, and bringing this chart to us, are you bringing to us information which has been approved and accepted as the Atomic Energy Commission's calculations, or is this your personal calculation? Are you doing it independently as a scientist?

Mr. EISENBUD. This is an independent calculation, but with the assistance of my colleagues and consultation with others within the Commission. We do not have the machinery within the Commission for producing a single so-called Commission computation.

There are a number of investigators within the Commission, and a great many more among the contractors of the Commission, all of whom are taking the same body of information and attempting to interpret it in their own way.

Representative HOLIFIELD. But the committee can understand that this is a statement approved by the Atomic Energy Commission?

Mr. EISENBUD. No, sir. I believe I am testifying in my individual capacity today, although——

Representative HOLIFIELD. That is the point I wanted to find out. That is fine.

Mr. EISENBUD. Yes, sir.

Senator ANDERSON. Just one minute. Are you not in the New York Operations Office of the Atomic Energy Commission?

Mr. EISENBUD. Yes, sir.

Senator ANDERSON. You are an employee of the Atomic Energy Commission?

Mr. EISENBUD. Yes, sir; but this is my statement. I am not reading a statement on behalf of anyone else.

Senator BRICKER. By that you mean that not all of the scientists in this field who are engaged in Atomic Energy Commission work would agree completely with you?

Mr. EISENBUD. I think they would agree with regard to the general conclusions. I think there would be a considerable measure of disagreement regarding details. But I think, as we hear about disagreement, we must ask ourselves, Does this disagreement really affect the conclusion?

Representative HOLIFIELD. Before we leave this part—and this is not to embarrass you, sir, but this is that the record may stand on its own feet, so to speak—you are presently on leave from the Atomic

Energy Commission, and you are acting as consultant to the United Nations?

Mr. EISENBUD. No, sir. I returned to active duty on Monday of this week. My 90 days with the United Nations were up.

Representative HOLIFIELD. You have been over the past certain period of time a consultant to the U. N. on this matter?

Mr. EISENBUD. No, not on this matter. I was senior scientific adviser to the Preparatory Commission drafting the program for the new agency.

Senator ANDERSON. Your name was on the agency payroll all the time; was it not?

Mr. EISENBUD. I was on the United Nations Preparatory Commission payroll for the 90 days, on leave from the Commission. That period expired on Monday of this week.

Representative HOLIFIELD. Thank you, Doctor.

Mr. EISENBUD. Now, the strontium 90 content of the milk from this milkshed averaged 3.5 micromicrocuries per gram of calcium in the period just preceding the sampling of the soils in October 1956 when the estimate of total fallout in this milkshed was made.

On this basis, then, we would expect this level of 3½ micromicrocuries per gram during this period to increase by a factor of 2.3 over the next months or years. So that the ultimate concentration in this milkshed will be 8.3 micromicrocuries per gram. I am sorry. It would be 8 micromicrocuries per gram. That will be ultimately somewhere about here [indicating], a little higher or a little lower. This is our best estimate at the present time.

Representative HOLIFIELD. That would be a factor of how many?

Mr. EISENBUD. 2.3 over this level in here [indicating]. This is our base—the average of these months.

Chairman DURHAM. That is based on the fact that all of it falls out?

Mr. EISENBUD. Yes, sir.

Representative HOLIFIELD. May I ask you why you use the 1955–56 level as a base rather than going back to 1954 as a base?

Mr. EISENBUD. We use this level as a base because this was the period when we were doing soil sampling in this milkshed, and what I want to do is relate the total amount on the ground, what is in the stratosphere, and this milk level during this period.

To show you this is a consistent pattern, sir, we did the same thing back here [indicating] a year ago in data which have been published. At that time we estimated that the maximum would be 8.3. So that 1 year has gone by, we have a lot of additional data, and we have revised our estimate from 8.3 to 8. Of course, there is no difference between the two.

When I say it is going to 8.3 or 8, this does not rule out the possibility that it may go to 10, but it certainly would not go to 80.

Based on the data now available, a child being nourished on milk containing 8 micromicrocuries of strontium 90 per gram of calcium would be expected to develop a skeleton containing strontium 90 in somewhat lower concentrations than this value, because the human metabolism discriminates in some measure against strontium.

Senator BRICKER. What is the relation of that discrimination to the amount of calcium that is in the milk? If there is more calcium in the milk is it a discrimination against strontium, greater or less?

Mr. EISENBUD. I am way out of my field now, but I think I can answer this competently. If the individual is on a calcium-deficient diet, then relatively more strontium would be taken up. But children who drink milk usually have a fairly large excess of calcium in their diet.

Senator BRICKER. The more calcium the less of the strontium that is assimilated?

Mr. EISENBUD. Yes—well, yes, sir. Based on data that have become available in human and animal experiments, a person on a diet of 8 micromicrocuries of strontium 90 per gram of calcium should develop a skeleton which has somewhere between 4 and 6 micromicrocuries per gram. So we start with a diet of 8, and it will drop to somewhere between 4 and 6. This seems to be the lower and upper limit.

Representative HOLIFIELD. Dr. Eisenbud, for the benefit of the lay readers who are not familiar with the term "millicuries," will you, or can you, in lay terms, express what this increased strontium 90 in milk means? When we talk about a tenfold increase in millicuries, this may be very alarming. Can we in some way express in layman's language this term so that the mothers, when they read this statement, will understand the significance of that measurement in relation to, let us say, a damaging amount? If you can draw any kind of comparison.

Mr. EISENBUD. I would prefer to leave the question of what level is damaging to the expert speakers who will come later on in the program, but I think that, for the purpose you have in mind, perhaps it would be worthwhile to point out that, when it is expressed simply as millicuries, which is a unit of activity, the natural radioactivity of milk, the radioactivity that has always been present not only in milk, but in other foods, is very much greater. It is my recollection that a quart of milk contains about, I think, a thousand disintegrations per minute. This is roughly 400 micromicrocuries. Now, a pound of beeksteak—again I am relying on my memory—has about 2,000 disintegrations per minute. That is almost a thousand micromicrocuries.

Senator ANDERSON. One time you use micromicro something else, and then you say "disintegrations." Can you keep it in pecks, quarts, or bushels?

Mr. EISENBUD. When we talk about the amount of strontium 90 on the earth's surface, we talk about millicuries, because we are talking about relatively large amounts.

Senator ANDERSON. Did you not start out to tell us what the natural amount of strontium 90 might be in milk, and then how much was added to it by these detonations? What is the relative ratio?

Mr. EISENBUD. The ratio of millicuries?

Senator ANDERSON. Well, whatever the unit of measurement is. In one study you found out it increased tenfold. What has happened in ordinary milk that you know about, against what might have come from strontium 90?

Mr. EISENBUD. All right. If you took a sample of milk before 1945, and put it in the Geiger counter, the background would go up because the milk always has a certain amount of radioactivity, just as all foods, rock, and tissue do. If you take a sample of milk now, and put it in the same Geiger counter, you cannot detect any difference. The difference is so small, against the background of radioactivity that is nor-

mally present, one cannot detect this strontium 90. You have to break it out chemically, and count it by low-level procedures after you have eliminated the natural radioactivity.

Senator ANDERSON. If it is so low that it is not measurable, is it at all dangerous?

Mr. EISENBUD. I think this gets us into a later section. For the record, and for orientation at this point, I can tell you that the maximum permissible exposure, or maximum permissible dose in human skeleton for public exposure, as recommended by various bodies that have considered this, is of the order of a hundred micromicrocuries per gram. One hundred.

Representative HOLIFIELD. How much do we have?

Mr. EISENBUD. Dr. Kulp later on this afternoon will no doubt tell you it is somewhat less than one. I am about to forecast how high it may go.

Representative HOLIFIELD. All right. Now let me ask you a question. In a normal calcium diet—I am going to refer this to children—in a normal calcium diet of a child, is it not true that most of the calcium is passed on through the body and is not retained?

Mr. EISENBUD. I understand that is so, sir.

Representative HOLIFIELD. In case there is strontium in that diet, mixed in that diet, is there a tendency for it to pass on through the body the same as calcium does? Or is there more of a tendency to retain it in the body than there is calcium?

Mr. EISENBUD. No, sir; it passes through like calcium. In fact, a little more of it passes through. That is why there is this discrimination. The human body prefers to take up calcium. Most of the time it cannot tell the difference between a strontium and a calcium atom, but sometimes it can. So, somewhere between 50 and 75 percent of the original ratio is retained, but, in general, it does pass through the body the same way as calcium does, and even a little more.

Representative HOLIFIELD. How does this compare to the natural radium that is in the milk?

Mr. EISENBUD. The uptake of radium is considerably less than the uptake of calcium.

Representative HOLIFIELD. Therefore, the retention of radium would be less, presumably, in the bone?

Mr. EISENBUD. Millicurie for millicurie, I would expect the uptake of radium to be less; yes, sir.

Representative HOLIFIELD. Then there is an affinity in the bone primarily for calcium, secondarily for strontium 90, and, in the third instance, for radium, in that order, you would say?

Mr. EISENBUD. Yes, sir. Others may correct me on this, but this is my opinion.

Senator BRICKER. Mr. Chairman?

Representative HOLIFIELD. Senator Bricker.

Senator BRICKER. Is there the same tendency on the part of plant-life in high calcium soils to reject the radioactive strontium as you find it in the human body?

Mr. EISENBUD. Yes. Really, I think that Dr. Alexander yesterday discussed this question in some detail, and pointed out that there was a discrimination in going from soil to plants, and then again within the body of the——

Senator BRICKER. Animal?

Mr. EISENBUD. Animal.

Senator BRICKER. And again, then, in the content of the milk there is a reduction, a natural reduction?

Mr. EISENBUD. Yes, sir.

Senator BRICKER. And then in the human body itself?

Mr. EISENBUD. Yes, sir.

Senator BRICKER. So you have four steps there, really, of discrimination against this alien element, you might say?

Mr. EISENBUD. Yes, sir.

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. Dr. Eisenbud, for the benefit of the record, can you give us, in plain words, the chemical structure of strontium 90?

Mr. EISENBUD. In the atmosphere it is usually as a silicate or carbonate, in the form of dust.

Representative VAN ZANDT. In the form of dust?

Mr. EISENBUD. Yes, sir.

Representative VAN ZANDT. Thank you.

Representative HOLIFIELD. Dr. Eisenbud, where is the milkshed you referred to, and how large is it?

Mr. EISENBUD. This is the milkshed located in the middle of New York State. The plant that processes this milk is located in western New York, and we regard these as representative of the New York milkshed.

In the full text of the report, sir, you will have tabulations of data like this for six milksheds in the United States and abroad, but I think you will find the numbers are in general agreement. For a while, one milkshed will get a little higher and then it will drop back to the others.

Senator ANDERSON. You said you were going to tell us what lies in the future.

Mr. EISENBUD. Yes, sir.

Senator ANDERSON. In order to make this a little topical, could you tell us for example from what you know of it, whether the British explosion that they had the other day—and I guess they are going to have more later on—how much of this strontium 90 it added which might deposit worldwide?

We had, finally, a little test yesterday, out in Nevada. Did it deposit strontium 90? In other words, are all of these things coming into the curve?

Many of us have been trying to point out that for a while only we were testing, then the Russians began to test, and apparently the Russians last year tested more than we did. Now the British are coming along. Pretty soon, of course, somebody else must come along. Do these add to it? And if so, how much.?

You are able to measure down to micromicrocuries. What do these add up to?

Mr. EISENBUD. When I have finished, I will have attempted to estimate how much strontium 90 is going to be in man from detonations to date. This covers a 6-year period. The only practical thing to do is to assume in the next 6 years the nations of the world are going to shoot as much as they have in the past 6 years.

Senator ANDERSON. Or tenfold, maybe.

Mr. EISENBUD. If we have the basic information, if we know what has happened up to now, we are in a position then to extrapolate. You can double it, or triple it, or stop entirely.

Senator ANDERSON. Here comes a brand new test by the British. Did it add some strontium 90?

Mr. EISENBUD. Yes, sir.

Senator ANDERSON. All right. It did?

Mr. EISENBUD. Yes.

Senator ANDERSON. That is good. How much did it add?

Mr. EISENBUD. We do not know. I do not know, sir.

Senator ANDERSON. Did you compute it on any basis?

Mr. EISENBUD. No. sir.

Senator ANDERSON. Was anybody curious about it?

Mr. EISENBUD. Yes, sir.

Senator ANDERSON. Did we not have—I do not want to get in the wrong field. Do we not have some way of measuring what was in it or estimating, let's say? Probably we had a way of measuring. Did we not have some way of finding out what was in it?

Mr. EISENBUD. If we have not done anything else, we will know about it when it comes down from the stratosphere.

Senator ANDERSON. I do not want to wait that long. I may die before it comes down. There is no way of estimating it now?

Mr. EISENBUD. Yes.

Senator ANDERSON. There is?

Mr. EISENBUD. Yes.

Senator ANDERSON. Is it impossible for you to give that to us? Is it secret?

Mr. EISENBUD. I do not have the information. You asked if it were possible to estimate. I said it is. That does not mean I can do it.

Senator ANDERSON. That is a completely honest answer. That is fine.

Mr. EISENBUD. All right.

Senator ANDERSON. How about the values—will we have some from the upcoming Plumbbob tests?

Mr. EISENBUD. Yes, sir; those tests will add to the fallout.

Senator ANDERSON. We have a bank account up in the sky sending down a little all the time, and then we keep adding to it, and disintegration is going on. Is anybody actually checking to see that the amount landing from previous accumulations and the amount that we are putting in now stays in balance with the decay?

Mr. EISENBUD. Yes, sir. We have enough information now, and large enough machinery so that now for the first time it is possible to predict what the fallout traces is going to be 2 years from now, 5 years from now.

Senator ANDERSON. I missed some of the previous testimony. Was it pointed out there is this difference, this heavy deposit in Wales, for example? And will that pattern be repeated around the world?

Mr. EISENBUD. The abnormality of deposits in Wales was not one due to the heavy deposits.

Senator ANDERSON. What was it due to?

Mr. EISENBUD. Dr. Alexander, who testified yesterday, went on a search around the world for an area in which the soil was conspicuously low in calcium, yet sufficiently fertile for at least sheep raising; and he

found this area in Wales. It is agriculturally a very unimportant area, but scientifically a very interesting one. He brought soil and bone back, and analyzed it, and the sheep bone had more strontium 90 than usual because of the low level of soil calcium.

Senator ANDERSON. That is what was found out about the human body: If there is a calcium deficiency, the greater the affinity for strontium; and if a calcium deficiency in soil, the affinity for strontium is greater.

Mr. EISENBUD. I think Dr. Kulp will deny that.

Senator ANDERSON. He will?

Mr. EISENBUD. He is going to testify.

Senator ANDERSON. All right.

Senator BRICKER. Just one more inquiry for the purpose of the record. I think there was testimony here a day or two ago, that the abnormal amount, not the abnormal amount, but the additional amount of strontium deposit in Washington at the time of the rain after the drought, was caused by the Russian tests. You would confirm that?

Mr. EISENBUD. I cannot confirm it, sir. We attempted to confirm it, and so far we have failed to do so. But it is a little early. It is a matter of a month or 2 months for the data to catch up with it. As of now, I cannot confirm it. The little bit of looking at it we have done—we do not have the data, but it is not surprising, I do not think, that from time to time there will be relatively heavy fallout here.

Representative HOLIFIELD. Dr. Eisenbud, I am going to ask you another question along the lines of the first ones I asked you. I do not want to oversimplify this matter, but first I am going to ask you: Do you have children?

Mr. EISENBUD. Yes, sir.

Representative HOLIFIELD. Now I am going to ask you if you would have any hesitancy at all in allowing your children to drink milk which has been produced on this milkshed?

Mr. EISENBUD. No, sir.

Representative HOLIFIELD. The reason I asked you that is because I want a plain understandable answer of your evaluation of the hazard that is involved, because when we are talking in tenfold increases of micromicrocuries, we are talking in a term which the average mother does not understand. But I think she could understand the fact that you, as a scientist, on your integrity as a scientist, would say that you would have no hesitancy in giving milk from this milkshed in any quantities that the child might want to your own children.

Mr. EISENBUD. None at all, sir.

Representative PRICE. Mr. Chairman, may I ask a question?

Representative HOLIFIELD. Mr. Price.

Representative PRICE. Dr. Eisenbud, does cesium 137 also show up in milk?

Mr. EISENBUD. Yes, sir. If I may defer this to Dr. Langham, who is going to testify later on this morning, I would like to do so.

Representative PRICE. Thank you.

Mr. EISENBUD. Let us use as a basis for discussion the 5 micromicrocuries of strontium 90 per gram of calcium, which we predict may be the maximum in perhaps 1970 or so. One can calculate that this amount of strontium 90 will deliver a dose of half a roentgen to the skeleton over a lifetime of 70 years. This compares with a normal skeletal irradiation—I want to repeat this for the record, sir: This

compares with a normal skeleton irradiation of 7 to 30 rads. So, I am comparing half a rad to somewhere between 7 and 30 rads resulting from potassium 40, carbon 14, cosmic rays, terrestrial gamma radiation, and radium. They are the natural radioactive constituents of our environment, to which man has been exposed throughout his development.

Senator BRICKER. That is the background radiation?

Mr. EISENBUD. Yes, sir. That is, the maximum foreseeable value of 5 micromicrocuries of strontium 90 per gram is equivalent to 1½ to 6 percent of the dose from natural sources of skeletal irradiation. It should be noted that this estimate includes a number of assumptions which are deliberately conservative. No allowance has been made for radioactive decay before the strontium 90 descends from the stratosphere. This may diminish the amount of available strontium 90 by about 25 percent. The assumption that all of the strontium 90 in milk originated by root uptake is another conservative assumption. The combined effect of these and other safety factors is apt to be appreciable.

The question has been raised as to whether a discussion of average fallout values is adequate to define the upper limit of hazard to people exposed to unusually heavy fallout. In this connection, the deviations from average are in the safe direction. This was brought out yesterday, independently, by Dr. Machta. Whereas fallout values greater than twice the mean are rarely reported in any given region, it is not uncommon to observe values which are of the order of 10 percent of the average. This forecast has been made on the basis of data from Northeastern United States. How applicable are these data to other parts of the United States or of the world?

Summaries of the concentration of strontium 90 in milk as observed in 6 domestic and 2 foreign sources of supply, as well as the data from the fallout measuring systems described earlier, indicate that the strontium 90 content of dietary calcium might be higher in certain areas. However, a factor of three applied to the estimates for Northeastern United States should be adequate to bracket the highest foreseeable value, from tests to date in any region of the world.

Thank you, Mr. Chairman.

Representative HOLIFIELD. Thank you Mr. Eisenbud.

Are there any further questions of the members?

Are there any questions from the staff?

Chairman DURHAM. Mr. Chairman.

Representative HOLIFIELD. As I have said before, we are reserving the right to study the statements that are presented, and then it is possible there will be further discussion on it during the hearings, or there will be letters addressed to those giving the presentations for answers to certain questions which may come up as a result of the analysis of the statement.

Mr. Durham.

Chairman DURHAM. No questions now.

Senator BRICKER. One question, Mr. Chairman.

Representative HOLIFIELD. Senator Bricker.

Senator BRICKER. It is your professional opinion, as an expert in this field, that we have reached a point of no danger, and you do not anticipate any danger point as far as the future is concerned?

Mr. EISENBUD. What I have described for you is the situation that I see on the basis of detonations to date. Now, if I predict 5 micromicrocuries per gram, it is very simple arithmetic to say how many times you would have to increase the rate of firing in order to get up to what we agree is a dangerous level. This would be a very considerable amount, and, at the present rate of testing, I personally am not apprehensive as to the long-range hazards.

Senator ANDERSON. You had a curve in there, it seemed to me, showing how this amount had increased, and it might again increase. If you also had a curve of testings, you would have an additional factor to take into consideration. We had a test several years ago out in the far Pacific, and then went for a long time without another one. Now, the Russians have 1 every 2 weeks, and we are scheduled to have maybe 2 a week here for a while. The British have started coming in. If you would plot all of these tests onto a curve, it would be moving up rapidly, and, with the amount of strontium 90 coming down out of the atmosphere also moving up rapidly, the two of them might change the figures a little, might they not?

You have projected these figures on the present rate of testing. It is like predicting the number of automobiles that would move if they all stayed at 10 miles an hour. They did not. They speeded them up. They got very much faster. Now the speedup on tests is very much faster, is it not?

Mr. EISENBUD. These calculations are, as you are aware based on testing up to late 1956, and there has been nothing in the testing schedule of the past year which would drastically alter these conclusions.

Senator ANDERSON. Wait a minute, now. I am just wondering I think I went out to the very first test in the Nevada proving grounds. We did not have too many test devices. Would not the amount of strontium 90 have some relationship to the number of devices tested?

Mr. EISENBUD. It has a relationship to the devices tested, and certainly the number of devices is part of the overall picture. Yes.

Senator ANDERSON. Yes; very, very substantially, because, if we make use of certain types of weapons, the strontium 90 fallout would not be proportionate to the power of the weapon at all. It is the devices that are somewhat smaller, maybe, that they have in development. So, would it not be important to chart the number of actual devices shot off in the plub-bob tests which will have many more than some of the earlier tests?

Mr. EISENBUD. I think it goes without saying, sir, that the basic purpose of these studies is to point out how many bombs you can detonate.

Senator ANDERSON. For the purpose of finding out, you say this line is moving up. We now proceed to freeze it at a level. But, with new countries coming in, with the multiplicity of our own shots, the tremendous multiplicity of the Russians, it is not going to stay level; it is going to go up with tremendous speed, apparently, in the future. What does that mean, biologically? That is one of the problems you have to consider.

Mr. EISENBUD. Yes, sir.

Senator ANDERSON. That is all.

Representative HOLIFIELD. The staff informs me you had two charts, one showing the Northern Hemisphere, and the other the Southern

Hemisphere, the radioactivity. Had you intended to produce them?

Mr. EISENBUD. We must be mixed up. I thought I did. This [indicating] shows for the soil samples and the gummed-film samples the latitudinal differences that exist.

I pointed out that the United States has in fact relatively heavy fallout in relation to other regions of the world. While this increase in the North Temperate Zone is obvious and has been known to us for some time, it is a difference of about, say, a small factor from the north Tropics, and very——

Representative HOLIFIELD. The chart does show that the latitude-longitude in which the United States is located is receiving, I would say, all the way from as much as five times the south Tropics to double the North Temperate?

Mr. EISENBUD. Yes.

Chairman DURHAM. Prior to 1945, was the amount of strontium actually known at that time?

Mr. EISENBUD. It did not exist, sir.

Chairman DURHAM. It did not exist?

Mr. EISENBUD. No.

Representative HOLIFIELD. Thank you very much, Mr. Eisenbud.

I will insert in the record at this point a study entitled "Summary of Analytical Results from the HASL Strontium Program, July-December 1956" by John H. Harley, Edward P. Hardy, Jr., Ira B. Whitney, and Merril Eisenbud.

(The study referred to follows:)

UNITED STATES ATOMIC ENERGY COMMISSION,
NEW YORK OPERATIONS OFFICE,
HEALTH AND SAFETY LABORATORY,
March 15, 1957.

SUMMARY OF ANALYTICAL RESULTS FROM THE HASL STRONTIUM PROGRAM JULY THROUGH DECEMBER, 1956

By John H. Harley, Edward P. Hardy, Jr., Ira B. Whitney, and Merril Eisenbud

ACKNOWLEDGMENTS

1. To Dr. Lyle T. Alexander, Chief, Soil Survey Laboratory, United States Department of Agriculture, for collecting and arranging for many of the samples reported here.
2. To the members of the Analytical Branch staff who performed most of the analyses reported, particularly Charles F. Baxter, Ira Cohen, William R. Collins, Jr., Gerald H. Hamada, Helen W. Keller, John J. Kelly, Americo Rodriguez, and Bernard Stern.
3. To Isotopes, Inc., and Nuclear Science & Engineering Corp., who performed certain analyses under contract to the Atomic Energy Commission. Such results are marked "(I)" and "(N)" in the tables of data.
4. To Mr. Eric W. Mood of the New Haven Department of Health for supplying dustfall samples collected by his organization.

This report summarizes the data on samples collected for the HASL strontium program during the period—July through December of 1956. Previous data was given in NYO-4751 (revised).

In addition to the analysis of samples collected for the HASL program, the Analytical Branch is responsible for the transmittal of samples to contract laboratories. These samples are part of the overall strontium program of the Division of Biology and Medicine. This activity, including preliminary treatment of certain samples, calculations and administration require the equivalent of one full-time man. Direct analyses and administration require 2 staff members for the gummed film program and 8 for the strontium program.

FALLOUT DOCUMENTATION

Gummed film network

Through the cooperation of the United States Weather Bureau and other groups, daily gummed film samples are collected at 40 stations within the continental United States and 75 stations in United States Territories and in other countries. Results through September 1955 have been reported previously.[1] The current data carries through September 1956.

Measurements are made by beta counting of ashed samples. Strontium 90 estimates are made by theoretical calculations, assuming a date of origin for the debris. The results are mapped in figure 1 for the United States, and figure 2 for the rest of the world.

In previous reports it was described how a factor of 1.6 was required to correct gummed film mixed fission product activity to agree with pot samples collected as HASL. This correction has been applied to gummed film results issued since that time for all stations.

Several groups of results in this report would indicate that a much larger correction factor would be in order, but this is not necessarily true. It is believed that the low strontium 90 values calculated from gummed film activity are due to the incorrect arbitrary burst assignments used in the calculations.

The recalculations of the gummed film values will be done, but it will be some time before the results are available. Therefore, gummed film data in this report, particularly after May 1956, should be considered as minimum estimates, and may be low by a considerable factor.

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[1] M. Eisenbud and J. H. Harley, Radioactive Dust From Nuclear Detonations, Science 117, 141–147 (1953). Ibid., Radioactive Fallout in the United States, Science 121, 677–680 (1955). Ibid., Radioactive Fallout Through September 1955, Science 124, 251–255 (1956).

FIGURE 1

ESTIMATE OF STRONTIUM 90 FALLOUT THROUGH SEPTEMBER 1956
MILLICURIES PER SQUARE MILE

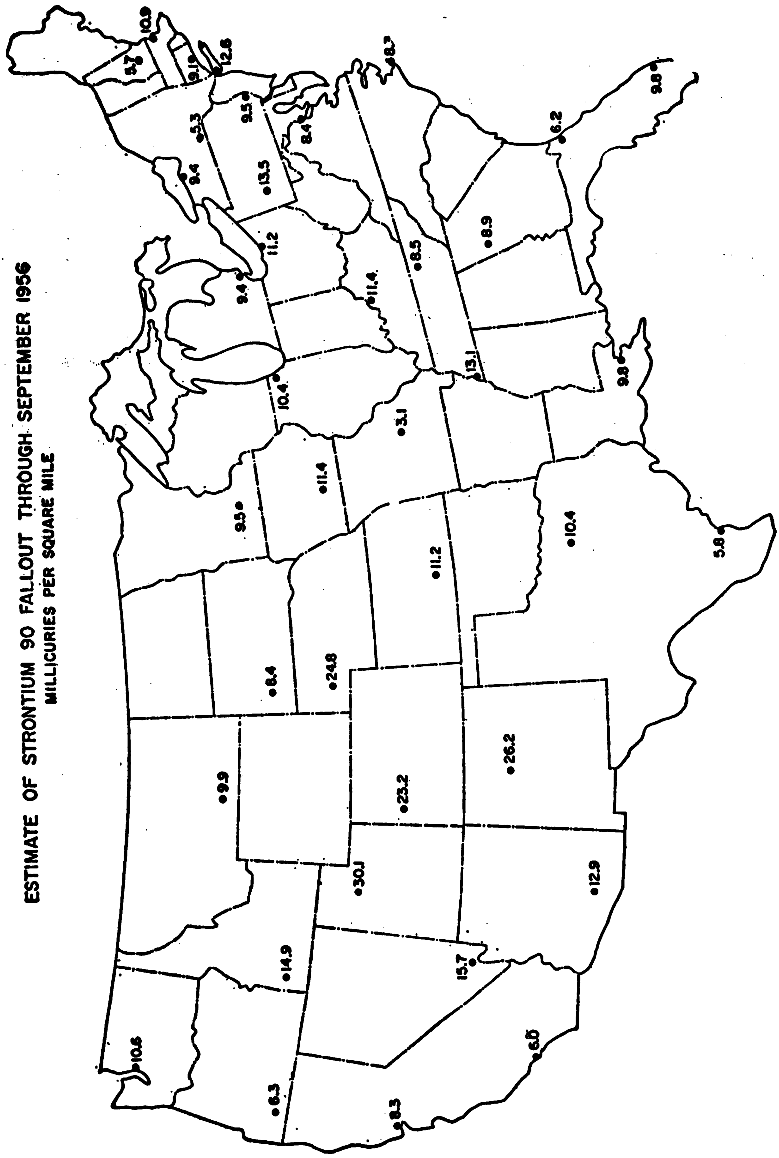
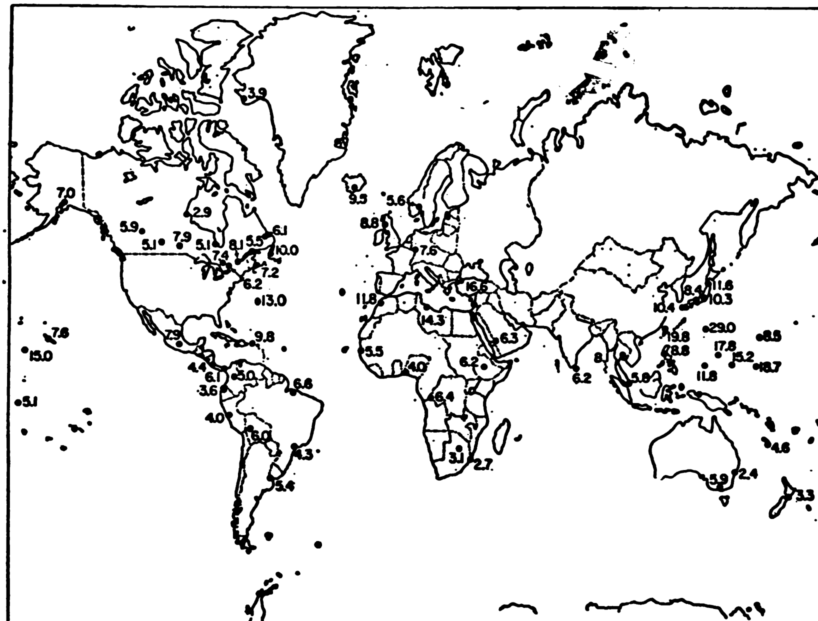


FIGURE 2



ESTIMATE OF STRONTIUM 90 FALLOUT THROUGH SEPTEMBER 1956
MILLICURIES PER SQUARE MILE

United States soil program

The advantages and disadvantages of gummed film for fallout measurement have been discussed many times previously. One possible calibration is the comparison of cumulative gummed film results with analyses of soils taken at identical locations. The first such collection was undertaken at 17 Weather Bureau stations in October 1955 and was repeated in October 1956. In both cases, 0–2 inch and 2–6 inch depths were taken, and strontium 90 leached with 6N HCl.

The 0–2 inch depths for the 1955 samples were reported in NYO–4751.[2] The 2–6 inch depths gave the analysts considerable difficulty due to interference from thorium chain isotopes and they were not reported. A procedure has been worked out for these soils and those having residual sample available will be reanalyzed.

The data for the 1956 samples is shown in table 1 and a plot of soil activity against the values estimated from the gummed film is shown in figure 3. As in the 1955 data, Albuquerque, Grand Junction, and Salt Lake City show lower soil values than predicted. The mean ratio of soil/film is 2.33 for the 14 "normal" samples. (Gummed film values already multiplied by 1.6 correction factor.)

If the 1955 samples are corrected to total mc./mi.^{2} using the 1956 ratio for total to topsoil activity, the mean ratio becomes 2.4. The value of 1.6 reported in NYO–4751 was for the 0–2 inch depths only.

[2] Harley, Hardy, Welford, Whitney, and Eisenbud, Summary of Analytical Results From the HASL Strontium Program to June 1956, NYO–4751 (1956), Technical Information Service Extension, Oak Ridge, Tenn.

TABLE 1.—*Strontium 99 data for 1956 continental United States survey soils—Strontium 90 extracted with 6N HCl at room temperature, sampled week of Oct. 8, 1956*

[Replicates represent individual soil aliquots taken after sampling]

Sampling site	Depth (inches)	d/m/gm soil*	mc/mi²	mc/mi² Average	mc/mi² Total
1. Albuquerque, N. Mex	0-2	0.078±0.001	7.5±0.1	7.3	
		0.075±0.001	7.2±0.1		
	2-10½	0.008±0.002	4.4±0.9	3.4	10.7
		0.005±0.002	2.4±0.8		
	0-2	0.10 ±0.004	10 ±0.4	10	
		0.10 ±0.004	10 ±0.4		
2. Atlanta, Ga	0-2	0.35 ±0.007	14 ±0.3	15	
		0.42 ±0.009	16 ±0.4		
	2-6	0.018±0.004	2.8±0.6	3	18.0
		0.021±0.003	3.3±0.5		
3. Binghamton, N. Y	0-2	0.32 ±0.007	17 ±0.4	18	
		0.35 ±0.007	18 ±0.4		
	2-6	0.019±0.003	4.4±0.8	5	23.0
		0.024±0.005	5.6±1.1		
4. Boise, Idaho	0-2	0.23 ±0.006	20 ±0.6	22	
		0.26 ±0.006	23 ±0.6		
	2-6	0.012±0.002	3.1±0.6	3.5	25.5
		0.015±0.002	4.0±0.6		
5. Des Moines, Iowa	0-2	0.31 ±0.007	23 ±0.5	23	
		0.31 ±0.007	23 ±0.5		
	2-6	0.028±0.002	7.6±0.7	7.1	30.1
		0.024±0.003	6.6±0.7		
6. Detroit, Mich	0-2	0.26 ±0.006	20 ±0.5	20	
		0.27 ±0.006	20 ±0.5		
	2-6	0.038±0.003	7.3±0.5	7.8	27.8
		0.044±0.003	8.4±0.6		
7. Grand Junction, Colo	0-2	0.10 ±0.001	7.8±0.1	7	
		0.091±0.001	7.1±0.1		
		0.11 ±0.019	†8.2±1.4		
		0.070±0.013	†5.1±1.0		
	2-10½	≤0.002	≤0.45	≤.48	7.5
		≤0.002	≤0.51		
8. Jacksonville, Fla	0-2	‡0.11 ±0.009	‡7.3±0.6	7.3	
	2-6	0.013±0.004	2.7±0.9	3.4	10.7
		0.020±0.005	4.0±1.0		
9. Los Angeles, Calif	0-2	0.12 ±0.008	6.9±0.5	7.5	
		0.14 ±0.009	8.0±0.5		
	2-7	0.009±0.002	3.3±0.9	2.8	10.3
		0.006±0.002	2.2±0.7		
10. Memphis, Tenn	0-2	0.27 ±0.006	15 ±0.4	15	
		0.26 ±0.006	15 ±0.4		
	2-6	0.028±0.003	6.5±0.7	6.6	21.6
		0.029±0.003	6.6±0.7		
11. New Orleans, La	0-2	0.24 ±0.006	8.8±0.2	8.6	
		0.22 ±0.006	8.3±0.2		
	2-6	0.009±0.002	3.3±0.9	2.8	11.4
		0.006±0.002	2.2±0.7		
12. New York, N. Y	0-2	0.21 ±0.006	10 ±0.3	12	
		0.29 ±0.007	14 ±0.3		
	2-6	0.072±0.004	14 ±0.8	14	26.0
		0.068±0.004	14 ±0.8		
13. Philadelphia, Pa	0-2	0.17 ±0.005	12 ±0.4	12	
		0.16 ±0.005	11 ±0.4		
	2-6	0.029±0.003	7.3±0.8	6.8	18.8
		0.026±0.003	6.4±0.7		
14. Rapid City, S. Dak	0-2	0.29 ±0.006	20 ±0.4	22	
		0.34 ±0.006	23 ±0.4		
	2-6	0.053±0.004	12 ±1.0	11	33.0
		0.045±0.003	10 ±0.7		
15. Rochester, N. Y	0-2	§0.22 ±0.006	§16 ±0.4	16	
	2-6	0.013±0.002	2.5±0.4	2.5	18.5
		0.013±0.002	2.5±0.4		
16. Salt Lake City, Utah	0-2	0.32 ±0.007	22 ±0.5	22	
		0.33 ±0.007	23 ±0.5		
		0.31 ±0.007	22 ±0.5		
	2-8	0.016±0.002	5.7±0.7	5.8	27.8
		0.016±0.002	5.9±0.8		
17. Seattle, Wash	0-2	0.46 ±0.011	17 ±0.4	17	
		0.44 ±0.010	16 ±0.4		
	2-6	0.051±0.007	9.4±1.2	9.5	26.5
		0.052±0.004	9.6±0.7		

*Air dried.
†Alternative procedure used.
‡Sample lost.
§Not reported.

NOTE.—Each error term represents 1 standard deviation due to counting error.

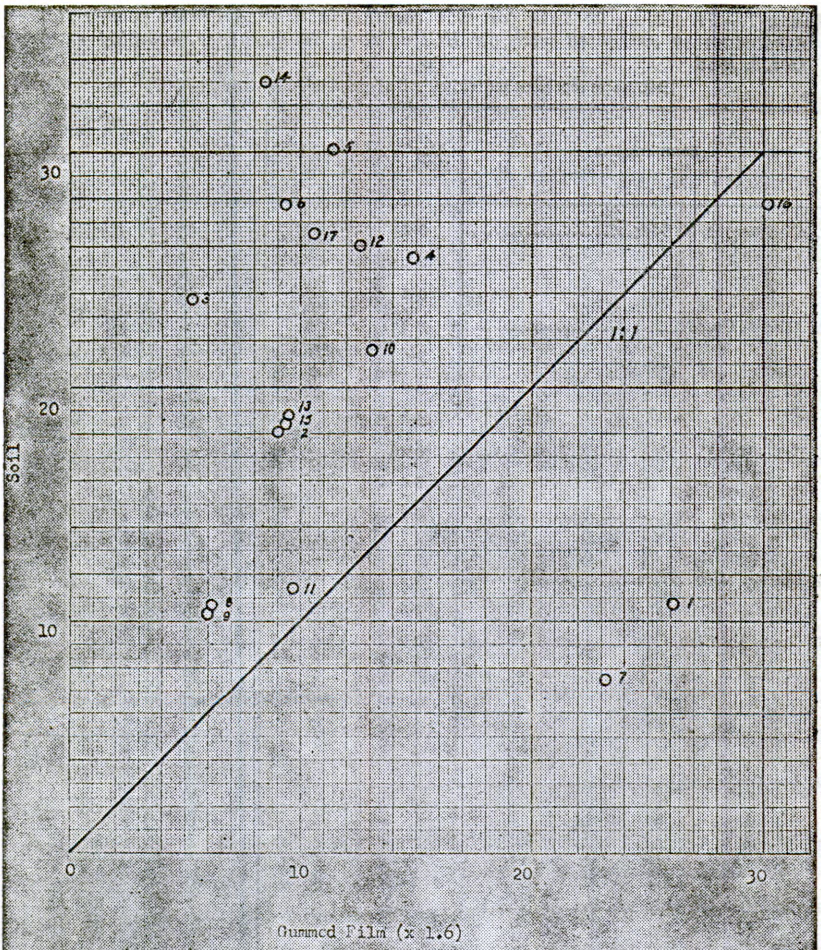


Fig. 3

Soil - Gummed Film Relationship for Sr-90 (mc/mi^2)

Further investigations are in progress on the reasons for the deviations shown by the three stations mentioned above. In addition, analysis of foreign soils will be made where available to extend the comparison.

Other comparison programs

Other sampling procedures for fallout documentation are by collection in a high-walled pot, or by collection of rainfall. These procedures may also be used for calibration of the gummed film. The first is represented by data from New York City by HASL, and the second by data from Pittsburgh by Nuclear Science & Engineering Corp. under AEC contracts. The detailed data will be presented later, but table 2 indicates the cumulative results useful for calibration purposes. For comparison, the New York City (La Guardia Airport) soil and gummed film data are included.

The pot and rainfall samples show that the 1.6 correction factor is not sufficient for this period. It must be emphasized, however, that this is not necessarily a failure of the original pot to film calibration figure of 1.6. It is more likely a failure to attribute measured MFP activity correctly to earlier tests, with a corresponding error in strontium-90 estimation.

TABLE 2.—*Gummed film comparisons (cumulative mc/mi²)*

	October 1955	October 1956	Δ	Ratio
LaGuardia:				
Soil	13.0	26.0	13.0	
Gummed film	2.6	7.9	5.3	2.45
HASL:				
Pot	14.9	25.9	11.0	
Gummed film	3.8	9.3	5.5	2.0
Pittsburgh:				
Rain	6.5	16.2	9.7	
Gummed film	2.6	8.5	5.9	1.65

NOTE.—Gummed film values not corrected for efficiency.

New Haven dustfall

The bureau of environmental sanitation of the New Haven Department of Health collects monthly dustfall samples at several stations in and around the city. The collectors are standard 1,500 ml. beakers, and duplicate samples are analyzed for dust content, one by evaporation of any rainfall and the other by filtration. The samples supplied to HASL were measured for total MFP activity and Sr–90, and the results are shown in table 3.

While the data are not as complete as desired, there are several interesting points:

1. Filtration or evaporation are equally effective for dustfall by weight.
2. Filtration loses both MFP and Sr–90 by solubility.
3. The agreement in activity values between stations is fairly good, and is independent of the dustfall.

A comparison of the mean Sr–90 values from the evaporated samples and the corresponding gummed film estimates (New Haven Airport) is shown in table 4. It appears that the gummed film activity should have been attributed to an early test series, rather than current tests, to give better agreement.

TABLE 3.—*New Haven dustfall experiment*

Sampling station	March			April		
	Activity, total β [*] mc/ml [2]	Sr^{90} mc/ml [2]	Gms. dust	Activity, total β [*] mc/ml [2]	Sr^{90} mc/ml [2]	Gms. dust
Airport No. 1:						
Evaporation	-----	-----	-----	55.55±1.37(1)	2.1 ±0.26	0.077
Filtration	11.11±0.85(1)	0.20±0.05	0.035	26.49±1.11(1)	0.14±0.06	0.075
Airport No. 2:						
Evaporation	-----	-----	-----	-----	-----	-----
Filtration	10.26±0.85(1)	0.09±0.06	0.025	-----	-----	-----
St. Rose's Convent:						
Evaporation	-----	-----	-----	48.71±1.54(1)	1.7 ±0.32	0.167
Filtration	14.53±0.94(1)	0.23±0.05	0.110	29.05±1.19(1)	0.25±0.1	0.159
Hall of records:						
Evaporation	-----	-----	-----	70.93±1.54(1)	1.4 ±0.08	0.143
Filtration	15.38±0.94(1)	0.14±0.06	0.096	Lost	0.14±0.05	0.114
Edward Malley Co., Bldg.:						
Evaporation	-----	-----	-----	69.22±1.62(1)	1.6 ±0.12	0.130
Filtration	16.24±0.94(2)	0.37±0.11	0.106	23.07±1.11(1)	[8]0.08	0.111
Brady Memorial Laboratory:						
Evaporation	-----	-----	-----	-----	-----	-----
Filtration	12.82±0.85(2)	0.21±0.07	0.206	-----	-----	-----
New Haven Hospital:						
Evaporation	-----	-----	-----	63.24±1.62(1)	0.94±029	0.109
Filtration	11.96±0.85(2)	0.14±0.06	0.081	25.64±1.03(2)	0.09±0.07	0.090
Grace-N. H. Memorial Bldg.						
Evaporation	-----	-----	-----	68.37±1.71(1)	6.0 ±0.37	0.109
Filtration	-----	-----	-----	27.35±1.03(2)	0.14±0.05	0.100
Grace-N. H. Memorial Bldg.:						
Evaporation	-----	-----	-----	-----	-----	-----
Filtration	-----	-----	-----	-----	-----	-----

Sampling station	May			June		
	Activity, total β [*] mc/ml [2]	Sr^{90} mc/ml [2]	Gms. dust	Activity, total β [*] mc/ml [2]	Sr^{90} mc/ml [2]	Gms. dust
Airport No. 1:						
Evaporation	40.17±1.28(1)	0.49±0.31	0.072	25.64±1.28(2)	0.53±0.06	0.054
Filtration	17.95±0.94(1)	0.38±0.09	0.046	12.82±0.85(2)	0.34±0.06	0.046
Airport No. 2:						
Evaporation	-----	-----	-----	-----	-----	-----
Filtration	-----	-----	-----	-----	-----	-----
St. Rose's Convent:						
Evaporation	43.58±1.37(1)	0.70±0.32	0.128	31.62±1.19(2)	0.66±0.08	0.163
Filtration	15.38±0.85(1)	0.18±0.07	0.112	19.66±1.03(2)	0.53±0.06	0.157
Hall of records:						
Evaporation	-----	-----	-----	26.49±1.11(2)	0.69±0.09	0.110
Filtration	23.92±1.11(1)	0.34±0.08	0.116	16.24±1.03(2)	0.50±0.12	0.112
Edward Malley Co., Bldg.:						
Evaporation	-----	-----	-----	30.76±1.20(2)	0.60±0.06	0.098
Filtration	17.09±0.85(1)	0.34±0.06	0.090	16.24±0.85(2)	0.42±0.06	0.096
Brady Memorial Laboratory:						
Evaporation	-----	-----	-----	-----	-----	-----
Filtration	-----	-----	-----	-----	-----	-----
New Haven Hospital:						
Evaporation	-----	-----	-----	-----	-----	-----
Filtration	17.09±0.94(1)	0.71±0.06	0.059	-----	-----	-----
Grace-N. H. Memorial Bldg.:						
Evaporation	-----	-----	-----	26.49±1.20(2)	0.36±0.06	0.104
Filtration	15.38±0.94(1)	0.39±0.07	0.068	17.09±0.94(2)	0.49±0.08	0.079
Grace-N. H. Memorial Bldg.:						
Evaporation	-----	-----	-----	24.78±0.94(2)	0.54±0.11	0.062
Filtration	-----	-----	-----	15.38±0.94(2)	0.37±0.08	0.064

TABLE 3.—*New Haven dustfall experiment*—Continued

Sampling station	July			August		
	Activity, total β* mc/mi²	Sr 90 mc/mi²	Gms. dust	Activity, total β* mc/mi²	Sr 90 mc/mi²	Gms. dust
Airport No. 1:						
Evaporation				Lost	0.51±0.07	
Filtration	23.07±1.11(3)	0.25±0.06	0.028	7.61±0.72(4)	80.10	0.015
Airport No. 2:						
Evaporation						
Filtration						
St. Rose's Convent:						
Evaporation				Lost	0.57±0.07	
Filtration	30.77±1.20(3)	0.17±0.06	0.095	8.38±0.77(4)	0.17±0.06	0.122
Hall of records:						
Evaporation						
Filtration	32.47±1.20(3)	0.26±0.1	0.080	9.40±0.77(4)	0.18±0.10	0.075
Edward Malley Co., Bldg.:						
Evaporation						
Filtration	22.22±1.03 (3)	0.15±0.06	0.067	6.92±0.73(4)	0.14±0.06	0.066
Brady Memorial						
Evaporation						
Filtration						
New Haven Hospital:						
Evaporation						
Filtration						
Grace-N. H. Memorial Bldg.:						
Evaporation				Lost	0.51±0.07	
Filtration	26.49±1.03(3)	0.25±0.05	0.069	8.38±0.68(4)	0.40±0.10	0.034
Grace-N. H. Memorial Bldg.:						
Evaporation						
Filtration	37.60±1.37(3)	0.22±0.05	0.046	6.24±0.64(4)	0.19±0.06	0.053

*See counting date references.
References and counting date.
(1) July 17, 1956.
(2) July 24, 1956.
(3) Aug. 24, 1956.
(4) Nov. 7, 1956.

TABLE 4.—*New Haven dustfall experiment*

Month	Sr-90 activity in mc/mi²	
	Mean dust-fall	Gummed film
1956—March		0.38
April	[1] 1.55	.59
May	.60	.12
June	.56	.01
July		.01
August	.53	.02

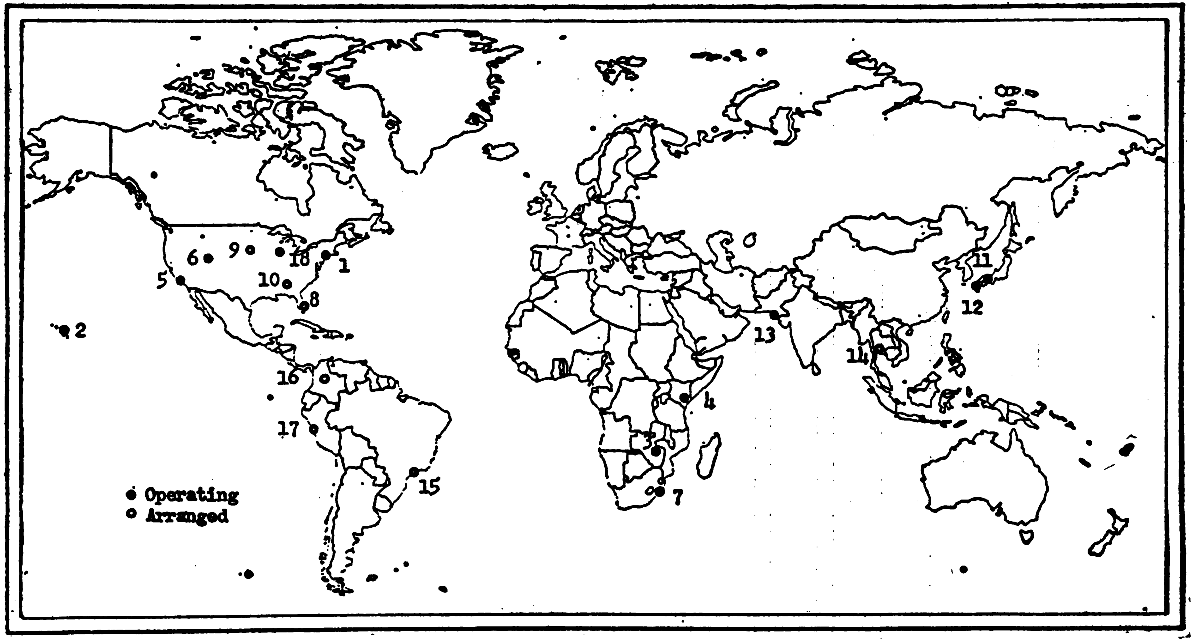
[1] Memorial building sample omitted.

NOTE.—Gummed-film values have not been corrected for efficiency.

Current pot program

The uncertainty as to the source of current fallout requires more extensive calibration than in the past. The simplicity of gumed film for daily network operation makes it necessary that it be retained for routine sampling. The possible errors in attributing activity to a given source is expected to be overcome by a broader network of pot-sampling stations. The operating and projected stations is the HASL program are shown in figure 4. These samples will be collected monthly and analyzed for MFP and Sr–90 and probably for Cs–137.

Cooperative tests will start in April 1957 on a fallout collector designed by Dr. Bo Aler, or Sweden. Fallout collects in a plastic funnel and is passed through a filter and ion-exchange column to collect the activity. Such a collector would be simple to maintain and to ship to a central laboratory for processing.



1. New York, N.Y.
2. Honolulu, Hawaii
3. Causeway, Southern Rhodesia
4. Kikuyu, Kenya
5. West Los Angeles, Cal.
6. Salt Lake City, Utah
7. Durban, South Africa
8. Coral Gables, Fla.
9. Vermillion, S. D.
10. Birmingham, Ala
11. Hiroshima, Japan
12. Nagasaki, Japan
13. Karachi, Pakistan
14. Bangkok, Thailand
15. Rio de Janeiro, Brazil
16. Bogota, Colombia
17. Lima, Peru
18. Chicago, Ill.

Summary

The sets of data on fallout from the United States soils, the New Haven dustfall, and the pot and rainfall from New York City and Pittsburgh all indicate that the current gummed-film estimates of Sr–90 deposition are in error. This is particularly true for activity attributed to tests in the summer of 1956. The actual Sr–90 measurements show the debris to be much older, and the gummed film data must be recalculated on this basis.

It was not possible to recalculate the mass of data involved for this report, but this will be done. In the meantime, the gummed-film estimates of Sr–90 must be considered as minimum values, subject to correction.

Futher samples and analyses on pot and soil samples are required before conclusions can be drawn as to the extent and uniformity of fallout outside the United States. Such work is in progress and will be reported when the results are completed.

PASTURE PROGRAM

Not all of the 1956 samples for the HASL pasture program have been received, and no Sr–90 data will be reported until all analyses are complete.

There have been several improvements in analytical techniques since the 1953 and 1954 pasture samples were run originally. Therefore, all samples still available will be reanalyzed in the coming months. When complete, the 4 years data will be reported as a unit.

One approach to estimating the eventual equilibrium state of Sr–90 in the biological cycle is through measurement of stable strontium and calcium. Some of the original pasture samples have been run for stable strontium and are reported in preliminary form in table 5. No attempt at interpretation will be made until further analyses are completed. (See also section on milk analyses.)

TABLE 5.—*Normal strontium, pasture samples (atoms Sr/1000 atoms Ca)*

	Tifton	Ithaca
1953 soil:		
Avail. Ca	69	5.6
Total Ca	1.9	.8
1955 plant	.73	1.1
1953 bone	.21	.22
1955 bone	.35	.26

NEW YORK CITY MONITORING PROGRAM

Pot sampling

A high-walled stainless steel pot is maintained on the roof of HASL for fallout sampling. The results of weekly collections are given in table 6 and the cumulative curve is shown in figure 5.

It was found that agreement between duplicate pots is not good. This is mostly caused by the low activity levels. Such deviations are expected to be reduced by exposure for 1-month periods and by counting on lower background equipment. The latter is shown partially by the data after September 24, 1956, but further improvement will be possible as more counting equipment will allow longer counting times.

Tapwater

New York City tapwater has been analyzed since August 1954. The results obtained since issuance of NYO–4751 are given in table 7. The weighted average value for the 6-month period is 0.19 μc/liter.

TABLE 6.—*Pot and gummed film results from HASL*

Date	Pot mc/mi^2		Film mc/mi^2	Date	Pot mc/mi^2		Film mc/mi^2
Jan. 2, 1956	0.104			July 2, 1956	0.264		
Jan. 9, 1956	.289	2.7	0.8	July 9, 1956	.080	0.6	0.08
Jan. 16, 1956	.201			July 16, 1956	.062		
Jan. 23, 1956	.301			July 23, 1956	.120		
Jan. 30, 1956	.188			July 30, 1956	.119		
Feb. 6, 1956	.308	1.0	1.5	Aug. 6, 1956	.068		
Feb. 13, 1956	.226			Aug. 13, 1956	.301	.5	.05
Feb. 20, 1956	.276			Aug. 20, 1956	.068		
Feb. 27, 1956	.251			Aug. 27, 1956	.096		
Mar. 5, 1956	.509			Sept. 3, 1956	.085		
Mar. 12, 1956	.729	1.9	1.3	Sept. 10, 1956	.194	.3	.04
Mar. 19, 1956	.302			Sept. 17, 1956	.019		
Mar. 26, 1956	.073			Sept. 24, 1956	.018		
Apr. 2, 1956	.295			Oct. 1, 1956	.157		
Apr. 9, 1956	.176	.8	1.1	Oct. 8, 1956	.030	.3	.03
Apr. 16, 1956	.201			Oct. 15, 1956	.050		
Apr. 23, 1956	.099			Oct. 22, 1956	.086		
Apr. 30, 1956	.377			Oct. 29, 1956	.383		
May 7, 1956	.068	1.0	.11	Nov. 5, 1956	.078		
May 14, 1956	.214			Nov. 12, 1956	.142	.9	.03
May 21, 1956	.377			Nov. 19, 1956	.244		
May 28, 1956	.176			Nov. 26, 1956	.047		
June 4, 1956	.070			Dec. 3, 1956	.092		
June 11, 1956	.141	.8	.01	Dec. 10, 1956	.25		
June 18, 1956	.276			Dec. 17, 1956	.18	.6	-------------
June 25, 1956	.141			Dec. 26, 1956	.035		

NOTE.—Gummed film results are not corrected for efficiency.

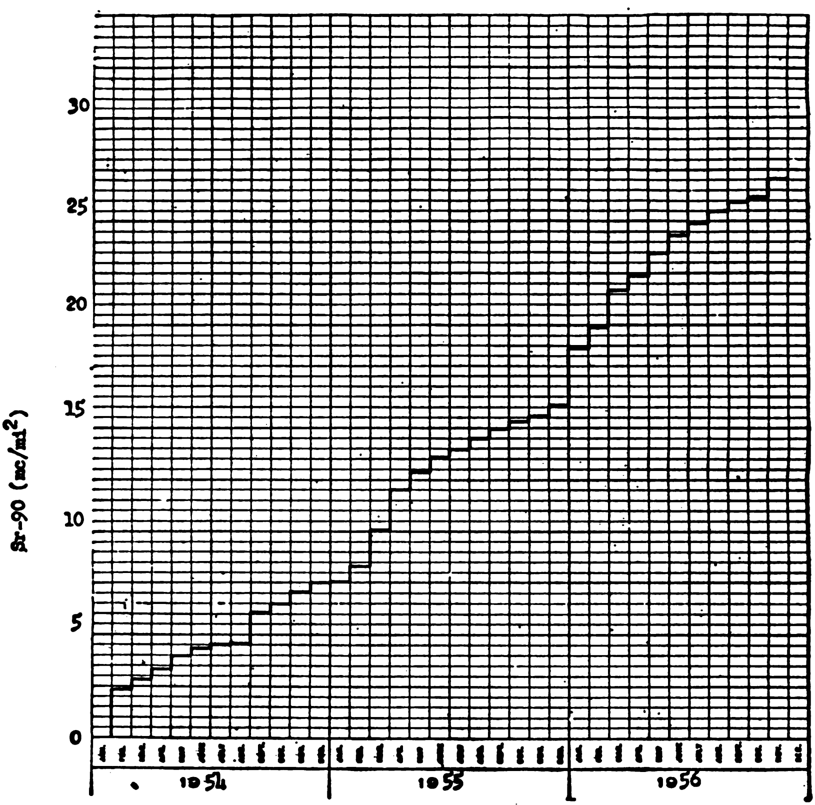


FIGURE 5.—Cumulative Sr–90 from pot collections at HASL.

TABLE 7.—*New York City tapwater*

HASL No.	Sampling period	Total activity		Sr-90, d/m/l
		c-date	d/m/l	
3785	June 27 to July 17, 1956	Aug. 30, 1956	4.1±0.2	0.50±0.08
4216	Aug. 27 to Sept. 12, 1956	Sept. 19, 1956	3.9±0.2	0.28±0.06
4286	Sept. 12 to 22, 1956		2.6±0.3	0.03±0.02
4299	Sept. 22 to Oct. 5, 1956	Oct. 19, 1956	8.4±0.3	0.57±0.03
4404	Oct. 5 to 17, 1956	do	7.5±0.3	0.44±0.03
4585	Oct. 16 to 30, 1956	do	4.2±0.2	Lost
4699	Oct. 30 to Nov. 13, 1956	Nov. 19, 1956	5.0±0.3	0.56±0.04
4850	Nov. 14 to 27, 1956	Nov. 30, 1956	5.4±0.2	0.49±0.03

Milk

The results of milk analyses performed since the issuance of NYO-4751 are given in table 8 and the continuous curve for dried milk is shown in figure 6.

The level increased during the summer of 1956 and has maintained its level after the normal pasture season. This probably indicates that the cows are feeding on vegetation exposed during the summer and will stay at the same level until the next pasture season.

In NYO-4751, some concern was shown that wet milk purchased in New York City showed higher levels than the dried milk being analyzed. This is apparently due to the difference in source area of the two supplies. As a check, however, Dr. Alexander obtained wet milk, dried milk, and the washings from the drier during a run at Columbus, Wis. These samples are being analyzed to check possible loss of strontium 90 during processing and to test uniformity of run.

Wet milk samples were not run during this reporting period, but a more extensive sampling program for the city supply was started at the first of 1957.

Some of the dried milk samples have been analyzed for stable strontium. The preliminary data are given in table 9.

Tests have shown that dry ashing of milk results in loss of cesium 137, even at low temperatures. No cesium 137 results on milk will be reported until improved techniques are available.

TABLE 8.—*Dried milk samples*

HASL No.	Analyzed by—	Sample date	Ca percent ash	S. U.	Sr-89/Sr-90	Sr-89 c-date
3720	HASL	June 1956	14.01	3.0±0.7	1.0	Oct. 3, 1956
3832	HASL	July 1956	16.93	2.7±0.7	5	Do.
3833	HASL	August 1956	18.51	3.1±0.7	7	Do.
4149 I	HASL	Sept. 3, 1956		5.1±0.7	11.0	Do.
4149 II	HASL	Sept. 13, 1956		5.5±0.5	8.5	Nov. 13, 1956
4149 III	HASL	Sept. 17, 1956		4.1±0.7	11.0	Do.
4149 IV	HASL	Sept. 24, 1956		4.8±0.5	12.0	Do.
4301-1	(I)	Oct. 4, 1956	17.0	5.05±0.06		
4301-2	(I)	Oct. 11, 1956	15.6	5.37±0.12		
4301-3	(I)	Oct. 18, 1956	18.7	5.68±0.11		
4301-4	(I)	Oct. 26, 1956	20.0	5.52±0.08		
4698	(N)	November 1955	16.0	5.6±0.3		

Source: Perry, N. Y.

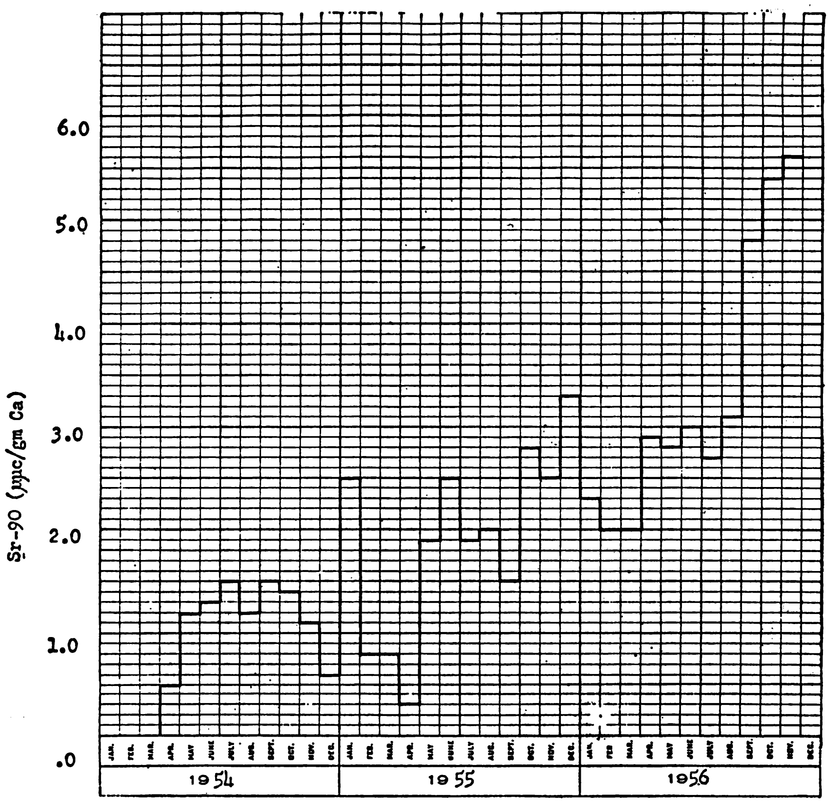


FIGURE 6.—Monthly analyses of dried milk from Perry, N. Y.

TABLE 9.—*Normal Sr in milk*

HASL No.	Location	Process date	Percent ash	Percent Ca in ash	Percent Sr in ash	Percent Sr / Percent Ca x 10³	Atoms Sr / Atoms Ca x 10³
3558	New York City, wet.	March 1956	------	15.1	0.0043±0.0003	0.28	0.13
3719	do	May 1956	------	15.1	.0046± .0002	.30	.14
3720	Perry, N. Y	June 1956	5.9	17.2	.0034± .0002	.20	.092
3832	do	July 30, 1956	5.4	16.9	.0045± .0006	.27	.12
3833	do	August 1956	5.7	18.0	.0057± .0006	.32	.14
4352	do	Aug. 28, 1956.	4.9	17.0	.0058± .0005	.34	.16
4698	do	November 1956.	5.9	16.0	.0059± .0006	.37	.17
3778	Columbus, Wis	May 1956	5.8	16.0	.0033± .0002	.21	.097
3800	do	July 1956	5.9	17.6	.0024± .0001	.14	.064
3776	Mandan, N. Dak	June 1956	11.7	9.28	.0039± .0008	.42	.19
4057	State College, Miss.	July 1956	8.1	17.5	.0077± .0006	.44	.20
3736	Portland, Oreg	May 1956	8.2	16.4	.0053± .0010	.32	.15
3737	do	June 1956	7.5	19.0	.0057± .0010	.30	.14
4293	Hikurangi, New Zealand.	--------------	7.9	13.5	.0035± .0002	.26	.12

Urine

Urine samples from laboratory personnel and others in the city have been run at intervals. The values found for Sr–90 and Cs–137 are given in table 10. The period of sampling is too short to indicate any trend.

TABLE 10.—*Urine analyses*

NEW YORK NAVAL SHIPYARD EMPLOYEES

HASL No.	Total volume, liters	S-date	Sr–90 d/m/l	Cs–137 d/m/l
3911	4	Aug. 10, 1956	1.3±0.25	
3912	4	do	1.1±0.28	21±1.5
3913	4	do	≤0.24	
3914	4	do	1.2±0.25	
3915	4	do	2.3±0.32	25±1.6
3916	4	do	1.1±0.30	16±1.7
3917	4	do	≤0.20	
3918	3	(1)		30±2.0
3919	2	(1)	1.2±0.50	
3926	4	Aug. 17, 1956	0.95±0.28	26±1.6
3927	4	do	0.82±0.25	26±1.9
3928	4	do	0.92±0.28	21±1.5

HASL EMPLOYEES

Type of sample	Total volume, liters	S-date	Sr–90 d/m/l
Pooled	5	June 1956	1.4±0.2
Do	5	do	1.9±0.2
Do	2	do	1.0±0.2
GH	5	September 1956	0.6±0.2
ST	5	do	1.0±0.2
Pooled	5	do	1.3±0.2

[1] Lost.

OTHER MONITORING

Milk supplies

Dried milk from several sources in the United States and abroad has been analyzed. A complete summary of the data is given in table 11.

The high value for Mandan led to analysis of soil and animal bone for this region. The bone ran 24 $\mu\mu$c/g. Ca and the soil 6.4 $\mu\mu$c/g. Ca with 10 mc/mi.[2] Neither the fallout nor the activity relative to calcium is high for the soil, yet the bone value is what might be expected from the high level in the milk. The milk samples from other sites are reasonably close to the level found for New York.

Fish samples

Canned tuna of known origin plun canned Alaska salmon and bonito are analyzed on a regular basis. The results of all analyses to date are given in table 12. There are no apparent trends.

Pittsburgh rain collection

Rainfall has been collected since February 25, 1955, at Pittsburgh. The collections are made by Nuclear Science and Engineering Corp. with a pair of tubs having a total area of 5.16 square feet. The collection periods are of irregular length and the analyses are performed at NSE. The cumulative data are shown in Figure 7.

The accumulation of Sr–90 at Pittsburgh is in reasonable agreement with the results obtained at HASL. At the end of 1956, Pittsburgh showed 17 mc/mi.[2], while HASL showed 18 for the same period.

TABLE 11.—*Dried milk analysis*

	Sr-90, μc/g.Ca						
	1	2	3	4	5	6	7
1955—January						3.0	
February						1.0	
March						2.0	1.8
April						1.8	2.9
May	2.6	4.1	1.0	7.3	1.7	1.9	
June	4.7	4.6	4.6	9.2	2.6	.8	5.5
July	4.4	3.9	.8	6.3			2.6
August	4.1		1.2	5.8		.8	
September	3.2		3.3	4.7		2.0	
October			4.4	6.9		7.5	
November			3.7	7.4		2.5	
December			3.0	10.0		3.5	
1956—January			3.0	3.5		2.7	4.0
February			3.5	8.1			
March	6.3		3.4	11.0		3.5	
April	6.7		3.4	9.6	5.2	(N) 3.0	4.6
May	4.9		2.8	17.0	6.4		4.5
June	4.4		3.4	8.7	5.0		5.0
July	6.1		4.2	6.6		(N) 2.3	
August	(I) 3.8		(I) 4.7	8.6			
September	(N) 4.8			(N) 10.7			
October				(N) 8.9			

Locations:

1. State College, Miss.
2. St. Louis, Mo.
3. Columbus, Wis.
4. Mandan, N. Dak.
5. Portland, Oreg.
6. Japan.
7. United Kingdom.

TABLE 12.—*Sr^{90} in canned fish*

HASL No.	Type		Date received at HASL	Sr-90, d/m/kg.-wet [1]
3636	Alaska pink salmen		May 26, 1956	≤0.98
3739	do		June 18, 1956	3.9±1.5
3760	do		July 3, 1956	2.9±1.2
3762	do		July 5, 1956	
3832	do		Aug. 6, 1956	1.5±0.73
4146	do		Sept. 4, 1956	1.9±1.2
4297	do		Oct. 3, 1956	4.6±0.7
4790	do		Nov. 19, 1956	4.2±0.6
3637	Bonito		May 26, 1956	5.3±2.3
3638	do		do	8.5±3.7
3660	do		June 2, 1956	2.2±1.4
3761	do		July 3, 1956	3.7±1.3
3824	do		Aug. 6, 1956	0.77±0.50
4147	do		Sept. 4, 1956	2.4±0.93
4298	do		Oct. 3, 1956	1.2±0.7
4791	do		Nov. 19, 1956	≤0.7
3606	Yellowfin	Northeast Pacific, Gulf of Tehauntepec.	Apr. 20, 1956	3.5±1.2
3606	do	Region of Marquesas Islands	do	≤1.5
3608	Albacore	Western Pacific	do	5.9±2.4
3721	do	do	June 19, 1956	2.5±0.8
3722	Yellowfin	Caught off Cape San Lucas	do	1.8±1.1
4055	do	Lower California, eastern Pacific	Aug. 1, 1956	≤1.7
4056	Albacore	Western Pacific	do	3.8±1.5
4152	Tuna	Cocos Island, Costa Rica, Nicaragua	Sept. 7, 1956	1.4±0.95
4153	do	Western Pacific	do	2.2±0.84
4792	do	Eastern Pacific	Nov. 19, 1956	5.2±0.7
4793	do	Western Pacific	do	4.0±0.8

[1] Wet refers to weight as received.

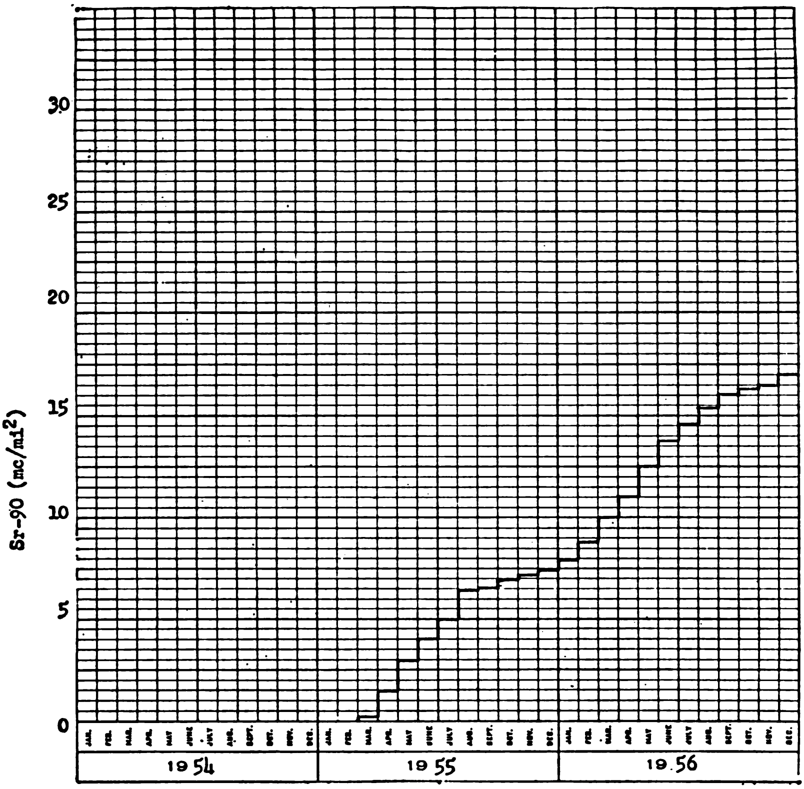


FIGURE 7.—Cumulative Sr–90 from rainfall collections at Pittsburgh.

BEAN EXPERIMENT

During the summer of 1956, three plants, snap beans, lima beans, and black-eyed peas were grown at the Beltsville Laboratory of the United States Department of Agriculture. The leaf, stalk, pod, and fruit of each were analyzed separately and the snap bean and black-eyed pea samples were run in duplicate. The results on the plants are given in table 13, while the soil analysis is shown in table 14.

Normalizing the data to stalk=1, the mean strontium unit values for leaf, fruit, and pod become 0.85, 0.52, and 0.54, respectively. The mean Sr–89/Sr–90 ratios are 1.9, 0.2, 0.7, and 1.4 for the leaf, fruit, stalk, and pod. While such averaging may not be completely justified, it would lead to the following conclusions:

1. The high stalk activity may indicate uptake that is partially blocked from the rest of the plant.
2. The leaves show higher strontium unit values than the pod and fruit, indicating some leaf retention.
3. The Sr–89 values indicate that the leaf and pod activity is younger than the fruit activity.

It is also of interest to note that the Sr–89 values indicate that some of the topsoil activity is younger than the bottom fraction.

TABLE 13.—*Radiostrontium and calcium in selected tissues of bean and black-eyed pea plants*

Plant	Section	Percent ash of dry weight	Percent Ca in ash	Average percent Ca in ash	D/m/g ash Sr-90	Sr units	Average SU	Sr-89/Sr-90 ratio
Snap bean:								
1st half bag [1]	Leaf	82.0	12.0 11.0	12.0	20.0 ±0.5 19.0 ± .5	76.0±1.7 80.0±1.8	78	3.0 2.9
	Bean	4.2	1.4 1.4	1.4	2.0 ± .18 1.6 ± .26	65.0±5.9 52.0±8.6	58	.87 .79
	Stalk	11.0	14.0 15.0	14.0	25.0 ± .5 26.0 ± .5	82.0±1.5 80.0±1.4	81	1.2 1.1
	Pod	9.2	9.3 8.0	8.6	11.0 ± .4 11.0 ± .6	53.0±1.7 61.0±3.1	57	1.6 1.4
2d half bag [2]	Leaf	52.0	3.8 4.2	4.0	3.9 ± .3 4.8 ± .3	45.0±4.0 50.0±3.0	48	2.0 1.4
	Bean	8.5	1.6 1.8	1.7	1.5 ± .3 1.2 ± .2	43.0±9.0 30.0±5.0	37	---------- .60
	Stalk	12.0	15.0 16.0	16.0	23.0 ± .6 22.0 ± .6	68.0±1.8 64.0±1.8	66	.39 .44
	Pod	10.0	7.9 7.8	7.9	9.3 ± .4 9.4 ± .4	55.0±2.0 55.0±2.0	55	.56 .38
Lima bean [3]	Leaf	27.0	15.0 14.0	15.0	11.5 ± .45 11.1 ± .41	35.0±1.4 36.0± .9	36	.99 .98
	Bean	4.2	1.5 1.4	1.5	.21± .19 .48± .23	6.4±5.9 15.0±7.3	11	----------
	Stalk	9.0	14.0 15.0	15.0	13.9 ± .46 12.7 ± .47	45.0±1.4 39.0±1.4	42	.20 .16
	Pod	9.1	7.7 8.1	7.9	.78± .33 .53± .37	4.5±1.8 3.0±2.3	3.8	----------
Black-eye pea:								
Bag 1 [4]	Leaf	16.0	22.0 23.0	23.0	16.0 ± .5 17.0 ± .5	33±0.9 34± .9	34	1.7 1.8
	Bean	3.4	1.7 2.0	1.9	.79± .25 1.2 ± .25	21±6.4 27±5.9	24	----------
	Stalk	8.3	8.4 7.9 7.9 7.7	8.0	6.3 ± .35 6.6 ± .35 6.0 ± .33 6.9 ± .35	34±1.8 38±1.8 35±1.8 41±2.3	37	.64 .37 .70 .82
	Pod	6.2	8.1 8.2	8.2	3.7 ± .27 4.2 ± .35	21±1.4 23±1.8	22	2.3 2.1
Bag 2 [5]	Leaf	17.0	21.0 19.0	20.0	13.0 ± .5 14.0 ± .5	28± .9 34±1.4	31	2.3 2.0
	Bean	3.3	2.0 2.0	2.0	.79± .21 .56± .19	18±5.0 13±4.5	16	----------
	Stalk	6.6	6.2 10.0	8.1	6.5 ± .4 6.7 ± .4	48±2.7 30±1.8	39	1.1 1.0
	Pod	5.8	7.2 7.3	7.3	3.1 ± .27 2.9 ± .31	20±1.8 18±1.8	19	2.9 2.8

[1] As of Oct. 15, 1956.
[2] As of Jan. 21, 1957.
[3] As of Jan. 24, 1957.
[4] As of Jan. 23, 1957.
[5] As of Jan. 22, 1957.

TABLE 14.—*Radiostrontium and calcium in soil from the bean and pea plant plots*

Depth	Gms. Ca/gm. soil	Sr-90 D/m/gm. soil	SU	Sr-90 mc./mi.^{2}	Sr-89/Sr-90	Sr-89 c-date
0-2 inches	89 x 10^{-6}	0.080±0.004	410±2.0	6.0±0.3	----------	----------
	62 x 10^{-6}	.083± .005	610±3.7	6.2± .3	----------	----------
	80 x 10^{-6}	.077± .004	440±2.3	5.8± .3	----------	----------
	----------	.071± .006	----------	5.3± .4	3.0	Dec. 27, 1956
	----------	.090± .008	----------	6.8± .6	2.3	Do.
2-6 inches	----------	.049± .003	----------	10 ± .6	.33	Jan. 18, 1957
	----------	.051± .003	----------	10 ± .6	.21	----------
	----------	.051± .003	----------	10 ± .6	----------	----------
	----------	.039± .003	----------	8.2± .6	----------	----------

LABORATORY PROGRAM

Several phases of Analytical Branch operations other than direct analyses are related to the strontium program. Progress on these projects is reported here for completeness.

Wisconsin milk experiment

Dr. Alexander collected samples during a milk drying run at Columbus, Wis., on November 13, 1956. Dried milk was taken at the start of the 16-hour run, and every hour to the end. Whole milk from the start and end and caustic washes from the cleanup were also taken.

Since processing consumes about 1 tank per hour and 1 tank represents the largest blend made, the test should indicate the variability that might be expected in Sr–90 and Cs–137 for a normal run. Since our normal sampling is a 5-pound can selected weekly, this variability will be extremely important.

Cs–137 analysis

Analysis of certain samples for Cs–137 will help in understanding parts of the strontium program, as well as having value in themselves.

The estimation of gamma dose from fallout will rely heavily on Cs–137 determinations for old debris. The gummed film is adequate for evaluating fresh activity, but analysis of pot samples for Cs-137 will assist in assigning an age to the material. The concentration of Cs–137 in foods and in human urine will be of direct interest.

The analytical methods are satisfactory for all types of sample except milk. There is considerable loss of cesium in the dry ashing process, even at 500° C. Still lower temperatures and a wet ashing process are being evaluated.

Laboratory manual

The methods for analysis of Sr–90, Sr–89, Cs–137, calcium, strontium, and mixed fission products are included in the Analytical Branch laboratory manual, which is being issued in looseleaf form as NYO–4700.

Standard samples

Standard samples are being prepared to assist laboratories starting work on strontium analysis. These are:

1. Milk ash
2. Bone ash (animal)
3. Bone ash (simulated human)
4. Bone ash (blank $Ca_3(PO_4)_2$)
5. Vegetation ash (hay)
6. Mixed fission products (for separation studies)
7. Soil
8. Standard Sr–90—Y–90 solution in ampoules (100 d/m)

Samples 1, 3, and 4 are being analyzed at 3 or 4 laboratories. Samples 2, 6, and 8 are in preparation. These samples will be forwarded to any laboratory, on request, with a statement of Sr–90 and calcium content. From 200 to 500 bottles of each sample will be available, each one adequate for duplicate or triplicate analyses.

Soil comparison

Dr. Alexander, of the Department of Agriculture, is preparing large samples of soil for determination of Sr–90 and calcium by several methods. These will assist in evaluation and selection of the best available methods of analysis. Previous comparisons have been limited in scope, but have shown large deviations between methods, and one broad experiment is now required.

Intercomparison report

A summary of all laboratory intercomparisons on all samples run to date has been completed, and will be issued as an NYO report. Agreement between laboratories is generally good, but there are enough major variations to warn against placing reliance on single results.

Control milk

The Analytical Branch has been running an analysis monthly on large batches of dried milk. The first lot, run from June of 1954 through April of 1955, showed values of 0.91 ± 0.11 $\mu\mu c/g$ Ca for 10 analyses. The second lot, run from August

of 1956 through December of 1956, showed values of 4.6 ± 0.7 $\mu\mu c/g$ Ca for 16 analyses. Although there are insufficient data for a complete statistical analysis, the results indicate some deviations greater than expected from counting error. Future control samples will be ashed and blended before use to remove the variation between cans in the same lot.

Representative HOLIFIELD. Before we proceed with our next witness, I have several statements I would like to insert in the record. The first is by Dr. W. F. Libby, a Commissioner of the Atomic Energy Commission, then a statement and a report by Dr. E. A. Martell, of the Air Force Cambridge Research Center, and finally a statement from the Naval Research Laboratory.

(The statements referred to follow:)

STATEMENT BY W. F. LIBBY

TOPIC IX

I would like to submit seven charts for the record.

Chart 1 shows "collectioln pot data for Pittsburgh." This is sometimes referred to as rainfall data. A washtub or similar container is placed at a carefully selected site and the material which falls into it (dust, soot, rain, etc.) is collected periodically and analyzed for strontium 90. It is a differential method since it gives the amount of strontium 90 that has been deposited in the interval between collections.

The curve on the chart is a cumulative one, since the strontium 90 reported for each sample collected from the tub is added to the total of all the previous reports. Thus, a steadily rising curve is obtained, which at any time gives the total of strontium 90 that has been collected at that site.

At the bottom of the chart are indicated the announced dates of detonations by the United States, United Kingdom and U.S.S.R. Dots indicate whether the detonations were air burst or surface burst, if that information is available.

Chart 2 is similar to chart 1, but the data are those for New York City. In the case of the New York data, we have, on occasion, obtained and analyzed samples of soil and found them in good agreement with the strontium 90 levels estimated from this curve.

Chart 3 is another way of presenting collection pot data. In this chart we have plotted each month's accumulation individually without adding it to the previous month, so that on this plot the individual points represent the monthly rate of fallout. We have included the first few results from the worldwide network of collection pot stations which we are in the process of establishing. In general, there has been a station in the gummed film network for some years at these same sites.

Chart 4 is a plot of the strontium 90 content of some soil samples taken in Asia Minor in 1955 from the annual rainfall at those same sites. This chart shows a very interesting relationship between the amount of rain and the amount of fallout.

Chart 5 is a spot map of the United States in which we have indicated the strontium 90 content of the soil in October 1956. These soil samples were obtained and analyzed in a cooperative venture of the AEC Health and Safety Laboratory and the United States Weather Bureau.

Chart 6 is similar to chart 5; however, we have used the collection pot data of charts 1 and 2 to estimate the increment of fallout between October 1956 and April 1957. Thus, we have been able to estimate the probable soil values at these same stations for April 1957.

Chart 7 is similar to chart 1. The data are those for Chicago for the years 1953–55 and are the data collected while the method was being worked out and checked. Note the agreement between the two soil samples and the rainfall curves.

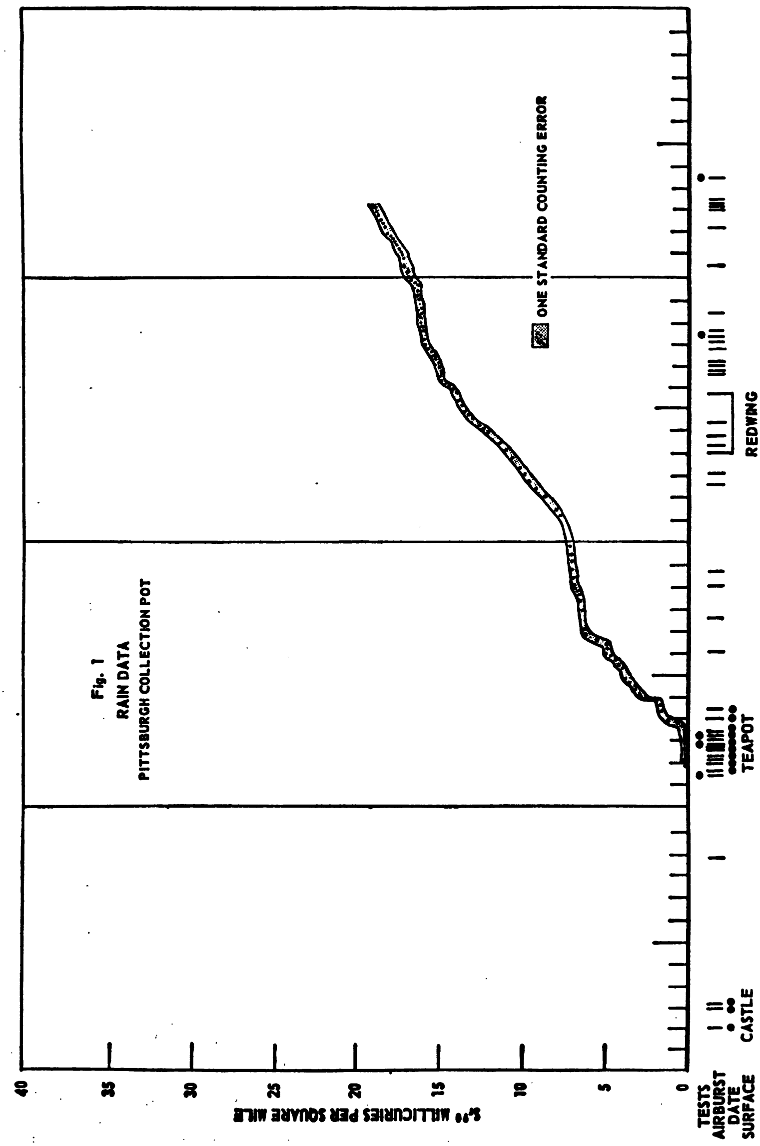


Fig. 1
RAIN DATA
PITTSBURGH COLLECTION POT

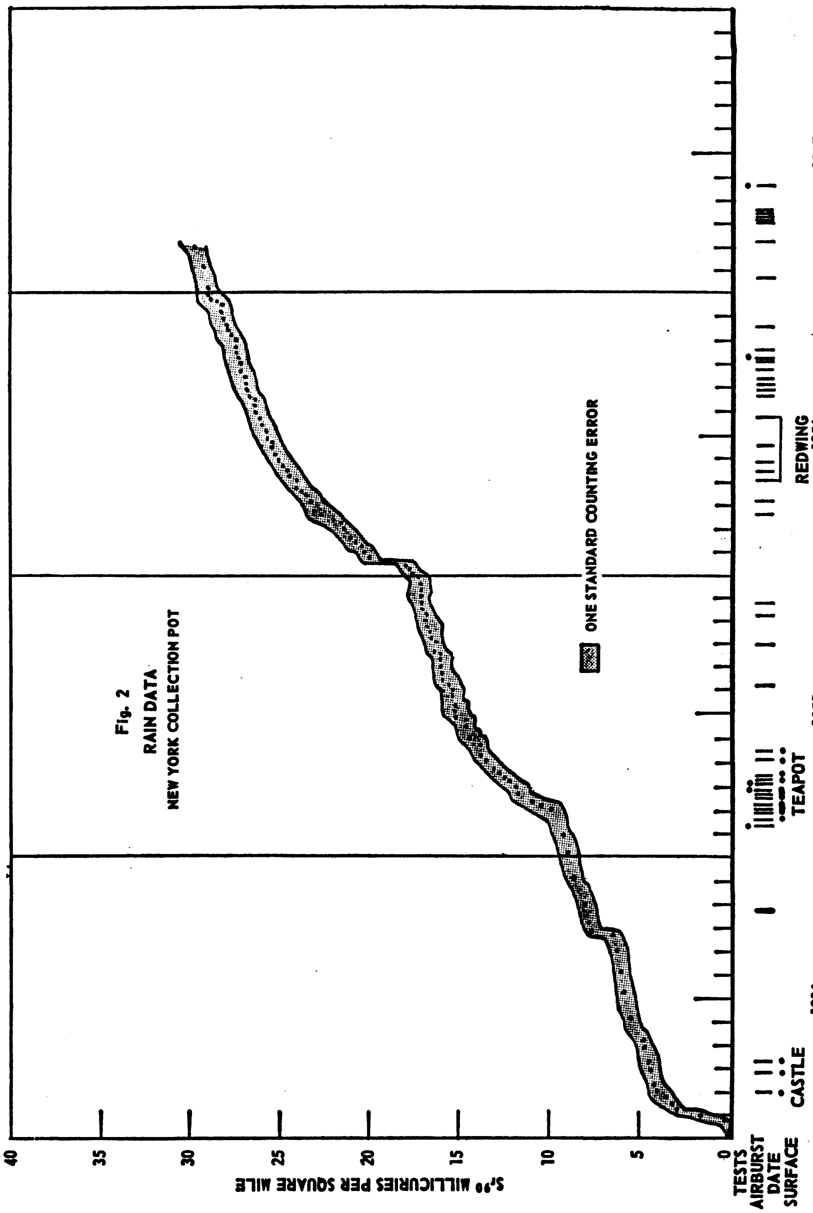


Fig. 2
RAIN DATA
NEW YORK COLLECTION POT
ONE STANDARD COUNTING ERROR
S_r^{90} MILLICURIES PER SQUARE MILE
0
5
10
15
20
25
30
35
40
TESTS
AIRBURST
DATE
SURFACE
CASTLE
1954
TEAPOT
1955
REDWING
1956
1957

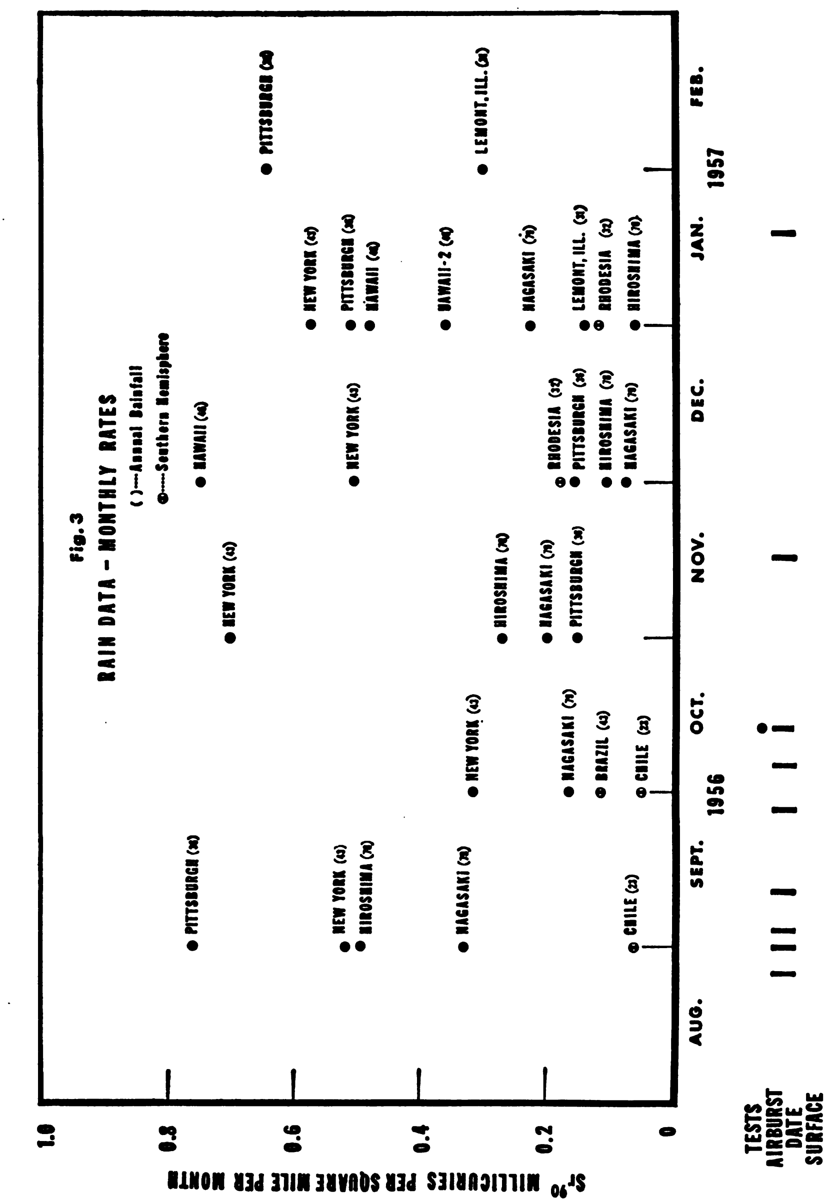


Fig. 3
RAIN DATA - MONTHLY RATES
( )—Annual Rainfall
⊖----Southern Hemisphere
Sr90 MILLICURIES PER SQUARE MILE PER MONTH
0
0.2
0.4
0.6
0.8
1.0
AUG.
SEPT.
OCT.
NOV.
DEC.
JAN.
FEB.
1956
1957
PITTSBURGH
NEW YORK
HIROSHIMA
NAGASAKI
CHILE
BRAZIL
HAWAII
RHODESIA
HAWAII-2
LEMONT, ILL.
TESTS
AIRBURST
DATE
SURFACE

Fig. 4

CORRELATION BETWEEN RAINFALL AND S_r^{90} IN SOIL

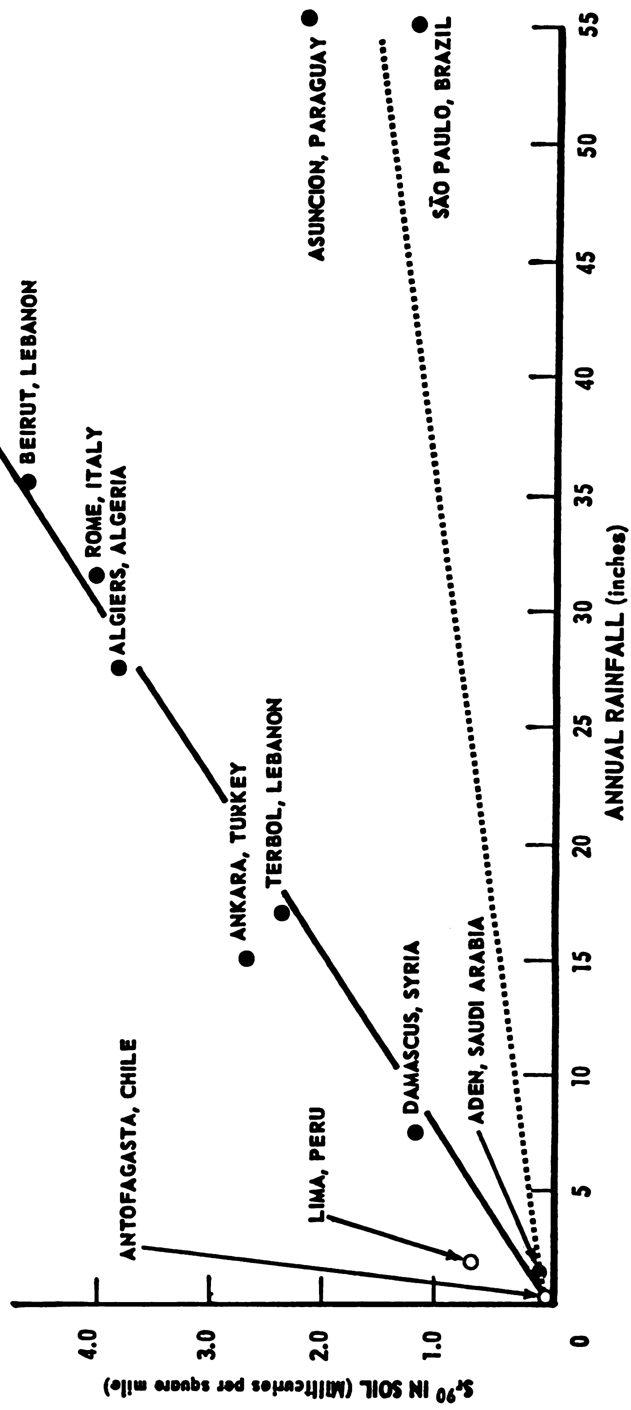
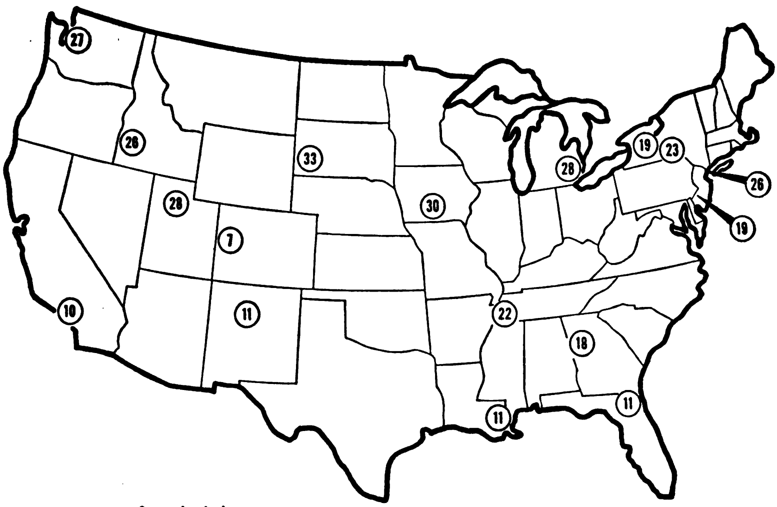


Fig. 5

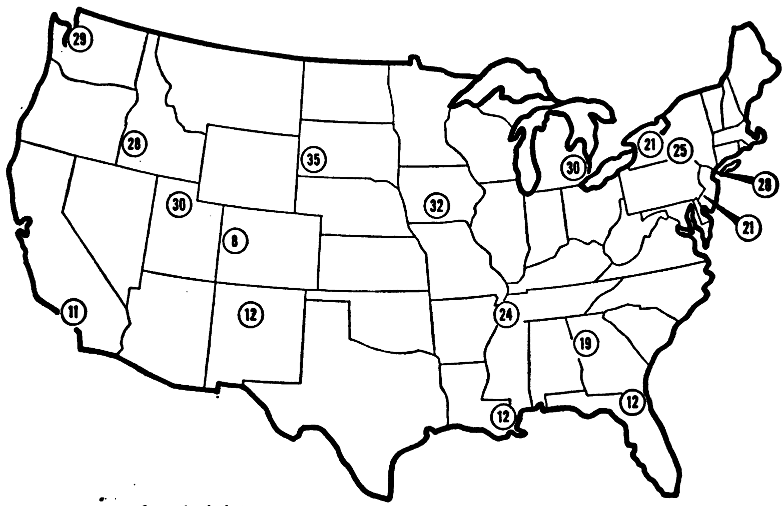
Sr^{90} IN U. S. SOIL (HASL - OCT. 8, 1956) (HCl EXTRACTION METHOD)



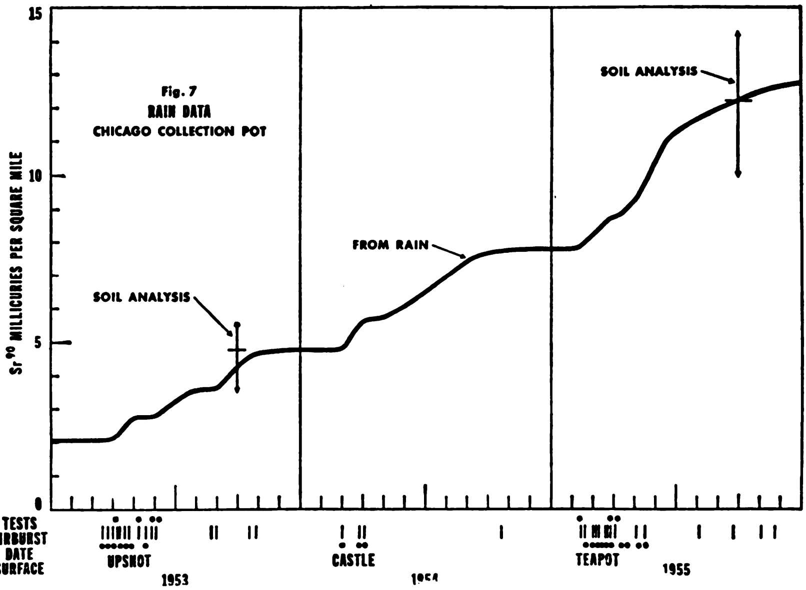
Numbers are in mc/mi² at individual site.

Fig. 6

ESTIMATED Sr^{90} IN U. S. SOIL AS OF APRIL 1957



Numbers are in mc/mi² at individual site.



STATEMENT OF DR. E. A. MARTELL

(Ph. D. radiochemistry, University of Chicago 1950; program director, radiation effects and fallout studies, Armed Forces special weapons project, 1950 to 1954; Chicago Sunshine project director, 1954 to 1956; Chief, Atmospheric Nuclear Chemistry Section, Special Projects Laboratory, Geophysics Research Directorate, Air Force Cambridge Research Center, Bedford, Mass., August 1956 to present)

I-A. REPORTS ON STRONTIUM 90 FALLOUT DATA

1. E. A. Martell, "Project Sunshine Bulletin Number 11," University of Chicago, December 1, 1955, SECRET; "Project Sunshine Bulletin Number 11—Strontium 90 Concentration Data for Biological Materials, Soils, Waters and Air Filters," AECD-3763, January 1957.
2. E. A. Martell, "The Chicago Sunshine Method—Absolute Assay of Strontium 90 in Biological Materials, Soils, Waters and Air Filters," University of Chicago, May 1956.
3. E. A. Martell, "Project Sunshine Bulletin Number 12," University of Chicago, August 1, 1955; "Project Sunshine Bulletin Number 12—Strontium 90 Concentration Data for Biological Materials, Solids, Waters and Air Filters," AECU-3387, January 1957.

I-B. UNPUBLISHED, UNCLASSIFIED FALLOUT DATA

Analysis of strontium 90 in Antarctica snow cores, collected early in 1956, are in progress. Results should assist in establishing deposition rate and total deposit of delayed fallout in Antarctica. Partial results can be submitted to the Joint Committee on Atomic Energy by letter report upon request.

II. INTERPRETATION OF GEOPHYSICAL ASPECTS OF DELAYED FALLOUT

Present stratospheric burden of strontium 90 and other long-lived fission products can be only crudely estimated in view of the large uncertainties in the amounts introduced and in the rate of removal. However, Dr. W. F. Libby's estimates of 2.4 megacuries of strontium 90 as the total stratospheric burden in June 1954 and again in June 1956 are undoubtedly good approximations.

The rate and extent of stratospheric mixing are unknown. Thus the present and subsequent vertical and lateral distribution of stratospheric fission product debris are uncertain. The United States Atomic Energy Commission balloon sampling program which has been in progress over the last half year should provide information on the distribution.

The mechanism of transport of stratospheric debris into the lower atmosphere is not clearly understood. Thus the path and rate of deposition of delayed fallout cannot be described with confidence at the present time. Proposed mechanisms include slow worldwide mixing through the tropopause and, alternatively, entrainment of stratospheric debris into the jet stream. Differences in tropopause definition and jet stream activity for the Northern and Southern Hemispheres may lead to differences in consequent ground distribution in the two hemispheres. If jet stream entrainment were the dominant mechanism in the Northern Hemisphere it would result in higher deposition in the middle latitudes, a result not inconsistent with observed ground levels.

Rainfall scavenging is clearly the dominant mechanism which controls the deposition of delayed fallout once it has mixed into the lower atmosphere. The soil level of delayed fallout should correlate reasonably well with total rainfall within any limited geographical area for which the seasonal pattern of rainfall is similar.

Southern Hemisphere fallout is substantially all delayed fallout. The limited data for South American soils and Antarctica snow cores indicate roughly uniform fallout over the Southern Hemisphere, with the distribution modified principally by the rainfall pattern.

Interpretation of Northern Hemisphere data is complicated by the contribution of fallout from the numerous small atomic weapons tests. Highest observed levels of fall out are reported for the Northern United States, with about 30 millicuries of strontium 90 per square mile in the fall of 1956. It is difficult to attribute more than a minor proportion of this amount to fallout from small weapons tests. The Northern United States may be receiving a substantially larger amount of delayed fallout than the average world deposition.

An important gap in the information on the worldwide distribution of strontium 90 is the fallout over the oceans which comprise some three-fourths of the earth area. It is not possible to anticipate whether the oceans are receiving more or less than their proportionate share of delayed fallout.

In conclusion, the large uncertainties in the atmospheric circulation and mixing history for delayed fallout, coupled with uncertain contribution of small weapons tests in the middle latitudes for the Northern Hemisphere, limit the reliability of any statement of the fraction and the distribution of delayed fallout which has already come down. Similarly, no reliable estimate of future ground levels can be made. At best the delayed fallout will be distributed roughly uniformly over the world, with the distribution modified by the rainfall pattern. It is also possible that the Northern Hemisphere is receiving a disproportionately large share, with the highest levels in the high rainfall areas of the middle latitudes.

III. COMMENTS ON PROCEDURE FOR SAMPLING DELAYED FALLOUT

Upper air sampling devices currently in use are satisfactory for synoptic evaluation of the distribution of stratospheric debris. However, in view of the unknown size distribution of the submicron particulates in the stratosphere and the inherent inefficiency of upper air sampling devices, the current techniques are inadequate to assess the total concentration at any point in space and thus will not allow reliable assessment of the total stratospheric burden. New upper air sampling systems currently under development should overcome these limitations.

Ground collection and analysis techniques for the indirect assessment of delayed fallout are complicated by the experimental difficulties in the determination of fallout over the large ocean areas and in the identification of delayed fallout in the presence of fallout from summary weapons tests.

Project Sunshine Bulletin, No. 12[1]: Strontium 90 Concentration Data for Biological Materials, Soils, Waters and Air Filters

E. A. Martell, August 1, 1956 (revised January 1957)—The Enrico Fermi Institute for Nuclear Studies, the University of Chicago, Chicago, Ill.

INTRODUCTION

In this revised edition of Chicago Sunshine Bulletin No. 12 are presented all Sr–90 concentration data obtained by the University of Chicago Project Sunshine

[1] This work was supported by the Division of Biology and Medicine, U. S. Atomic Energy Commission, under contract AT (11–1)–281.

group for the period December 1, 1955, to termination of the work in August 1956. For cumulative results to December 1, 1955, see Chicago Sunshine Bulletin No. 11.[2] Results for samples assayed by the Nuclear Science and Engineering Corp., Pittsburgh, Pa., under subcontract, are included and are designated by the letter "P" following the Chicago Laboratory number (i. e., CL xxx–P).

This revised report includes all results and discussion contained in the original version of Chicago Sunshine Bulletin No. 12, August 1, 1956, plus additional results submitted to the Division of Biology and Medicine, United States Atomic Energy Commission, in a letter report, Chicago Sunshine Results, Final List, dated August 3, 1956. Data of the last two columns, table 6, pages 27 and 28, have been corrected to take into account the difference in ratio of available Sr–90 to available calcium in the electrodialyzed sample as compared with the original soil. The correction is substantial for calcareous soils. A new result is reported for the Brawley, California soil, CL 1127 (p. 44, this report), establishing a lower maximum level of Sr–90 for this arid area.

The data for all biological samples and soils are presented in units of one one-thousandth of the occupational tolerance dose of Sr–90 for an average man of 1,000 grams total body content of calcium. Thus the "sunshine unit" equals one one-thousandth microcuries of Sr–90 per kilogram of calcium or 2.2 disintegrations per minute of Sr–90 per gram of calcium.

Water and air concentration data are reported in disintegrations per minute of Sr–90 per unit volume.

A description of the chemical procedures and the absolute counting method employed by the Chicago Sunshine project group has been reported.[3]

A considerable portion of the data presented in this report has been discussed by W. F. Libby.[4 5] Some of the data of Bulletins 11 and 12 are summarized in graphical or tabular form and are briefly discussed in the following section of this report.

DISCUSSION OF RESULTS

I. Sr–90 surface air concentration data

Large air blower samples, collected over the last several years at the Naval Research Laboratory, Washington, D. C., by I. H. Blifford and associates, were made available to us for Sr–90 assay. Collections were made on Army Chemical Corps type V filters of 200 square inches area and of heavy asbestos fiber composition. Collection volumes ranged from about 1 to 5 million cubic feet of air for collection periods of 1 day to 1 week. The large blower samples which were analyzed from Sr–90 were collected from four locations: Washington, D. C.; Kodiak, Alaska; Port Lyautey, French Morocco; and Yokosuka, Japan. A summary of the Sr–90 concentration data, together with the location, collection period, and sample volume for each sample, is presented in the last section of this report.

The Washington, D. C., Sr–90 air-concentration data are presented in figure 1. For these samples, the volumes were computed from recorded flow rate data. Figure 2 shows the variation in total flow with length of the collection due to dust loading of filters at the Washington, D. C., station. This curve was obtained from Mr. Blifford at the Naval Research Laboratory, who indicated quite large variations of individual collection volumes from the average values shown.

The data for the three foreign stations are presented in figure 3. For these, the collection volumes were not monitored and the volume of each sample was estimated by assuming the effect of dust loading observed at Washington, D. C. (fig. 2), applied equally well at these other locations. The necessity of making this assumption imposes a restriction on comparison of relative air concentrations for the four locations but does allow us to consider the change in air concentration with time at each location.

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[2] E. A. Martell, Project Sunshine Bulletin No. 11, Dec. 1, 1955, secret; AECD–3763, January 1957.

[3] E. A. Martell, The Chicago Sunshine Method; Absolute Assay of Sr–90 in Biological Materials, Soils, Waters, and Air Filters, AECU–3262, May 1956.

[4] W. F. Libby, Radioactive Strontium Fallout, Proceedings National Academy of Science, 42, 365 (1956).

[5] W. F. Libby, Current Research Findings on Radioactive Fallout, speech before AAAS, Washington, D. C., October 12, 1956.

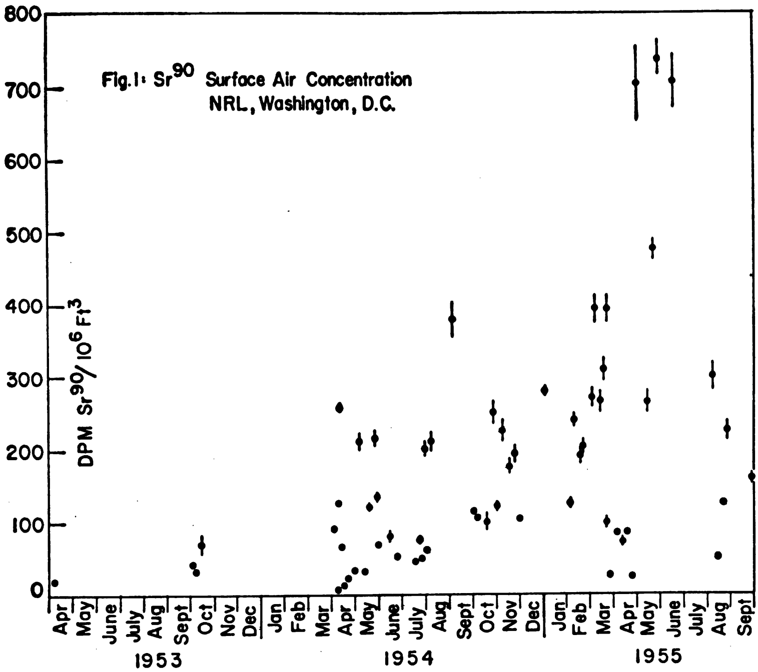


Fig.1: Sr^{90} Surface Air Concentration
NRL, Washington, D.C.
DPM $Sr^{90}/10^6\ Ft^3$
0
100
200
300
400
500
600
700
800
Apr May June July Aug Sept Oct Nov Dec Jan Feb Mar Apr May June July Aug Sept Oct Nov Dec Jan Feb Mar Apr May June July Aug Sept
1953
1954
1955

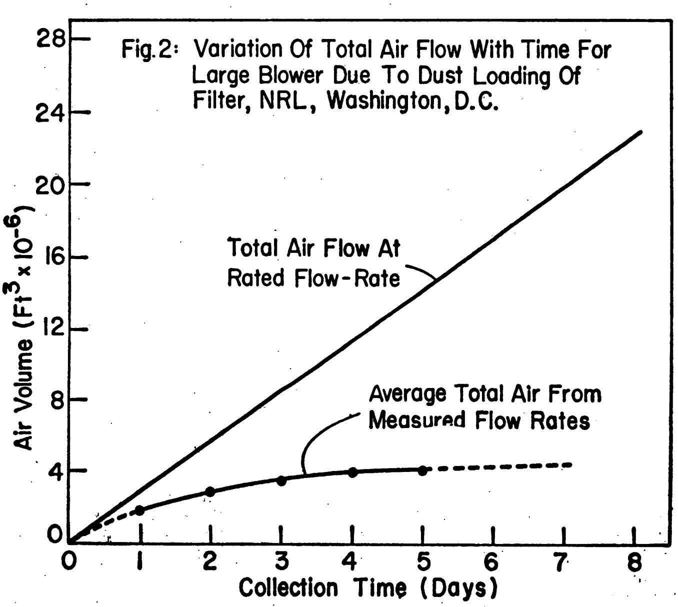
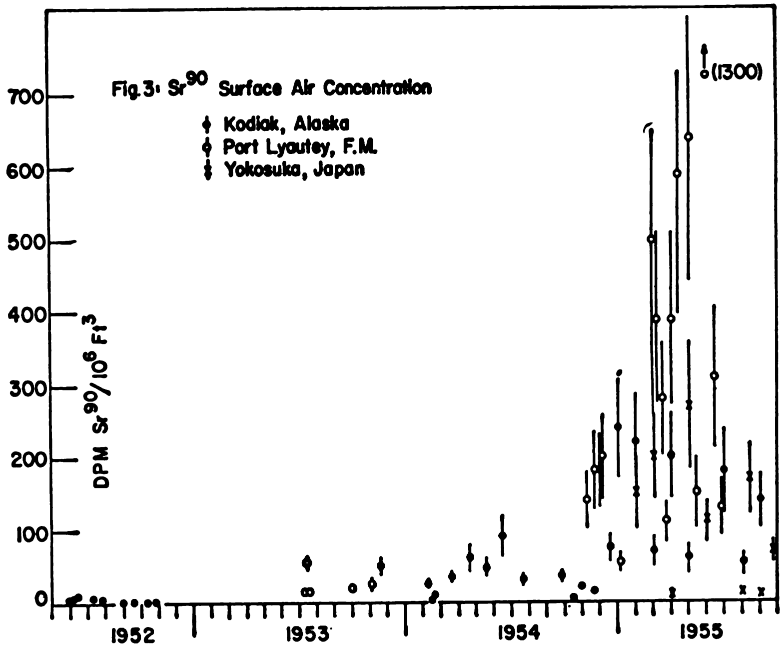


Fig.2: Variation Of Total Air Flow With Time For Large Blower Due To Dust Loading Of Filter, NRL, Washington, D.C.
Total Air Flow At Rated Flow-Rate
Average Total Air From Measured Flow Rates
Air Volume ($Ft^3 \times 10^{-6}$)
Collection Time (Days)
0
4
8
12
16
20
24
28
0 1 2 3 4 5 6 7 8



The data for all four locations show generally the same concentration levels at any given time and all show a gradual and substantial increase during 1954. The concentrations observed at Kodiak, Alaska, during and following the spring 1952 tests are strikingly low by comparison with the 1954 and 1955 concentration data. The average Sr-90 activity in 10^6 cubic feet of surface air appears to have increased from a pre-Ivy test level of about 4 dpm. to a pre-Castle level of some 40 dpm. and a post-Castle level of some 200 dpm.

Comparison of the Sr-90 air concentration data with the daily total fission product beta activity data obtained by the NRL group indicates "apparent" ages for the mixed fission products of from 1 month to several years with wide variations during any given month. No conclusions with respect to the Sr-90 production date may be drawn from such data for a number of reasons. The size distribution of the original bomb debris is dependent on the energy yield, orientation, and environment of the weapon and for each case the mean size of particulates carrying Sr-90 is expected to be smaller than that of mixed fission products. Furthermore, the particulates may be further fractionated by the action of rains, depending on the efficiency of scavenging by rains as a function of particle size. Another complication is that imposed by the size-collection efficiency of the device itself. Additional difficulties are imposed by the close spacing of tests during the last several years. Finally, there are large day-to-day variations in surface concentration of Sr-90 and other fission products, apparently due to scavenging of surface air by vegetation and to the time interval between rains.

In spite of the many complicating factors indicated above which limit the meaningfulness of any individual measurement of Sr-90 concentration and its relation to total fission product activity or to other individual fission product concentration, the general features of the Sr-90 surface air concentration data shed considerable light on atmospheric circulation and storage of long lived fission products. The Sr-90 air concentration history shows a marked general increase following the Ivy and Castle tests and thus appears to directly relate to the increased stratospheric storage following these tests. The general equiv-

alence of the concentrations observed for the four stations, widely separated in geographical location, is strong indication that relatively old debris rather than fresh fission product activity is involved. Except for a few of the highest concentration values observed, the Sr–90 data do not reflect any considerable contribution from individual small-weapons tests for which tropospheric washout rates of several weeks or less have been estimated.

An air filter device, horizontally oriented near the ground surface in general, will not collect large particles which fall directly or particles trapped in raindrops. Instead, it will collect a portion of those particles which mix downward between rains and persist in surface air plus some of the particles carried in the air downdraft associated with rains for which the scavenging efficiency of rains is low. Thus, the Sr–90 surface air concentration data do not necessarily relate to the total deposition rate in any direct manner.

The Sr–90 concentrations observed in surface air during the fall 1954 and spring 1955 are an order of magnitude lower than the limited United States and British measurements of upper troposphere concentration during the same period. The numbers are not necessarily inconsistent when consideration is given to dilution by cleaner surface air during downward mixing, to reduction resulting from scavenging by rains, and to removal from surface air by the filtration action of vegetation foliage and the action of fog and dew. The mean troposphere concentration of Sr–90 is undoubtedly a factor of 5 to 10 times higher than surface air values and thus corresponds to a total tropospheric air burden of 1 megaton of fission or so. These considerations provide additional basis for the argument that the Sr–90 surface air concentration data relate to stratospheric debris since the total fission yield represented by this source will generally mask the Sr–90 produced in tests of small atomic weapons. The low concentrations observed at Kodiak during June and July 1952 provide the most convincing argument that Nevada tests, and thus small weapons tests generally, contribute negligibly to the Sr–90 concentration observed on surface air filters.

Thus, it appears that the measurement of Sr–90 on surface air filters provides a direct measure of the stratospheric burden and that the apparent seasonal variation in the Sr–90 surface air concentration may relate to seasonal variation in mixing through the tropopause and/or tropospheric washout rate.

II. Sr–90 in Chicago rains

A. *Summary of Chicago rain data, 1953 through 1955.*—Table 1 shows the estimated monthly and yearly totals of Sr–90 deposited per square foot in the Chicago area, together with totals for rains actually measured.

The 1953 rain samples were roof runoff samples collected by the Chicago Tritium research group. For these, it is assumed that the result in disintegrations per minute was constant for each rain, and the Sr–90 deposited per square foot was computed from the total precipitation in inches reported by the Weather Bureau for the University of Chicago station.

The 1954 samples were individual rains collected in a galvanized washtub on the roof of the Jones chemistry laboratory building. For these, the total sample activity divided by the collection area was taken as the Sr–90 deposited per square foot for individual rains.

For both 1953 and 1954 rains, the total monthly deposit was estimated by multiplying the deposit in disintegrations per minute per square foot per inch of rain for the rains measured by the total monthly precipitation in inches.

Table 2 shows the total monthly deposits for 1955 rains. At all three locations the 1955 collections were made in galvanized tubs, and the total monthly precipitation was taken. For these, the deposit per square foot was computed from total sample activity divided by collection area. The 1955 collections were made with the tubs exposed continuously, and thus any dry material falling out between rains was included in the sample.

TABLE 1.—*Sr^{90} deposited by 1953 and 1954 Chicago rains*

	1953 Chicago rains		1954 Chicago rains	
	Inches of rain	DPM Sr-90/ft.[1]	Inches of rain	DPM Sr-90/ft.[1]
January	1.20 (0)		1.01 (0)	
February	1.45 (0.22)	0.99 (0.15)	2.17 (0)	
March	3.59 (1.72)	7.15 (3.42)	4.44 (2.49)	64.0 (35.8)
April [2]	2.83 (1.59)	47.8 (42.3)	4.53 (1.85)	15.7 (6.4)
May	2.04 (0.75)	6.0 (2.2)	2.25 (0.80)	21.3 (7.6)
June	4.49 (1.87)	37.1 (15.5)	2.73 (1.55)	27.0 (15.4)
July	3.95 (1.96)	22.4 (11.1)	6.37 (3.89)	18.7 (11.4)
August [3]	1.32 (0.88)	2.16 (1.44)	6.12 (0.60)	45.3 (4.43)
September	2.17 (0.78)	39.0 (13.7)	0.98 (0)	
October	1.58 (0.45)	45.2 (12.9)	11.69 (6.5)	21.2 (11.8)
November	1.53 (0.39)	2.0 (0.51)	1.37 (0.13)	7.4 (0.70)
December	2.44 (0)		2.05 (1.80)	27.0 (23.8)
Total		>210		>250

[1] Values in parentheses are totals for measured rains.
[2] On April 1953 rain of 0.94 inch deposited 39.4 dpm. Sr-90/ft.[2]. This rain was omitted in the estimation of the contribution of April rains which were not collected.
[3] The estimates for August and November 1954 are based on a very low fraction of the monthly precipitation, with the result that the estimates are probably high for August and low for November. Such errors should approximately average out over the year.

TABLE 2.—*DPM $Sr^{90}/Ft.^2$ deposited by 1955 rains and snows*

Month	Chicago	Pittsburgh	Washington, D. C.
January	6.6 (1.20 inches)		
February	[1]18.8 (1.61 inches)		30.0 (1.74 inches)
March	44.9 (2.17 inches)	25.2 (3.40 inches)	58.5 (3.49 inches)
April	30.7 (2.42 inches)	88.6 (4.84 inches)	75.3 (2.33 inches)
May	105.0 (2.66 inches)	120.1 (1.82 inches)	
June	[1]67.8 (2.77 inches)	80.3 (2.82 inches)	
July	31.6 (2.63 inches)	78.6 (2.35 inches)	
August	13.0 (6.60 inches)	133.0 (6.95 inches)	
September	10.8 (1.57 inches)	8.3 (1.84 inches)	
October	27.5 (6.12 inches)	31.7 (3.27 inches)	
November	18.2 (1.77 inches)	15.6 (2.79 inch)	
December	5.5 (0.47 inch)	7.9	

[1] Based on assay of ¾ of total monthly rainfall.

B. *Test of rain-collection method.*—If the collection of precipitation is to be used as a method of following the deposition of fallout Sr-90, it would be highly desirable to increase the collection period to about 1 month. During a long period of collection, the collector would be alternately dry and wet. Under these circumstances, it may be considered that a significant amount of fallout would be blown out of the collector during dry periods between rains or accumulated in excess when the collector contained water.

As a test of these possibilities, a number of collections made at Chicago during March, April, and May 1955 included, in addition to the standard open tub, a second tub covered with a plastic sheet with a small center hole, and a third tub with the water level maintained at an inch or more by periodically adding water.

Results for the "covered" tub and "wet" tub collections have been reported in Chicago Bulletin No. 11 and in this report. In the case of the "wet" tub, the volume reported is that obtained from the collector area and depth of precipitation reported by the Weather Bureau for the University of Chicago station.

The total Sr-90 activity collected by each of these methods can be directly compared since the collection areas are equal. The data are as follows:

1. Period 1000, Mar. 16, 1955, to 0915, Mar. 21, 1955:

CL 459–P	Open tub	29.0±2.0 dpm total.
CL 460–P	Covered tub	31.8±2.2 dpm total.

2. Period 1530, Mar. 21, 1955, to 1000, Apr. 4, 1955:

CL 462 and 466	Open tub	45.1±4.0 dpm total.
CL 477	"Wet" tub	35.2±2.2 dpm total.

3. Period 0950, Apr. 4, 1955 to 1400, Apr. 14, 1955:

CL 551–P	Open tub	29.0±1.9 dpm total.
CL 552–P	Covered tub	34.3±2.4 dpm total.

4. Period 2100, Apr. 14, 1955, to 1630, Apr. 20, 1955:

CL 562–P	Open tub	53.5±3.3 dpm total.
CL 563–P	Covered tub	34.4±1.8 dpm total.

5. Period 1800, Apr. 28, 1955, to 0930, May 13, 1955:

CL 617–P	Open tub	67±7 dpm total.
CL 618–P	Covered tub	83±9 dpm total.
CL 619–P	"Wet" tub	72.8±4.9 dpm total.

6. Period 1100, May 13, 1955, to 1130, May 23, 1955:

CL 627–P	Open tub	90±5 dpm total.
CL 628–P	Covered tub	70±5 dpm total.

7. Period 1700, May 23, 1955, to 1030, June 6, 1955:

CL 648	Open tub	210±8 dpm total.
CL 645	Covered tub	239±12 dpm total.
CL 646	"Wet" tub	221±8 dpm total.

The agreement in total Sr^{90} activity collected by each of the several methods is gratifying. The only significant discrepancy is the low value for CL 563–P which might be explained by the occurrence of a very heavy rainfall with a possible resultant loss of sample due to splashing off the plastic cover. Another possibility is the occurrence of dry fallout which could have been blown off the plastic cover by surface winds. It appears that the Sr^{90} is water soluble at the time of collection and is not absorbed on the walls of the vessels. It is further indicate that, once deposited in rains, the Sr^{90} fallout does not blow around significantly.

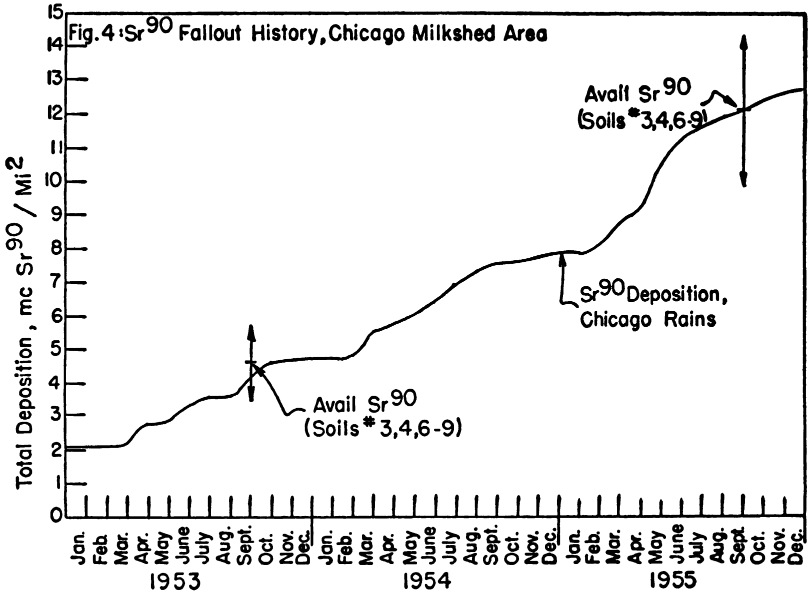
These results testify to the adequacy of the water-collection and chemical-separation procedures used and indicate that an open tub provides for reliable collection of precipitation for extended periods of time. Since the Sr activity is apparently quite soluble, large volume collections can be safely aliquotted to reduce the sample shipment problem. Thus, the collection of precipitation appears to be a practicable method to augment, or even replace, the sticky paper collector as a general method of worldwide monitoring.

C. *Solubility of Sr^{90} deposited in rains.*—The above indirect evidence for the solubility of the Sr^{90} activity in fallout is confirmed by the analysis and measurement of the insoluble residues from Chicago rains. The insoluble residues from two active Chicago rains, CL 320 and CL 407 and 408, initially separated by filtration, were fused with Na_2CO_3 and dissolved in HCl. Following a fuming nitric acid separation of strontium, the samples were reserved for yttrium 90 growth and milked for the Y^{90} activity. The results are shown in table 3 together with the Sr^{90} activity of the water soluble fraction.

TABLE 3.—*Solubility of Sr–90 in rains*

	CL 320	CL 407–8
1. Total Sr–90 activity in solution (dpm)	49.2±0.5	29.9±1.5
2. Total Sr–90 activity in insoluble residue (dpm)	≤0.6	0.7±0.1
3. Percent of total activity in insoluble residue	≤1.2	2.3±0.4

D. *Chicago fallout history from rain and soil data.*—The Sr^{90} data for Chicago rains and Chicago milkshed soils are summarized in figure 4 which gives the approximate total soil burden of Sr^{90} and its change with time over the 3-year period, January 1953 to December 1955. The curve is normalized to the average available Sr^{90} in 6 typical Chicago milkshed soils collected on September 29 and 30, 1955. The total monthly fallout in Chicago rains are taken from tables 1 and 2 above, and the method of measurement and data are discussed in the accompanying section. The Chicago milkshed soil data are discussed below.



The average available Sr^{90} observed for the same 6 soils samples in late September 1953 is also plotted. The agreement in this case may be partly fortuitous since the available Sr^{90} in the soils, that fraction extractable into normal neutral ammonium acetate, is somewhat less than the total.

From figure 3 it is clear that most of the fallout has been deposited since the beginning of 1953. The change in average annual slope indicates at least qualitatively the increasing annual contribution of stratospheric debris to total fallout. The general agreement between the change in soil level and cumulative fallout in rains during the intervening period is graphic indication that the scavenging of the atmosphere by rains is the primary mechanism of fallout. Sampling of rains with tubs continuously exposed will include collection of dry fallout of large particles which may be appreciable for considerable distances downwind of test areas during the first few days following an atomic test.

III. Results for 1955 Chicago milkshed samples

A. *1955 Chicago milkshed soils.*—Table 4 summarizes the results obtained for the Chicago milkshed soils collected September 29 and 30, 1955. The available calcium in grams per square foot was computed from Beltsville analyses of available calcium per unit weight of sample and total weight and area of sample collected. Strontium 90 analyses were made by the Chicago Sunshine Laboratory.

The samples were taken from the same fields of farms sampled in 1953 and again in 1954. Farms Nos. 3, 6, and 7 had been plowed recently. For each of these, 15 plugs, 3.5 inches in diameter, were taken to a depth slightly greater than apparent plow depth. On farm No. 7, two sets of samples were taken alternately spaced holes to provide a check on the reliability of sampling recently plowed fields. The agreement in the results of the duplicate samples is very striking.

The results in dpm per square foot apply to the available strontium 90 (i. e., extractable in normal neutral ammonium acetate) and not necessarily the total strontium 90 deposited per square foot. Although the Chicago rain data indicate that the strontium 90 is deposited in soluble form, it is recalled that 1953 soil samples fused with Na_2CO_3 following successive NH_4AC and HCl extraction showed appreciable residual strontium 90.

The average for the first 6 farms listed in table 4 is 970±180 dpm available strontium 90 per square foot, corresponding to about 12 millicuries per square mile for this area. The September 30, 1953, average for these same 6 soils was approximately 5 millicuries per square mile. The increase of 7 millicuries per square mile for the 2-year period is in surprisingly good agreement with the

total deposition of strontium 90 in Chicago rains over the same time interval (see fig. 4).

TABLE 4.—*Available Sr–90 in 1955 Chicago milkshed soils*

Beltsville number	Chicago laboratory number	Farm	Depth	Sunshine units	Available Ca g/ft²	Available Sr–90 dpm/ft² Layer	Total
			Inches				
1503	1019	Swanson (No. 3)[1] Winnebago County, Ill	0–8	6.83±0.08	82.80	1,240	1,240
1500	956	Holcomb (No. 4) Rock County, Wis	0–2	26.7± 1.0	15.00	881	1,150
1501	957	do	2–6	3.81±0.14	31.80	267	
1502	1018	Premo (No. 6)[1] Columbia County, Wis	0–6	15.0±0.5	25.50	843	843
		Kurpeski (No. 7)[1] McHenry County, Ill.:					
1496	886	Sample A	0–6.5	11.9±0.5	30.90	810	828
1497	887	Sample B	0–6.5	12.6±0.5	30.50	846	
1504	1020	Austin (No. 8) McHenry County, Ill	0–2	49.9±1.5	6.92	760	925
1505	1021	do	2–6	9.6±0.4	7.80	165	
1498	888	McKee (No. 9) McHenry County, Ill	0–2	9.8±0.4	31.20	673	844
1499	955	do	2–6	0.99±0.04	78.40	171	
1508	1022	Van Winkle (No. 11) Will County, Ill	0.2	65.1±2.6	4.40	630	742
1509	1023	do	2–6	10.0±0.4	5.10	112	
1510	1024	Carver (No. 12) Will County, Ill	0–2	64.5±1.3	3.70	525	698
1511	1025	do	2–6	14.0±0.7	5.60	173	

[1] Field recently plowed.

The soils for farms Nos. 11 and 12 are not typical for the Chicago milkshed area but are coarse sandy soils included in the selection because of their very low calcium level. These two soils show the lowest 1955 level and the lowest 2-year increase in available strontium 90. While it is possible that they received less fallout, it is more likely that something in the chemistry of these two soils acts to reduce the friction of "available" strontium 90. That these soils may be anomalous is further indicated by the unexpectedly low concentration of strontium 90 in the alfalfa grown on them (see table 5 and discussion on p. 22 below).

The strontium 90 measurements for the twelve Chicago milkshed soils collected in late September 1953 have been summarized and discussed elsewhere.[3] The 1953 soils show an average of about 50 percent of the total available strontium 90 in the top 1-inch layer for unplowed soils. The 1955 soils show about 80 percent of the total available strontium 90 in the top 2 inches of unplowed soils. Since this observation applies equally well to the soils showing the lowest total available strontium 90 (farms Nos. 11 and 12), it appears that the leaching of strontium 90 to greater depths by natural processes is very slow.

B. *1955 Chicago milkshed alfalfas.*—Results for the alfalfa samples are presented in table 5 together with the results for the soils from which they were collected. The soils are listed in order of decreasing concentration of available calcium in the 0 to 2 inch depth surface layer, as shown in column 2.

The soil levels of available strontium 90 in sunshine units are listed in the third column together with the depth of the soil sample assayed. The results for soil samples of 0 to 2-inch depth increase fairly regularly with decreasing calcium concentration. For the three recently plowed soils, the sunshine unit values of 0 to 2 inch depth are undoubtedly some 2 to 3 times the values given.

In controlled experiments, Menzel[4] has shown that the available strontium 90 to available calcium ratio in plants is about half the corresponding soil ratio. Under controlled field conditions, the relation of plant and soil level will be influenced by many complicating factors including possible leaf retention of strontium 90 fallout, variable root depth and soil moisture depth, the sharp gradient of strontium 90 concentration in the soil surface layer, and the application of fertilizers containing calcium. These factors undoubtedly account for considerable scatter in the results.

In the last column of table 5, the strontium 90 to calcium level in alfalfas are arbitrarily compared to the same ratio in the 0 to 2 inch layer of the soils on which they were grown. The results appear quite reasonable with the exception of those for McKee, Van Winkle, and Carver farms.

[4] Menzel, G. R., Soil Science 77, 419 (1954).

The high concentration for the McKee farm plant material may be explained by the fact that the sample taken was a mixture of bromegrass and ladino clover with shallower root depth than that of alfalfas and thus may relate to a higher soil level. Furthermore, the growth was short and sparse and was gleaned from several scattered patches apart from the point of soil sampling. Thus, differences in soil calcium or strontium 90 or a higher leaf retention effect may be involved. That this sample is not characteristic of the average McKee farm vegetation is indicated by the result of 0.51±0.03 S. U. for the bone of a McKee farm steer killed in September 1955 (see CW 813–P and compare with CL 1011 and 1012). The three 1955 Chicago milkshed animal bone samples show the lowest strontium 90 level for the farm with the highest soil calcium level and the highest bone level for that of the lowest calcium, as would normally be expected.

Dr. L. T. Alexander, Chief of the Department of Agriculture Soil Survey Laboratory, Beltsville, Md., has suggested that the low results for alfalfas grown on the two Plainfield sand soils (farms Nos. 11 and 12) may be due to the greater root depth which would result in their getting most of their calcium well below the surface and hence out of reach of the majority of the strontium 90 activity.

TABLE 5.—*Comparison of Sr–90 level in related alfalfa and soil samples*

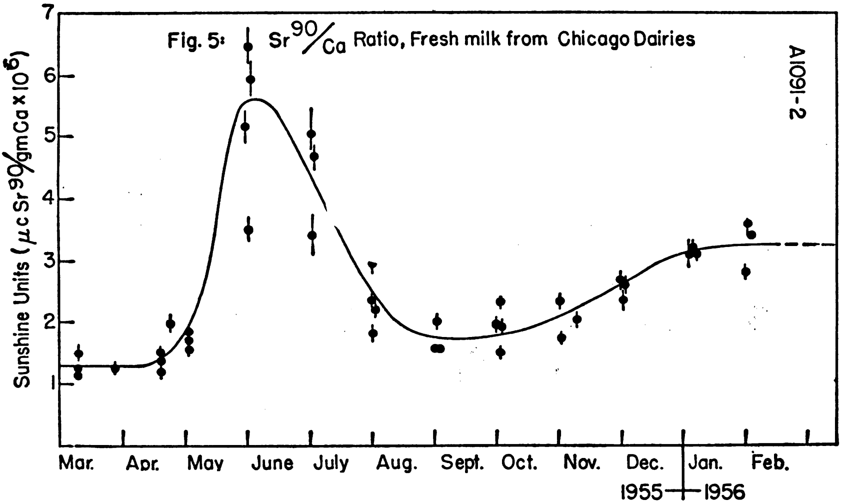
Farm	Available Ca 0 to 2-inch depth (g/ft.²)	Sr–90 soil level, in sunshine units	Sr–90 plant level, in sunshine units	Ratio [1]
McKee (No. 9), McHenry County, Ill.	31.20	(0 to 2-inch), 9.8±0.4	30.5±1.70	3.10
Swanson (No. 3),[2] Winnebago County, Ill.	~20.70	(0 to 8-inch), 6.83±0.08	13.6± .80	(~.80)
Holcomb (No. 4), Rock County, Wis.	15.00	(0 to 2-inch), 26.7±1.0	19.2±1.00	.72
Kurpeski (No. 7),[1] McHenry County, Ill.	~9.40	(0 to 6.5-inch), 12.3±0.4	7.05±.33	(~.02)
Premo No. 6),[1] Columbia County, Wis.	~8.20	(0 to 6-inch), 15.0±0.5	25.5±1.30	(~.70)
Austin (No. 8), McHenry County, Ill.	6.92	(0 to 2-inch), 49.9±1.5	38.0±2.00	.76
Van Winkle (No. 11), Will County, Ill.	4.40	(0 to 2-inch), 65.1±2.6	4.74±.21	.073
Carver (No. 12), Will County, Ill.	3.70	(0.2-inch), 64.5±1.3	2.73±.18	.042

[1] Ratio=sunshine units plant/sunshine units soil over 0 to 2-inch depth. (For the 3 unplowed soils, the 0 to 2-inch depth concentration of Sr–90 is arbitrarily taken as 2.5 times that determined for the greater depth.

[2] Field recently plowed.

IV. Fresh milk from Chicago dairies

Samples of 2 to 3 gallons of fresh milk were obtained monthly from several of the larger Chicago dairies for 1 year beginning in March 1955. The results in sunshine units are presented in figure 5 which shows the very interesting seasonal variation in the Sr–90 level of forage and dairy products.



The January-February 1956 Sr–90 level appears to be a good average of the levels observed over the 1955 growing season. That is to be expected, since over the winter months the cows feed on hay gathered at various times during the previous growing season. Similarly, the March-April 1955 level must represent the average for 1954.

The sharp rise in the spring of 1955 may be due in part to leaf retention of fallout from the concurrent Nevada tests. However, the rise may be explainable in terms of other factors. When first pastured in the spring, the cows change from feed grown an average 8 months earlier to new growth, representing an increase in Sr–90 level due to the total additional Sr–90 fallout during the 8 months' period. Furthermore, the spring pasturage for the most part may be unplowed fields.

The fall-off during the summer may be due to several factors: A large proportion of feed from recently plowed fields, greater root depth and thus lower Sr–90 level for more mature plants, and lower Sr–90 retention in milk as a result of richer calcium diet. To the extent that leaf retention is involved, it should have lower effect in summer consistent with the lower observed fallout during the summer months (see 1955 Chicago rain data, table 2 above).

The increase from the March 1955 level to the February 1956 level is somewhat greater than the corresponding increase in the total Sr–90 soil level from July 1954 to July 1955 (see fig. 4). The greater increase in milk level may be the consequence of higher fallout, and thus higher leaf retention, in 1955 than in 1954. Another possible explanation would be the reduced effect of plowing on lowering the average Sr–90 level at root depth in successive years.

The data in figure 5 indicate that the sampling was surprisingly good, since the only considerable scatter occurs during the sharp spring rise. The monitoring of milk from large dairies appears to provide a good record of plant and dairy product activity levels over the growing season for a given area and also provides a basis for comparison of the activity of plants and dairy products from widely scattered areas.

V. Sr–90 content of foreign soils

The results of Sr–90 analyses for all foreign soil samples assayed since the publication of Chicago Sunshine Bulletin No. 11, December 1, 1955, are summarized in table 6. Results for earlier foreign soil measurements, which include most of the data for soils collected during and prior to the spring of 1954, have been reported by W. F. Libby.[4]

The results reflect the substantial increase in the worldwide distribution of Sr–90 fallout following the Castle tests. The increase is particularly striking for the Southern Hemisphere samples which show about an order of magnitude increase in the Sr–90 soil content for the period from spring 1954 to January 1956.

Two of the South American samples (Beltsville No. 56456 from Lima, Peru, and No. 56447 from Antofagasta, Chile) are representative of areas of very low rainfall. Comparing their results with those for the São Paulo, Brazil, and Antofagasta, Chile, soils indicates a striking dependence of the fallout on rainfall. The result for the Brawley, Calif., soil (CL 1127, reported on p. 44 ,this report), which shows ≤0.4 sunshine units, corresponding to ≤0.6 millicuries of Sr–90 per square mile, is equally convincing in this respect. It would appear that, except within several thousand miles downwind of test areas where large particles of dry debris may fall out directly for a short period following a test shot, precipitation must be the only important mechanism of fallout. This dependence can be further tested by relating the Sr–90 soil level to rainfall information for areas remote from test sites. The latitude dependence of fallout, pointed out by W. F. Libby,[4] should be taken into account when such a correlation is attempted.

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[4] W. F. Libby, Radioactive Strontium Fallout, Proceedings National Academy of Science, 42, 365 (1956).

TABLE 6.—*Sr-90 content of foreign soils*

Beltsville No.	Location	Collection date	Depth	Sr-90 (sunshine units)	g Ca/ft²	Mc/mi²
			Inches			
551284	Turkey No. 1	February 1954	0–2	2.44±0.13	33.20	2.30
551285	Turkey No. 2	do	0–2	1.52±0.05	29.50	1.30
551286	Turkey No. 3	do	0–2	1.29±0.06	30.40	1.10
55772	Italy	February 1955	0–4	1.64±0.05	88.20	4.00
55877	Turkey	do	0–4	1.28±0.07	106.40	3.80
55878A	Ankara, Turkey	do	0–4	2.4 ±0.2	31.80	2.70
55878B	do	do	0–4	1.01±0.12	19.80	
55591A	Beirut, Lebanon	do	0–4	4.4 ±0.2	32.10	4.60
55591B	do	do	0–4	1.22±0.13	20.50	
55592A	Terbol, Lebanon	do	0–4	2.04±0.13	29.00	2.40
55592B	do	do	0–4	1.56±0.12	17.90	
55590	Damascus, Syria	do	0–4	1.40±0.10	31.70	1.20
55614	Paris, France	do	0–4	0.69±0.05	41.60	.80
55643	Tokyo, Japan	do	0–4	3.74±0.34	4.34	.45
55644	do	do	0–4	5.86±0.29	6.08	.99
55645	Dakar, French West Africa	do	0–4	3.71±0.14	4.52	.47
55647	Algiers, Algeria	do	0–4	1.20±0.08	32.60	1.10
55648	do	do	0–4	2.90±0.20	47.10	3.80
55646	Dakar, French West Africa	do	0–4	9.31±0.74	1.12	.29
55672	Bombay, India	do	0–4	0.40±0.02	7.65	.08
55673	do	do	0–4	0.64±0.05	10.80	.19
55777	Durban, South Africa	do	0–4	4.43±0.19	13.70	1.70
55786	Aden, Saudi Arabia	do	0–4	1.02±0.06	4.21	.12
54288	Belo Horizonte, Brazil	March 1954	0–4	5.3 ±2.1	.76	.11
54289	do	do	0–4	7.1 ±0.7	.84	.17
56448	São Paulo, Brazil	January 1956	0–6	3.04±0.27	14.20	1.20
56450	Asuncion, Paraguay	do	0–6	11.3 ±0.8	6.2	1.95
56456	Lima, Peru	do	0–6	0.60±0.04	42.7	.71
56447	Antofagasta, Chile	do	0–½	0.44±0.04	1.66	.02

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956

Sample	*Sunshine units*
I. Human bone: In all cases, the date of sample corresponds to the date of death or post mortem.	
A. Newborn (under 30 days):	
1. United States:	
a. Furnished by Dr. Shields Warren, Cancer Research Institute, New England Deaconess Hospital, Boston, Massachusetts:	
(1) CL 993: Age 10 days, A55–317, December 22, 1955, vertebrae, 5.14 g ash, 1.96 g Ca	0.70± 0.04
(2) CL 999–P: Age 9 days, A55–326, December 30, 1955, vertebrae and ribs, 4.55 g ash, 1.62 g Ca	.45± .10
2. Foreign, Southern Hemisphere:	
a. Santiago, Chile: Furnished by Dr. Juan Vial, Catholic University School of Medicine, Santiago, Chile. Collection arrangements made by Dr. R. B. Watson, Rockefeller Foundation, Rio de Janeiro, Brazil:	
(1) CL 717–P: Age 4 days, August 2, 1955, femur, ribs, and sternum, 0.91 g ash, 0.37 g Ca	1.2± .3
(2) CL 718–P: Age 2 days, August 10, 1955, femur, ribs, sternum, vertebral column, and parietal, 7.1 g ash, 2.7 g Ca	.17± .03
(3) CL 720–P: Age 2 days, August 11, 1955, ribs, sternum, vertebral column, and parietal, 6.7 g ash, 2.65 g Ca	.15± .05
(4) CL 722–P: Age 5 days, August 12, 1955, ribs, sternum, vertebral column, and parietal, 6.1 g ash, 2.39 g Ca	.31± .04
(5) CL 723–P: Age 3 days, August 13, 1955, ribs, sternum, vertebral column, and parietal, 6.3 g ash, 2.33 g Ca	.14± .04
(6) CL 726–P: Age 9 days, August 18, 1955, ribs, sternum, vertebral column, and parietal, 4.5 g ash, 1.67 g Ca	.33± .06

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
I. Human bone—Continued	
A. Newborn—Continued	
2. Foreign, Southern Hemisphere—Continued	
b. Lima, Peru: Furnished by Dr. Alberto Hurtado, Maternity Hospital of Lima. Collection arrangements made by Dr. R. B. Watson, Rockefeller Foundation, Rio de Janeiro, Brazil:	
(1) CL 812–P: Age few hours, July 4, 1955, ribs and femur, 0.903 g ash, 0.334 g Ca	<.45
(2) CL 809–P: Age few hours, July 6, 1955, ribs and femur, 1.26 g ash, 0.46 g Ca	.44±.16
(3) CL 804: Age few hours, August 3, 1955, ribs and femur, 5.1 g ash, 1.96 g Ca	≤.2
(4) CL 807: Age 6 days, A No. 55–247, August 4, 1955, ribs and femur, 2.4 g ash, 1.11 g Ca	≤.6
(5) CL 806: Age few hours, A No. 55–251, August 8, 1955, ribs and femur, 1.26 g ash, 0.49 g Ca	≤.7
(6) CL 810–P: Age 8 days, A No. 55–246, August 5, 1955, 1.83 g ash, 0.68 g Ca	.36±.12
(7) CL 811: Age few hours, A No. 55–245, August 5, 1955, ribs and femur, 2.4 g ash, 0.99 g Ca	.7±.1
(8) CL 808–P: Age 10 days, A No. 55–365, September 9, 1955, ribs and femur, 1.08 g ash, 0.4 g Ca	.64±.28
(9) CL 805–P: Age 14 days, A No. 55–367, September 10, 1955, ribs and femur, 1.2 g ash, 0.52 g Ca	2.16±.40
B. Children (30 days to 15 years):	
1. United States:	
a. Furnished by Dr. Shields Warren, Cancer Research Institute, New England Deaconess Hospital, Boston, Massachusetts:	
(1) CL 850–P: Age 8½ years, Massachusetts, A35102, April 15, 1955, vertebrae, 5.47 g ash, 2.26 g Ca	.13±.04
(2) CL 851: Age 2 10/12 years, Massachusetts, A55–103, April 16, 1955, vertebrae, 5.1 g ash, 1.84 g Ca	≤.21
(3) CL 852–P: Age 7 months, Rhode Island, A55–104, April 18, 1955, vertebrae, 5.18 g ash, 1.82 g. Ca	.82±.08
(4) CL 855–P: Age 14 months, Massachusetts, A55–108, April 21, 1955, vertebrae, 4.42 g ash, 1.23 g Ca	.65±.12
(5) CL 856–P: Age 7 years, Massachusetts, A55–109, April 21, 1955, vertebrae, 6.41 g ash, 2.37 g Ca	.22±.04
(6) CL 854–P: Age 2¾ years, Massachusetts, A55–110, April 24, 1955, vertebrae, 3.69 g ash, 1.38 g Ca	.29±.08
(7) CL 862–P: Age 6 years, New York, A160953, April 29, 1955, ribs, 6.97 g ash, 2.64 g Ca	.28±.03
(8) CL 853–P: Age 4 years, Massachusetts, A55–116, April 30, 1955, vertebrae, 2.55 g ash, 0.97 g Ca	.63±.19
(9) CL 860–P: Age 1½ years, Massachusetts, A55–117, May 7, 1955, vertebrac, 6.56 g ash, 2.40 g Ca	.23±.04

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
I. Human bone—Continued	
B. Children (30 days to 15 years)—Continued	
1. United States—Continued	
a. Furnished by Dr. Shields Warren—	
(10) CL 859–P: Age 3⅓ years, California, A55–122, May 8, 1955, vertebrae, 4.31 g ash, 1.57 g Ca	<. 10
(11) CL 858–P: Age 6 weeks, Massachusetts, A128, May 10, 1955, vertebrae, 2.30 g ash, 0.85 g Ca	. 71 ±. 20
(12) CL 857–P: Age 5⅚ years, New Hampshire, A417286, May 18, 1955, vertebrae, 3.88 g ash, 1.29 g Ca	. 40 ±. 12
(13) CL 992–P: Age 11½ months, Massachusetts, A55–304, December 14, 1955, vertebrae, 3.71 g ash, 1.21 g Ca	. 69 ±. 12
(14) CL 995: Age 10 years, Massachusetts, A55–319, December 22, 1955, vertebrae, 4.21 g ash, 1.52 g Ca	1. 4 ±. 2
(15) CL 994–P: Age 13 years, North Carolina, A55–318, December 23, 1955, vertebrae, 31.45 g ash, 11.5 g Ca	. 26 ±. 07
(16) CL 996: Age 3 years, Massachusetts, A55–320, December 23, 1955, ribs and vertebrae, 5.33 g ash, 2.04 g Ca	. 60 ±. 04
(17) CL 998–P: Age 2½ years, Massachusetts, A55–325, December 28, 1955, vertebrae and ribs, 9.90 g ash, 3.02 g Ca	. 47 ±. 09
(18) CL 1000–P: Age 11 years, Massachusetts, A55–327, December 30, 1955, vertebrae and ribs, 33.48 g ash, 10.3 g Ca	. 21 ±. 03
(19) CL 1001: Age 6 years, New York, A56–1, January 1, 1956, vertebrae, 4.95 g ash, 1.85 g Ca	. 6 ±. 1
(20) CL 1002: Age 1½ years, Massachusetts, A56–2, January 1, 1956, vertebrae, 2.25 g ash, 0.82 g Ca	1. 7 ±. 3
(21) CL 1003: Age 7 weeks, Massachusetts, A56–3, January 2, 1953, vertebrae, 2.97 g ash, 1.04 g Ca	. 80 ±. 08
(22) CL 1004–P: Age 2¾ years, Rhode Island, A56–5, January 4, 1956, vertebrae, 6.8 g ash, 1.91 g Ca	. 26 ±. 06
(23) CL 1006–P: Age 5 months, New Hampshire, A56–6, January 6, 1956, vertebrae and ribs, 6.96 g ash, 2.16 g Ca	. 97 ±. 08
(24) CL 1007–P: Age 2½ years, Massachusetts, A56–10, January 10, 1956, vertebrae, 5.98 g ash, 1.73 g Ca	. 42 ±. 06
C. Adults (over 15 years):	
1. United States:	
a. Furnished by Dr. Shields Warren, Cancer Research Institute, New England Deaconess Hospital, Boston, Massachusetts:	
(1) CL 863–P: Three samples combined, age range 40–44 years, Massachusetts, ribs, sterna and vertebrae, 51.2 g ash, 17.7 g Ca	0. 046 ± 0. 012
(*a*) No. 161066, age 44 years, May 5, 1955.	
(*b*) No. 55A56, age 40 years, May 26, 1955.	
(*c*) No. 161952, age 40 years, June 8, 1955.	

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
I. Human bone—Continued	
C. Adults (over 15 years)—Continued	
1. United States—Continued	
a. Furnished by Dr. Shields Warren—Continued	
(2) CL 663–P: Four samples combined, age range 49–57 years, 88.47 g ash, 33.2 g Ca	.036±.009
(*a*) No. 159893, age 57 years, Rhode Island, March 12, 1955.	
(*b*) No. 159920, age 54 years, Massachusetts, March 15, 1955.	
(*c*) No. 160704, age 49 years, Massachusetts, April 19, 1955.	
(*d*) No. 55A40, age 57 years, Massachusetts, April 22, 1955.	
(3) CL 864–P: Six samples combined, age range 51–57 years, Massachusetts, vertebrae, 75.38 g ash, 25.6 g Ca	.076±.007
(*a*) No. 160934, age 53 years, April 29, 1955.	
(*b*) No. 161007, age 52 years, May 3, 1955.	
(*c*) No. 55A49, age 57 years, May 9, 1955.	
(*d*) No. 55A50, age 51 years, May 13, 1955.	
(*e*) No. 161984, age 55 years, June 10, 1955.	
(*f*) No. 163751, age 51 years, September 7, 1955.	
(4) CL 866–P: Three samples combined, age range 61–69 years, Massachusetts, femurs, 131.1 g ash, 49.9 g Ca	.029±.005
(*a*) No. 161533, age 69 years, May 21, 1955.	
(*b*) No. 161659, age 61 years, May 26, 1955.	
(*c*) No. 161820, age 67 years, June 3, 1955.	
(5) CL 865–P: Six samples combined, age range 63–69 years, Massachusetts, vertebrae and clavicle, 75.0 g ash, 26.8 g Ca	.11± .03
(*a*) No. 161093, age 67 years, May 6, 1955.	
(*b*) No. 161134, age 67 years, May 7, 1955.	
(*c*) No. 55A51, age 69 years, May 14, 1955.	
(*d*) No. 55A55, age 69 years, May 23, 1955.	
(*e*) No. 161675, age 67 years, May 27, 1955.	
(*f*) No. 161787, age 63 years, June 2, 1955.	
(6) CL 867–P: Five samples combined, age range 71–77 years, Massachusetts, femurs, 228.2 g ash, 91.1 g Ca	.025± .003
(*a*) No. 161028, age 76 years, May 6, 1955.	
(*b*) No. 161564, age 77 years, May 23, 1955.	
(*c*) No. 161660, age 72 years, May 26, 1955.	

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
I. Human bone—Continued	
C. Adults (over 15 years)—Continued	
1. United States—Continued	
a. Furnished by Dr. Shields Warren—Continued	
(C) CL 867–P—Continued	
(*d*) No. 161789, age 71 years, June 2, 1955.	
(*e*) No. 162106, age 71 years, June 15, 1955.	
(7) CL 868–P: Seven samples combined, age range 70–81 years, vertebrae, sternum and ribs, 148.3 g ash, 55.4 g Ca	.031± .007
(*a*) No. 161001, age 70 years, Connecticut, May 2, 1955.	
(*b*) No. 161294, age 73 years, Rhode Island, May 13, 1955.	
(*c*) No. 161539, age 74 years, Massachusetts, May 23, 1955.	
(*d*) No. 161605, age 80 years, Massachusetts, May 24, 1955.	
(*e*) No. 161887, age 81 years, Massachusetts, June 7, 1955.	
(*f*) No. 161920, age 70 years, New England, June 8, 1955.	
(*g*) No. 161985, age 75 years, Massachusetts, June 9, 1955.	
b. Furnished by Dr. R. Hasterlik, Argonne Cancer Research Hospital, University of Chicago, Chicago, Illinois:	
(1) CL 638: Four samples combined, age range 40–47 years, Chicago, 52.3 g ash, 19.79 g Ca	≤.02
(*a*) No. 9386, age 46 years, April 7, 1955.	
(*b*) No. 9390, age 47 years, April 12, 1955.	
(*c*) No. 9414, age 40 years, May 6, 1955.	
(*d*) No. 9426, age 41 years, May 20, 1955.	
2. Foreign, Southern Hemisphere:	
(a) Brazil: Furnished by Dr. Paolo Conto, Faculdade de Medicina, Universidade de Recife, Pernambuco, Brazil. Collection arrangements made by Dr. R. B. Watson, Rockefeller Foundation, Rio de Janeiro, Brazil:	
(1) CL 770–P: Age 18 years, Recife, January 1, 1955, clavicle, 5.89 g ash, 2.25 g Ca	.14± .05
(2) CL 767: Age 24 years, Recife, January 20, 1955, humerus, 8.32 g ash, 3.32 g Ca	≤.09
(3) CL 768. Age 24 years, Recife, March 13, 1955, metacarpals and phalanges, 4.71 g ash, 1.88 g Ca	≤.16
(4) CL 766: Age 26 years, Recife, August 31, 1955, femur, 12.24 g ash, 4.75 g Ca	≤.07
b. Brazil: Furnished by Dr. Jairo Câmara, Faculdade de Medicina, Universidade de Minas Gerais, Belo Horizonte, Brazil. Collection arrangements made by Dr. R. B. Watson, Rockefeller Foundation, Rio de Janeiro, Brazil:	
(1) CL 739: Age 34 years, Marque, July 20, 1955, sternum and cartilage, rib fragments, 9.7 g ash, 3.14 g Ca	≤.02
(2) CL 764–P: Age 30 years, Belo Horizonte, September 9, 1955, 14.27 g ash, 5.06 g Ca	.12± .02

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
I. Human bone—Continued	
D. Dentine:	
1. CL 665–P: Permanent teeth from persons over 15 years of age, Bristol, England, furnished by Dr. Shields Warren, Cancer Research Institute, New England Deaconess Hospital, Boston, Massachusetts, No. L3, collected January to April 1955, 32.32 g ash, 12.5 g Ca	.026±.011
II. Animal bone:	
A. United States:	
1. Chicago Milkshed Farms: Collected by Dr. E. A. Martell:	
a. CL 813–P: Leg bone of Holstein-Angus, age 1½ years, McKee Farm (No. 9), McHenry County, Illinois, killed September 22, 1955, 1374 g ash, 35.3 g Ca	.51±.03
b. CL 1012–P: Steer leg bones, age 19 months, Swanson Farm (No. 3), Winnebago County, Illinois, killed middle of November 1955, 279.8 g ash, 19.5 g Ca	2.09±.11
c. CL 1011–P; Steer leg bone, age 15 months, Grabow Farm (No. 1), Rock County, Wisconsin, killed January 15, 1956, 440.2 g ash, 122.6 g Ca	5.50±.27
2. Interlaboratory check sample, Cornell lamb bone, New York, age 6 months, furnished by Dr. Lyle T. Alexander, Beltsville No. 551512, killed September 15, 1955. Four replicates assayed as individual samples:	
a. CL 841: Cornell lamb bone No. 31, 24.86 g ash, 8.96 g Ca	4.45±.34
b. CL 842: Cornell lamb bone No. 32, 25.07 g ash, 9.26 g Ca	5.14±.23
c. CL 843–P: Cornell lamb bone No. 33, 24.86 g ash, 8.26 g Ca	6.98±.34
d. CL 844–P: Cornell lamb bone No. 34, 24.95 g ash, 11.1 g Ca	4.45±.24
3. Interlaboratory check sample, calf bone, age 9 months, Tifton, Georgia, Beltsville No. 551168, killed last week in October 1955:	
a. CL 972: 31.8 g ash, 12.05 g Ca	12.9± .6
b. CL 973: 30.87 g ash, 11.42 g Ca	10.3± .3
c. CL 974–P: 32.93 g ash, 12.2 g Ca	12.7± .7
d. CL 975–P: 31.45 g ash, 11.9 g Ca	11.4± .6
B. Foreign, Southern Hemisphere:	
1. CL 1130–P: Lamb bones, Montaro Valley, Huan Cayo, Peru, collected by Dr. M. Drosdoff, ICA, USOM to Peru, Beltsville No. 56475, received May 23, 1956, 144.2 g ash, 18.43 g Ca	7.48±.44
III. Animal products:	
A. Cheese:	
1. United States:	
a. CL 707–P: Domestic Swiss, Wisconsin, 15 lbs. purchased from V. Berg, Chicago, August 25, 1955, manufactured April 19, 1955, 281.2 g ash, 51.0 g Ca	10.4± .4
b. CL 836: Domestic Münster, Dodge County, Wisconsin, 19.5 lbs. purchased from V. Berg, Chicago, November 30, 1955, manufactured October 11, 1955, 271.5 g ash, 20.0 g Ca	6.7± .3
c. CL 838: Domestic Swiss, Green County, Wisconsin, 13.75 lbs. purchased from V. Berg, Chicago, November 30, 1955, manufactured September 2, 1955, 212.5 g ash, 56.4 g Ca	6.8± .2

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample — *Sunshine units*

III. Animal products—Continued
- A. Cheese—Continued
  - 1. United States—Continued
    - d. CL 1036–P: Domestic Münster, Wisconsin, 17¾ lbs. purchased from V. Berg, Chicago, March 16, 1956, manufactured January 1956, 122.2 g ash, 6.53 g Ca — 8.37±.21
    - e. CL 1038–P: Domestic Swiss, Wisconsin, 18¼ lbs. purchased from V. Berg, Chicago, March 16, 1956, manufactured December 1955, 160.2 g ash, 12.8 g Ca — 4.7± .28
  - 2. Foreign, Northern Hemisphere:
    - a. CL 294–P: Imported Danish Blue, Denmark, 19¼ lbs. purchased from V. Berg, Chicago, October 21, 1954, manufactured in April 1954, 174.86 g ash, 16.6 g Ca — .65±.05
    - b. CL 708–P: Imported Swiss, Switzerland, 12 lbs. purchased from V. Berg, Chicago, August 25, 1955, manufactured in February 1955, 217.5 g ash, 40.0 g Ca — 2.27±.13
    - c. CL 839: Imported Swiss, Switzerland, 12.5 lbs. purchased from V. Berg, Chicago, November 30, 1955, manufactured May 1955, 157.3 g ash, 42.53 g Ca — 9.33±.24
    - d. CL 1037–P: Imported Swiss, Switzerland, 15 lbs. purchased from V. Berg, Chicago, March 16, 1956, manufactured in June 1955, 77.9 g ash, 13.7 g Ca — 1.06±.05
    - e. CL 849: Imported Danish Blue, Denmark, 18.25 lbs. purchased from V. Berg, Chicago, November 30, 1955, manufactured in July 1955, 138.8 g ash, 13.5 g Ca — 2.6 ±.2
    - f. CL 1035–P: Imported Danish Blue, Denmark, 18½ lbs. purchased from V. Berg, Chicago, March 16, 1956, manufactured Fall-Winter, 1955, 159.1 g ash, 5.44 g Ca — 11.0± .7
  - 3. Foreign, Southern Hemisphere:
    - a. CL 669–P: Cheddar, Perth, Australia, 2 lbs., Beltsville No. 55841, received June 20, 1955, 34.7 g ash, 7.59 g Ca — 1.26±.13
- B. Milk:
  - 1. Fresh milk from Chicago dairies:
    - a. CL 673–P: Bowman Dairy, purchased June 1, 1955, 43.5 g ash, 8.08 g Ca — 3.5± .2
    - b. CL 701–P: Borden Dairy, purchased August 1, 1955, 64.8 g ash, 12.0 g Ca — 1.82±.08
    - c. CL 702–P: Wanzer Dairy, purchased August 1, 1955, 56.3 g ash, 11.5 g Ca — 2.94±.17
    - d. CL 703–P: Bowman Dairy, purchased August 1, 1955, 77.7 g ash, 12.8 g Ca — 2.34±.19
    - e. CL 746–P: Bowman Dairy, purchased October 3, 1955, 81.7 g ash, 11.8 g Ca — 1.92±.10
    - f. CL 747–P: Wanzer Dairy, purchased October 3, 1955, 57.0 g ash, 9.8 g Ca — 1.96±.09
    - g. CL 748–P: Borden Dairy, purchased October 3, 1955, 29.1 g ash, 4.54 g Ca — 2.34±.12
    - h. CL 749–P: Pure Milk Ass'n., purchased October 3, 1955, 54.7 g ash, 8.0 g Ca — 1.47±.08
    - i. CL 826–P: Bowman Dairy, purchased November 1, 1955, 82.7 g ash, 15.2 g Ca — 1.72±.09
    - j. CL 827–P: Wanzer Dairy, purchased November 1, 1955, 85.5 g ash, 14.9 g Ca — 2.34±.11
    - k. CL 829–P: Pure Milk Ass'n., purchased November 9, 1955, 54.2 g ash, 9.2 g Ca — 2.03±.09

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
III. Animal products—Continued	
B. Milk—Continued	
1. Fresh milk from Chicago daries—Continued	
l. CL 869–P: Bowman Dairy, purchased December 1, 1955, 26.8 g ash, 4.91 g Ca	2.35± .15
m. CL 870–P: Wanzer Dairy, purchased December 1, 1955, 83.8 g ash, 12.6 g Ca	2.58± .15
n. CL 875–P: Pure Milk Ass'n., purchased December 7, 1955, 52.8 g ash, 8.40 g Ca	2.68± .14
o. CL 958: Borden Dairy, purchased January 3, 1956, 79.2 g ash, 11.5 g Ca	3.12± .25
p. CL 959: Bowman Dairy, purchased January 3, 1956, 25.8 g ash, 3.9 g Ca	7.7± .4
q. CL 960: Wanzer Dairy, purchased January 3, 1956, 25.8 g ash, 4.07 g Ca	3.2± .1
r. CL 967: Pure Milk Ass'n., purchased January 5, 1956, 55.8 g ash, 8.47 g Ca	3.10± .09
s. CL 1014: Wanzer Dairy, purchased February 1, 1956, 21.8 g ash, 3.69 g Ca	2.80± .08
t. CL 1015: Bowman Dairy, purchased February 1, 1956, 27.7 g ash, 4.22 g Ca	3.6± .2
u. CL 1016: Borden Dairy, purchased February 1, 1956, 31.4 g ash, 5.06 g Ca	3.40± .14
v. CL 1017–P: Pure Milk Ass'n., purchased February 9, 1956, 9.65 g ash, 1.38 g Ca	2.62± .17
w. CL 1028–P: Bowman Dairy, purchased March 1, 1956, 22.2 g ash, 3.22 g Ca	3.05± .20
x. CL 1029–P: Wanzer Dairy, purchased March 1, 1956, 62.8 g ash, 7.94 g Ca	2.45± .13
y. CL 1030–P: Borden Dairy, purchased March 2, 1956, 24.2 g ash, 3.49 g Ca	2.99± .29
z. CL 1031–P: Pure Milk Ass'n., purchased March 5, 1956, 52.0 g ash, 7.86 g Ca	1.25± .12
aa. CL 1057–P: Bowman Dairy, purchased April 2, 1956, 47.2 g ash, 7.43 g Ca	2.20± .13
bb. CL 1058–P: Borden Dairy, purchased April 2, 1956, 37.4 g ash, 5.68 g Ca	3.21± .18
cc. CL 1063–P: Pure Milk Ass'n, purchased April 2, 1956, 40.3 g ash, 6.34 g Ca	2.33±.13
dd. CL 1064–P: Wanzer Dairy, purchased April 2, 1956, 66.4 g ash, 7.40 g Ca	2.61±.16
ee. CL 1069–P: Wanzer Dairy, purchased May 1, 1956, 23.9 g ash, 3.14 g Ca	2.23±.13
ff. CL 1070–P: Bowman Dairy, purchased May 1, 1956, 69.6 g ash, 7.28 g Ca	1.53±.13
2. Other United States milks:	
a. Interlaboratory check sample, powdered whole, New York, furnished by Health and Safety Laboratory, New York Operations Office, U. S. Atomic Energy Commission, processed October 29, 1955:	
(1) CL 889–P: 45.2 g ash, 7.12 g Ca	2.4± .2
(2) CL 890–P: 46.2 g ash, 7.12 g Ca	2.6± .2
(3) CL 891: 33.1 g ash, 6.62 g Ca	2.4± .2
(4) CL 892: 44.3 g ash, 7.68 g Ca	2.5± .2
3. Foreign, Southern Hemisphere:	
a. CL 694–P: Powdered skim, Waiton, New Zealand, Beltsville No. 551052, manufactured February 7, 1955, 88.8 g ash, 17.8 g Ca	.77±.07
b. CL 693–P: Powdered whole, Waiton, New Zealand, Beltsville No. 551051, manufactured February 8, 1955, 51.8 g ash, 11.2 g Ca	.70±.07

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
IV. Botanical:	
A. Chicago Milkshed: Collected by Dr. L. T. Alexander, Plant Industry Station, U. S. Department of Agriculture, Beltsville, Maryland and Dr. E. A. Martell, Enrico Fermi Institute for Nuclear Studies, University of Chicago, Chicago, Illinois, September 29 and 30, 1955:	
1. CL 754–P: Alfalfa, Swanson Farm, (No. 3) Winnebago County, Illinois, 90.6 g ash, 11.4 g Ca	13. 6± .8
2. CL 755–P: Alfalfa, Holcomb Farm (No. 4), Rock County, Wisconsin, 20.8 g ash, 3.79 g Ca	19. 2±1. 0
3. CL 750–P: Red clover, Premo Farm (No. 6), Columbia County, Wisconsin, 119.4 g ash, 20.5 g Ca	25. 5±1. 3
4. CL 752–P: Alfalfa, Kurpeski Farm (No. 7), McHenry County, Illinois, 172.2 g ash, 20.4 g Ca	7. 05±. 33
5. CL 757–P: Alfalfa, Austin Farm (No. 8), McHenry County, Illinois, 63.6 g ash, 14.6 g Ca	38. 0±2. 0
6. CL 751–P: Lidino clover and bromegrass mixture, McKee Farm (No. 9), McHenry County, Illinois, 62.6 g ash, 10.2 g Ca	30. 5±1. 7
7. CL 756–P: Alfalfa, Van Winkle Farm (No. 11), Will County, Illinois, 116.1 g ash, 15.3 g Ca	5. 76±. 29
8. CL 753–P: Alfalfa, Carver Farm (No. 12), Will County, Illinois, 53.2 g ash, 11.2 g Ca	2. 73±. 18
B. Other United States:	
1. Interlaboratory check sample, Bermuda grass, Tifton, Georgia, Beltsville No. 551528, harvested in June 1955:	
a. CL 968–P: 124.4 g ash, 4.71 g Ca	42. 0±1. 9
b. CL 970: 124.4 g ash, 4.39 g Ca	40. 0±2. 0
2. Interlaboratory check sample, mixed hay, New York, furnished by Dr. L. Alexander, Beltsville No. 551513, grown during August and September 1955:	
a. CL 847–P: Cornell No. 3, 90.5 g ash, 12.0 g Ca	19. 7± .8
b. CL 848–P: Cornell No. 4, 89.7 g ash, 12.0 g Ca	17. 9± .9
3. Brawley, California: Furnished by Edward Noble, Southwestern Irrigation Field Station. Collection arrangements made by Dr. Lyle T. Alexander, Plant Industry Station, U. S. Department of Agriculture, Beltsville, Maryland, January 5, 1956:	
a. CL 1059–P: Lettuce, 57.3 g ash, 2.68 g Ca	.39±. 05
b. CL 1060–P: Broccoli, 74.4 g ash, 7.90 g Ca	.25±. 08
c. CP 1061–P: Peas, 56.1 g ash, 5.67 g Ca	1. 34±. 08
d. CL 1062–P: Alfalfa, 49.9 g ash, 5.43 g Ca	2. 13±. 22
V. Soil: All soil samples listed below were obtained by Dr. L. T. Alexander and extracted at the Soil Survey Laboratory, U. S. Department of Agriculture, Beltsville, Maryland. During the Spring of 1956, the Beltsville laboratory discontinued the ammonium acetate extraction procedure in favor of electrodialysis extraction. The method of extraction is indicated for each sample. Soil Survey Laboratory analyses on these samples include the determination of Na, K, Mg, Ca and Sr:	
A. Chicago Milkshed: Collected by Dr. L. Alexander, Soil Survey Laboratory, Plant Industry Station, U. S. Department of Agriculture, Beltsville, Maryland, and Dr. E. A. Martell, Enrico Fermi Institute for Nuclear Studies, University of Chicago, Chicago, Illinois, September 29 and 30, 1955:	
1. CL 1019: Swanson Farm (No. 3), Winnebago County, Illinois, Carrington-like silt loam, Beltsville No. 551503, NH_4AC extraction of 4 lbs, soil, 0–8″ depth, 20.0 g oxalate, 7.54 g oxide, 5.39 g Ca	6. 83±. 8

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	Sunshine units
V. Soil—Continued	
A. Chicago Milkshed—Continued	
2. Holcomb Farm (No. 4), Rock County, Wisconsin, Carrington silt loam:	
a. CL 956: Beltsville No. 551500, NH_4AC extraction of 4 lbs. soil, 0–2″ depth, 12.1 g oxalate, 6.82 g oxide, 3.06 g Ca	26.7±1.0
b. CL 957: Beltsville No. 551501, NH_4AC extraction of 4 lbs. soil, 2–6″ depth, 12.1 g oxalate, 6.21 g oxide, 3.18 g Ca	3.81±.14
3. CL 1018: Premo Farm (No. 6), Columbia County, Wisconsin, Miami silt loam, Beltsville No. 551502, NH_4AC extraction of 4 lbs. soil, 0–6″ depth, 11.25 g oxalate, 3.94 g oxide, 2.79 g Ca	15.0± .5
4. Kurpeski Farm (No. 7), McHenry County, Illinois, Miami silt loam:	
a. CL 886: Beltsville No. 551496, Sample A, NH_4AC extraction of 4 lbs. soil, 0–6 or 7″ depth, 8.51 g oxalate, 3.21 g oxide, 2.24 g Ca	11.9± .5
b. CL 887: Beltsville No. 551497, Sample B, NH_4AC extraction of 4 lbs. soil, 0–6 or 7″ depth, 8.90 g oxalate, 3.35 g oxide, 2.36 g Ca	12.6± .5
5. Austin Farm (No. 8), McHenry County, Illinois, Miami silt loam:	
a. CL 1020: Beltsville No. 551504, NH_4AC extraction of 4 lbs. soil, 0–2″ depth, 5.71 g oxalate, 2.21 g oxide, 1.57 g Ca	49.9± 1.5
b. CL 1021: Beltsville No. 551505, NH_4AC extraction of 4 lbs. soil, 2–6″ depth, 4.08 g oxalate, 1.6 g oxide, 1.19 g Ca	9.6± .4
6. McKee Farm (No. 9) McHenry County, Illinois, Drummer silty clay loam:	
a. CL 888: Beltsville No. 551498, NH_4AC extraction of 4 lbs. soil, 0–2″ depth, 30.25 g oxalate, 11.31 g oxide, 7.04 g Ca	9.8± .4
b. CL 955: Beltsville No. 551499, NH_4AC extraction of 4 lbs. soil, 2–6″ depth, 30.6 g oxalate, 17.44 g oxide, 7.91 g Ca	.99± .04
7. Van Winkle Farm (No. 11), Will County, Illinois, Plainfield sand:	
a. CL 1022: Beltsville No. 551508, NH_4AC extraction of 4 lbs. soil, 0–2″ depth, 4.31 g oxalate, 1.66 g oxide, 1.19 g Ca	65.1± 2.6
b. CL 1023: Beltsville No. 551509, NH_4AC extraction of 4 lbs. soil, 2–6″ depth, 2.44 g oxalate, 0.94 g oxide, 0.67 g Ca	10.0± 0.4
8. Carver Farm (No. 12), Will County, Illinois, Plainfield sand:	
a. CL 1024: Beltsville No. 551510, NH_4AC extraction of 4 lbs. soil, 0–2″ depth, 4.43 g oxalate, 1.70 g oxide, 1.22 g Ca	64.5± 1.3
b. CL 1025: Beltsville No. 551511, NH_4AC extraction of 4 lbs. soil, 2–6″ depth, 2.98 g oxalate. 1.15 g oxide, 0.97 g Ca	14.0± 0.7
B. Other United States soils:	
1. Interlaboratory check sample, Ithaca, New York, September 15, 1955, electrodialysis extraction of 4 lbs. soil, 0–2″ depth:	
a. CL 1039: Beltsville No. 551186A, 18.28 g oxalate, 6.93 g oxide, 4.96 g Ca	21.5± 1.3
b. CL 1040–P: Beltsville No. 551186B, 18.22 g oxalate, 6.84 g oxide, 4.89 g Ca	17.8± 1.0

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
V. Soil—Continued	
B. Other United States soils—Continued	
2. Interlaboratory check sample, Tifton, Georgia, November 2, 1955, electrodialysis extraction of 4 lbs. soil, 0–4'' depth:	
a. CL 1041–P: Beltsville No. 551523A, 0.698 g oxalate, 0.26 g oxide, 0.186 g Ca	164.0±12.0
b. CL 1042: Beltsville No. 551523B, 0.59 g oxalate, 0.22 g oxide, 0.16 g Ca	178.2± 7.1
3. CL 1127: Brawley, California, Beltsville No. 56316, collected January 5, 1956, electrodialysis extraction of 4 lbs. soil, 0–6'' depth, 16.56 g oxalate, 6.35 g oxide, 4.54 g Ca	(≤.8),[1]≤.4
C. Foreign, Northern Hemisphere:	
1. CL 1065: England, Beltsville No. 54675B, obtained by Dr. L. Alexander in July 1954, extracted by fusion of a 100 gram subsample of a 4 lb. electrodialyzed soil sample, 0–3'' depth, 4.15 g oxalate, 1.59 g oxide, 0.136 g. Ca. (1.2±0.3 dpm Sr–90 total sample activity)	4.0±1.0
2. CL 1095–P: Calcareous, Paris, France, Beltsville No. 55614, obtained by Dr. L. Alexander, February 16, 1955, electrodialysis extraction of 4 lbs. soil, 0–4'' depth, 29.30 g oxalate, 11.06 g oxide, 7.90 g Ca	.69± .05
3. CL 881: Italy, Beltsville No. 55772, obtained by Dr. L. Alexander, February 5, 1955, NH_4AC extraction of 4 lbs. soil, 0–4'' depth, 43.1 g oxalate, 16.54 g oxide, 11.5 g Ca	1.64± .05
4. CL 882: Turkey, Beltsville No. 55877, obtained by Dr. L. Alexander, February 7, 1955, NH_4AC extraction of 4 lbs. soil, 0–4'' depth, 44.6 g oxalate, 16.96 g oxide, 12.0 g Ca	1.28± .07
5. CL 883: Turkey, Beltsville No. 551284, obtained by Dr. L. Alexander, February 28, 1954, NH_4AC extraction of 4 lbs. soil, 0–2'' depth, 42.8 g oxalate, 16.31 g oxide, 11.5 g Ca	2.44± .13
6. CL 884: Turkey, Beltsville No. 551285, obtained by Dr. L. Alexander, February 28, 1954, NH_4AC extraction of 4 lbs. soil, 0–2'' depth, 47.5 g oxalate, 18.28 g oxide, 12.8 g Ca	1.52± .05
7. CL 885: Turkey, Beltsville No. 551286, obtained by Dr. L. Alexander, February 28, 1954, NH_4AC extraction of 4 lbs. soil, 0–2'' depth, 53.9 g oxalate, 20.67 g oxide, 13.6 g. Ca	1.29±0.06
8. CL 1046–P: Calcareous, Kohler Yard, Turkey, Beltsville No. 55878A, obtained by Dr. L. Alexander, February 7, 1955, electrodialysis extraction (48-hour period) of 4 lbs. soil, 0–4'' depth, 17.11 g oxalate, 5.50 g oxide 4.72 g Ca	2.4±0.2
9. CL 1047–P: Calcareous, Kohler Yard Turkey, Beltsville No. 55878B, obtained by Dr. L. Alexander, February 7, 1955, electrodialysis extraction (additional 24-hour period) of 4 lbs. soil, 0–4'' depth, 10.64 g oxalate, 4.03 g oxide, 2.88 g Ca	1.01±0.12
10. CL 1094–P: Calcareous, Damascus Syria, Beltsville No. 55590, obtained by Dr. L. Alexander, February 11, 1955, electrodialysis extraction of 4 lbs. soil, 0–4'' depth, 20.30 g oxalate, 7.70 g oxide, 5.50 g Ca	1.40±0.10

[1] New result obtained following strontium separation and barium chromate scavenging to remove radium activity.

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
V. Soil—Continued	
C. Foreign, Northern Hemisphere—Continued	
11. CL 1048–P: Calcareous, Beirut, Lebanon, Beltsville No. 55591A, obtained by Dr. L. Alexander, February 10, 1955, electrodialysis extraction (48-hour period) of 4 lbs. soil, 0–4″ depth, 14.05 g oxalate, 5.35 g oxide, 3.83 g Ca	4.4±0.2
12. CL 1049–P: Calcareous, Beirut, Lebanon, Beltsville No. 55591B, obtained by Dr. L. Alexander, February 10, 1955, electrodialysis extraction (additional 24-hour period) of 4 lbs. soil, 0–4″ depth, 9.0 g oxalate, 3.41 g oxide, 2.44 g Ca	1.22±0.13
13. CL 1050–P: Calcareous, Terbol, Lebanon, Beltsville No. 55592A, obtained by Dr. L. Alexander, February 10, 1955, electrodialysis extraction (48-hour period) of 4 lbs. soil, 0–4″ depth, 16.68 g oxalate, 6.31 g oxide, 4.52 g Ca	2.04±0.13
14. CL 1051–P: Calcareous, Terbol, Lebanon, Beltsville No. 55592B, obtained by Dr. L. Alexander, February 10, 1955, electrodialysis extraction (additional 24-hour period) of 4 lbs. soil, 0–4″ depth, 10.32 g oxalate, 3.91 g oxide, 2.8 g Ca	1.56±0.12
15. CL 1140–P: Aden, Saudi Arabia, Beltsville No. 55786, obtained by Dr. L. Alexander, February 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 13.99 g oxalate, 5.45 g oxide, 3.69 g Ca	1.02±0.06
16. CL 1099–P: Calcareous, Algeria, Beltsville No. 55647, obtained by Dr. L. Alexander, February 15, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 20.0 g oxalate, 7.57 g oxide, 5.41 g Ca	1.20±0.08
17. CL 1100–P: Calcareous, Algeria, Beltsville No. 55648, obtained by Dr. L. Alexander, February 15, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 26.75 g oxalate, 10.16 g oxide, 7.26 g Ca (less 1 g Ca added during last step of electrodialysis procedure)	2.90±0.20
18. CL 1098–P: Dakar, F. W. Africa, Beltsville No. 55645, obtained by Dr. L. Alexander, February 13, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 8.66 g oxalate, 3.26 g oxide, 2.33 g Ca (less 1 g Ca added during last step of electrodialysis procedure)	3.71±0.14
19. CL 1136–P: Dakar, F. W. Africa, Beltsville No. 55646, obtained by Dr. L. Alexander, February 14, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 4.38 g oxalate, 1.75 g oxide, 1.27 g Ca (less 1 g Ca added during last step of electrodialysis procedure)	9.31±0.74
20. CL 1137–P: Bombay, India, Beltsville No. 55672, obtained by Dr. L. Alexander, February 14, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 29.99 g oxalate, 11.45 g oxide, 8.03 g Ca	.40±0.02
21. CL 1138–P: Bombay, India, Beltsville No. 55673, obtained by Dr. L. Alexander, February 14, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 35.5 g oxalate, 13.62 g oxide, 9.61 g Ca	.64±0.05
22. CL 1096–P: Tokyo, Japan, Beltsville No. 55643, obtained by Dr. L. Alexander, February 10, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 4.997 g oxalate, 1.89 g oxide, 1.35 g Ca	3.74±0.34
23. CL 1097–P: Tokyo, Japan, Beltsville No. 55644, obtained by Dr. L. Alexander, February 10, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 5.74 g oxalate, 2.17 g oxide, 1.55 g Ca	5.86±0.29

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	Sunshine units
V. Soils—Continued	
D. Foreign, Southern Hemisphere:	
1. CL 1044–P: Brazil, Beltsville No. 54288, obtained by Dr. L. Alexander, March 2, 1954, NH_4AC extraction of 4 lbs. soil, 0–4″ depth, 0.32 g oxalate, 0.12 g oxide, 0.09 g Ca	5.3 ±2.1
2. CL 1045–P: Brazil, Beltsville No. 54289, obtained by Dr. L. Alexander, March 2, 1954, NH_4AC extraction of 4 lbs. soil, 0–4″ depth, 0.99 g oxalate, 0.38 g oxide, 0.27 g Ca	7.1±0.7
3. CL 1135–P: São Paulo, Brazil, Beltsville No. 56448, obtained by Dr. L. Alexander, January 30, 1956, electrodialysis extraction of 4 lbs. soil, 0–6″ depth, 7.32 g oxalate, 2.90 g oxide, 1.96 g Ca	3.04±0.27
4. CL 1129: Lima, Peru, Beltsville No. 56456, obtained by Dr. L. Alexander, January 9, 1956, electrodialysis extraction of 4 lbs. soil, 0–6″ depth, 15.57 g oxalate, 6.01 g oxide, 4.29 g Ca	0.60±0.04
5. CL 1128: Antofagasta, Chile, Beltsville No. 56447, obtained by Dr. L. Alexander, January 13, 1956, electrodialysis extraction of 4 lbs. soil, 0–½″ depth, 9.35 g oxalate, 3.58 g oxide, 2.56 g Ca	0.44±0.04
6. CL 1134–P: Asunción, Paraguay, Beltsville No. 56450, obtained by Dr. L. Alexander, January 27, 1956, electrodialysis extraction of 4 lbs. soil, 0–6″ depth, 5.79 g oxalate, 2.42 g oxide, 1.53 g Ca (less 1 g Ca added during last step of electrodialysis procedure)	11.3±0.75
7. CL 1139–P: Durban, Natal, S. Africa, Beltsville No. 55777, obtained by Dr. L. Alexander, February 15, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 6.42 g oxalate, 2.55 g oxide, 1.69 g Ca	4.43±0.19
VI. Precipitation:	
A. Chicago, Illinois: Samples were collected in galvanized tubs placed on the roof of Jones Chemistry Laboratory, University of Chicago. For all samples, the equivalent precipitation in inches derived from sample volume and collection area is given:	*dpm/gal*
1. CL 290: Rain, 0.264 gal., 0.143″ equivalent, collected October 15, 1954, to October 18, 1954	10.3±0.3
2. CL 382: Rain, 1.62 gal., 0.82″ equivalent, collected 1645, December 31, 1954, to 0700, January 6, 1955	4.8±0.4
3. CL 356, 357 and 358: Snow, 0.304 gal., 0.16″ equivalent, collected January 25, 1955, to February 3, 1955	21.2±2.1
4. CL 383: Snow, 1.17 gal., 0.60″ equivalent, collected 0400, February 3, 1955, to 1500, February 10, 1955	5.9±0.8
5. CL 436–P: Rain and hail, 0.526 gal., 0.27″ equivalent, collected 1445, March 3, 1955, to 1000, March 4, 1955	29.6±4.8
6. CL 419–P: Rain, 1.53 gal., 0.78″, collected 1400, March 7, 1955, to 1430, March 21, 1955 ("wet" tub)[2]	48.5±3.0
7. CL 461–P: Rain, 1.04 gal., 0.53″, collected 1530, March 10, 1955, to 0900, March 16, 1955 ("wet" tub)[2]	42.2±2.0
8. CL 477–P: Rain and snow, 1.68 gal., 0.86″, collected 1530, March 21, 1955, to 1000, April 4, 1955 ("wet" tub)[2]	21.0±1.6
9. CL 592–P: Rain, 1.0 gal., 0.51″ equivalent, collected 0730, April 21, 1955, to 1715, April 28, 1955	42.0±2.0
10. CL 617–P: Rain, 0.098 gal., 0.05″ equivalent, collected 1800, April 28, 1955, to 0930, May 13, 1955	685.0±72.0
11. CL 618–P: Rain, 0.43 gal., 0.22″ equivalent, collected 1800, April 28, 1955, to 0930, May 13, 1955 (covered tub)[2]	193.0±21.0

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
VI. Precipitation—Continued	
A. Chicago, Illinois—Continued	
12. CL 619–P: Rain, 0.548 gal., 0.28″, collected 1800, April 28, 1955, to 0930, May 13, 1955 ("wet" tub)[2]	133.0±9.0
13. CL 627–P: Rain, 0.85 gal., 0.43″ equivalent, collected 1100, May 13, 1955, to 1130, May 23, 1955	106.0±6.0
14. CL 628–P: Rain, 0.90 gal., 0.46″ equivalent, collected 1100, May 13, 1955, to 1130, May 23, 1955 (covered tub)[2]	78.0±5.0
15. CL 645–P: Rain, 3.48 gal., 1.78″ equivalent, collected 1700, May 23, 1955, to 1030, June 6, 1955 (covered tub)[2]	69.0±3.0
16. CL 646–P: Rain, 4.94 gal., 2.52″, collected 1700, May 23, 1955, to 1030, June 6, 1955 ("wet" tub)[2]	44.8±1.6
17. CL 648–P: Rain, 0.77 gal., 0.39″ equivalent, collected 1700, May 23, 1955, to 1030, June 6, 1955	270.0±12.0
18. CL 647–P: Rain, 0.35 gal., 0.18″ equivalent, collected 1200, June 6, 1955, to 1245, June 7, 1955	22.0±5.0
19. CL 649–P: Rain, 0.37 gal., 0.19″ equivalent, collected 1245, June 7, 1955, to 0950, June 9, 1955	60.0±3.0
20. CL 661–P: Rain, 1.12 gal., 0.57″ equivalent, collected 0950, June 9, 1955, to 1035, June 10, 1955	52.0±3.0
21. CL 692–P: Rain, 2.38 gal., 1.21″ equivalent, collected 1200, June 14, 1955, to 1030, July 25, 1955	42.0±2.0
22. CL 705–P: Rain, 7.16 gal., 3.67″ equivalent, collected 1030, July 25, 1955, to 1400, August 7, 1955	2.8±0.2
23. CL 706–P: Rain, 0.435 gal., 0.22″ equivalent, collected 1400, August 7, 1955, to 1100, August 22, 1955	30.5±2.3
24. CL 727: Rain, 4.29 gal., 0.22″ equivalent, collected 1100, August 22, 1955, to 0900, August 30, 1955	1.74±0.10
25. CL 743–P: Rain, 0.66 gal., 0.34″ equivalent, collected 1100, August 30, 1955, to 1350, September 21, 1955	24.0±1.7
26. CL 744–P & 745–P: Rain, 2.07 gal., 106″ equivalent, collected 1350, September 21, 1955, to 1458, September 29, 1955	8.6±2.0
27. CL 761–P: Rain, 1.97 gal., 1.00″ equivalent, collected 1458, September 29, 1955, to 1611, October 5, 1955	2.2±0.3
28. CL 762–P: Rain, 2.83 gal., 1.44″ equivalent, collected 1611, October 5, 1955, to 1033, October 6, 1955	5.3±0.5
29. CL 763–P: Rain, 3.42 gal., 1.75″ equivalent, collected 1033, October 6, 1955, to 1110, October 7, 1955	4.70±0.29
30. CL 771–P: Rain, 1.07 gal., 0.55″ equivalent, collected 1110, October 7, 1955, to 1027, October 12, 1955	9.02±0.52
31. CL 772–P: Rain, 1.16 gal., 0.59″ equivalent, collected 1027, October 12, 1955, to 1021, October 17, 1955	8.4±0.5
32. CL 799–P: Rain, 0.79 gal., 0.40″ equivalent, collected 1021, October 17, 1955, to 0811, October 24, 1955	26.5±1.5
33. CL 825–P: Rain, 1.04 gal., 0.53″ equivalent, collected 0811, October 24, 1955, to 1010, October 31, 1955	10.1±0.5
34. CL 828–P & 830–P: Rain, 1.84 gal., 0.94″ equivalent, collected 1010, October 31, 1955, to 1030, November 17, 1955	21.0±1.0
35. CL 831–P: Snow, 0.535 gal., 0.27″ equivalent, collected 1030, November 17, 1955, to 0949, November 21, 1955	22.3±1.0

[2] See discussion, Test of Rain Collection Method (p. 622).

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
VI. Precipitation—Continued	
A. Chicago, Illinois—Continued	
36. CL 837–P: Rain, 0.147 gal., 0.075″ equivalent, collected 0949, November 21, 1955, to 0920, November 23, 1955	36. 0±4. 7
37. CL 840–P: Snow, 0.122 gal., 0.062″ equivalent, collected 0920, November 23, 1955, to 1055, November 28, 1955	11. 7±2. 7
38. CL 871–P: Snow, 0.116 gal., 0.059″ equivalent, collected 1055, November 28, 1955, to 1100, December 2, 1955	28. 6±3. 4
39. CL 872–P: Snow, 0.31 gal., 0.16″ equivalent, collected 1100, December 2, 1955, to 1120, December 5, 1955	19. 0±1. 4
40. CL 954–P: Snow, 0.15 gal., 0.077″ equivalent, collected 1120, December 5, 1955, to 0800, December 21, 1955	53. 0±7. 0
41. CL 976–P: Snow, 0.082 gal., 0.042″ equivalent, collected 0800, December 21, 1955, to 1115, January 16, 1956	440. 0±25. 0
B. Insoluble residue from Chicago rains:	
1. Insoluble residue from rain, CL 320: 1.82 gal., 0.930″ equivalent, collected December 24, 1954, to December 27, 1954. The filtered insoluble residue was fused with sodium carbonate before assay:	
Total Sr–90 activity insoluble residue: ≤0.6 dpm.	
Total Sr–90 activity in solution: 49.2±0.5 dpm.	
2. Insoluble residue from rain, CL 407 and 408: 0.71 gal., 0.36″ equivalent, collected February 18, 1955, to 0900, February 21, 1955. The filtered insoluble residue was fused with sodium carbonate before assay:	
Total Sr–90 activity insoluble residue: 0.7±0.1 dpm.	
Total Sr–90 activity in solution: 29.9±1.5 dpm.	
C. Washington, D. C.: Collected at the Naval Research Laboratory. Samples were taken by direct fall into galvanized tubs on the roof of one of the NRL buildings. The precipitation in inches is that reported by the local Weather Bureau station for the period of sample collection:	
1. CL 790: Rain, 2.40 gal., 0.61″, collected 2000, April 11, 1955, to 0400, April 12, 1955	34. 3±1. 4
2. CL 791–P: Rain, 2.92 gal., 0.68″, collected 0030 to 1500, April 14, 1955	25. 6±3. 5
3. CL 792: Rain, 0.845 gal., 0.21″, collected 2315 to 2400, April 14, 1955	39. 0±1. 7
4. CL 793–P: Rain, 1.60 gal., 0.23″, collected 2100, April 21, 1955, to 0245, April 22, 1955	46. 7±2. 2
5. CL 794: Rain, 0.697 gal., 0.21″, collected 0415, April 24, 1955, to 0300, April 25, 1955	195. 0±4. 3
6. CL 795–P: Rain, 2.30 gal., 0.39″, collected 1500, April 25, 1955, to 0700, April 26, 1955	15. 5±0. 6
7. CL 797: Rain, 0.771 gal., 0.19″, collected 1630 to 2030, May 20, 1955	112. 9±2. 0
8. CL 798–P: Rain, 1.17 gal., 0.29″, collected 1245 to 1400, May 22, 1955	1. 92±0. 22
D. Pittsburgh, Pennsylvania: Collected by the Nuclear Science and Engineering Corporation. Samples were taken by direct fall into galvanized tubs on the roof of the laboratory building. The precipitation in inches is that reported by the local Weather Bureau station for the period of sample collection:	
1. CL 876–P: Rain, PL–20–RW, 2.47 gal., 0.48″, collected 1200, July 10, 1955, to 1000, July 19, 1955	16. 6±1. 5

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
VI. Precipitation—Continued	
E. Pittsburgh, Pennsylvania—Continued	
2. CL 877–P: Rain, PL–21–RW, 5.70 gal., 0.65″, collected 1000, July 19, 1955, to 1100, July 25, 1955	18.8±1.4
3. CL 878–P: Rain, PL–22–RW, 4.40 gal., 0.60″, collected 1100, July 25, 1955, to 0900, July 28, 1955	8.2±0.5
4. CL 879–P: Rain, PL–23–RW, 1.98 gal., 0.18″, collected 0900, July 28, 1955, to 1000, August 6, 1955	8.7±1.0
5. CL 961–P: Rain, PL–24–RW, 0.86 gal., 0.18″, collected 1000, August 6, 1955, to 1500, August 8, 1955	10.8±1.0
6. CL 962–P: Rain, PL–25–RW, 4.91 gal., 1.98″, collected 1500, August 8, 1955, to 1630, August 11, 1955	5.55±0.32
7. CL 982–P: Rain, PL–26–RW, 7.6 gal., 2.24″, collected 1630, August 11, 1955, to 1030, August 16, 1955	31.0 ±3.0
8. CL 983–P: Rain, PL–27–RW, 4.0 gal., 1.71″, collected 1030, August 16, 1955, to 1700, August 23, 1955	83.0±5.0
9. CL 963–P: Rain, PL–28–RW, 0.395 gal., 0.66″, collected 1700, August 23, 1955, to 1600, August 31, 1955	31.6±2.6
10. CL 964–P: Rain, PL–29–RW, 4.4 gal., 1.84″, collected 1600, August 31, 1955, to September 28, 1955	9.75±0.59
11. CL 965–P: Rain, PL–30–RW, 2.0 gal., 0.76″, collected September 28, 1955, to October 10, 1955	10.7±0.6
12. CL 966–P: Rain, PL–31–RW, 3.9 gal., 1.32″, collected October 10, 1955, to October 18, 1955	14.2±0.6
13. CL 984–P: Rain, PL–32–RW, 0.465 gal., 0.09″, collected October 18, 1955, to October 20, 1955	34.0±1.6
14. CL 985–P: Rain, PL–33–RW, 1.14 gal., 0.42″, collected October 20, 1955, to 1615, October 24, 1955	16.1±0.8
15. CL 986–P: Rain, PL–34–RW, 0.508 gal., 0.28″, collected 1615, October 24, 1955, to 1700, October 29, 1955	50.5±2.5
16. CL 987–P: Rain, PL–35–RW, 0.715 gal., 0.40″, collected 1700, October 29, 1955, to 0845, October 31, 1955	38.4±1.9
17. CL 988–P: Rain, PL–36–RW, 0.465 gal., 0.31″, collected 0845, October 31, 1955, to 1630, November 12, 1955	60.0±2.9
18. CL 989–P: Rain, PL–37–RW, 0.402 gal., 0.08″, collected 1630, November 12, 1955, to 1630, November 14, 1955	22.6±1.4
19. CL 990–P: Rain, PL–38–RW, 6.18 gal., 2.25″, collected 1630, November 14, 1955, to 1100, November 21, 1955	4.43±0.19
20. CL 1032–P: Rain, PL–39–RW, 2.06 gal., 0.35″, collected 1100, November 21, 1955, to 1400, December 3, 1955	17.0 ±1.0
21. CL 991–P: Rain, PL–40–RW, 0.0238 gal., 0.07″, collected 1400, December 3, 1955, to 1400, December 14, 1955	319.0±24.0
22. CL 1033–P: Rain, PL–41–RW, 0.025 gal., 0.05″, collected 1400, December 14, 1955, to 1400, December 24, 1955	520.0±60.0
23. CL 1066–P: Rain, PL–42–RW, 4.34 gal., 2.63″, collected 1400, December 24, 1955, to 1600, February 3, 1956	29.0±3.0
24. CL 1088–P: Rain, PL–43–RW, 6.05 gal., 2.10″, collected 1600, February 3, 1956, to 1000, February 13, 1956	16.9±1.3
25. CL 1089–P: Rain, PL–44–RW, 4.10 gal., 2.95″, collected 1000, February 13, 1956, to 1400, February 27, 1956	32.2±1.9

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

Sample	*Sunshine units*
VI. Precipitation—Continued	
D. Pittsburgh, Pennsylvania—Continued	
26. CL 1090-P: Rain, PL-45-RW, 4.55 gal., 0.76″, collected 1400, February 27, 1956, to March 6, 1956	52.0±3.0
27. CL 1091-P: Rain, PL-46-RW, 6.60 gal., 3.13″, collected 1800, March 6, 1956 to 1200, March 24, 1956	28.0±2.0
28. CL 1092-P: Rain, PL-47-RW, 2.55 gal., 1.21″, collected 1200, March 24, 1956, to 1000, April 1, 1956	47.0±4.0
VII. Water other than precipitation:	
A. CL 1093-P: Tapwater, PL-51-TW, 13.2 gal., Pittsburgh, Pennsylvania, collected by Nuclear Science and Engineering Corporation, March 2, 1956, to March 13, 1956	1.33±.14
B. CL 732: Sea water, surface sample, 40 liters, Atlantic Ocean, collected at 48°49′ N., 48°07′ W., by U. S. Coast Guard, Station 5844, April 28, 1955 (depth of thermocline —100 meters):	
Total Sr-90 Activity: 4.3±0.3 dpm.	

VIII. Air concentration: Air filter samples provided by Mr. I. H. Blifford, Naval Research Laboratory, Washington, D. C. Collections were made on Army Chemical Corps Type V filters of 200 square inch area and of heavy asbestos fiber composition.

A. Sr-90 Surface Air Concentration, Washington, D. C.

CL No.	Collection period	Volume ($ft^3 \times 10^{-6}$)	DPM Sr-90/10^6 ft^3
204-D	Apr. 5-8, 1953	4.5	18.6± 0.7
204-A	Oct. 2-6, 1953	1.7	41.1± 3.0
204-B	Oct. 6-9, 1953	3.4	30.5± 1.1
130	Oct. 12-15, 1953	3.4	70.0±12.0
514-P	Apr. 3-5, 1954	2.92	91.0± 7.0
204E	Apr. 8-10, 1954	2.6	6.4± 0.2
204C	Apr. 9-11, 1954	1.7	125.0± 5.0
204F	Apr. 10-12, 1954	3.4	258.0± 6.0
515-P	Apr. 12-14, 1954	1.95	65.5± 4.6
204-G	Apr. 15-17, 1954	3.7	11.0± 0.5
204-H3	Apr. 17-19, 1954	2.8	20.7± 0.6
516-P	Apr. 29-May 1, 1954	3.0	32.2± 2.6
895-P	May 5-7, 1954	2.33	210.0±12.0
517-P	May 11-13, 1954	2.76	31.3± 2.2
896-P	May 17-19, 1954	2.59	120.0± 7.0
518-P	May 24-26, 1954	2.61	216.0±11.0
897-P	May 28-30, 1954	3.80	133.0± 7.0
519-P	June 1-3, 1954	2.90	68.3± 4.1
898-P	June 14-17, 1954	4.45	79.0± 6.0
899-P	June 23-26, 1954	3.79	51.0± 3.0
520-P	July 16-17, 1954	1.88	47.0± 2.4
521-P	July 24-26, 1954	2.56	73.5± 5.2
522-P	July 26-29, 1954	3.66	48.0± 3.9
900-P	July 30-Aug. 2, 1954	2.95	200.0±10.0
901-P	Aug. 2-7, 1954	5.41	59.0± 5.0
902-P	Aug. 7-9, 1954	2.92	210.0±13.0
903-P	Aug. 28-29, 1954	1.82	380.0±25.0
904-P	Oct. 1-3, 1954	3.39	112.0± 7.0
905-P	Oct. 5-8, 1954	3.56	104.0± 6.0
906-P	Oct. 16-18, 1954	2.69	198.0±14.0
907-P	Oct. 26-28, 1954	2.26	251.0±17.0
401-P	Nov. 1-3, 1954	2.9	120.0± 7.0
908-P	Nov. 7-8, 1954	1.15	225.0±14.0
909-P	Nov. 15-16, 1954	1.28	175.0±10.0
910-P	Nov. 22-24, 1954	1.96	194.0±11.0
402-P	Dec. 1-2, 1954	1.6	103.0± 4.0
411-P	Jan. 3-4, 1955	1.26	281.0± 6.0
412-P	Feb. 5-6, 1955	1.7	127.0± 5.0
413-P	Feb. 10-12, 1955	2.9	241.0±10.0
913-P	Feb. 17-18, 1955	1.51	191.0±11.0
523-P	Feb. 22-23, 1955	1.41	202.0±11.0
524-P	Mar. 3-4, 1955	1.76	270.0±13.0
525-P	Mar. 7-8, 1955	1.54	394.0±20.0
526-P	Mar. 13-14, 1955	1.07	267.0±16.0
527-P	Mar. 16-17, 1955	1.62	310.0±15.0
914-P	Mar. 21-23, 1955	2.27	98.0± 7.0
528-P	Mar. 22-23, 1955	1.74	393.0±20.0
529-P	Mar. 27-28, 1955	1.80	24.0± 5.0

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

VII. Air concentration—Continued

A. Sr–90 Surface Air Concentration, Washington, D. C.—Continued

CL No.	Collection period	Volume ($ft^3 \times 10^{-4}$)	DPM Sr-90/10^6 ft^3
773-P	Apr. 4-5, 1955	1.32	84.0± 4.0
774-P	Apr. 11-12, 1955	1.93	71.5± 3.3
775-P	Apr. 18-19, 1955	2.27	85.0± 6.0
776-P	Apr. 25-26, 1955	1.82	22.5± 1.4
777-P	May 2-3, 1955	1.34	709.0±52.0
778-P	May 10-11, 1955	1.54	265.0±12.0
779-P	May 17-18, 1955	1.37	478.0±16.0
780-P	May 24-25, 1955	1.69	755.0±33.0
917-P	June 16-17, 1955	1.43	710.0±40.0
918-P	Aug. 5-8, 1955	3.0	300.0±20.0
919-P	Aug. 12-16, 1955	4.51	49.0± 4.0
920-P	Aug. 19-22, 1955	3.5	124.0± 6.0
921-P	Aug. 26-29, 1955	3.6	226.0±16.0
922-P	Sept. 26-27, 1955	1.53	158.0± 9.0
923-P	Sept. 29-30, 1955	1.69	124.0± 8.0

B. Sr–90 Surface Air Concentration, Foreign Locations: There is considerable uncertainty in the air volumes of samples collected at Kodiak, Alaska, Port Lyautey, French Morocco, and Yokosuka, Japan, because the flow rate is not directly recorded. For the earliest reports of air filter data for these three locations, the rated flow rate times the total collection period was taken as the collected air volume. Because the flow rate falls off substantially as dust accumulates on the filter, those samples were overestimated in volume and thus the reported air concentration data were too low. It is considered that a better estimate of their air volume is provided by the average Washington, D. C. volumes for equivalent collection periods. On this basis, the relative air concentration data should be considerably improved, although their absolute value may be in error by as much as 50 percent or so. All the earlier reported air filter data for Kodiak, Port Lyautey, and Yokosuka have been estimated on this basis, and the new results are presented below.

1. KODIAK, ALASKA

CL No.	Collection period	Volume ($ft^3 \times 10^{-4}$)	DPM Sr-90/10^6 ft^3
924-P	May 27-June 3, 1952	~4.4	~4.8
926-P	June 5-July 1, 1952	~4.5	~6.7
925-P	June 11-17, 1952	~4.3	~9.5
927-P	July 8-16, 1952	~4.4	~6.8
928-P	July 24-29, 1952	~4.2	~4.9
929-P	Aug. 29-Sept. 4, 1952	~4.2	~1.1
930-P	Sept. 18-25, 1952	~4.2	~1.1
931-P	Oct. 9-16, 1952	~4.2	~1.0
932-P	Oct. 23-30, 1952	~4.2	0.7±0.2
131	Nov. 18-23, 1953	~4.2	~50
205C	Feb. 9-15, 1954	~4.3	~27
205D2	Feb. 15-18, 1954	~3.6	~2.2
205E	Feb. 18-22, 1954	~4.0	~10
933-P	Mar. 17-22, 1954	~4.2	~36
934-P	Apr. 19-26, 1954	~4.4	~61
935-P	May 17-24, 1954	~4.4	~48
936-P	June 14-21, 1954	~4.4	~90
937-P	July 19-26, 1954	~4.4	~31
939-P	Sept. 24-26, 1954	~3.0	~35
940-P	Oct. 15-18, 1954	~3.6	~6.1
403-P	Oct. 30-Nov. 1, 1954	~3.0	~21
941-P	Nov. 20-22, 1954	~3.0	~17
404-P	Dec. 1-2, 1954	~1.9	~180
942-P	Dec. 16-19, 1954	~3.6	~74
414-P	Jan. 1-2, 1955	~1.9	~240
415-P	Feb. 1-2, 1955	~1.9	~230
535-P	Mar. 1-3, 1955	~3.0	~71
781-P	Apr. 1-3, 1955	~3.0	~200
782-P	Apr. 30-May 2, 1955	~3.0	~62
783-P	June 30-July 1, 1955	~1.9	~180
943-P	Aug. 5-7, 1955	~3.0	~53
944-P	Sept. 1-3, 1955	~3.0	~140

Chicago sunshine results for period Dec. 1, 1955, to Aug. 1, 1956—Continued

VIII. Air concentration—Continued
B. Si-90 Surface Air Concentration—Continued

2. PORT LYAUTEY, FRENCH MOROCCO

206B	July 9-11, 1953	~3.0	~14
206C	July 11-13, 1953	~3.0	~54
206D	July 13-16, 1953	~3.6	~15
206A2	Sept. 30-Oct. 1, 1953	~1.9	~22
206E1	Nov. 2-9, 1953	~4.4	~26
405-P	Nov. 8-9, 1954	~1.9	~140
949-P	Nov. 21-22, 1954	~1.9	~180
406-P	Dec. 3-4, 1954	~1.9	~200
416-P	Jan. 4-6, 1955	~3.0	~53
530-P	Feb. 28-Mar. 2, 1955	~3.0	~500
531-P	Mar. 6-8, 1955	~3.0	~390
532-P	Mar. 16-18, 1955	~3.0	~280
533-P	Mar. 22-24, 1955	~3.0	~110
784-P	Apr. 1-3, 1955	~3.0	~390
950-P	Apr. 15-17, 1955	~3.0	~590
785-P	May 1-3, 1955	~3.0	~640
951-P	May 15-17, 1955	~3.0	~150
786-P	May 31-June 2, 1955	~3.0	~1300
952-P	June 14-16, 1955	~3.0	~310
953-P	June 29-July 1, 1955	~3.0	~130

3. YOKOSUKA, JAPAN

417-P	Feb. 1-3, 1955	~3.0	~150
534-P	Mar. 1-3, 1955	~3.0	~200
787-P	Apr. 1-3, 1955	~3.0	~12
788-P	May 1-3, 1955	~3.0	~270
789-P	June 1-3, 1955	~3.0	~110
945-P	Aug. 1-3, 1955	~3.0	~14
946-P	Aug. 15-17, 1955	~3.0	~170
947-P	Sept. 1-3, 1955	~3.0	~12
948-P	Sept. 23-25, 1955	~3.0	~70

Chicago sunshine results, final list (originally submitted to the Divisions of Biology and Medicine, USAEC, in letter report of August 3, 1956)

Sample — *Sunshine units*

I. Human bone: In all cases, date given corresponds to date of death or post mortem:
  A. United States: Furnished by Dr. Shields Warren, Cancer Research Institute, New England Deaconess Hospital, Boston, Massachusetts:
    1. CL 873-P: Combination of 2 samples, age range 65-67 years, 149.9 g ash, 58.3 g Ca — 0.026 ± 0.005
      a. No. 160739, 67 yrs., April 20, 1955, Massachusetts, tibia and fibula.
      b. No. 161622, 65 yrs., May 25, 1955, Massachusetts, femur.
    2. CL 874-P: Combination of 3 samples, age range 71-78 years, 306.2 g ash, 121.8 g Ca — 0.020 ± 0.005
      a. No. 161203, 71 yrs., May 10, 1955, Massachusetts, femur and tibia shanks.
      b. No. 161205, 72 yrs., May 10, 1955, Massachusetts, femur and tibia shanks.
      c. No. 161526, 78 yrs., May 21, 1955, Massachusetts, femur and tibia shanks.
    3. CL 1159-P: Age 51 years, New York, No. 162471, July 5, 1955, left tibia, 66.5 g ash, 5.85 g Ca — 2.26 ± 0.16
    4. CL 1154-P: Age 13 years, W. Buxton, Maine, A-56-13, January 16, 1956, rib and vertebral fragments, 17.50 g ash, 6.64 g Ca — 0.14 ± 0.01
    5. CL 1146-P: Age 2 9/12 years, Lewiston, Maine, A-56-16, January 18, 1956, rib and vertebral fragments, 8.86 g ash, 3.32 g Ca — 0.54 ± 0.05
    6. CL 1155-P: Age 13 7/12 years, Norfolk, Massachusetts, A-56-20, January 26, 1956, rib and vertebral fragments, 19.35 g ash, 7.47 g Ca — 0.36 ± 0.03

Chicago sunshine results, final list (originally submitted to the Divisions of Biology and Medicine, USAEC, in letter report of August 3, 1956)—Con.

Sample	*Sunshine units*
I. Human bone—Continued	
A. United States—Continued	
7. CL 1151–P: Combination of 2 samples, age range 7–10 years, 19.34 g ash, 6.62 g Ca	0.14±0.02
a. A–56–41, 7⁸⁄₁₂ yrs., Feb. 22, 1956, Concord, Massachusetts, rib and vertebral fragments.	
b. A–56–26, 9¹⁰⁄₁₂ yrs., Feb. 3, 1956, Brighton, Massachusetts, rib and vertebral fragments.	
8. CL 1147–P: Combination of 2 samples, age range 3–4 years, 14.34 g ash, 5.15 g Ca	0.32±0.03
a. A–56–25, 3⁸⁄₁₂ yrs., Feb. 3, 1956, Norwood, Massachusetts, vertebral fragments.	
b. A–56–46, 3¹⁰⁄₁₂ yrs., Feb. 23, 1956, Needham, Massachusetts, vertebral fragments.	
9. CL 1141–P: Combination of 2 samples, age range 3–6 days, 9.96 g ash, 3.78 g Ca	0.54±0.05
a. A–56–43, 3 days, Feb. 23, 1956, Boston, Massachusetts, rib and vertebral fragments.	
b. A–56–35, 6 days, Feb. 16, 1956, Lynn, Massachusetts, rib and vertebral fragments.	
10. CL 1150–P: Combination of 2 samples, age range 7–7½ years, 24.8 g ash, 8.23 g Ca	0.095±0.011
a. A–56–51, 7⁸⁄₁₂ yrs., Feb. 29, 1956, Canton, Massachusetts, rib and vertebral fragments.	
b. Barbara Mann, 7½ yrs., early 1956, Wallingford, Connecticut, rib and vertebral fragments.	
11. CL 1145–P: Combination of 3 samples, age range 1 to 2⁸⁄₁₂ years, 21.65 g ash, 9.29 g Ca	0.96±0.07
a. A–56–37, 1³⁄₁₂ yrs., Feb. 17, 1956, Warrick, Rhode Island, scalp, rib and vertebral fragments.	
b. A–56–64, 1⁵⁄₁₂ yrs., March 10, 1956, North Adams, Massachusetts, scalp, rib and vertebral fragments.	
c. A–56–31, 2⁸⁄₁₂ yrs., early 1956, Natick, Massachusetts, scalp, rib and vertebral fragments.	
12. CL 1148–P: Combination of 2 samples, age range 4 to 4⁷⁄₁₂ years, 18.08 g ash, 5.83 g Ca	0.17±0.01
a. A–56–40, 4 yrs., Feb. 19, 1956, Massachusetts, rib and vertebral fragments.	
b. A–56–75, 4⁷⁄₁₂ yrs., March 26, 1956, Hyannis, Massachusetts, rib and vertebral fragments.	
13. CL 1143–P: Combination of 2 samples, age range 5 weeks to 5 months, 12.11 g ash, 4.17 g Ca	1.13±0.12
a. A–56–71, 5 wks., March 20, 1956, Humarock, Massachusetts, scalp, rib and vertebral fragments.	
b. A–56–47, 5 mo., Feb. 25, 1956, Waltham, Massachusetts, scalp, rib and vertebral fragments.	
14. CL 1149–P: Combination of 3 samples, age range 4½–7 years, 29.1 g ash, 10.92 g Ca	0.21±0.03
a. A–56–49, 4⁵⁄₁₂ yrs., Feb. 20, 1956, Burlington, Vermont, rib and vertebral fragments.	
b. A–56–89, 5¹⁰⁄₁₂ yrs., Apr. 17, 1956, Fair Haven, Vermont, rib and vertebral fragments.	
c. A–56–27, 6¹¹⁄₁₂ yrs., Feb. 3, 1956, Barre, Vermont, rib and vetebral fragments.	
15. CL 1142–P: Combination of 2 samples, age range 7–9 days, 6.42 g ash, 2.28 g Ca	0.47±0.04
a. A–56–54, 7 days, March 3, 1956, Brockton, Massachusetts, rib and vertebral fragments.	
b. A–56–60, 9 days, March 7, 1956, Newton, Massachusetts, rib and vertebral fragments.	
16. CL 1158–P: Age 3⁸⁄₁₂ years, New Orleans, Louisiana, A–56–57, March 5, 1956, vertebral fragments, 4.21 g ash, 2.41 g Ca	0.29±0.04

Chicago sunshine results, final list (originally submitted to the Divisions of Biology and Medicine, USAEC, in letter report of August 3, 1956)—Con.

Sample	*Sunshine units*
I. Human bone—Continued	
A. United States—Continued	
17. CL 1157–P: Age 19 years, Woonsocket, Rhode Island, A–56–61, March 8, 1956, rib and vertebral fragments, 25.4 g ash, 9.24 g Ca	0. 060±0. 009
18. CL 1144–P: Combination of 2 samples, age range 6–7 months, 19.15 g ash, 7.15 g Ca	0. 60±0. 05
a. A–56–87, 6 mo., Apr. 13, 1956, Newport, Rhode Island, scalp, rib and vertebral fragments.	
b. A–56–56, 7 mo., March 5, 1956, Boston, Massachusetts, scalp, rib and vertebral fragments.	
19. CL 1153–P: Combination of 3 samples, age range 12–13 years, 37.5 g ash, 13.85 g Ca	0. 051±0. 006
a. Donna Rice, 12 yrs., early 1956, Athol, Massachusetts, rib and vertebral fragments.	
b. A–56–95, 12$\frac{4}{12}$ yrs., Apr. 23, 1956, Haverhill, Massachusetts, rib and vertebral fragments.	
c. A–56–59, 13 yrs., March 7, 1956, Boston, Massachusetts, rib and vertebral fragments.	
20. CL 1156–P: Combination of 2 samples, age range 14–15 years, 39.0 g ash, 14.7 g Ca	0. 056±0. 008
a. A–56–94, 14$\frac{3}{12}$ yrs., Apr. 24, 1956, Whitinsville, Massachusetts, rib and vertebral fragments.	
b. A–56–65, 15 yrs., March 13, 1956, Greenwood, Massachusetts, rib and vertebral fragments.	
20. CL 1152–P: Combination of 2 samples, age range 10½–11½ years, 24.5 g ash, 9.03 g Ca	0. 14±0. 02
a. A–56–80, 10$\frac{5}{12}$ yrs., Apr. 2, 1956, Norwich, Connecticut, rib, sternum and vertebral fragments.	
b. A–56–84, 11$\frac{4}{12}$ yrs., Apr. 6, 1956, Norwood, Massachusetts, rib, sternum and vertebral fragments.	
B. Foreign:	
1. CL 765–P: Age 56 years, Felixlandia, Minas Gerais, Brazil, furnished by Dr. Jairo Camara, Departamento de Anatomia, Faculdade de Medicina, Universidade de Minas Gerais, Belo Horizonte, Brazil, collection arranged by Dr. R. B. Watson, Rockefeller Foundation, Rio de Janeiro, Brazil, August 1, 1955, 25.5 g ash, 9.29 g Ca	0. 037±0. 017
2. CL 1160–P: Age 2½ years, Brazil, furnished by Dr. Shields Warren, Cancer Research Institute, New England Deaconess Hospital, Boston, Massachusetts, James Tussin, vertebral fragments, 7.15 g ash, 2.197 g Ca	0. 50±0. 05
II. Animal bone:	
A. CL 1162–P: Lamb bones, age 6 months, Los Banos, Philippines, furnished by Dr. Lyle T. Alexander, U. S. Department of Agriculture, Beltsville, Maryland, Beltsville No. 56506, killed April 30, 1956, 165.5 g ash, 61.36 g Ca	1. 90±0. 11
III. Milk:	
A. United States: Fresh Milk from Chicago Dairies:	
1. CL 1071–P: Pure Milk Association, purchased May 7, 1956, 22.6 g ash, 3.64 g Ca	1. 65±0. 11
B. Foreign:	
1. CL 1101–P: "Vigor" powdered, Sao Paulo, Brazil, Beltsville No. 56449, furnished by Dr. Lyle T. Alexander, U. S. Department of Agriculture, Beltsville, Maryland, 19.94 g ash, 4.14 g Ca	0. 90±0. 06
2. CL 1131–P: Nestle powdered whole, Magdelena, Argentina, Beltsville No. 56476, furnished by Dr. Lyle T. Alexander, 50.0 g ash, 10.9 g Ca	2. 72±0. 15

Chicago sunshine results, final list (originally submitted to the Divisions of Biology and Medicine, USAEC, in letter report of August 3, 1956)—Con.

Sample	*Sunshine units*
III. Milk—Continued	
B. Foreign—Continued	
3. CL 1132–P: Powdered whole, Trenque Lauquen, Argentina, Beltsville No. 56477, furnished by Dr. Lyle T. Alexander, 52.6 g ash, 9.20 g Ca	1. 22±0. 11
4. CL 1133–P: Powdered skim, Trenque Lauquen, Argentina, Beltsville No. 56478, furnished by Dr. L. T. Alexander, 64.3 g ash, 10.73 g Ca	1. 00±0. 08
IV. Soil:	
A. Furnished by Dr. Lyle T. Alexander, Soil Survey Laboratory, U. S. Department of Agriculture, Beltsville, Maryland:	
1. CL 1164–P: Oslo, Norway, Beltsville No. 55781, December 6, 1954, electrodialysis extraction of 4 lbs. soil, 0–2″ depth, 19.29 g oxalate, 7.23 g oxide, 5.16 g Ca	1. 20±0. 10
2. CL 1165–P: Oslo, Norway, Beltsville No. 55782, December 6, 1954, electrodialysis extraction of 4 lbs. soil, 0–2″ depth, 23.06 g ocalate, 8.82 g oxide, 6.30 g Ca	0. 99±0. 06
3. CL 1166–P: Oslo, Norway, Beltsville No. 55783, December 6, 1954, electrodialysis extraction of 4 lbs. soil, 0–2″ depth, 17.44 g oxalate, 6.66 g oxide, 4.76 g Ca	1. 57±0. 08
4. CL 1167–P: Aden, Saudi Arabia, Beltsville No. 55787, February 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 3.84 g oxalate, 1.49 g oxide, 1.06 g Ca	0. 69±0. 09
5. CL 1168–P: Madras, India, Beltsville No. 55790, February 14, 1955, elecrodialysis extraction of 4 lbs. soil, 0–4″ depth, 6.73 g oxalate, 2.60 g oxide, 1.86 g Ca	4. 5±0. 5
6. CL 1169–P: Madras, India, Beltsville No. 55791, February 14, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 4.00 g oxalate, 1.54 g oxide, 1.10 g Ca, (less 1 g Ca added during last step of electrodialysis procedure)	36. 0±2. 0
7. CL 1163–P: Durban, Natal, South Africa, Beltsville No. 55778, February 15, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 5.74 g oxalate, 2.18 g oxide, 1.56 g Ca (less 1 g Ca added during last step of electrodialysis procedure)	15. 0±0. 08
8. CL 1170–P: Perth, Australia, Beltsville No. 55839, February 15, 1955, electrodialysis extraction of 4 lbs. soil, 0–4″ depth, 4.49 g oxalate, 1.72 g oxide, 1.23 g Ca (less 1 g Ca added during last step of electrodialysis procedure)	14. 7±1. 1
V. Precipitation: Samples collected by Nuclear Science and Engineering Corporation, Pittsburgh, Pennsylvania:	
	dpm/gal
A. CL 1171–P: Rain, PL–48–RW, 4.1 gal., 1.80″, collected 1000, April 1, 1956, to 0900, April 7, 1956	35. 0±2. 0
B. CL 1172–P: Rain, PL–49–RW, 1.78 gal., 0.87″, collected 0900, April 7, 1956, to 1130, April 21, 1956	77. 0±5. 0
C. CL 1173–P: Rain, PL–50–RW, 3.46 gal., 1.50″, collected 1130, April 21, 1956, to 2000, April 30, 1956	41. 0±4. 0
D. CL 1174–P: Rain, PL–52–RW, 2.29 gal., 2.63″, collected 2000, April 30, 1956, to 2000, May 14, 1956	8. 09±5. 0
E. CL 1175–P: Rain, PL–53–RW, 5.29 gal., 2,57″, collected 2000, May 14, 1956, to 2000, May 28, 1956	74. 0±3. 0
F. CL 1176–P: Rain, PL–54–RW, 0.40 gal., 0.38″, collected 2000, May 28, 1956, to 1800, May 31, 1956	38. 0±3. 0
G. CL 1177–P: Rain, PL–55–RW, 1.46 gal., 0.84″, collected 1800, May 31, 1956, to 2000, June 4, 1956	66. 0±3. 0
H. CL 1178–P: Rain, PL–56–RW, 3.41 gal., collected 2000, June 4, 1956, to 1000, June 15, 1956	57. 0±3. 0

Chicago sunshine results, final list (originally submitted to the Divisions of Biology and Medicine, USAEC, in letter report of August 3, 1956)—Con.

Sample	*Sunshine units*
VI. Irrigation water:	
A. CL 981–P: 4.84 gal., Brawley, California, furnished by Edward Noble, Southwestern Irrigation Field Station, Brawley, California, collection arranged by Dr. L. T. Alexander, January 5, 1956	1.5±0.7
VII. Antarctic Snow Cores, 1956: Collected by E. E. Goodale, U. S. Weather Bureau.	

CL No.	Depth	Location [1]	Volume (liters)	dpm/liter
1102	0 to 5 inches	McMurdo Sound	7.90	14.3±1.4
1103	5 to 11 inches	do	7.92	1.1±0.1
1117	0 to 6 inches	Kainan Bay	7.69	1.9±0.2
1118	6 to 12 inches	do	7.19	1.7±0.2
1119	12 to 18 inches	do	7.00	0.40±0.08
1120	18 to 24 inches	do	7.00	0.2
1123	36 to 42 inches	do	6.90	0.2

[1] McMurdo Sound: Location: 77°51′S, 166°35′E; Collection Date: Mar. 7, 1956. Kainan Bay: Location: 78°10′S, 162°30′W; Collection Date: Jan. 1, 1956.

Sample	*dpm/10⁶ ft³*
VIII. Surface Air Concentration: Air filter samples provided by Mr. I. H. Blifford, Naval Research Laboratory, Washington, D. C.	
A. Air filters, Washington, D. C.:	
1. CL 915–P: Collected June 2–3, 1955, volume 1.65×10^6 ft³, total dpm 224±12	1350. ±7.0
2. CL 916–P: Collected June 8–10, 1955, volume 3.17×10^6 ft³, total dpm 66±5	21.0±2.0
3. CL 1052–P: Six small filters, collected February 23–28, 1955, 0.258×10^6 ft³ total air volume	244.0±29.0
B. Air filters, Kodiak, Alaska:	
1. CL 938–P: Collected August 16–23, 1954, $\sim 4.4\times 10^6$ ft³	~33
C. Air filters, Pearl Harbor, T. H.:	
1. CL 1053–P: Fifteen small filters, collected January 1–20, 1954, 0.645×10^6 ft³ total air volume	34.0±4.5
2. CL 1054–P: Ten small filters, collected January 21–31, 1954, 0.43×10^6 ft³ total air volume	51.9±6.0
3. CL 1055–P: Twelve small filters, collected February 1–14, 1954, 0.52×10^6 ft³ total air volume	41.6±4.8
4. CL 1056–P: Twelve small filters, collected February 15–28, 1954, 0.52×10^6 ft³ total air volume	25.4±3.8

Naval Research Laboratory

Information on Fallout

As prepared by Luther B. Lockhart, Jr. (see biography p. 324), Head, High Polymers Branch, Chemistry Division

(Enclosure: (1) Abstract of report entitled, "Atmospheric Radioactivity Along the 80th Meridian, 1956"; (2) NRL Memo Report 626 and monthly letter reports entitled, "Radioactivity of Air and Fallout Samples Collected at Sites on the 80th Meridian.")

1. The U. S. Naval Research Laboratory has had in effect since 1948 a long-range program on the radioactivity of the air. This program has included the study of methods of collecting and estimating both the natural and fission product activity of the air and also the routine measurement of the gross B-activity of the air since early 1950. Documents relating to atmospheric radioactivity and fallout include the following:

(*a*) NRL Report 4036 (September 1952), "On the Natural Radioactivity in the Air," I. H. Blifford, Jr., L. B. Lockhart, Jr., and H. B. Rosenstock.

(*b*) NRL Report 4069 (October 1952), "The Collection of Long-lived Natural Radioactive Products from the Atmosphere," P. King, L. B. Lockhart, Jr., et al.

(*c*) NRL Report 4607 (Nov. 4, 1955), "Relationship between the Air Concentration of Radioactive Fission Products and Fallout," I. H. Blifford, Jr., L. B. Lockhart, Jr., and R. A. Baus.

(*d*) NRL Report 4760 (June 4, 1956), "Radioactivity of the Air," I. H. Blifford, Jr., H. Friedman, L. B. Lockhart, Jr., and R. A. Baus.

(*e*) NRL Memo Report 626 (August 1956), "Radioactivity of Air and Fallout Samples Collected on the 80th Meridian," I. H. Blifford, Jr., and L. B. Lockhart, Jr.

(*f*) Supplementary monthly letter reports entitled, "Radioactivity of Air and Fallout Samples Collected on the 80th Meridian."

(*g*) Publication in Science, vol. 123, No. 3208, pp. 1120–21 (1956), "Collection of Atomic Bomb Debris from the Atmosphere by Impaction on Screens," I. H. Blifford, Jr., L. B. Lockhart, Jr., and R. A. Baus.

(*h*) Publication in Science, vol. 123, No. 3198, pp. 619–622 (Apr. 13, 1956), "Fallout Dosages at Washington, D. C.," by I. H. Blifford, Jr., and H. B. Rosenstock.

(*i*) Publication in Nature, vol. 177, pp. 990–992 (May 26, 1956), "Relationship Between Air Concentration of Radioactive Fission Products and Fallout," by I. H. Blifford, Jr., L. B. Lockhart, Jr., and R. A. Baus.

2. There is available some unpublished data on the subject of fission product concentration in the air at various sites along the 80th meridian which is being collected through a joint NRL-AEC-Weather Bureau program. An abstract of a report in preparation is attached (enclosure (1)). The gross measurements of the β-activity of the air are reported in NRL Memo Report 626 and subsequent monthly letter reports (enclosure (2)). Preliminary radiochemical analyses have been performed on a few weekly air filter collections of radioactivity from Peru, Guayaquil, Panama, and Washington. The results of these analyses are summarized in Table I. Sr–90 data are not available at present but will be included when the necessary low-level counting equipment is put into operation.

3. A single experimental collection at Washington, D. C., of "fallout" and "rainout" over a 2-week period has been analyzed and the results reported in table II. Other information is available in rough form on the gross β-activity (fission products) of collections made by the air-filter, cloth-screen, and gummed-film methods at several locations along the 80th meridian. A comparison of weekly averages of the relative gross fission product concentrations in the air at ground level at a number of locations along the 80th meridian is available which indicates that this activity is concentrated in the mid-latitudes of the Northern Hemisphere.

LUTHER B. LOCKHART, Jr.,
Head, High Polymers Branch, Chemistry Division.

TABLE I.—*Preliminary radiochemical analyses of some air-filter collections*

[Atoms per cu. ft. of air [1]]

	Ce^{141}	Sr^{89}	Ce^{144}	Cs^{137}
Washington, D. C.:				
June 4–10, 1956	Low	6×10^2	3.3×10^3	1.7×10^4
July 10–15, 1956	Low	2.2×10^3	2.5×10^3	3.2×10^4
Aug. 1–6, 1956	70	1.8×10^3	1.7×10^3	2.3×10^4
Miraflores, C. Z.:				
July 9–15, 1956	Low	Low	1×10^2	1×10^4
July 30–Aug. 5, 1956	Low	4×10^2	1.0×10^3	8×10^3
Guayaquil, Ecuador:				
July 9–15, 1956	20	1.6×10^2	3×10^2	5×10^3
July 30–Aug. 5, 1956	20	3×10^2	5×10^2	5×10^3
Sept. 3–9, 1956	2.7×10^2	2.6×10^2	5×10^2	5×10^3
Lima, Peru:				
Oct. 1–7, 1956	1.2×10^2	1.9×10^2	5.6×10^2	1.1×10^4
Oct. 8–14, 1956	1.0×10^3	1.2×10^3	1.8×10^3	2.0×10^4

[1] Arranged in ascending order of half lives to show excess of longerlived fission products. Corrected for decay to middle of collecting period.

TABLE II.—*Analysis of radioactivity deposited at NRL during period Mar. 12–26, 1957*

[Collecting area 64 sq. ft. Rainfall during period 1.06″]

Isotope	Half life	D/m [1]	D/m/sq. ft./day [1]	Atoms/sq. ft./day
Ba-140	12.8 d.	11,600	13	3.5×10^5
Ce-141	28 d.	21,600	24	1.4×10^6
Sr-89	54 d.	7,000	8	9×10^5
Ce-144	275 d.	11,600	13	7.4×10^6
Sr-90	28 yr.	890	1	2×10^7
Cs-137	37 yr.	1,900	2	6×10^7
Pb-210 [2]	22 yr.	990	1	2×10^7

[1] Corrected for decay to 3/26/57.
[2] Natura. radioactivity resulting from radon decay.

ABSTRACT

Measurements of atmospheric radioactivity and fallout at a number of sites along the 80th meridian (west) are reported for the year 1956. These results were obtained through the combined efforts of the United States Naval Research Laboratory and the Meteorological Services of Chile, Peru, and Ecuador with the cooperation of the United States Weather Bureau and the United States Atomic Energy Commission.

Radioactivity levels at the various sites during 1956 are reported for three different collecting systems: air filters, cloth screens, and gummed films. Extremely wide variations in the gross radioactivity of fission products in the air have been noted, with the highest levels occurring in the Northern Hemisphere. The presence of some of the peaks of activity at various localities has been correlated with known atomic explosions.

Radiochemical techniques are being developed for the routine analysis of collections for certain isotopes. Preliminary results of such analyses of a few filter collections indicate that the air at all of the collecting sites contains excessive quantities of the longer-lived fission products.

RADIOACTIVITY OF AIR AND FALLOUT SAMPLES COLLECTED ON THE 80TH MERIDIAN

By I. H. Blifford, Jr. and L. B. Lockhart, Jr., August 1956,
Naval Research Laboratory, Washington 25, D. C.

ABSTRACT

A brief review is given of the status of the project for collecting atmospheric radioactivity samples at various sites along the 80th meridian. Radioactivity data for the months of May and June 1956 are presented.

Problem status: This is an interim report; work on this problem is continuing.

Authorization: NRL Problem A02–13. Project No. NR 612,130.

In accordance with arrangements between the meteorological services of Chile, Equador, and Peru and the United States Weather Bureau, the United States Naval Research Laboratory, in participation with the United States Atomic Energy Commission, is supplying equipment for collecting fission product radioactivity in the air and as fallout. The collected samples are being returned to Washington, D. C., for measurement.

This report is a brief review of the status of the work as of August 1, 1956, and includes a summary of the radioactive assay of samples collected through June 30, 1956. Monthly bulletins will be provided in the future as promptly as possible after the measurements have been completed and the data tabulated.

The following collecting devices are being supplied: (1) small pumps and filter papers for sampling the atmosphere, (2) standard gummed paper fallout devices developed by the AEC, and (3) experimental collectors using cloth screens attached to wind vanes. Table 1 shows the status of each of the present and proposed stations. Supplies for approximately 3 months' operation were sent in

the initial shipment to each station and an additional 6 months' supply will be forwarded in August.

It may be of interest to note that NRL is operating radioactivity measurement equipment of other types at United States Naval bases in Hawaii, Japan, Alaska, and the Philippine Islands, and at Little America, Antarctica (in cooperation with the United States Weather Bureau). Similar measurements are being made in Brazil in cooperation with the National Research Council of Brazil. Preliminary data indicate that fission product radioactivity in the air at Little America was about 3 percent of that at Washington, D. C., during May and June 1956.

On receipt at Washington, D. C., each sample is ignited in a furnace at 700° C. and the residue assayed for radioactivity (assuming a beta energy of 1 MeV) using an end-window Geiger counter. Corrections for geometry and sample weight are made. No correction is made for the loss of volatile products upon ignition. In order to allow sufficient time for receipt of the samples and for the decay of the short-lived fission products and natural radioactivities, each sample is counted on the fourteenth day after collection.

The air filter results are given in disintegrations per minute (d/m) of fission product β-activity per cubic foot of air and are presented in tables 2 and 3 for all samples received for May and June 1956. The filter papers retain very nearly 100 percent of the fission particulates and have negligible natural radioactivity at the time of measurement. The airflow rate is 40 cubic feet per minute. Reduction in airflow due to plugging of the filter is negligible at this flow rate. The activities of individual filters having measurable radioactivity but less than approximately 0.0020 d/m per ft.^{3} are tabulated as *traces*.

The fallout measurements using gummed films are reported in disintegrations per minute of fission product β-activity per square foot and are given in tables 4 and 5. No corrections for the collection efficiency of the gummed films have been made. Unexposed films have been found to have no measurable radioactivity. The activities of individual samples containing radioactivity but with less than 40 d/m per ft.^{2} are tabulated as *traces*.

For the month of June 1946, the residue from the ignition of all of the gummed papers from each station was combined and a weighted fraction assayed for radioactivity on July 25, 1956. The total fallout radioactivity during June 1956 for each location is given in table 6. It is to be observed that the activity of the Guayaquil sample was extremely low and that it was not practical for it to be measured to the same accuracy as for samples from other locations.

The cloth screen results are presented in total disintegrations per minute of fission product β-activity and are given in tables 7 and 8 for May and June 1956. A correction has been made for a small amount of natural radioactivity in the unexposed cloth. The activities of individual samples containing radioactivity but with less than 40 d/m are tabulated as *traces*.

The tabulated values for samples collected over periods longer than 24 hours represent the total measured activity divided by the number of 24-hour periods in the collection. Days for which samples were not available have been indicated by a dash. The maximum counting error (standard deviation) is not greater than ±20 percent for any measurement. For the majority of the samples, the counting error is between 5 and 10 percent.

TABLE 1.—*Date of start of radioactivity collections*

Location	Filter	Type of collector	
		Gummed paper	Cloth screen
Punta Arenas	(1)	(2)	(2).
Santiago de Chile	(1)	July 15, 1956	July 15, 1956
Lima	(1)	May 29, 1956	May 29, 1956
Guayaquil	May 24, 1956	May 24, 1956	May 24, 1956
Quito	(2)	(2)	(2).
Panama, C. Z.	June 1, 1956	June 1, 1956	June 1, 1956
Puerto Rico	(1)	(1)	(1).
Miami, Fla.	(1)	(1)	(1).
Washington, D. C.	May 1, 1956	May 1, 1956	May 1, 1956
Newfoundland	(1)	(1)	(1).
Thule, Greenland	(1)	(1)	(1).

[1] Not shipped.
[2] Shipped but not in operation on Aug. 1, 1956.

TABLE 2.—*Daily record of fission product β-activity collected by the filter method, May 1956*

[d/m/cu. ft.]

Day	Guayaquil	Washington, D. C.	Day	Guayaquil	Washington, D. C.
1	-----	-----	17	-----	.054
2	-----	0.11	18	-----	.005
3	-----	.052	19	-----	[1].057
4	-----	.053	20	-----	[1].057
5	-----	[1].034	21	-----	[1].057
6	-----	[1].034	22	-----	.090
7	-----	[1].034	23	-----	.11
8	-----	.058	24	(2)	.026
9	-----	.11	25	0.002	.098
10	-----	.14	26	0	[1].11
11	-----	.044	27	(2)	[1].11
12	-----	[1].060	28	(2)	[1].11
13	-----	[1].060	29	(2)	.036
14	-----	[1].060	30	.002	.024
15	-----	.056	31	(2)	-----
16	-----	.034			

[1] 3-day collection. [2] Trace.

TABLE 3.—*Daily record of fission product β-activity collected by the filter method, June 1956*

[In d/m/cu. ft.]

Day	Guayaquil	Panama, C. Z.	Washington, D. C.	Day	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0.004	-----	0.047	16	-----	0.009	[1]0.095
2	.002	0.018	[1].037	17	0.018	.010	[1].095
3	.002	.046	[1].037	18	.003	.007	[1].095
4	(2)	.030	[1].037	19	.005	.005	.14
5	.003	.024	.028	20	.013	.006	.075
6	(2)	.005	.050	21	-----	.008	-----
7	(2)	(2)	.073	22	.013	.008	.068
8	(2)	(2)	.074	23	.011	.007	[1].038
9	.004	.005	[1].055	24	.010	.006	[1].038
10	.004	(2)	[1].055	25	.084	.004	[1].038
11	.004	0	[1].055	26	.016	.004	.036
12	.004	.003	.071	27	.007	.024	.036
13	.004	0	.13	28	.014	.030	.054
14	.004	.011	.17	29	.006	.026	.059
15	.005	.018	.16	30	.003	.024	[1].059

[1] 3-day collection. [2] Trace.

TABLE 4.—*Daily record of the fission product β-activity collected by the gummed paper method, May 1956*

[In d/m/sq. ft.]

Day	Lima	Guayaquil	Washington, D. C.	Day	Lima	Guayaquil	Washington, D. C.
1	-----	-----	-----	17	-----	-----	(1)
2	-----	-----	-----	18	-----	-----	150
3	-----	-----	110	19	-----	-----	[2]78
4	-----	-----	(1)	20	-----	-----	[2]78
5	-----	-----	[2]60	21	-----	-----	[2]78
6	-----	-----	[2]60	22	-----	-----	(1)
7	-----	-----	[2]60	23	-----	-----	130
8	-----	-----	140	24	-----	(2)	90
9	-----	-----	0	25	-----	(2)	(1)
10	-----	-----	110	26	-----	(2)	[1]35
11	-----	-----	1,100	27	-----	(2)	[1]35
12	-----	-----	[2]35	28	-----	(2)	[1]35
13	-----	-----	[2]35	29	(2)	(1)	0
14	-----	-----	[2]35	30	(2)	100	70
15	-----	-----	0	31	140	(2)	-----
16	-----	-----	490				

[1] Trace. [2] 3-day collection.

TABLE 5.—*Daily record of the fission product β-activity collected by the gummed-paper method, June 1956*

[In d/m/sq. ft.]

Day	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	(1)	0	----------	0
2	40	(1)	100	[2] 61
3	(1)	(1)	200	[2] 61
4	0	(1)	140	[2] 61
5	0	0	270	0
6	70	0	100	0
7	(1)	0	110	(1)
8	40	(1)	100	(1)
9	40	0	40	[2] 20
10	40	(1)	70	[2] 20
11	(1)	0	100	[2] 20
12	740	0	40	50
13	(1)	(1)	50	0
14	90	(1)	200	680
15	200	(1)	(1)	90
16	910	----------	140	[2] 100
17	130	(1)	80	[2] 100
18	40	0	40	[2] 100
19	----------	(1)	50	60
20	70	0	(1)	260
21	0	----------	70	200
22	0	(1)	40	90
23	40	0	(1)	[2] 47
24	40	(1)	(1)	[2] 47
25	(1)	(1)	(1)	[2] 47
26	(1)	(1)	(1)	170
27	0	(1)	40	(1)
28	0	0	(1)	60
29	(1)	0	440	60
30	(1)	0	(1)	[2] 20

[1] Trace.
[2] 3-day collection.

TABLE 6.—*Total fission product β-activity of gummed paper samples collected during June 1956*

[In d/m/sq. ft.]

Location:	*Total activity*
Lima	840
Guayaquil	[1] 130
Panama, C. Z.	1500
Washington, D. C.	1300

[1] Counting accuracy ±40 percent.

TABLE 7.—*Daily record of fission product β-activity collected by the cloth screen method, May 1956*

[In d/m]

Day	Lima	Guayaquil	Washington, D. C.	Day	Lima	Guayaquil	Washington, D. C.
1	----------	----------	----------	17	----------	----------	----------
2	----------	----------	----------	18	----------	----------	310
3	----------	----------	90	19	----------	----------	[1] 190
4	----------	----------	0	20	----------	----------	[1] 190
5	----------	----------	[1] 190	21	----------	----------	[1] 190
6	----------	----------	[1] 190	22	----------	----------	(2)
7	----------	----------	[1] 190	23	----------	----------	350
8	----------	----------	980	24	----------	----------	590
9	----------	----------	170	25	----------	(2)	190
10	----------	----------	700	26	----------	(2)	[1] 370
11	----------	----------	570	27	----------	(2)	[1] 370
12	----------	----------	[1] 500	28	----------	(2)	[1] 370
13	----------	----------	[1] 500	29	(2)	(2)	90
14	----------	----------	[1] 500	30	(2)	(2)	180
15	----------	----------	200	31	(2)	(2)	----------
16	----------	----------	260				

[1] 3-day collection. [2] Trace.

TABLE 8.—*Daily record of fission product β-activity collected by the cloth-screen method, June 1956*

[In d/m]

Day	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0	0	----------	90
2	0	40	(1)	[2] 170
3	0	(1)	390	[2] 170
4	0	(1)	140	[2] 170
5	0	(1)	0	0
6	370	270	(1)	0
7	0	0	0	110
8	0	0	0	(1)
9	60	50	0	[2] 170
10	540	850	0	[2] 170
11	0	570	0	[2] 170
12	0	0	0	60
13	170	170	0	(1)
14	1,700	70	0	150
15	1,200	130	0	570
16	2,500	----------	100	[2] 560
17	1,900	130	(1)	[2] 560
18	780	220	0	[2] 560
19	----------	70	0	700
20	1,000	130	0	170
21	740	----------	0	2,100
22	180	330	0	150
23	460	110	0	[2] 160
24	970	360	0	[2] 160
25	220	100	0	[2] 160
26	90	290	0	0
27	230	50	0	0
28	(1)	(1)	270	0
29	100	90	0	240
30	180	130	0	[2] 220

[1] Trace.
[2] 3-day collection.

NAVAL RESEARCH LABORATORY,
Washington 25, D. C., September 10, 1956.

Subject: Radioactivity of air and fallout samples collected at sites on the 80th meridian during July 1956; NRL problem A02-13, project No. NR 612130; interim report on.

Reference: (a) NRL memo report 626 of August 1956 entitled, "Radioactivty of Air and Fallout Samples Collected on the 80th Meridian," by I. H. Blifford, Jr., and L. B. Lockhart, Jr.

Figure 1: Daily record of fission product β-activity collected by the filter method.

Figure 2: Daily record of fission product β-activity collected by the cloth screen method.

Figure 3: Daily record of fission product β-activity collected by the gummed paper method.

Figure 5: Total fission product β-activity of gummed paper samples collected during July 1956.

1. Radioactivity measurements of air and fallout samples collected at various sites along the 80th meridian (west) during the month of July 1956 are presented in figures 1 through 4.

2. A discussion of the method of analysis and the accuracy of the results is given in reference (a). No attempt is being made at this time to interpret the data collected.

H. FRIEDMAN
(For I. H. Blifford, Jr.).
L. B. LOCKHART, Jr.

FIGURE 1.—*Daily record of fission product β-activity collected by the filter method, July 1956*

[In d/m/cu. ft.]

Day	Guayaquil	Panama, C. Z.	Washington, D. C.	Day	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0.005	0.009	[1] 0.059	17	0.018	----------	0.045
2	.014	.008	[1] .059	18	.016	0.034	.053
3	.005	.005	.28	19	.010	.046	.063
4	.005	(2)	.032	20	.016	.079	.040
5	.008	----------	.032	21	.005	.040	[1] .032
6	.005	.005	.032	22	.005	.040	[1] .032
7	.003	.007	[1] .028	23	.020	.028	[1] .032
8	.002	.010	[1] .028	24	.009	.024	[1] .039
9	.004	.009	[1] .028	25	.016	.018	.038
10	.024	.009	.048	26	.024	.031	.050
11	.026	.016	.026	27	(2)	.24	.051
12	.010	.014	.049	28	.022	.010	[1] .061
13	.012	----------	.065	29	.018	.016	[1] .061
14	.020	.018	[1] .040	30	.030	.018	[1] .061
15	.009	.020	[1] .040	31	.050	.010	.026
16	.018	.028	[1] .040				

[1] 3-day collection.
[2] Trace.

FIGURE 2.—*Daily record of fission product β-activity collected by the cloth screen method, July 1956*

[In d/m]

Day	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	----------	50	300	160	[1] 220
2	----------	500	470	180	[1] 220
3	----------	200	90	0	----------
4	----------	300	300	0	700
5	----------	830	270	0	700
6	----------	180	130	0	540
7	----------	200	100	0	[1] 630
8	----------	240	70	0	[1] 630
9	----------	290	100	(2)	[1] 630
10	----------	160	350	0	1,500
11	----------	630	350	0	50
12	----------	2,500	190	80	0
13	0	1,500	330	0	70
14	0	2,600	780	0	[1] 100
15	(2)	2,700	230	----------	[1] 100
16	0	0	920	270	[1] 100
17	0	2,300	830	0	810
18	80	480	270	570	440
19	0	(2)	160	230	270
20	0	900	990	1,000	290
21	100	960	210	1,400	[1] 1,700
22	0	2,200	270	210	[1] 1,700
23	(2)	0	0	350	[1] 1,700
24	80	990	260	210	1,100
25	0	850	450	(2)	580
26	0	0	700	(2)	240
27	----------	3,100	780	430	280
28	0	1,500	190	290	[1] 940
29	0	1,200	1,100	0	[1] 940
30	0	2,900	420	210	[1] 940
31	0	680	960	(2)	250

[1] 3-day collection.
[2] Trace.

FIGURE 3.—*Daily record of fission product β-activity collected by the gummed paper method, July 1956*

[In d/m/sq. ft.]

Day	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	----------	(1)	0	290	[2] 20
2	----------	70	(1)	350	[2] 20
3	----------	(1)	0	100	1,200
4	----------	(1)	0	70	200
5	----------	(1)	40	40	200
6	----------	(1)	(1)	(1)	220
7	----------	(1)	0	50	[2] 190
8	----------	(1)	(1)	(1)	[2] 190
9	----------	(1)	0	160	[2] 190
10	----------	50	0	100	370
11	----------	0	0	40	0
12	----------	190	(1)	190	0
13	(1)	120	0	(1)	(1)
14	(1)	120	0	180	[2] 170
15	(1)	100	0	160	[2] 170
16	(1)	290	1,000	100	[2] 170
17	120	(1)	140	50	210
18	(1)	100	0	580	300
19	0	(1)	0	1,000	(1)
20	0	100	(1)	600	60
21	(1)	80	(1)	1,500	[2] 930
22	(1)	110	(1)	160	[2] 930
23	0	660	2,200	110	[2] 930
24	250	80	(1)	340	580
25	120	(1)	(1)	210	320
26	(1)	60	(1)	110	110
27	----------	260	(1)	480	130
28	0	80	(1)	730	[2] 250
29	70	140	(1)	(1)	[2] 250
30	(1)	190	50	(1)	[2] 250
31	(1)	50	50	160	(1)

[1] Trace.
[2] 3-day collection.

FIGURE 4.—*Total fission product β-activity of gummed paper samples collected during July 1956*

[In d/m/sq. ft.]

Location:	*Total activity* [1]
Lima	1,300
Guayaquil	3,300
Panama, C. Z.	4,700
Washington, D. C.	3,800

[1] Measured on Aug. 16, 1956.

UNITED STATES NAVAL RESEARCH LABORATORY,
Washington 25, D. C., September 26, 1956.

Subject: Radioactivity of air and fallout samples collected at sites on the 80th meridian during August 1956; NRL problem A02-13, project No. NR 612130; interim report on

Figure 1: Daily record of fission product β-activity collected by the filter method.

Figure 2: Daily record of fission product β-activity collected by the cloth screen method.

Figure 3: Daily record of fission product β-activity collected by the gummed paper method.

Figure 4: Total fission product β-activity of gummed paper samples collected during August 1956.

1. Radioactivity measurements of air and fallout samples collected at various sites along the 80th meridian (west) during the month of August 1956 are presented in figures 1 through 4.

H. FRIEDMAN
(For I. H. Blifford, Jr.).
L. B. LOCKHART, Jr.

FIGURE 1.—*Daily record of fission product β-activity collected by the filter method, August 1956*

[In d/m/cu. ft.]

Day	Guayaquil	Panama, C. Z.	Washington, D. C.	Day	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0.040	0.022	0.031	17	0.020		0.061
2	.025	.039	.061	18	.012		[1] .097
3	.015	.020	.037	19	.013		[1] .097
4	.008	.010	[1] .031	20	.010	0.028	[1] .097
5	.012	.013	[1] .031	21	.014		.033
6	.012	.023	[1] .031	22	.018	.018	.043
7	.012	.032	.056	23	.017	.035	.033
8	.018	.054	.013	24	.032	[2]	.057
9	.020	.045	.034	25	.003	.008	[1] .037
10	.023	.063	.065	26	.019	.015	[1] .037
11	.025	.016	[1] .052	27	.017	.025	[1] .037
12	.025	.021	[1] .052	28	.010	.023	.047
13	.031	.021	[1] .052	29	.009	.067	.089
14	.023	.024	.059	30	.026	.008	.073
15	.015	.023	.30	31	.024	.010	.070
16	.023	.012	.074				

[1] 3-day collection. [2] Trace.

FIGURE 2.—*Daily record of fission product β-activity collected by the cloth-screen method, August 1956*

[In d/m]

Day	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0	0	1,500	0	190
2		530	970	360	1,300
3	0	800	660	900	810
4	0	1,500	370	0	[1] 510
5	0	1,500	520	200	[1] 510
6	0		600	[2]	[1] 510
7	0	420	680	290	300
8		1,500	660	620	60
9	0	3,400	1,000	340	280
10	0	3,000	880	290	460
11	0	990	940	160	[1] 430
12	0	1,400	0	840	[1] 430
13	0	1,300	2,100	710	[1] 430
14	0	1,400	680	1,700	580
15	[2]		850	[2]	3,500
16	0	6,400	620	320	390
17	0	2,300	930	60	140
18	0	890	540	40	[1] 650
19	0	320	480		[1] 650
20	0		380	260	[1] 650
21	0	1,900	360	400	0
22	0	1,200	320	50	650
23	0	0	520	0	100
24	0	1,800	1,300	100	0
25	0	1,900	80	0	[1] 390
26	0		960	[2]	[1] 390
27		1,700	430	260	[1] 390
28	160	2,400	270	210	380
29	240	2,700	530	810	780
30	0	2,000	760	0	290
31	0	1,800	920	0	780

[1] 3-day collection.
[2] Trace.

FIGURE 3.—*Daily record of fission product β-activity collected by the gummed paper method, August 1956*

[In d/m/sq. ft.]

Day	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	100	280	(1)	430	580
2	----------	80	(1)	170	40
3	0	100	(1)	760	240
4	(1)	80	60	100	[2] 260
5	(1)	120	240	(1)	[2] 260
9	(1)	----------	(1)	(1)	[2] 260
7	0	80	(1)	160	70
8	(1)	(1)	100	(1)	70
9	(1)	190	70	190	(1)
10	(1)	140	80	470	70
11	(1)	110	(1)	140	[2] 70
12	(1)	70	80	610	[2] 70
13	(1)	260	(1)	(1)	[2] 70
14	90	260	(1)	1,000	100
15	(1)	----------	(1)	900	340
16	0	450	(1)	250	660
17	0	120	40	40	50
18	0	70	(1)	40	[2] 330
19	40	60	0	----------	[2] 330
20	80	----------	(1)	80	[2] 330
21	0	60	(1)	320	(1)
22	0	100	50	170	100
23	0	50	(1)	40	(1)
24	(1)	190	(1)	190	50
25	0	110	(1)	50	[2] 40
26	(1)	----------	120	(1)	[2] 40
27	----------	210	(1)	500	[2] 40
28	310	260	0	380	70
29	0	860	360	420	60
30	50	80	(1)	50	50
31	(1)	160	60	(1)	90

[1] Trace. [2] 3-day collection.

FIGURE 4.—*Total fission product β-activity of gummed paper samples collected during August 1956*

[In d/m/sq. ft.]

Location:	*Total activity*
Santiago	590
Lima	1,800
Guayaquil	[1] 240
Panama, C. Z.	3,100
Washington, D. C.	2,300

[1] Counting accuracy ±40 percent.

UNITED STATES NAVAL RESEARCH LABORATORY,
Washington 25, D. C., October 30, 1956.

Subject: Radioactivity of air and fallout samples collected at sites on the 80th meridian during September 1956; NRL problem AO2-13, project No. NR 612130; interim report on

Figure 1: Daily record of fission product β-activity collected by the filter method.

Figure 2: Daily record of fission product β-activity collected by the cloth screen method.

Figure 3: Daily record of fission product β-activity collected by the gummed paper method.

Figure 4: Total fission product β-activity of gummed paper samples collected during July 1956.

1. Radioactivity measurements of air and fallout samples collected at various sites along the 80th meridian (west) during the month of September 1956 are presented in figures 1 through 4.

I. H. BLIFFORD, Jr.
L. B. LOCKHART, Jr.

FIGURE 1.—*Daily record of fission product β-activity collected by the filter method, September 1956*

[In d/m/cu. ft.]

Day	Guayaquil	Panama, C. Z.	Washington, D. C.	Day	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0.018	0.024	[1] 0.072	16	0.005	0.007	[1] 0.160
2	.016	.052	[1] .072	17	.012	.020	[1] .160
3	.018	.034	[1] .072	18	.014	.005	.140
4	.024	.022	[1] .072	19	.016	.005	.039
5	.016	.018	.070	20	.013	.006	.006
6	.016	.004	.084	21	.007	.010	.082
7	.026	.011	.044	22	.014	.012	[1] .094
8	.012	.004	[1] .200	23	.016	.012	[1] .094
9	.009	.004	[1] .200	24	.022	.005	[1] .094
10	.009	.015	[1] .200	25	.009	.005	.160
11	.014	.024	.061	26	.008	.010	.098
12	.038	.016	.093	27	.008	.018	.099
13	.016	.018	.120	28	.006	.010	.420
14	.022	.013	.140	29	.009	.002	[1] .320
15	.002	.036	[1] .160	30	.008	.008	[1] .320

[1] 3-day collection.

FIGURE 2.—*Daily record of fission product β-activity collected by the cloth screen method, September 1956*

[In d/m]

Day	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0	450	640	0	[1] 830
2	0	370	450	80	[1] 830
3	0	160	0	210	[1] 830
4	0	1,900	750	0	[1] 830
5	0	730	360	160	350
6	0	220	240	0	880
7	0	470	1,000	100	1,300
8	0	([2])	240	0	[1] 1,700
9	0	----------	([2])	0	[1] 1,700
10	0	1,500	320	0	[1] 1,700
11	0	340	360	([2])	370
12	0	800	960	0	2,200
13	0	940	500	0	350
14	0	770	([2])	0	1,800
15	0	260	600	100	[1] 3,100
16	0	620	170	0	[1] 3,100
17	0	160	290	0	[1] 3,100
18	0	420	400	0	----------
19	0	540	520	0	410
20	0	240	640	0	----------
21	0	1,200	100	0	2,600
22	0	1,100	590	0	[1] 1,500
23	0	730	370	0	[1] 1,500
24	0	270	310	0	[1] 1,500
25	0	190	160	0	810
26	0	420	80	0	1,400
27	0	0	140	0	5,400
28	0	590	160	0	2,300
29	0	800	320	0	[1] 300
30	0	1,300	300	0	[1] 300

[1] 3-day collection.
[2] Trace.

FIGURE 3.—*Daily record of fission product β-activity collected by the gummed paper method, September 1956*

[In d/m/sq. ft.]

Day	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	([1])	50	([1])	60	([1] [2])
2	70	110	([1])	190	([1] [2])
3	([1])	150	([1])	310	([1] [2])
4	0	140	([1])	60	([1] [2])
5	([1])	100	0	190	([1])
6	0	100	([1])	320	200
7	0	------------	0	290	430
8	170	------------	550	([1])	[2] 130
9	170	------------	0	([1])	[2] 130
10	([1])	------------	0	([1])	[2] 130
11	0	------------	([1])	140	50
12	0	------------	60	([1])	630
13	50	------------	([1])	0	90
14	([1])	([1])	([1])	([1])	80
15	0	50	0	60	[2] 530
16	0	70	0	([1])	[2] 530
17	([1])	([1])	([1])	70	[2] 530
18	0	50	([1])	([1])	530
19	([1])	50	0	([1])	([1])
20	0	50	0	([1])	530
21	([1])	110	0	50	70
22	([1])	50	([1])	([1])	[2] 460
23	([1])	([1])	([1])	([1])	[2] 460
24	0	50	0	0	[2] 460
25	0	0	0	([1])	410
26	0	([1])	0	80	60
27	([1])	0	([1])	70	590
28	0	50	0	0	360
29	900	50	0	([1])	[2] 130
30	70	([1])	0	70	[2] 130

[1] Trace.
[2] 3-day collection.

FIGURE 4.—*Total fission product β-activity of gummed paper samples collected during September 1956*

[In d/m/sq. ft.]

Location:	*Total activity* [1]
Santiago	360
Lima	750
Guayaquil	([2])
Panama, C. Z.	1,200
Washington, D. C.	4,200

[1] Measured on Oct. 18, 1956.
[2] Trace.

UNITED STATES NAVAL RESEARCH LABORATORY,
Washington 25, D. C., November 26, 1956.

Subject: Radioactivity of air and fallout samples collected at sites on the 80th meridian during October 1956; NRL problem AO2-13, project No. NB612 130; interim report on

Figure 1: Daily record of fission product β-activity collected by the filter method.

Figure 2: Daily record of fission product β-activity collected by the cloth-screen method.

Figure 3: Daily record of fission product β-activity collected by the gummed-paper method.

1. Radioactivity measurements of air and fallout samples collected at various sites along the 80th meridian (west) during the month of October 1956 are presented in figures 1 through 3.

I. H. BLIFFORD, Jr.
L. B. LOCKHART, Jr.

FIGURE 1.—*Daily record of fission product β-activity collected by the filter method, October 1956*

[In d/m/cu. ft.]

Day	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0.036	0.009	(1)	0.095
2	.003	.006	0.007	.21
3	.016	.010	.006	.098
4	.009	.009	.012	.17
5	.008	.006	.019	.16
6	.018	[2] .004	.005	[2] .12
7	[2] .020	[2] .004	(1)	[2] .12
8	[2] .020	.003	(1)	[2] .12
9	.034	.003	.008	.12
10	.072	.006	.007	.12
11	.12	.008	.004	.030
12	[2] .13	.063	.004	.097
13	[2] .13	.055	(1)	[2] .15
14	.12	.032	(1)	[2] .15
15	.068	.028	.005	[2] .15
16	.074	.039	.006	.22
17	.049	.036	[2] .013	.089
18	.028	.045	[2] .013	.15
19	.023	.024	.006	.12
20	.007	.016	(1)	[2] .076
21	[2] .020	.016	(1)	[2] .076
22	[2] .020	.008	.004	[2] .076
23	.017	.008	.004	.006
24	.017	.013	.007	.022
25	.046	.009	.004	.081
26	.035	.014	(1)	.19
27	.029	.028	.004	[2] .066
28	[2] .023	.019	(1)	[2] .066
29	[2] .023	.016	(1)	[2] .066
30	.029	.014	.006	.097
31	.025	.022	.005	.061

[1] Trace.
[2] Combined collections.

FIGURE 2.—*Daily record of fission product β-activity collected by the cloth screen method, October 1956*

[In d/m]

Day	Punta Arenas	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	----------	0	2,700	270	0	1,000
2	----------	0	540	260	0	1,300
3	----------	0	1,200	190	0	590
4	----------	0	50	160	0	290
5	300	0	260	(1)	0	3,600
6	0	0	1,200	[2] 110	50	[2] 1,200
7	(1)	0	[2] 1,900	[2] 110	0	[2] 1,200
8	0	0	[2] 1,900	50	0	[2] 1,200
9	0	70	940	1,000	0	720
10	330	130	4,700	0	120	2,100
11	0	50	9,600	0	0	1,800
12	50	0	[2] 10,000	1,800	0	370
13	210	0	[2] 10,000	1,100	0	[2] 530
14	(1)	(1)	4,400	780	0	[2] 530
15	0	0	3,400	400	170	[2] 530
16	130	0	2,100	680	0	120
17	200	0	770	1,400	(1)	1,200
18	130	0	2,200	860	0	2,700
19	0	0	930	540	0	920
20	50	0	830	310	80	[2] 440
21	0	0	[2] 410	440	0	[2] 440
22	0	0	[2] 410	0	0	[2] 440
23	0	0	260	310	0	510
24	0	0	550	430	0	(1)
25	190	0	430	380	0	(1)
26	60	(1)	630	140	0	310
27	400	0	520	370	0	[2] 1,700
28	0	0	[2] 500	520	0	[2] 1,700
29	(1)	0	[2] 500	360	0	[2] 1,700
30	0	0	530	470	0	2,000
31	----------	0	660	700	0	840

[1] Trace.
[2] Combined collections.

FIGURE 3.—*Daily record of fission product β-activity collected by the gummed paper method, October 1956*

[In d/m/sq. ft.]

Day	Punta Arenas	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	----------	60	100	0	([1])	380
2	----------	([1])	70	([1])	60	50
3	----------	([1])	([1])	([1])	0	([1])
4	----------	0	0	0	([1])	130
5	([1])	70	([1])	([1])	70	380
6	([1])	0	50	([1 2])	([1])	[2] 60
7	([1])	0	[2] 80	([1 2])	([1])	[2] 60
8	50	([1])	[2] 80	160	0	[2] 60
9	([1])	0	50	0	([1])	([1])
10	80	([1])	160	([1])	90	([1])
11	([1])	160	260	50	0	40
12	([1])	([1])	[2] 250	70	70	50
13	270	60	[2] 250	50	([1])	[2] 20
14	60	260	50	([1])	([1])	[2] 20
15	0	0	80	([1])	([1])	[2] 20
16	0	----------	60	([1])	([1])	60
17	([1])	----------	100	260	180	40
18	50	----------	50	([1])	[2] 40	200
19	([1])	----------	40	([1])	[2] 40	07
20	([1])	----------	80	120	([1])	[1] 50
21	40	----------	[2] 40	50	([1])	[1] 50
22	90	----------	[2] 40	([1])	100	[1] 50
23	60	----------	80	40	50	200
24	([1])	670	90	50	([1])	90
25	([1])	----------	100	([1])	([1])	150
26	([1])	----------	50	([1])	40	210
27	50	([1])	([1])	([1])	([1])	[1] 170
28	([1])	([1])	[2] 40	([1])	60	[1] 170
29	([1])	([1])	[2] 40	([1])	([1])	[1] 170
30	([1])	70	80	60	([1])	120
31	----------	([1])	80	([1])	([1])	220

[1] Trace.
[2] Combined collections.

UNITED STATES NAVAL RESEARCH LABORATORY,
Washington 25, D. C., January 10, 1957.

Subject: Radioactivity of air and fallout samples collected at sites on the 80th meridian during November 1956; NRL problem AO2-13, project No. NR 612130; interim report on

Figure 1: Daily record of fission product β-activity collected by the filter method.

Figure 2: Daily record of fission product β-activity collected by the cloth-screen method.

Figure 3: Daily record of fission β-activity collected by the gummed-paper method.

1. Radioactivity measurements of air and fallout samples collected at various sites along the 80th meridian (west) during the month of November 1956 are presented in figures 1 through 3.

I. H. BLIFFORD, Jr.
L. B. LOCKHART, Jr.

FIGURE 1.—*Daily record of fission product β-activity collected by the filter method, November 1956*

[In d/m/cu. ft.]

Day	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	------------	0.032	0.032	0.004	0.026
2	------------	.028	.026	(1)	.012
3	------------	.026	.028	.004	[2] .038
4	------------	.019	[2] .020	.008	[2] .038
5	------------	.032	[2] .020	.015	[2] .038
6	------------	.026	.018	.030	.056
7	------------	.024	.046	.022	.069
8	------------	.018	.014	.052	.088
9	------------	.028	.014	.048	.066
10	------------	.036	.009	.011	[2] .058
11	------------	.056	.026	(1)	[2] .058
12	------------	------------	.024	.004	[2] .058
13	------------	.046	.042	.005	[2] .058
14	------------	.039	.052	.004	.042
15	------------	.030	.034	.004	.104
16	------------	.024	.022	.005	.157
17	0.010	.022	.009	.026	[2] .035
18	.006	.016	.018	.016	[2] .035
19	.006	.014	.009	.008	[2] .035
20	.024	.022	.007	(1)	.070
21	.006	.008	.007	.006	.100
22	.008	.020	.009	.004	.088
23	.005	.018	.016	[2] .008	.103
24	.008	0	.016	[2] .008	[2] .026
25	.005	.014	.012	.012	[2] .026
26	.014	.008	[2] .010	.052	[2] .026
27	.009	------------	[2] .010	.008	.037
28	.010	.028	.010	.020	.048
29	.010	.010	[2] .012	.022	.080
30	.009	.013	[2] .012	.018	.038

[1] Trace.
[2] Combined collections.

FIGURE 2.—*Daily record of fission product β-activity collected by the cloth screen method, November 1956*

[In d/m]

Day	Punta Arenas	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	60	0	450	810	0	50
2	50	0	1,500	850	0	520
3	90	0	1,100	630	0	[1] 430
4	160	(2)	240	[1] 350	(2)	[1] 430
5	110	0	810	[1] 350	0	[1] 430
6	80	100	990	980	0	90
7	90	0	420	240	0	(2)
8	90	0	150	190	(2)	230
9	110	------------	370	150	(2)	1,300
10	(2)	0	530	350	0	[1] 950
11	50	0	2,300	450	0	[1] 950
12	(2)	0	------------	710	0	[1] 950
13	0	0	900	1,000	0	[1] 950
14	80	0	400	450	0	290
15	------------	0	110	420	0	450
16	------------	0	80	220	0	1,100
17	(2)	0	210	750	0	[1] 620
18	40	0	240	190	(2)	[1] 620
19	40	0	450	140	0	[1] 620
20	90	0	730	90	0	(2)
21	------------	0	430	40	0	710
22	70	0	410	410	0	1,800
23	50	0	310	310	0	1,200
24	0	0	710	80	0	[1] 70
25	300	0	110	170	0	[1] 70
26	140	0	70	[1] 200	0	[1] 70
27	360	0	------------	[1] 200	0	0
29	------------	0	900	130	0	720
29	80	0	250	[1] 170	0	130
30	------------	0	180	[1] 170	0	[1] 210

[1] Combined collections.
[2] Trace.

FIGURE 3.—*Daily record of fission product β-activity collected by the gummed paper method, November 1956*

[In d/m/sq.ft.]

Day	Punta Arenas	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	(1)	(1)	110	(1)	(1)	50
2	(1)	(1)	80	(1)	(1)	490
3	50	(1)	50	(1)	(1)	[2] 50
4	(1)	80	50	(1)	0	[2] 50
5	(1)	0	(1)	(1)	60	[2] 50
6	(1)	(1)	50	(1)	60	(1)
7	0	(1)	(1)	(1)	(1)	50
8	0	(1)	0	(1)	80	(1)
9	(1)	---	50	(1)	(1)	140
10	(1)	0	(1)	0	0	[2] 40
11	(1)	70	70	(1)	0	[2] 40
12	(1)	0	---	(1)	(1)	[2] 40
13	0	(1)	(1)	60	(1)	[2] 40
14	(1)	0	(1)	(1)	0	(1)
15	---	(1)	(1)	(1)	(1)	(1)
16	---	(1)	(1)	(1)	140	(1)
17	150	0	(1)	0	70	[2] 90
18	0	(1)	(1)	(1)	0	[2] 90
19	0	(1)	(1)	(1)	110	[2] 90
20	0	0	(1)	0	0	50
21	---	0	(1)	0	50	(1)
22	(1)	0	0	(1)	0	130
23	(1)	(1)	(1)	(1)	(1)	---
24	0	(1)	0	(1)	(1)	[2] 40
25	0	50	(1)	(1)	0	[2] 40
26	(1)	(1)	(1)	0	50	[2] 40
27	0	0	---	0	0	0
28	---	(1)	(1)	0	(1)	(1)
29	(1)	0	0	(1)	70	(1)
30	---	(1)	(1)	(1)	50	(1)

[1] Trace.
[2] Combined collections.

UNITED STATES NAVAL RESEARCH LABORATORY,
Washington 25, D. C., January 30, 1957.

Subject: Radioactivity of air and fallout samples collected at sites on the 80th meridian during December 1956; NRL problem A02-13, project No. NR 612130; interim report on.

Figure 1: Daily record of fission product β-activity collected by the filter method.

Figure 2: Daily record of fission product β-activity collected by the cloth screen method.

Figure 3: Daily record of fission product β-activity collected by the gummed paper method.

1. Radioactivity measurements of air and fallout samples collected at various sites along the 80th meridian (west) during the month of December 1956 are presented in figures 1 through 3.

I. H. BLIFFORD, Jr.
L. B. LOCKHART, Jr.

FIGURE 1.—*Daily record of fission product β-activity collected by the filter method, December 1956*

[In d/m/cu. ft.]

Day	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0.005	0.013	0.009	0.016	[1] 0.036
2	.005	[1] .011	.016	.026	[1] .036
3	.012	[1] .011	[1] .014	.041	[1] .036
4	.013	.024	[1] .014	.045	.100
5	.009	.022	.012	.025	.095
6	.006	.016	.022	.028	.052
7	.010	.011	[1] .017	.046	.064
8	.006	.015	[1] .017	.016	[1] .051
9	.009	.015	.022	[1] .037	[1] .051
10	.008	.016	.022	[1] .037	[1] .051
11	.009	.024	.018	.030	.073
12	.009	.018	.015	.037	.081
13	.013	.014	.015	.038	.042
14	.008	.010	.018	.014	.031
15	.006	.014	.018	.014	[1] .003
16	.005	[1] .021	.020	.006	[1] .003
17	.005	[1] .021	.016	.007	[1] .003
18	.006	.014	[1] .013	.010	.029
19	.007	.012	[1] .013	.030	.082
20	.008	.012	[1] .012	.032	.101
21	.005	.008	[1] .012	.018	.079
22	.009	.012	.013	.014	[1] .020
23	.018	[1] .002	.018	.042	[1] .020
24	.006	[1] .002	.013	.050	[1] .020
25	.005	[1] .008	.012	.050	[1] .020
26	.003	[1] .008	[1] .012	.079	[1] .020
27	.003	.009	[1] .012	.058	.043
28	(2)	.010	.012	.043	.063
29	(2)	.013	.011	.034	[1] .042
30	.005	.006	.013	.032	[1] .042
31	.010	.006	.013	.036	[1] .042

[1] Combined collection. [2] Trace.

FIGURE 2.—*Daily record of fission product β-activity collected by the cloth screen method, December 1956*

[In d/m]

Day	Punta Arenas	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	50	0	60	330	0	[1] 210
2	0	0	[1] 50	230	0	[1] 210
3	0	0	[1] 50	[1] 180	0	[1] 210
4	0	0	230	[1] 180	0	500
5	------------	0	240	240	0	210
6	0	0	170	120	0	100
7	0	0	(2)	[1] 340	0	70
8	(2)	0	240	[1] 340	0	[1] 200
9	0	0	260	270	0	[1] 200
10	(2)	0	320	210	0	[1] 200
11	50	0	290	280	0	660
12	(2)	0	100	310	0	700
13	(2)	0	180	220	0	170
14	70	0	140	220	(2)	890
15	50	0	140	170	0	[1] 70
16	0	0	[1] 80	330	0	[1] 70
17	------------	0	[1] 80	280	0	[1] 70
18	0		170	[1] 260	0	90
19	0	0	(2)	[1] 260	0	1,600
20	80	0	100	[1] 160	0	300
21	50	0	50	[1] 160	0	(2)
22	(2)	0	140	140	0	[1] 600
23	190	0	[1] 30	310	0	[1] 600
24	0	------------	[1] 30	140	0	[1] 600
25	(2)	0	[1] 25	240	0	[1] 600
26	0	0	[1] 25	[1] 140	0	[1] 600
27	50	0	(2)	[1] 140	(2)	750
28	0	0	90	80	0	520
29	110	0	100	140	0	------------
30	220	0	160	100	0	------------
31	(2)	0	60	(2)	0	------------

[1] Combined collection. [2] Trace.

FIGURE 3.—*Daily record of fission product β-activity collected by the gummed paper method, December 1956*

[m d/m sq. ft.]

Day	Punta Arenas	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	([1])	50	0	0	([1])	[2] 20
2	0	([1])	([1])	([1,2])	([1])	[2] 20
3	([1])	([1])	([1])	([1,2])	80	[2] 20
4	50	([1])	([1])	([1])	([1])	130
5	--------	([1])	([1])	([1])	0	230
6	0	0	([1])	0	([1])	50
7	([1])	0	0	([1,2])	0	90
8	([1])	0	0	([1,2])	0	[2] 180
9	60	([1])	([1])	0	[2] 0	[2] 180
10	0	([1])	0	130	[2] 0	[2] 180
11	([1])	([1])	([1])	100	100	350
12	0	0	([1])	0	70	660
13	40	0	0	([1])	80	50
14	([1])	0	0	([1])	([1])	230
15	0	([1])	--------	0	50	[2] 60
16	0	0	[2] 0	([1])	50	[2] 60
17	0	([1])	[2] 0	([1])	([1])	[2] 60
18	0	0	0	[2] 0	([1])	([1])
19	([1])	([1])	([1])	[2] 0	([1])	([1])
20	70	([1])	([1])	[2] 0	([1])	([1])
21	0	0	0	[2] 0	50	70
22	0	50	([1])	([1])	0	[2] 150
23	0	0	[2] 0	60	([1])	[2] 150
24	0	50	[2] 0	0	0	[2] 150
25	([1])	--------	([1,2])	60	0	[2] 150
26	0	0	([1,2])	[2] 30	([1])	[2] 150
27	([1])	([1])	160	[2] 30	140	190
28	0	0	([1])	([1])	0	([1])
29	([1])	([1])	([1])	0	([1])	[2] 50
30	([1])	0	0	([1])	([1])	[2] 50
31	([1])	([1])	0	([1])	0	[2] 50

[1] Trace.
[2] Combined collection.

UNITED STATES NAVAL RESEARCH LABORATORY,
Washington 25, D. C., March 6, 1957.

Subject: Radioactivity of air and fallout samples collected at sites on the 80th meridian during January 1957; NRL problem A02-13, project No. NR 612130; interim report on.

Figure 1: Daily record of fission product β-activity collected by the filter method.

Figure 2: Daily record of fission product β-activity collected by the cloth screen method.

Figure 3: Daily record of fission product β-activity collected by the gummed paper method.

1. Radioactivity measurements of air and fallout samples collected at various sites along the 80th meridian (west) during the month of January 1957 are presented in figures 1 through 3.

2. These measurements are being carried out as part of the United States Naval Research Laboratory program of atmospheric radioactivity studies. They are being made with the assistance of the Meteorological Services of Chile, Ecuador, and Peru and in cooperation with the United States Weather Bureau, Department of Commerce. Partial financial support is provided by the Division of Biology and Medicine, United States Atomic Energy Commission.

I. H. BLIFFORD, Jr.
L. B. LOCKHART, Jr.

FIGURE 1.—*Daily record of fission product β-activity collected by the filter method, January 1957*

[In d/m/cu ft.]

Day	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0.010	[1] 0.007	0.013	0.041	[1] 0.051
2	.008	[1].007	.016	.036	[1].051
3	.008	.002	.010	.040	.060
4	.005	.007	.012	.052	.043
5	.012	.008	.004	.026	[1].056
6	.012	.013	.013	.030	[1].056
7	.009	.005	.012	.062	[1].056
8	----------	.009	.009	.037	.038
9	.007	.006	.010	.036	.051
10	.010	.011	.009	.036	.045
11	.010	.016	.007	.020	.043
12	.010	.010	.010	.032	[1].060
13	.012	.010	.011	.032	[1].060
14	.010	.009	.004	.042	[1].060
15	.006	.004	.012	.032	.064
16	.014	----------	.010	.030	.042
17	.008	----------	.009	.036	.069
18	.008	----------	.008	.030	.048
19	.004	----------	.005	.030	[1].053
20	.008	----------	.010	.043	[1].053
21	.010	----------	.008	.040	[1].053
22	.010	----------	.012	.007	[1].053
23	.006	----------	.020	.032	.024
24	.009	----------	[1].009	.098	.054
25	.010	----------	[1].009	.060	.066
26	.006	----------	.022	.078	[1].074
27	0	----------	.020	.044	[1].074
28	.005	----------	.016	.046	[1].074
29	.003	----------	.016	.037	.034
30	.006	----------	.012	----------	.024
31	.007	----------	.010	----------	.053

[1] Combined collections.

FIGURE 2.—*Daily record of fission product β-activity collected by the cloth screen method, January 1957*

[In d/m]

Day	Punta Arenas	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	----------	0	[1] 70	40	(2)	[1] 2,200
2	----------	0	[1] 70	100	0	[1] 2,200
3	----------	0	(2)	140	0	2,000
4	----------	0	60	100	0	530
5	----------	0	0	0	0	[1] 560
6	0	0	100	70	0	[1] 500
7	0	0	0	(2)	0	[1] 560
8	(2)	0	(2)	(2)	0	430
9	(2)	0	(2)	70	0	270
10	50	0	0	0	0	190
11	(2)	0	100	80	140	----------
12	0	0	140	0	0	[1] 620
13	(2)	0	(2)	70	0	[1] 620
14	0	0	100	0	0	[1] 620
15	0	0	50	(2)	0	2,200
16	----------	50	(2)	(2)	0	1,200
17	----------	0	170	0	0	590
18	0	0	0	0	0	170
19	0	0	0	0	0	[1] 340
20	0	0	0	(2)	0	[1] 340
21	0	0	170	(2)	0	[1] 340
22	(2)	0	70	80	(2)	[1] 340
23	0	0	(2)	220	0	230
24	0	0	210	[1] 0	260	2,100
25	0	0	60	[1] 0	0	360
26	0	0	(2)	40	0	[1] 590
27	40	0	----------	0	0	[1] 590
28	0	0	----------	50	(2)	[1] 590
29	40	0	60	----------	0	90
30	0	0	0	0	0	520
31	0	0	60	0	(2)	240

[1] Combined collections. [2] Trace.

FIGURE 3.—*Daily record of fission product β-activity collected by the gummed paper method, January 1957*

[In d/m/sq. ft.]

Day	Punta Arenas	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	-----	(1)	(1 2)	40	(1)	[2] 190
2	-----	(1)	(1 2)	(1)	40	[2] 190
3	-----	70	(1)	(1)	80	230
4	-----	(1)	(1)	0	50	(1)
5	-----	(1)	0	(1)	60	[2] 170
6	-----	(1)	(1)	(1)	0	[2] 170
7	-----	0	0	0	0	[2] 170
8	-----	0	0	0	0	(1)
9	-----	0	(1)	(1)	(1)	90
10	-----	(1)	(1)	150	(1)	130
11	-----	(1)	(1)	160	(1)	60
12	-----	(1)	(1)	0	(1)	[2] 20
13	-----	0	(1)	(1)	(1)	[2] 20
14	-----	(1)	0	(1)	(1)	[2] 20
15	-----	0	(1)	0	(1)	(1)
16	-----	120	(1)	0	0	(1)
17	-----	0	0	(1)	-----	0
18	-----	0	0	0	0	(1)
19	-----	40	(1)	0	0	[2] 50
20	-----	0	(1)	0	0	[2] 50
21	-----	0	0	0	(1)	[2] 50
22	-----	(1)	(1)	0	(1)	[2] 50
23	-----	0	0	(1)	(1)	70
24	-----	0	0	(1 2)	0	60
25	-----	0	(1)	(1 2)	(1)	(1)
26	-----	-----	(1)	80	0	[2] 50
27	-----	-----	(1 2)	50	0	[2] 50
28	-----	-----	(1 2)	(1)	(1)	[2] 50
29	-----	-----	0	-----	0	60
30	0	-----	(1)	0	0	0
31	-----	-----	(1)	0	0	120

[1] Trace.
[2] Combined collections.

U. S. NAVAL RESEARCH LABORATORY,
Washington 25, D. C., March 29, 1957.

Subject: Radioactivity of air and fallout samples collected at sites on the 80th meridian during February 1957; NRL problem A02-13, project No. NR 612130; interim report on.

Figure 1: Daily Record of fission product β-activity collected by the filter method.

Figure 2: Daily record of fission product β-activity collected by the cloth screen method.

Figure 3: Daily record of fission product β-activity collected by the gummed paper method.

1. Radioactivity measurements of air and fallout samples collected at various sites along the 80th meridian (West) during the month of February 1957 are presented in Figures 1 through 3.

2. These measurements are being carried out as part of the United States Naval Research Laboratory program of atmospheric radioactivity studies. They are being made with the assistance of the Meteorological services of Chile, Ecuador and Peru and in cooperation with the United States Weather Bureau, Department of Commerce. Partial financial support is provided by the Division of Biology and Medicine, United States Atomic Eenergy Commission.

I. H. BLIFFORD, Jr.
L. B. LOCKHART, Jr.

FIGURE 1.—*Daily record of fission product β-activity collected by the filter method, February 1957*

[In d/m/cu. ft.]

Day	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0.006	------------	------------	0.028	0.020
2	.008	------------	0.028	.032	[1] .030
3	.013	------------	.012	.040	[1] .030
4	.012	[2]	.005	.043	[1] .030
5	.024	O	.006	.037	.071
6	.012	[2]	.008	.050	.084
7	.009	[2]	.012	.052	.042
8	.008	[2]	.005	.062	.028
9	.008	[2]	------------	.065	[1] .042
10	.006	------------	.003	.060	[1] .042
11	.008	------------	.003	.059	[1] .042
12	.010	[2]	[2]	.041	.081
13	.010	[2]	.013	.063	.034
14	.012	------------	.009	.079	.085
15	.008	------------	.009	.089	.045
16	.006	------------	.009	.073	[1] .045
17	.008	------------	.003	.079	[1] .045
18	.005	.003	.003	.120	[1] .045
19	.003	------------	.003	.079	.100
20	.004	------------	.003	.082	.115
21	.003	------------	[2]	.089	.044
22	.008	------------	[2]	.071	[1] .047
23	.006	------------	.012	.087	[1] .047
24	.008	------------	.018	.109	[1] .047
25	.006	------------	.018	.079	[1] .047
26	.006	------------	.009	.075	.066
27	.010	------------	.019	.086	.039
28	.016	------------	.024	.086	.048

[1] Combined collection. [2] Trace.

FIGURE 2.—*Daily record of fission product β-activity collected by the cloth-screen method, February 1957*

[In d/m]

Day	Punta Arenas	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	0	0	60	------------	140	[1]
2	110	0	0	------------	140	[2] 170
3	0	0	0	[1]	40	[2] 170
4	[1]	0	[1]	0	0	[2] 170
5	0	0	0	0	50	890
6	0	0	0	0	80	580
7	[1]	0	0	0	[1]	[1]
8	140	0	0	0	50	70
9	------------	0	0	0	0	[2] 480
10	0	0	60	0	50	[2] 480
11	[1]	0	0	0	0	[2] 480
12	60	0	0	0	0	950
13	------------	0	0	0	80	320
14	[1]	0	[1]	0	60	70
15	[1]	0	[1]	[1]	[1]	730
16	300	0	0	[1]	50	[2] 620
17	0	0	------------	0	170	[2] 620
18	------------	0	50	0	80	[2] 620
19	260	0	[1]	0	.80	1,800
20	[1]	0	[1]	0	260	1,010
21	0	0	60	0	0	570
22	210	0	0	0	0	[2] 140
23	140	0	0	0	0	[2] 140
24	260	60	60	0	0	[2] 140
25	0	0	[1]	0	0	[2] 140
26	0	0	0	60	[1]	[1]
27	[1]	0	50	[1]	0	0
28	220	0	100	0	[1]	60

[1] Trace.
[2] Combined collection.

FIGURE 3.—*Daily record of fission product β-activity collected by the gummed paper method, February 1957*

[In d/m/sq. ft.]

Day	Punta Arenas	Santiago	Lima	Guayaquil	Panama, C. Z.	Washington, D. C.
1	(1)	----------	0	----------	0	60
2	0	----------	(1)	(1)	70	[2] 80
3	----------	----------	0	(1)	0	[2] 80
4	----------	----------	0	0	0	[2] 80
5	0	----------	(1)	0	----------	(1)
6	0	----------	0	0	----------	170
7	0	----------	0	0	----------	(1)
8	0	----------	0	0	0	(1)
9	----------	----------	0	0	(1)	[2] 80
10	0	----------	0	(1)	(1)	[2] 80
11	(1)	----------	0	0	(1)	[2] 80
12	0	----------	0	(1)	(1)	(1)
13	----------	----------	(1)	(1)	(1)	60
14	0	----------	0	0	50	(1)
15	0	----------	(1)	0	(1)	120
16	----------	----------	0	0	(1)	[2] 40
17	0	----------	----------	0	0	[2] 40
18	0	----------	(1)	0	(1)	[2] 40
19	0	----------	0	0	0	170
20	(1)	----------	(1)	0	(1)	230
21	(1)	----------	0	(1)	0	720
22	(1)	----------	0	(1)	0	(1, 2)
23	(1)	----------	(1)	(1)	0	(1, 2)
24	(1)	----------	(1)	(1)	(1)	(1, 2)
25	0	----------	0	(1)	0	(1, 2)
26	0	----------	0	0	(1)	120
27	0	----------	(1)	0	0	120
28	0	----------	(1)	(1)	(1)	190

1 Trace.
2 Combined collection.

(Following is an article by Dr. Patterson and Dr. Blifford of the Naval Research Laboratory.)

[Reprinted from Science Magazine, March 3, 1957]

ATMOSPHERIC CARBON-14

According to Libby,[1] most of the neutrons which escape into the surrounding atmosphere from an atomic or thermonuclear explosion interact with the nitrogen of the atmosphere to produce C^{14} through the nuclear reaction.

$$N^{14}+n\rightarrow C^{14}+H^{1}$$

Because of the extensive use of C^{14} dating techniques, small additions of this material to the atmosphere may be important.

Libby[1] further estimates that, even to double temporarily the atmospheric radiocarbon content, megatons of fission of the order of 1000 would be required. With the measurement techniques now in use, it is possible to make measurements of the present equilibrium level of C^{14} in contemporary biological materials to an accuracy of approximately 1 percent. From these considerations it seems probable that only thermonuclear explosions will produce sufficient C^{14} to give measurable increases.

On the assumption that any C^{14} formed in weapons tests would be present in the air as CO_2,[2] collections of this gas from the atmosphere were begun in 1952. A vacuum pump was used to draw filtered air through a solution of sodium hydroxide (80 g of NaOH in 2 gal of water) at a flow rate of 10 to 12 lit/min. At the end of the collection period, the solution contained 15 to 20 lit of CO_2 as Na_2CO_3, which was precipitated as $CaCO_3$ by the addition of $CaCl_2$. The precipitated calcium carbonate was separated by decantation and filtration, was dried, and was placed in sealed containers. By drawing the air through two

1 W. F. Libby, "The radioactive fallout and radioactive strontium," speech delivered at Northwestern University, Evanston, Ill., 19 Jan. 1956.
2 W. F. Libby, Radiocarbon Dating (Univ. of Chicago Press, Chicago, Ill., ed. 2, 1956).

collecting bottles in series, the efficiency of collection of CO_2 from the air was found to be approximately 80 percent.

The counting technique employed was that developed by Suess,[3] in which the carbon-containing material is converted first to carbon dioxide and then to purified acetylene. The acetylene is used as the counting gas in a well-shielded proportional counter with an anticoincidence arrangement for reducing the cosmic-ray background. It was found that excellent stability and good repetitive accuracy could be obtained over periods of a year or more.

We were fortunate[4] in obtaining small quantities of strontium carbonate prepared from fossil carbon (lignite coal) and from contemporary carbon which had been measured previously.[5] These samples were converted to acetylene and served as standards for all of our measurements. For each of the experimental values reported here, the counting rate of the atmospheric carbon was compared with two measurements on the contemporary carbon standard and with two measurements on the fossil carbon standard. One standard measurement was made within 3 days before, and the other within 3 days after, the sample count. In some cases, the reported values were derived from more than one measurement, and in other instances two completely separate samples were prepared from the same atmospheric carbon. All of the errors shown were computed from the total number of counts and are expressed as the 9/10 error in this quantity.

In order to test the reproducibility of the entire process, three simultaneous atmospheric carbon samples were collected and separately processed. The counting rates obtained were well within the expected statistical error.

By means of the afore-described techniques, four 1-week samples collected during the period from October to December 1952 in French Morocco and four similar collections in Alaska gave average values for the sample/standard ratio of 0.97 ± 0.01. Four samples collected at Washington, D. C., during this same period gave an average of 0.95 ± 0.01, while three collections made in the Hawaiian Islands and three in the Philippine Islands gave average sample/standard ratios of 1.00 ± 0.02. In no case did the C^{14} content of the atmospheric sample exceed that of the standard. The lower values (particularly at Washington, D. C.) are believed to reflect the dilution effect of the burning of fossil fuels.

[3] H. E. Suese, Science 120, 5 (1954).

[4] We wish to thank M. Rubin, for supplying the standard samples; L. B. Lockhart, Jr., H. Friedman, and R. A. Baus, for helpful discussions; Paul Gustafson, who assisted in the preparation of some of the samples; and the Naval personnel who collected the CO_2 samples.

[5] H. E. Suess, Science 122, 415 (1955); the sample of contemporary carbon was prepared from wood grown before 1900 and was sample number W-214, Table 1, of this reference.

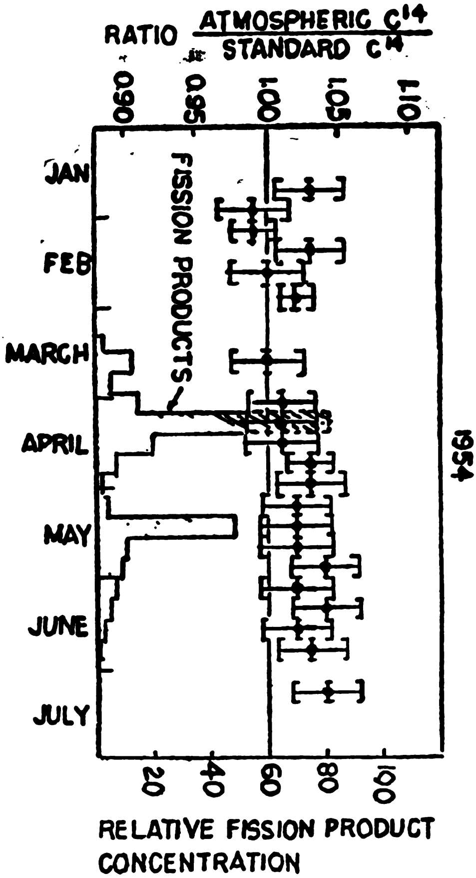

FIG. 1.—C^{14} content of atmospheric CO_2 samples collected at Subic Bay, Philippine Islands.

We were able to obtain CO_2 collections from the Naval station at Subic Bay in the Philippine Islands during the thermonuclear tests of 1954. They are of interest, since it was possible to measure simultaneously the ground-level concentration of fission products. The direction of the winds below 20,000 feet from the Pacific proving grounds was such that the observed radioactivity should have been the result of low-altitude fission debris. The counting rates of atmospheric carbon, referred to standard carbon, for CO_2 samples that were collected from January to July of 1954 are illustrated in Fig. 1, along with the relative fission-product concentration for the same period. It is clear that the concentration of C^{14} did not increase as markedly as did that of the fission products. However, it does appear that it was somewhat higher after the tests than at the time of the 1952 collections. It is possible that most of the C^{14} which was formed was entrained in the hot gases of the fireball and injected into the stratosphere so that relatively little was present in the ground-level cloud.

In Fig. 2 are shown the relative atmospheric/standard carbon counting rates from collections made in Washington, D. C., from January 1955 to February 1956. Samples collected in the months of May through November 1955 were significantly higher than the standard in C^{14} content and, in one instance, as high as +18 percent. The fact that the concentration was at a minimum during the colder months may indicate a seasonal decrease, resulting from reduced plant transpiration and the burning of fossil fuels. Almost all of the measurements gave higher values than those of October-December 1952.

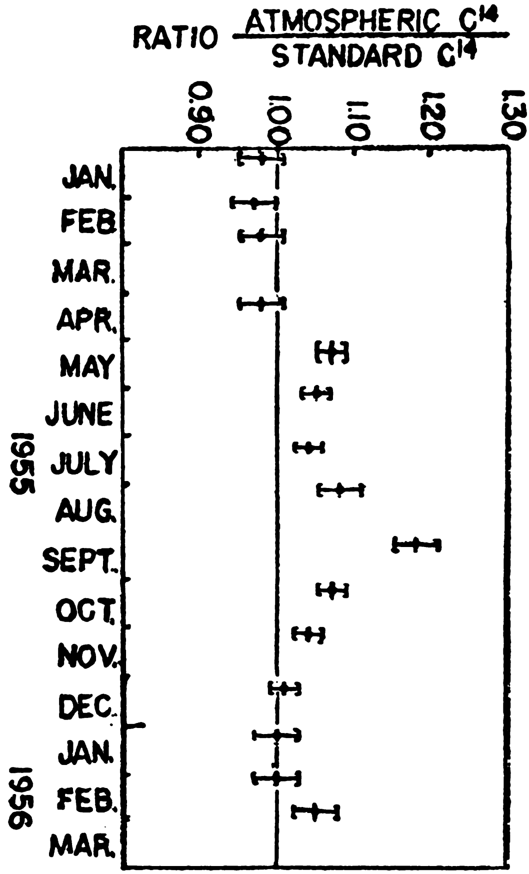

FIG. 2—C^{14} content of atmospheric CO_2 samples collected at Washington D. C.

Previous measurements of atmospheric C^{14} by Kulp[6] indicated no significant deviation from contemporary wood for 12 samples collected under a variety of conditions. Rafter,[7] on the other hand, found that four CO_2 samples collected in New Zealand in 1954 and 1955 had higher concentrations of C^{14} than had contemporary wood (+4.7 percent for one sample). Our collections at Washington, D. C., during the summer of 1955 gave values for the C^{14} content of atmospheric CO_2 appreciably higher than those previously reported. It seems difficult to account for these high values on the basis of isotopic fractionation, and therefore the increase in the C^{14} content of atmospheric CO_2 from 1952 to 1956 is probably the result of the addition of radiocarbon from thermonuclear sources. The delayed appearance of the C^{14} increase at ground level may indicate a stratospheric reservoir of this isotope.

R. L. PATTERSON, JR.
I. H. BLIFFORD, JR.

U. S. Naval Research Laboratory,
Washington, D. C.

Representative HOLIFIELD. Our next witness is Dr. J. L. Kulp, Columbia University and Lamont Geological Laboratory.

Dr. Kulp, we are happy to have you before the committee this morning.

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[6] J. L. Kulp, Quart. Progr. Repts. Lamoel Geol. Observatory Nos. 9, 10, 11 (15 Jan. 1953).
[7] T. A. Rafter, New Zealand J. Sci. Technol. B37, 20 (1955).

STATEMENT OF DR. J. L. KULP, COLUMBIA UNIVERSITY AND DIRECTOR, LAMONT GEOLOGICAL OBSERVATORY[2]

Dr. KULP. Thank you Mr. Chairman.

With Dr. Eisenbud's permission, I would like to use two of his charts.

In my opinion, the strontium 90 problem can best be divided into three phases. Phase 1 is the definition of the present level of strontium 90 in human beings.

The second phase is the determination of the factors which bring about this particular level, because, as we determine these factors, it then becomes possible for us to predict for any given future situation how much man will get into his body.

The third phase of the strontium 90 problem is the biological aspect of exactly how much injury will occur from various levels of radiation.

Since 1953, we at Columbia University have been engaged in a research project sponsored by the Atomic Energy Commission, in which we have been addressing ourselves to the first two of these three phases, and it is of these two that I wish to speak this morning.

Primarily, I will be speaking about phase 1, namely, the present level of strontium 90. I will have a few things to say about the distribution factors, but Dr. Eisenbud has already covered much of this.

Now, to set the framework for this discussion, I would like first to write down these units again.

In terms of micromicrocuries of strontium 90 per gram of calcium 1,000 is the present accepted maximum permissible concentration for an industrial worker. It is recommended that for populations that this be a hundred micromicrocuries.

The reason I would like to put these down at the outset is that we will continually refer to the levels in man, various men, various ages, in various countries, relative to these numbers. So we need to have them clearly in mind.

The 1,000 value is the industrial level that is now accepted by the National Committee on Radiation Protection, and the 100 value is the recommended level for population.

Our study to date has involved the analysis of about 1,100 human bone samples. These human bone samples have come from 25 stations all over the world and range over a maximum of geographical and dietary types. These localities range from Chile to various places

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in the United States, Japan, Taiwan, India, and various sites in Europe.

I would like to break down the discussion of this data as follows:

First, to mention the age distribution for a given locality; secondly, the current levels, the current world average levels; thirdly, the change of these levels with time; fourthly, the change with geographical locations; and finally, the change with diet and other factors.

First, then, the age distribution.

For any given locality, whether it be New York City or Santiago, Chile, if we plot the strontium 90 concentration against the age we observe that there is a peak at approximately 5 years which then falls off and is level from 20 years through 60 and there is an indication of a slight increase above 60 years. So we may divide this curve essentially into these important parts:

Part A represents the beginning of equilibration of a young child with his diet. Bones are being formed rapidly in this range of zero to 4 years, and the child is getting most of his calcium from milk, at least in western civilization. The child is nearly in equilibrium with the milk of his diet.

Secondly, there is a period of dilution where from 4 years to maybe 20 years you are still building a certain fraction of the calcium into your bone. Thirdly, you have the average adult range, where essentially we are not building much calcium. So the uptake from strontium 90 is about the same whether you deal with a 25-year-old or 60-year-old person.

Then, due to various physiological processes for very old people there seems to be a very slight increase due to bone turnover. But this is the general pattern you get no matter what locality you are dealing with.

This curve will be shifted up and down depending on the fallout and the diet at that particular locality.

The difference between the young children and the average adult is about a factor of 7 or 8 in most cases.

Now, I wish to confine most of my remaining analyses to these two groups.

These data [indicating] refer to the adults, the 20 to 60 group, which are quite consistent everywhere we have studied, and the young children, zero to 4, because these are closest to the equilibrium with the diet. The children therefore give us the best measure of what the whole population will reach some day. Quite clearly this level will continue to rise as the bones of adults equilibrate with the environment, but they are doing it very much more slowly because of the large dilution of the dead bone which we have in our bodies pre-1954.

Next, what the are current levels worldwide?

The current levels for people who died in 1956 are as follows:

For the world, we have 2 categories, zero to 4, and the 20 to 60. We will consider the world, the North American average, and Chile.

The reason we use Chile rather than a South American average is simply at the moment we have a very large number of samples from here. So the statistics are good.

These results, based on approximately 400 samples, range as follows: 0.52; 0.70; 0.21; 0.08; 0.09; 0.04.

Now you will see immediately two things:

There is a factor of 7 or 8 between the children that are nearly in equilibrium, and the adults, you see, in North America are higher than the world average. Also North America is higher than Chile by about a factor of 4.

This is a very important fact because the difference between North America and Chile in the actual content of human bone is only different by about a factor of 4, or in the case of adults about a factor of 2. Yet is is quite clear from the soil data and the gummed paper data which Dr. Eisenbud presented that there may actually be a factor of as much as 5 or 10, in terms of fallout, between Chile and New York.

What are the reasons for this discrepancy?

The reason is that in our mechanized modern society, we bring food from considerable distances, and so the food tends to be pooled, and a particular single soil sample may not be representative at all of what the population is eating in that area.

A much more significant measure of what they are eating is the measurement of the milk consumption, as Dr. Eisenbud's laboratory has so completely done.

So we find that the geographical distribution is much more uniform than we might expect, and this is somewhat in disagreement with the emphasis of Dr. Machta yesterday of the differences between various places in fallout, but this is again a second order effect.

Next I would like to say something about the change with time.

We have a number of measurements which show the gradual increase of strontium 90 in human bone as time has proceeded from 1953 to the present.

I would like to give simply three illustrations of this.

The first illustration is that of New York City adults. In 1953, the average was about 0.04. In 1954, it was 0.07, and in 1955, it was 0.11, and in 1956 it was 0.13 micromicrocuries per gram of calcium.

So you see again, this general increase. However, as you note, this increase may be a factor of 3, whereas the milk shows a larger factor, posibly of 6. This is due, in part, I think, to this lag of equilibration, that is, the equilibration does not quite keep up with the change in the diet.

If we take quite a different case, that of Swiss children, in 1955 we find a considerable change, although the number of samples is limited to about a dozen, going in 1955 from 0.50 to in 1956 1.02. This again is for the zero to 4-year-olds.

Finally, to illustrate the change with time, I would like to take world averages for all children zero to 4 and compare 1955 with 1956:

In North America, 0.57 to 0.70;

In South America, 0.27 to 0.38;

In Europe, 0.40 to 0.42.

The overall worldwide average rose from 0.43 to 0.52.

This is the world change from 1955 to 1956, and it essentially means a 20-percent increase in the worldwide average from 1955 to 1956.

How do we explain this?

There is a considerable lag, of course, between the concentration of strontium 90 in the body and the time strontium 90 falls out. It is observed that in 1954 and 1955 the curve did not change; it is almost level. This was due in part to a very large increase in the Castle tests

of 1954, and then a leveling out. So this difference of only 20 percent is reflecting in part this rather level period.

Actually we do not have any measurements on children that are anywhere near equilibrium with this number.

One further word about geographical location.

The main thing to be said about geographical location, I think, is the amazing uniformity, the striking uniformity worldwide. Of our 25 stations, the maximum difference is a factor of 3 between the average zero to 4 children, let's say, in Chile and, maybe, Zurich, Switzerland. So that the maximum range in urban centers is very much less than the range in total fallout.

One other point on geography regarding the chairman's question to Dr. Eisenbud on Japan, and that is the average for Japanese children away from the atom-bombed area, is somewhat lower than the United States, consistent with the lower soil and fallout information.

Now let us consider the distribution around the mean.

It has been pointed out by a number of scientists that simply giving these average values does not tell the whole story, because even though the average value for children in North America, 1956, is only seven-tenths of a unit as compared to 100 units, nevertheless there is a question as to what is the distribution around this mean.

Clearly, if the distribution is very broad, then you may have an appreciable number of children that are very much higher than this. Therefore, I would like to consider what this distribution actually looks like, and what are the reasons for this distribution.

In order to do this, I would like to plot 2 curves, 1 for Swiss and Boston children, and 1 for German and British Columbia adults.

Chairman DURHAM. Do you mind a question at this point?

Dr. KULP. No, sir.

Chairman DURHAM. I believe yesterday we had evidence, of course, that the actual fallout of strontium 90 was nonuniform.

Dr. KULP. Right.

Chairman DURHAM. As I read your statement there, and analyze it, it is the fact that the analysis is uniform to a certain extent.

Dr. KULP. It is more uniform.

Chairman DURHAM. How is that?

Dr. KULP. It is more uniform.

Chairman DURHAM. More uniform?

Dr. KULP. Right. And the reason for this, I believe, is that, as we have shown in some independent experiments at Columbia, virtually all of the fallout comes down with rain, and you do not grow agricultural crops in a desert. So this helps to equalize the situation. Secondly, foodstuffs are transported great distances. Even some South American countries eat some wheat produced in Canada.

Senator ANDERSON. Doctor, your statement that you do not grow agricultural crops in the desert. Are you at all familiar with irrigation?

Dr. KULP. Yes, sir.

Senator ANDERSON. Do you know whether or not the State of California has moved ahead as one of the largest of the agricultural States, and, roughly, about how much of the agricultural produce of California comes from irrigated areas in Fresno, Salinas, and places like that?

Dr. KULP. Yes. I was coming to the point, if you will give me just a minute.

Senator ANDERSON. As long as you have it in mind.

Dr. KULP. This, of course, is largely truck farming, and other vegetables and fruits, rather than wheat.

Senator ANDERSON. People eat this so-called truck, do they not?

Dr. KULP. Oh, yes. I wish to come back to this point. It is a rather interesting one, and it shows something quite unexpected and quite favorable.

Senator ANDERSON. You said you were going to deal with some Swiss and Boston children, and some German and British Columbia children?

Dr. KULP. Adults.

Senator ANDERSON. Adults?

Dr. KULP. Yes, to look at the distribution around this mean we are talking about.

Senator ANDERSON. We will get to the point later sometime, but did you make a study of bones in British Columbia?

Dr. KULP. Yes, sir.

Senator ANDERSON. Did you find one of those that had very much greater deposit of strontium 90 in his bones than was normal?

Dr. KULP. Yes, sir.

Senator ANDERSON. Seventy times as much?

Dr. KULP. Yes, sir.

Senator ANDERSON. Did you discard that sample?

Dr. KULP. We did not discard it. We looked at it with a jaundiced eye, because the sample was measured in the very early stages of the work, and it was an extremely small sample.

Senator ANDERSON. A jaundiced eye is a diseased eye. [Laughter.] Why not look at it with a good eye?

Dr. KULP. A critical eye might be a better statement. The fact of the matter is——

Senator ANDERSON. Why not look at it with an impartial eye?

Dr. KULP. Impartial or critical is fine.

Senator ANDERSON. But they are not alike, are they?

Dr. KULP. Impartial and critical?

Senator ANDERSON. Are they exactly alike? Well, never mind. I am only hopeful we might look at it with an unprejudiced eye. Put it that way.

Here is a sample that is 70 times greater——

Dr. KULP. That is right.

Senator ANDERSON. Than any other sample you have had. So you say, "We will throw that one out."

Dr. KULP. No. I am very careful not to say we throw it out.

Senator ANDERSON. What do you do with it?

Dr. KULP. In our scientific report, the first report of this data, we were careful to include it.

Senator ANDERSON. Did you evaluate it fully or give it a smaller rating than something else?

Dr. KULP. No. It was clearly stated as a particular determination, and it was pointed out that although there is always the possibility of a single determination being in error, there is also the possibility, since we are dealing with the statistical phenomena, that this might be

a real high number. It seems very unlikely as more data has amassed, because we get nothing else like this with better samples and better techniques and the diet of the North American would not include anything that we know of that would lead to such a high value in an individual. Nevertheless, we cannot dismiss it, because we have checked back with the laboratory notebooks, and there is no excuse for throwing it away.

Senator ANDERSON. Of course, when the atomic bomb fell on Japan, they had never seen anything like that, either.

Dr. KULP. That is right.

Now, if I may proceed with these two distribution curves.

First, we will have the Swiss and Boston children. Interestingly enough, these two groups of children in 1956 have identical averages, and almost identical distribution curves.

We have the number of cases on the vertical ordinate and the horizontal obscissa is the strontium-90 in the units of micromicrocuries per gram of calcium.

Now, the distribution curve looks something like this (fig. 1). In other words, the initial portion of the curve is a very normal symmetrical sort of a thing. It has a peak at around 6 of these units, and it comes down to about 1.2. Then there are a few samples up as high as the highest value of 2.2.

Now you get the identical curve from both Boston and Zurich. The age distribution is from zero to 9 years here. So far as we can tell there is nothing unusual about either locality.

Now I would like to superimpose on this the curve for these adults, which is quite different in character, and then I will try to explain what I think is the reason for this difference (figure 2).

(The graphs referred to follows:)

FIGURE 1

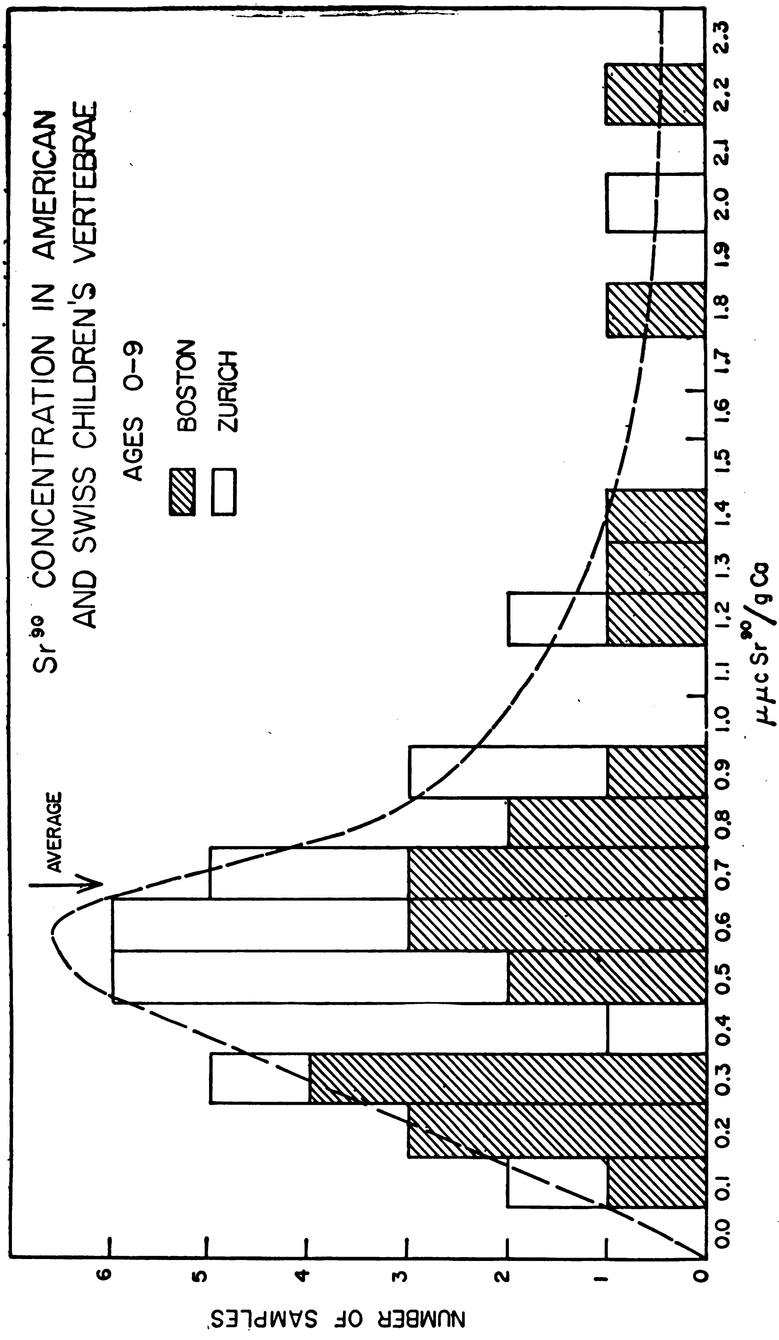
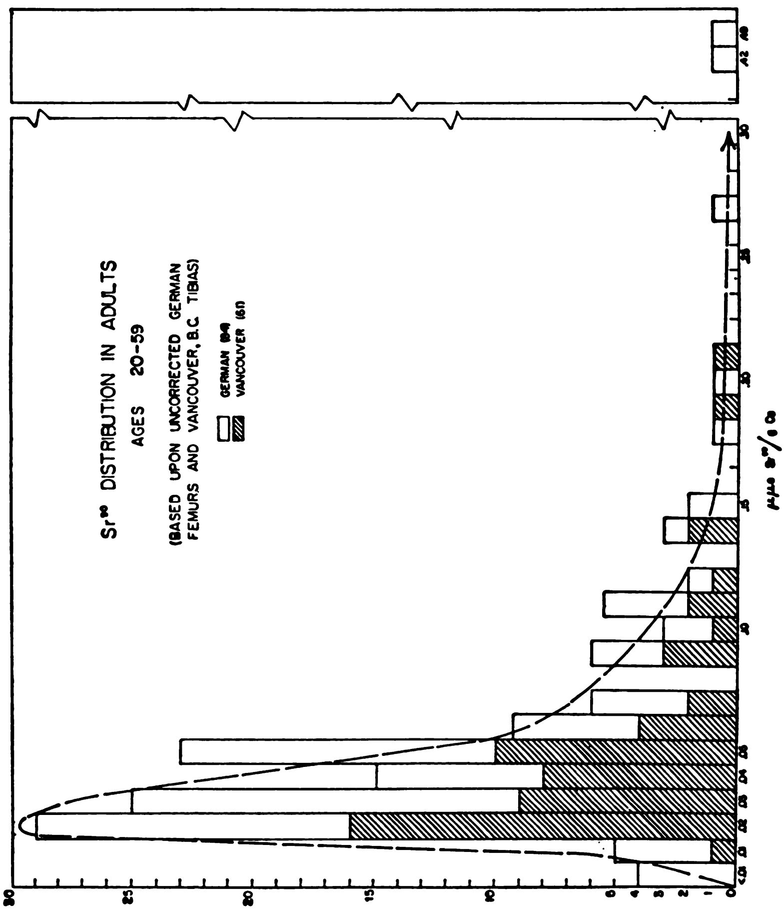

Sr^{90} CONCENTRATION IN AMERICAN
AND SWISS CHILDREN'S VERTEBRAE
AGES 0-9
BOSTON
ZURICH
AVERAGE
NUMBER OF SAMPLES
0
1
2
3
4
5
6
0.0 0.1 0.2 0.3 0.4 0.5 0.6 0.7 0.8 0.9 1.0 1.1 1.2 1.3 1.4 1.5 1.6 1.7 1.8 1.9 2.0 2.1 2.2 2.3
$\mu\mu c\ Sr^{90}/g\ Ca$

FIGURE 2


Sr90 DISTRIBUTION IN ADULTS
AGES 20-59
(BASED UPON UNCORRECTED GERMAN FEMURS AND VANCOUVER, B.C. TIBIAS)
GERMAN (84)
VANCOUVER (61)
NUMBER OF SAMPLES

Dr. KULP. The adult situation is closer to this on a different scale. The horizontal scale extends to about 0.2 instead of 2.

The reason for these variations are due to three sources: First, differences in diet.

This tends to be averaged out in a metropolitan community.

Secondly, you get some slight differences, due to age, but this is restricted here to 9 years.

But finally, the most important reason for variation of this kind is due to individual variations among the bones of the body. For example, if you take 2 ribs, 1 rib may have as much as twice the strontium 90 as another rib in an adult. In a child you do not get as much variation, because the child is almost in equilibrium. In other words, every bit of bone that is being built is very similar to the next bit of bone as far as the strontium 90 to calcium ratio is concerned. Whereas in an adult you may have a large section of bone which is not active at all, another section where it is quite active. So if you were to break up the bone, you would find an irregular distribution of strontium 90 in the bone.

We have done quite a few studies in an attempt to see what this variation is like, and we find that in children the variations from one bone to another generally do not exceed maybe 20 or 30 percent. Thus, we would expect a more symmetrical and narrower distribution in the case of children than in an adult.

You see the mean here is 0.6. Then the distribution around the mean is only about plus or minus 50 percent.

In the case of the adults, however, you have this large variation from one bone to another, and thus you observe a symmetrical curve. The mean develops a tremendous tail (see figure 2), due to the large variations from one bone to another.

The conclusion we draw from this is that it could be more desirable to analyze whole skeletons which we are now doing in 200 cases in 1 locality. Obviously in this sort of business it is difficult getting the proper samples. But in view of the fact that even the Atomic Energy Commission or the contractors cannot indulge in grave-robbery, it is not possible everywhere in the world to get the whole body sample, which is the ideal sample. Thus we must take the individual bones and understand how much they vary.

We believe that the variation is quite narrow for a given locality, and that the variation we are observing in the adults is largely due to variations from one bone to another.

We conclude, then, that so far as the present concentration of strontium 90 in man is concerned, that in urban populations the spread is very narrow; that it probably does not greatly exceed the normal variation in ordinary strontium to calcium.

Next, I would like to say just a little bit about the cycle of strontium 90, in particular with reference to some original work which we have done at Columbia.

In the past year we have measured all of the rains which have fallen in the area, and we have calculated the strontium 90 fallout per square foot as brought down by rain. We have also computed by measurement the strontium 90 per square foot that has been added to the soil of the New York area.

These two numbers agree rather well, and indicate that during the last year or year and a quarter about 10 millicuries per square mile

have come down. About three of these are probably due to stratospheric drip, and much of the rest is due to Russian testing. At least we did not report any testing during that period.

The present level in the United States——

Senator ANDERSON. So that a third is from what has been tested previously, and two-thirds came from the recent tests of the Russians?

Dr. KULP. Apparently, or at least half. Let's be more conservative, and say at least half in the last year seems to have come from tropospheric fallout from Russian testing.

Senator ANDERSON. You used the figure of a third of it came from drip, you said, from the stratosphere.

Dr. KULP. Yes.

Senator ANDERSON. And you thought the rest was from the Russian testing.

Dr. KULP. We did obtain from some carryover, probably, from our own testing.

Senator ANDERSON. Is not that important to us, to try to evaluate that also?

Dr. KULP. Yes.

Now, the next thing that we should point out, is that the average in the United States at the present time is, say, 25 to 30 millicuries per square mile, as Dr. Eisenbud has shown here; and that at the present rate of drip from the stratosphere we are adding a little bit more. If there is no more testing, we are adding a little bit more strontium 90 than is decaying away.

Senator ANDERSON. That is somewhat important. You say if there are no further tests, the present rate of drip would give us a little more strontium 90 than is decaying away?

Dr. KULP. For a time. And then eventually——

Senator ANDERSON. Well, can we state that for the present, at the present time that is happening? So that if there is testing going on, we are substantially adding, then, are we not, to the amount of strontium 90?

Dr. KULP. That is correct. Before finishing, I would like to compare these figures of varied predictions.

Senator ANDERSON. The statement has frequently been made, that we would be all right if we could keep a balance between the strontium 90 coming down to us from the stratosphere, plus the carryover from our previous tests, plus current testing as against the rate of decay.

Dr. KULP. Right.

Senator ANDERSON. And if we are not adding to it at any greater rate than the rate of decay we were all right. We were in balance. But it is your testimony that if we do not test any more, we will still have a greater drip, as you call it, than the rate of decay?

Dr. KULP. Slightly greater; yes.

Now, the next thing we must do is to examine what these discrimination factors are in going from the food all the way to man.

Representative HOLIFIELD. At this time, Dr. Kulp, I am going to ask you to look over the transcript, after you have received it, and prepare the charts you are referring to, so that we can have copies of them, and have them printed in the record at the appropriate place.

Dr. KULP. Yes.

Representative HOLIFIELD. Otherwise, your testimony in referring to these charts which you write on the board and the rates, will not be included.

Dr. KULP. Let us consider vegetation in general, for a moment. There are two routes of strontium 90 in vegetation to man. One route is through fodder to the cow, and then to the man through milk. Discrimination against strontium 90, considering strontium 90 per gram of calcium—the discrimination against strontium 90 in this step is about 7. The discrimination against strontium 90 in going from fodder to the cow's milk is a factor of about 7, as determined by Dr. Comar of the Oak Ridge Institute, and several other workers. The discrimination from cow's milk to man's bones appears to be about 2.

Senator ANDERSON. Can we understand what a factor of about 7 means?

Dr. KULP. It means that the strontium 90 per gram of calcium is 7 times less in the cow's milk than it is in the feed for the cow; and it means that the strontium 90 per gram of calcium in deposited human bone is half what it is in the milk which the human consumes once the human is at equilibrium.

Now, there is another route through which strontium 90 can come to us from vegetation, and that is our other direct vegetable sources. Meat contributes very little strontium 90. Drinking water contributes very little. But we can get, in principle, detectable and significant amounts from other sources of food.

Just within the last 6 months, we have begun a measurement program, attempting to analyze all varieties of frozen foods, such as those which come from the Imperial Valley of California. Somewhat to our surprise, we found that the strontium 90 per gram of calcium is very much lower in these vegetables than in fodder.

Reflection on this, of course, made us realize that the cow's fodder by and large consists of grasses that utilize the top few inches of soil, whereas the truck-farm vegetables commonly have deeper roots and, furthermore, the truck farms are very carefully nutured and calcium is added to the soil. So whereas the cow's fodder is now on the order of 40 to 50 micromicrocuries per gram of calcium [indicating], and the milk is on the order of 5, the truck-farm vegetables are averaging about 6. It turns out that when you eat these kinds of vegetables, the discrimination is not 2, as it is with milk, but fortunately it is 4. Therefore, at the present time, about 80 percent of the child's calcium comes from milk and dairy products, and at least half of the calcium of the adult comes through this route, even for a real coffee drinker, nevertheless, almost all of the strontium 90 is coming via this route [indicating].

If, however, the truck farm vegetables were as high as the grasses, then the vegetables would be our major source of strontium 90.

Actually our main strontium 90 source is milk and milk products [indicating], even though it is discriminated against by a factor of 14. Most of the strontium 90 is coming from this route [indicating].

Representative HOLIFIELD. How does this pattern differ when you consider vegetation and vegetables and fodder and intake and uptake and so forth, from an area where it has plenty of rainfall as compared to an area that is irrigated, such as Imperial Valley? Is there a difference?

Dr. KULP. Actually, the Imperial Valley, for the same kind of vegetable, would be a lot lower.

Representative HOLIFIELD. You say a lot lower. Have you tested vegetables in milksheds, the New York milkshed, for instance; New Jersey? There are a lot of vegetables raised there, and I think they depend more on rainfall than they do on irrigation.

Dr. KULP. This program is only begun, but we have about two samples from the irrigated area of California, and we have several from Maryland, some of the Maryland truck farms. And the California ones are lower. But both of them compared to the milk are lower.

Representative HOLIFIELD. In other words, the milk is higher in the rainfall areas and the vegetables are higher in the rainfall areas. Do you have enough data in order to give us what the degree is instead of the word "lower"? Do you have enough data?

Dr. KULP. No; I do not think we have enough data to say anything beyond that it may be as much a factor as 5 lower.

Representative HOLIFIELD. In another statement we have here, the rainfall data for Brawley, which is in Imperial Valley, in 1955 was 1.70 inches. You see that is very low. I suppose in this area it would be around 40, maybe 45, would it not?

Dr. KULP. Yes.

Representative HOLIFIELD. And the rainfall does bring out of the atmosphere, according to your own testimony—it is the primary factor in bringing down radioactive particles, prime particles. There would be a sharp difference, I would suppose.

Dr. KULP. Yes; and this is the reason Dr. Eisenbud was saying why you can get extremes on the low end, but you do not get any extremes on the high end. The maximum fallout, due to the 40 to 60-inch rainfall, is up to 100 or 150, which is only a factor of 2 higher than the average. But you can go down an inch or less than an inch of rainfall in desert area which is lower by the factor of 40.

The next thing I would like to do as far as strontium 90 in man is concerned, is stick my neck out on a few predictions.

Again I would like to refer to these values——

Representative HOLIFIELD. When you say "these values," the record will not show what you mean.

Dr. KULP. The thousand micromicrocuries of strontium 90 per gram of calcium for the industrial permissible level, and the 100 which is recommended for general population.

As far as adult man is concerned, in 1956 he has about one-tenth of a unit, which is 1,000 times less than this 100, or whatever the experts will make it this next week. Our job is to try to determine what is there.

Secondly, the children, worldwide, in 1956, are about 0.5 and in the United States about 0.7.

I would like to divide this now into two groups because we will predict both for the United States and for the world.

So far as the United States is concerned, the present children are about 0.7. The average world is about 0.5, with, of course, Chile being lower than that.

Now, let us ask a question. We know these are not in equilibrium. The first question is: If there is no more testing, and everything comes down, and we all reach equilibrium, what will these numbers look like? My prediction is they will be about 3 and 1.

Chairman DURHAM. When? What date?

Dr. KULP. Within 10 years.

Senator ANDERSON. That jumps from 0.7 to a full 3?

Dr. KULP. Yes. This is not because we are waiting for three times as much to fall out; we are waiting for equilibrium.

Senator ANDERSON. That is based on no more testing?

Dr. KULP. Correct.

Senator ANDERSON. Just so we can see where we might go, how many years did you say that would be—5?

Dr. KULP. Five to ten years, as we approach this [indicating].

Senator ANDERSON. If instead of there being no more testing, you would make a graph of the growth of testing, and how to apply that to the figure of 3, what would you bring it to—30?

Dr. KULP. I would prefer, if you let me take the next step, which is to say what will these numbers be if the present rate of testing continues.

Senator ANDERSON. And then from that we can interpret what it might be if the curve moved up?

Dr. KULP. Yes.

This is "no testing."

The next step is: What if every 5-year interval from now on we have the same amount of testing we have had in the last 5 years? The numbers then come up to 24 and 8.

Senator ANDERSON. Are we to read that in connection with 100?

Dr. KULP. Yes.

Senator ANDERSON. So they would have a fourth as much?

Dr. KULP. Right.

Senator ANDERSON. If we happen to have acceleration of tests, as we are now doing, and the British get into the picture and the French and the Italians, and the Germans and everybody else, it would only take a very few years, 3 or 4 at the most, to get that up to the 100, would it not?

Dr. KULP. I think anyone can calculate from here for any predicted rate what it would be.

Senator ANDERSON. I was going to try to give a scientific guess, and I was going to have my own curbstone opinion. But, in any event, if it is going to come to 24 if we keep up the present rate of testing, we freeze it at the present level, and we know we are moving faster, we know it is going up to whatever the maximum permissible level is for children?

Dr. KULP. Right.

Senator ANDERSON. Thank you.

Representative HOLIFIELD. Let me understand. When do we get to the 24—in 5 years, is it?

Dr. KULP. No.

Representative HOLIFIELD. In how many years?

Dr. KULP. This would probably require 50 to 60 years at the present rate—every 5-year interval there is the same amount we have had in the last 5, and we keep on with this. Then in the year 2010, or something like that, we come up somewhere near these values.

Senator ANDERSON. If the rate is frozen for 6 years, we come up with the rate of 24, but if the present rate of acceleration continues——

Dr. KULP. One can make his own computations.

Senator ANDERSON. Wet get to a hundred in a very few years. Depending on one's own calculations. That is right. One man's guess is about as good as another's.

Dr. KULP. I do not think it is a guess. I think we have enough data now so we can predict within a factor of 2 what will be for any set of circumstances.

Senator ANDERSON. Has not someone fed this into the "Maniac" out at Los Alamos or a calculating machine somewhere else, to have it say if the rate of increase in testing—with the way these new countries are getting reactors under the Atoms for Peace program, and if they all get in the game, if we calculate all this new increase, all these new reactors coming in, and so forth, has not somebody tried to make a calculation?

You had to use a little bit of mathematics to make the calculation of 24 in 6 years. What happens if we increase at the same rate we have been increasing in the last 5 years in testing? Has anybody tested that or calculated that?

Dr. KULP. I do not know. I have not.

Senator ANDERSON. I would suppose there would be somebody that would feed it into one of these mechanical brains and try to see what it comes up with.

Dr. KULP. You will have to ask the Commission that, I think.

Senator ANDERSON. We will ask somebody that. You have not, in any event?

Dr. KULP. No.

Senator ANDERSON. It would not be in your field of competence?

Dr. KULP. Overall, it is not a difficult thing to do once basic perimeters are established.

Senator ANDERSON. I would not think so. I would think you would have all the information necessary for someone to drop it into a calculating machine and get the answer.

Dr. KULP. I would like to conclude these points——

Representative VAN ZANDT. May I ask several questions at this point, Mr. Chairman?

Dr. KULP. Yes.

Representative VAN ZANDT. In October 1956, Dr. Libby said that United States children will eventually accumulate up to 1 percent of the maximum permissible concentration of strontium 90.

Do I understand that this point 7 you have on the board is seven-tenths of 1 percent of the maximum permissible concentration?

Dr. KULP. Correct.

Representative VAN ZANDT. Then the numeral 3 there represents 3 percent?

Dr. KULP. Right.

Representative VAN ZANDT. And the 24 represents 24 percent of the maximum permissible concentration of strontium 90?

Dr. KULP. Correct.

This data is not in sharp disagreement with Dr. Libby's statement, because a year ago we would have predicted, maybe, 1½ here instead of 3. The reason it is 3 rather than 1 is simply because the Russians are quite active and more things have happened. His calculations were based on what had fallen out as of early 1956.

Representative VAN ZANDT. Dr. Kulp, you have described the routes by which strontium 90 can reach the human body, through vegetation and through milk?

Dr. KULP. Yes.

Representative VAN ZANDT. What about the human body absorbing the radiation from that which is breathed into the lungs?

Dr. KULP. This has been studied rather carefully by other groups than ours. You can probably get more of this information from the men next week. But the conclusion has been that the inhalation hazard is definitely negligible compared to the ingestion hazard.

Representative VAN ZANDT. What about the particles settling in a man's hair?

Dr. KULP. Trivial.

Representative VAN ZANDT. What about the particles settling on the inside of a man's clothing?

Dr. KULP. Trivial.

Representative VAN ZANDT. So the only way that he can really get a dose of strontium 90 is through the milk?

Dr. KULP. Right.

Representative VAN ZANDT. Or eating the vegetables?

Dr. KULP. Right.

We have not discussed maximum man, and this is a very difficult problem, but it is also a very important one, probably in the overall philosophical consideration of this subject.

We have talked about average man, we have talked about the distribution curve for people living near cities, but we do not have any samples that might approximate maximum man.

We have already said in our earlier discussions that the greatest variation of curves is due to the variation in the calcium content of the soil. Therefore, if we were to imagine a primitive culture where the calcium content of the soil was extremely low, and where the people of that area ate food which they grew only on their own half acre, then these people might be very much higher than anything we have been talking about here. However, they would have to be quite primitive.

Even in areas of North America they bring in appreciable food, but there are some obscure Indian tribes, of course, in the upper Amazon, that might very well live on food from their own village area. And this is where maximum man would be found, but nobody has the samples yet.

This variation is not due to differences in fallout; it is due to differences in the calcium content in the soil, and the lack of mixing of food products.

I would conclude, then, as far as the technical remarks are concerned, that, first, the current levels in man are fairly well defined, sufficiently well that I believe that, given any set of circumstances you wish to predict for the future, it is possible to compute within fair accuracy, say at least a factor of 2 or 3, what the levels in average man will rise to.

The distribution curve for modern urban societies is very narrow and, therefore, it is practical to talk about the average value of these measurements.

Much of the variation, apparently, can be due to the variation within the bones.

Chairman DURHAM. Doctor, that statement is based on the known amount of strontium today which exists?

Dr. KULP. Right. And we do not have the samples of maximum man. But certainly this cannot be represented by more than a very, very minute fraction of the world's population.

Finally, Mr. Chairman, if I might add 1 or 2 words as a citizen, rather than a scientist, for the record I would like to note three things:

First, that I do not think the difference in the opinion of scientists, such as Dr. Libby and Dr. Pauling, are anywhere near as great as some of the newspaper accounts would lead us to believe. I believe that as far as scientific data is concerned, they are probably in rather close agreement.

Dr. Libby has carefully said, or has pointed out that he is not saying there is no risk, and Dr. Pauling has admitted that the amount of radiation from strontium 90 is a very small fraction of the natural background. On these basic facts all are agreed.

Secondly, I would like to mention that I feel the whole problem of worldwide fallout from testing is completely trivial compared to the rather seemingly inevitable problem and continuing hazard of nuclear warfare.

Finally, I would like to emphasize that, if we assume that there is no threshold for bone cancer or leukemia, as some scientists now feel is probable——

Representative HOLIFIELD. What do you mean by "no threshold" please, for the record?

Dr. KULP. I mean that no matter how little radiation there is, there is some effect from it, that the number of cases are linear, or nearly linear right on down. This, of course, is what Dr. Pauling was using in one way or another in computing his number of deaths.

One can get quite different numbers, depending on assumptions, but philosophically there is no difference because if you admit there is no threshhold, there certainly are some deaths, and whether there are a 100 or 10,000 seems very irrelevant philosophically.

The point I wish to make is this: All of our environmental measurements, all of our models for the reactors, and the entire utilization of isotopes are based on maximum permissible concentration. That is, when we say the effluent from an industrial plant using atomic energy, the effluent in the river water must be below a certain value, that is set so that if a person drank that river water for the rest of his life, he would not exceed this MPC.

Representative HOLIFIELD. You are pointing to the top one now?

Dr. KULP. I am pointing to this—or this. No.

Representative HOLIFIELD. Please say it.

Dr. KULP. I think actually it is geared to this [indicating].

Representative HOLIFIELD. Now will you clarify your remark for the record so we will know what you are pointing to?

Dr. KULP. My point is that the present maximum permissible concentrations which are allowed for air, water, and soil in terms of disposal are maximum permissible concentrations calculated, such that a person living in an area and consuming that water and breathing that air for the rest of his life would not exceed the maximum permissible level of strontium 90 in the general population.

Representative HOLIFIELD. Which is?

Dr. KULP. One hundred micromicrocuries of strontium 90 per gram of calcium.

Representative HOLIFIELD. Fine.

Chairman DURHAM. Up to 70 years old, and then the rest of his life?

Dr. KULP. Yes, sir; the rest of his life.

Representative HOLIFIELD. That is from that one source.

Chairman DURHAM. No.

Dr. KULP. No; this is from any industrial application.

Representative HOLIFIELD. I understand. But you are applying it to that one source. That does not take into account how much he might get from other sources. You are referring it to the one industrial effluent disposal, are you not?

Dr. KULP. Right.

Representative HOLIFIELD. But he gets other contamination from other sources besides that particular industrial effluent.

Dr. KULP. Yes, but I would like to leave that out for the moment, and just consider the industrial.

Representative HOLIFIELD. All right.

Dr. KULP. My point is this; that if there is no threshhold, then we are taking the same risk essentially, by entering the atomic era industrially. In fact, we will probably wind up taking more risk than at least with present testing.

I would like to try to make this very clear, because I feel that in discussions I have read this point has not been made very strongly. The point is: If our maximum permissible concentration is significant here, namely, that 3 is small compared to 100, then 3 is also small compared to 100 in industrial applications. And if we are going to say that the maximum permissible concentration must be zero, then we cannot have any reactors, and we cannot have a nuclear era. Obviously, we are going to have a nuclear era and therefore we are going, if there is no threshold, to have deaths in the world as a whole from peacetime application, regardless of whether it is testing or not.

Chairman DURHAM. Doctor, is not there a difference there because of the fact that most of it is confined in the reactors safely, and in the release of atomic weapons you released into the air?

Dr. KULP. It is only confined safely within these prescribed limits. But if we say the limits are not right, they should be zero instead of 100, then we are no safer there than we are with anything else.

Mr. RAMEY. You do not have a strontium 90 problem, do you, from reactors as such, into the atmosphere, except in the event of an accident, and then that would just be a local problem, would it not?

Dr. KULP. We cannot go into the individual types of applications, but in any application that involves mixed fission products you have to get rid of radioactive wastes. And normally you get rid of high-level things, but then you have very low-level wastes which you can just throw away because it is below the MPC.

Representative HOLIFIELD. But your figures there are based on rules and regulations set up for the protection of the people, are they not?

Dr. KULP. Right.

Representative HOLIFIELD. But any violation of those rules would upset your calculation. I speak particularly of the sabotage of a

power reactor or the meltdown of a reactor such as has occurred in the experimental models. Now we are building these reactors of 100,000 to 260,000 potential kilowatt capacity. Assuming there was a sabotage, or assuming that one of these reactors would get out of control, then this would upset your whole calculation as far as the area contaminated from that particular reactor, would it not?

Dr. KULP. Yes. But the point I was trying to make was the contamination you would get from perfectly normal operations under all the present rules.

Representative HOLIFIELD. That is true. That is true. But accidents do occur, as you know. So while it is very comforting to be told that the rules are good, the speed limit has been set on the highways, but we know that people do exceed the speed limit and there are wrecks as a result.

Dr. KULP. And people get killed below the speed limit.

Representative HOLIFIELD. Those who go beyond the rules and regulations.

So that is a factor which we must also take into consideration as responsible Members of the United States Congress; is it not?

Dr. KULP. Yes.

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. Dr. Kulp, yesterday we were told that an all-out attack on the United States would require about 2,500 megatons of TNT equivalent, and it would destroy about 82 million people—50 percent by radiation, and 50 percent by blast and heat.

At the present time we have 2,500 megatons equivalent to TNT as far as radiation is concerned in the air. Based on your figure of 3 percent of the maximum permissible concentration, what would the figure look like after this 2,500 megatons of equivalent TNT had been deposited in the atmosphere?

Dr. KULP. Well, the difficulty here is the defining of the geographical area, but it would probably be more than a hundred times this.

Representative VAN ZANDT. A hundred times, with the possibility of a hundred times more?

Dr. KULP. This is not worldwide. Assuming a restricted area in the United States.

Representative VAN ZANDT. The Western Hemisphere.

Dr. KULP. Adjacent to where the detonation is concerned.

Representative VAN ZANDT. A hundred times more would be in the atmosphere. What would that do to those who would be left after such an attack?

Dr. KULP. Again I would refer you to the biological experts next week. It would appear on the present rules, if you got a hundred more times higher than this worldwide, this would have serious effects.

Representative VAN ZANDT. In other words, in addition to the 82 million people who were killed, and those that were suffering from radiation and heat, and so forth, who were not killed, the remainder of the population in the Western Hemsphere would be faced with possible death through this concentration of radiation that you speak of?

Dr. KULP. A certain fraction; yes.

Representative VAN ZANDT. Dr. Kulp, is it not true that you have the fact that, in addition to the reduction caused by the yield of the weapon, those who survive are also faced with this threat?

Dr. KULP. Yes.

Representative HOLIFIELD. Thank you very much, Dr. Kulp, for your presentation.

Dr. William F. Neuman, of the University of Rochester School of Medicine and Dentistry, will be the first witness this afternoon.

Before we recess I would like to insert in the record an article from Science magazine by Dr. Kulp, Walter R. Eckelmann, and Arthur R. Schulert entitled "Strontium 90 in Man," to gether with two letters to the editor of Science magazine. Also an article from the same publication entitled "Strontium Content of Human Bones," by Dr. Kulp and K. K. Turekian.

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STRONTIUM 90 IN MAN

J. Laurence Kulp, Walter R. Eckelmann, Arthur R. Schulert [1]

Radioactive fallout at great distances from atomic explosions produces both internal and external hazards to the human race. The external hazards result from the interaction of gamma rays in the environment on the genes of individuals, which produces an increased mutation rate. The increase in normal gamma background owing to fallout is very small, so far, and it is being carefully monitored (1). The tolerable level of external gamma radiation for genetic effects is not well defined (2). The internal hazard is primarily the development of bone cancer, because of the presence of strontium 90 (half-life 28 years). Libby (3) has discussed the general problem of strontium 90 in fallout and has presented considerable data (4, 5) on the concentration of this isotope in various parts of the chain from the atmosphere to man.

This article (6) summarizes the results obtained at Lamont Geological Observatory on the strontium 90 content in man (based on a worldwide sampling network) and attempts to evaluate the potential hazard. The work presented here is a part of a more comprehensive study of the geochemistry and biochemistry of strontium 90 that has been in existence at Lamont for several years (7, 8). The first experimental verification of measurable quantities of strontium 90 in animal bone, milk products, and soil was made at Lamont in August 1953 following the prediction by Libby, Eisenbud, and others in July 1953 (9) that it might be found in detectable quantities if low level techniques were employed.

At the present time strontium 90 can be found in all human beings, regardless of age or geographic location, provided that a sample of adequate size is available. As is shown here, these quantities are small compared with the maximum permissible concentration (MPC) (1.0 millimicrocuries of strontium 90 per gram of calcium) established by the National Committee on Radiation protection (10). However, the existence of measurable quantities makes it possible to analyze the present distribution of strontium 90 with regard to age, sex, diet, geography, and time. Such information is fundamental for making predictions about the probable effects of future nuclear explosions.

DISPERSAL

The route of strontium 90 from the time of fission to its uptake in human bones is known in broad outline. The explosion releases the strontium 90 into the air, where it is then carried for great distances. Eventually it is transported to the soil and becomes a part of the base-exchangeable alkaline-earth-metal ions in the upper few inches. Since plants take up this radioactive strontium along with their necessary calcium, human beings ingest strontium 90 from vegetables and milk products.

Kiloton explosions produce debris mainly in the lower atmospheres (troposphere), from which the strontium 90 is deposited in a few weeks (5). This

[1] The authors are on the staff of Lamont Geological Observatory, Columbia University, Palisades, N. Y.

debris is deposited in a restricted latitude. Thus the Nevada test series distributed strontium 90 largely over a narrow latitude band in the Northern Hemisphere, with a higher concentration in the United States than elsewhere. Megaton weapons, on the other hand, appear to put most of their strontium 90 into the stratosphere, where it is more or less uniformly distributed with respect to latitude. It then passes very slowly (about 10 percent per year) back into the troposphere (4), from which it is rapidly washed out. Megaton explosions, therefore, tend to equalize the worldwide fallout pattern. The local distribution will be modified by rainfall, vegetation cover, and topography, but the latest total fallout data (1) support a rather homogeneous distribution that shows maximum variations of only a factor of 3 at the longitudes of Africa and New York. The distribution in the Northern Hemisphere remains higher than that in the Southern Hemisphere, because of the kiloton shots from the Soviet and United States test sites.

It is now reasonably well established (5) that the scavenging action of rain is responsible for most of the deposition of tropospheric debris. Experiments made at this laboratory during the past 6 months confirm this conclusion. Other experiments at Chicago (11) and at Lamont (8) indicate that at least 60 to 70 percent of the strontium 90 which has fallen out in the United States is in the soluble form and is therefore available to plants. It is suspected that the megaton debris from the Pacific tests has a higher fraction of soluble strontium 90 than debris from other tests, but this remains to be confirmed. About 80 percent of the strontium 90 is found in the upper 2 inches of the soil, but in some cases detectable amounts may be carried down as far as 12 inches (8), because of the type of soil, topography, and drainage pattern. Measurements of the soil and plant content of strontium 90 per gram of available calcium in 1953 (4) can be interpreted as meaning that some plants have significant surface retention of strontium 90, or that the concentration of strontium 90 per gram of calcium in the 0- to 2-inch interval is not representative of the true environment of the roots. As the ground becomes progressively more contaminated with strontium 90, however, the surface effects become obscured. Thus, on the east coast of the United States during the Nevada tests in the spring of 1955, the surface fallout of strontium 90 was only a very small fraction of the total strontium 90 in the plants (8).

The total fallout of strontium 90 was estimated by Libby (5) at the end of 1955 to be about 13 millicuries per square mile in the upper midwestern region of the United States. The soil data of Hardy and Morse (4) suggest an average of about 15 millicuries per square mile for the United States. On the basis of the fallout of mixed fission products and an estimate of strontium 90 fractionation at long distances from test sites, Eisenbud and Harley (1) calculated a fallout of 13 millicuries per square mile for the United States in late 1955. By comparing the average total fallout on gummed paper for the United States in the fall of 1955 (1) with that for the world, a worldwide average deposition of strontium 90 on the soil of 8 millicuries per square mile can be calculated.

The amount of strontium 90 in the soil which gets taken up by the plant depends on the root depth, the calcium content of the soil, and the biological fractionation factor. Menzel (12) has shown that strontium is discriminated against by a factor of about 1.4 when it goes from soil to plant. The calcium content of the soil varies greatly. Values of 0.4 to 40 milliequivalents of exchangeable calcium per 100 grams of soil are common. Further, as noted in a foregoing paragraph, the concentration of strontium 90 drops rapidly with depth. These factors make it possible for the concentration of strontium 90 per gram of available calcium to vary by a factor of more than 100 for a given amount of fallout. Since the biological hazard may be stated in terms of concentration of strontium 90 per gram of calcium, it is clear, then, that merely to consider average values of strontium 90 in soil is not sufficient. Thus, although the fallout of strontium 90 per square foot in the New York area in 1955 varied by a factor of 7 (144 to 1010), the range in micromicrocuries of strontium 90 per gram of available calcium exceeded a factor of 40 (6 to 250) (8).

BIOCHEMICAL CONSIDERATIONS

Most people in the United States obtain their calcium through milk products. Here, fortunately, there is a discrimination factor of 7 against strontium from the plant that the cow consumes to the milk produced, according to experiments by Cosmar (13).

Regardless of the dietary source (milk products, vegetation, meat, or water), strontium 90 will follow calcium in the body, but it is discriminated against in going from the intestines to the blood by a factor of 2 to 3 (14, 15). Studies on human beings who have been given intravenous tracer doses of strontium 85 and calcium 45 simultaneously show that strontium is also discriminated against in the process of bone deposition. This, together with the fact that the body preferentially excretes strontium, results in a progressive enrichment of the bone in the calcium isotope following a single administration of the two tracers, the experimentally determined factor being 2.0 at 1 month and gradually increasing (16). On constant dietary intake, it would appear that an equilibrium enrichment of about 3 would be obtained in going from blood to bone, so that the total discrimination against strontium in going from the food to bone is about 8

Appreciable local variations in strontium 85 content per gram of calcium occur in individual bones after a single dose. The "hot spots" that appear on autoradiographs are probably of less consequence in the case of strontium 90 than they are in the case of radium, because the dimension of these localizations is usually much less than the range of the beta particle that is emitted from the strontium isotope. Table 1 shows that real differences in strontium 85 content per gram of calcium exists among the various bones of a particular skeleton. Although the data shown are for an individual who died 39 days after administration, the ratios proved to be relatively uniform for 7 other cases ranging from 3 hours to 125 days.

From the unpublished data of Trotter on the weights of individual bones in the human skeleton and from information on the percentage of calcium in the bones per gram of calcium exists among the various bones of a particular skeleton. load of strontium 90 from any given bone. The bone most frequently obtained at autopsy is the rib. It may be noted from table 1 that the concentration of strontium 85 per gram of calcium in the rib is twice that of the average body. Other bones frequently received in the worldwide survey are femur and vertebrae, which contain 0.72 and 4.2 times the average strontium 85 concentration of the whole skeleton, respectively.

These are the primary concepts and data that must be used in interpreting the worldwide human assay.

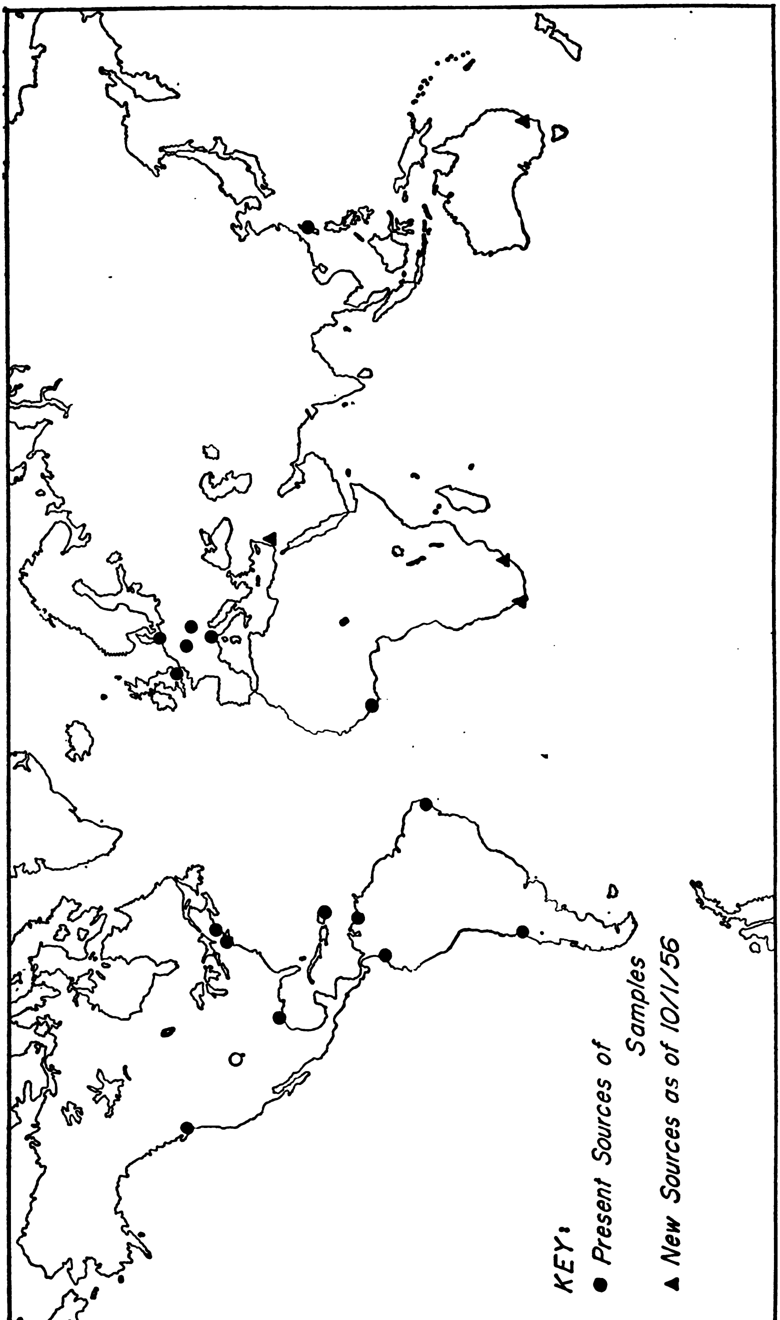

FIGURE 1. Worldwide network for collection of human bone samples. The triangles indicate new stations for which data are not yet available

TABLE 1.—*Relative size of bone and distribution of strontium 85 in human skeleton*

Bone	Percentage of skeleton (dry weight) [1]	Percentage of calcium of skeleton	Percentage of Sr-85 per gram of calcium [2]
Long bones [3]	52.5	57.9	0.0187
Femur	18.7	20.6	.0219
Humerus	6.9	7.6	.0171
Radius	2.3	2.5	.0170
Skull and mandible	17.9	19.7	[4] .0094
Rib	5.7	6.3	.0618
Vertebras	8.6	7.1	.128
Sternum	.3	.2	.138
Weighted average			.0303

[1] Data kindly supplied by Mildred Trotter, Washington University School of Medicine.
[2] Data from Schulert, Laszlo et al., giving concentrations 39 days after administration of isotope. Other data taken from 3 hours to 125 days after administration of isotope show similar relative distribution of strontium 85.
[3] All the limb bones plus the pelvis. In averaging, it was assumed that the 3 analyzed are representative of the total.
[4] Skull.

SAMPLING

Autopsy samples of human bones were obtained from 17 stations in a worldwide network (17) (Fig. 1). To date, more than 1,500 samples have been received, and about 600 analyses have been made; the bulk of the samples have come from about 10 stations. The size of sample ranged from 1 to 200 grams of wet bone. An attempt was made to get as wide a geographic and dietary distribution as possible, but the distribution was necessarily limited by our contacts with physicians in certain centers. Future sampling will utilize a wider network, and considerable use will be made of integrated samples.

The bones employed were usually ribs, but those from Germany were femur shafts, and those from Switzerland, England, and Denmark were vertebras. In all cases the entire rib, shaft, or vertebras section was ashed and analyzed to avoid local variations that do appear both laterally and vertically in the bone.

The early results suggested that there was negligible strontium 90 in persons over 40 years of age; hence, sampling was concentrated in the younger age groups. It is clear that this is no longer true and that a broader spectrum is now desirable.

A few samples of adult bone measured at Lamont and many stillborns analyzed at Chicago (4) date as early as 1953, but for most localities the samples were procured in 1955, so that a clear definition of the rate of change of strontium 90 concentration cannot yet be made.

ANALYSIS

The radiochemical procedure (18) consists briefly of ashing the bone, dissolving in hydrochloric acid, precipitating calcium oxalate, igniting to calcium oxide, resolving in hydrochloric acid, adding nonradioactive yttrium carrier, and milking the yttrium 90 daughter of strontium 90 as the oxalate. Usually the first milking brings down some foreign activities so that a second one is required for the quantitative assay of yttrium 90. Purity is checked by monitoring the decay of the yttrium 90 precipitate. The yttrium oxalate precipitate is counted in a convenient low-level system that has been described elsewhere (19). This procedure makes it possible to determine less than 1 disintegration per minute of the strontium 90 sample. At the level of 10 disintegrations per minute, per sample, the precision is a few percent. Even at low levels of activity, the variation in the individual samples is considerably larger than the experimental error.

The data reported in this paper were obtained at the Lamont Observatory and at two commercial laboratories: Isotopes Inc., Westwood, N. J.; and Nuclear Science & Engineering Corp., Pittsburgh, Pa.

RESULTS

All of the analyses of strontium 90 in human bone reported in this study are summarized by locality in figure 2. The results are given in micromicrocuries of

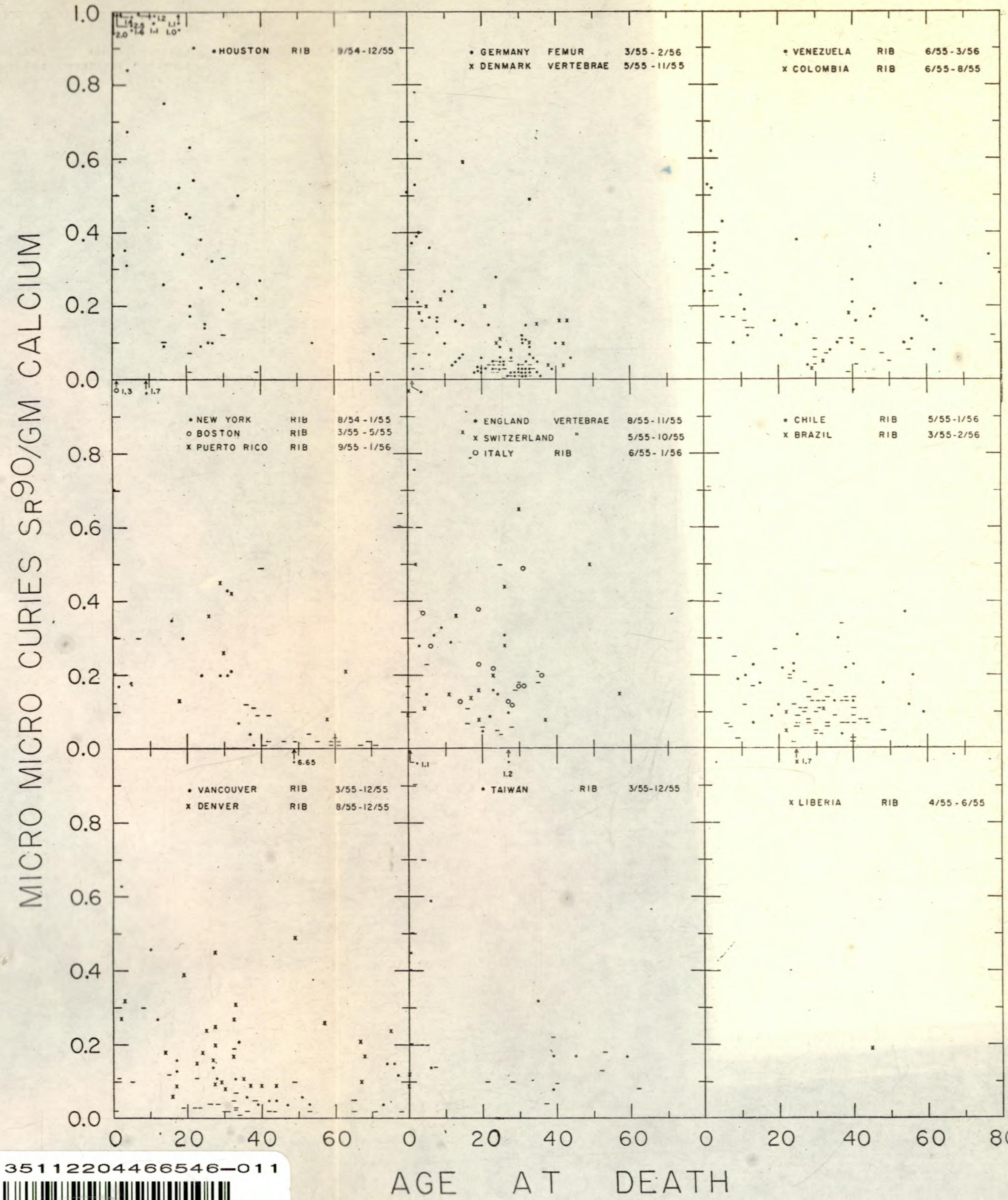

FIGURE 2. Strontium 90 content of human bone in 1955.

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strontium 90 per gram of calcium. The dashes mean that the sample contained equal to, or less than, this amount. The error in a determination is generally less than 20 percent, but for a few of the very small samples or those with only a few hundredths of a micromicrocurie of strontium 90 per gram of calcium, it may be considerably higher.

Table 2 shows the averages for all localities broken down into age groups. All values in this table are normalized to an average skeleton, using the aforementioned factors. The weighted averages for each age group were calculated for each continent and for the world. Finally, a total maximum world average of the concentration of strontium 90 in the human skeleton was obtained. The average deviation for a given age group and locality was commonly around 50 percent. All samples whose analyses were reported as "equal to or less than X" have been assumed to contain X micromicrocuries of strontium 90 per gram of calcium for the purposes of this averaging. Thus table 2 actually represents the maximum average strontinum 90 content. For most localities, however, this makes little difference, because the numbr of such analyses was small or the analyses were in the range of low concentration. In the case of Chile and Brazil, however, the actual average concentration is probably about 25 percent lower than this maximum value.

One sample with very high concentration of strontium 90 was not included in the average. This sample was from Vancouver, British Columbia (49 years; tibia), and it had a concentration of 6.6±0.3 micromicrocuries of strontium 90 per gram of calcium. This would give a skeletal average of about 9.1 micromicrocuries of strontium 90 per gram of calcium. Analytic error owing to contamination appears unlikely, since other samples that were processed at the same time showed very low activity.

DISCUSSION

From the analytic data shown in figure 2 and table 2, the following tentative conclusions may be drawn:

(1) The present worldwide average content of strontium 90 in man is about 0.12 micromicrocuries per gram of calcium, or one ten-thousandth of the presently accepted maximum permissible concentration.

(2) The averages for the different continents are surprisingly similar, indicating that already the stratospheric drip of strontium 90 from megaton explosions is swamping the local concentrations from both the Nevada and the Soviet test sites. There is evidence, however, that Chile and Brazil have clearly lower concentrations than those localities in the Northern Hemisphere for which a large number of samples are available. The close similarity between Houston, Tex., and Bonn, Germany, for which good sampling is available, emphasizes that the differences as a function of longitude in the Northern Hemisphere are small. Since Taiwan has appreciable fallout from Pacific and Soviet tests (1), it is not surprising that its values are similar to those for North America.

(3) There is clearly an age effect, at least in the first 20 years. Young children have 3 or 4 times more strontium 90 per gram of calcium, on the average, than adults. This effect reflects the larger proportion of active bone in children.

(4) As was expected, the average strontium 90 content of human bone does not vary from one locality to another more than the average concentration of mixed fission products (1). For identical periods of time there is a fair correlation between these 2 factors for the 17 localities that were sampled for human bone.

(5) By averaging all samples from persons above 10 years of age, a large enough set is available for comparison between localities. In North America, for example, it is readily seen that the concentration in Vancouver is essentially the same as that in Houston, whereas the concentration in Denver is definitely lower (despite its proximity to Nevada). The New York average is not strictly comparable; it is low because samples from individuals 40 to 60 years of age comprised half of the total. These are the only ones in the table that were obtained in the spring and fall of 1954. At that time the strontium 90 content of adult bone was distinctly lower than it was in late 1955. (Note that the concentration in New York City milk had increased at least by a factor of 2 toward the end of 1955 from the level in 1954 (4).

(6) There are large deviations from the mean in the strontium 90 content in individuals of a given locality. The average deviation for most 10-year-age sets is about 50 percent. Figure 3, a log normal plot of the North American samples, illustrates that some individuals may have at least 10 times the average concentration. This is most probably related to diet.

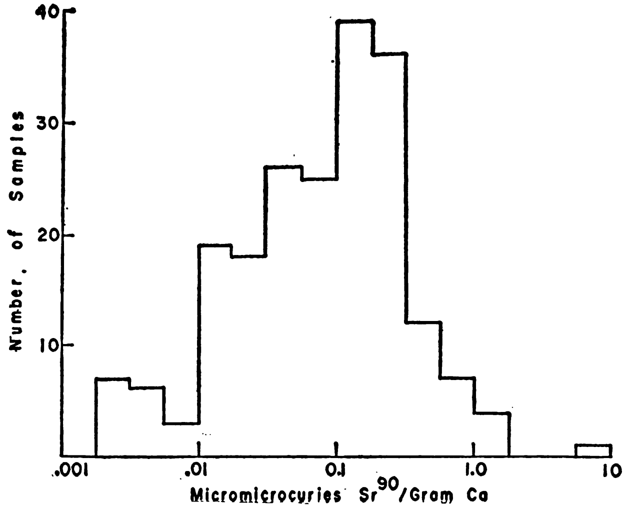

FIGURE 3. Distribution Pattern of Strontium 90 in Human Bone in North America

An attempt was made to find the time dependence of the strontium 90 contamination in human bone. The major difficulty is the limited time interval over which samples have been taken (mainly 1955). The New York samples of the spring and summer of 1954 in the older adults (40 to 60 years) were definitely lower (the average was less than 0.01 micromicrocurie of strontium 90 per gram of calcium) than in areas of similar total fallout in 1955, for example, Puerto Rico and Denver (averages were 0.14 and 0.08 micromicrocurie of strontium 90 per gram of calcium, respectively). Here an increment of 0.1 micromicrocurie of strontium 90 per gram of calcium, per year is evident. It now appears that stillborns may have a higher concentration than the average of the mother skeleton, at least during this early period of nonequilibrium. The Chicago data (4) suggests a 1953 stillborn average of about 0.12 micromicrocurie of strontium 90 per gram of calcium; this climbed to 0.17 by the spring of 1954. Six samples from Chile gave about 0.35 in mid-1955. The values for the comparable mother skeletons would have been 0.03 micromicrocurie per gram in Chicago in 1954 and 0.07 micromicrocurie per gram in Chile in 1955.

The samples from Germany were of sufficient size and number to make possible a significant comparision between the periods March to September 1955 and October 1955 to January 1956. The biggest difference was observed in the youngest age group, whose members would be the most sensitive to change in the strontium 90 concentration of the diet, because of their rapid growth. for the 0 to 9 age group, the averages were 0.21 and 0.34 micromicrocurie of strontium 90 per gram of calcium for March to September 1955 and for October 1955 to January 1956, respectively. A major increment in the known bone data should be observed in the present winter 1956–57 collection.

TABLE 2.—*Average strontium 90 content in man, 1955. (All values in micromicrocuries of strontium 90 per gram of calcium, normalized to the average whole skeleton. The numbers in parentheses give the total number of samples in the category.) The world average for all ages and locations is 0.12 micromicrocuries per gram of calcium; the average, including 1 sample of high value, is 0.14; and the world average for all samples assuming "equal to or less than" value are zero is 0.10*

Location and sample	Age at death								Average, 10-80
	0-4	5-9	10-19	20-29	30-39	40-49	50-59	60→	
EUROPE									
Germany (femur)	0.44(12)	0.25(6)	0.14(13)	0.065(33)	0.085(29)	0.14(2)			0.085(77)
Switzerland (vertebras)	.13(3)		.068(6)	.076(3)	.088(2)	.12(1)	0.036(1)		.076(13)
England (vertebras)	.19(3)	.060(4)	.048(2)	.026(9)					.032(11)
Denmark (vertebras)	.044(1)	.052(2)	.14(1)	.029(4)	.024(3)	.029(4)			.036(12)
Italy (rib)	.18(1)	.14(1)	.10(4)	.10(6)	.11(7)				.11(17)
Average	.33(20)	.15(13)	.11(26)	.06(55)	.084(41)	.08(7)	.04(1)		.078(130)
NORTH AMERICA									
Texas (rib)	0.49(13)		0.22(9)	0.14(17)	0.12(8)	0.13(1)	0.05(1)		0.15(36)
Denver (rib)	.12(5)	0.085(5)	.09(11)	.075(9)	.09(4)	.13(1)		0.07(7)	.085(32)
New York City (rib)	.44(2)	.16(3)	.17(2)	.10(2)	.075(8)	.005(4)	.01(4)		.06(20)
Boston (rib)	.40(2)								
Vancouver (tibia)	.46(2)		.28(5)	.10(4)	.066(11)	.06(5)	.06(3)	.092(7)	.10(35)
Puerto Rico (rib)	.06(1)		.065(1)	.21(2)	.13(3)	.15(2)	.04(1)	.16(6)	.14(15)
Average	.41(25)	.12(8)	.17(28)	.12(34)	.09(34)	.07(13)	.02(9)	.10(20)	.11(138)
SOUTH AMERICA									
Colombia (rib)				0.016(1)	0.04(5)	0.04(1)			0.035(7)
Chile (rib)	0.15(1)	0.06(5)	0.075(7)	.07(16)	.06(22)	.05(10)	0.10(4)		.065(59)
Brazil (rib)	.21(1)		.14(1)	.055(1)	.055(1)	.05(3)			.06(12)
Venezuela (rib)	.19(10)	.11(4)	.08(4)	.085(4)	.04(6)	.085(10)	.065(6)	.105(6)	.075(39)
Average	.19(12)	.085(9)	.08(15)	.07(27)	.055(34)	.065(24)	.08(10)	.105(6)	.065(117)
AFRICA									
Liberia (rib)				0.83(1)		0.093(1)			
ASIA									
Taiwan (rib)	0.34(5)	0.16(3)	0.25(1)	0.33(2)	0.08(7)	0.055(3)	0.08(3)		0.12(16)
WORLDWIDE									
Average	0.31(62)	0.14(33)	0.12(70)	0.09(118)	0.08(106)	0.07(47)	0.06(22)	0.09(26)	

The average American obtains about 80 percent of his calcium from dairy products and the remainder mainly from vegetation (20). In 1955 the strontium 90 concentration in milk in the United States was about 3.5 micromicrocuries per gram of calcium (4, 5). During the same period, field vegetation averaged about 20 micrimicrocuries per gram of calcium (5, 8) so that the average human population in the United States probably had a diet of about 7 micromi-

crocuries of strontium 90 per gram of calcium. Using the discrimination factor of 8 between diet and human bones, an equilibrium concentration of about 0.9 mircromicrocuries of strontium 90 per gram of calcium would be predicted for the average American. The actual concentration in 1955 was probably about 0.3 for young children and 0.1 for adults. These lower values reflect the time required for bones to equilibrate with the strontium 90 calcium ratio in the diet.

An estimate of the worldwide burden can be made by assuming that the average total strontium 90 fallout was 8 millicuries per square mile in 1955, and that the average amount of exchangeable calcium is 75 grams per cubic foot (4). Thus, if half of the fallout is in the upper 1 inch of soil, which contains 6 grams of calcium per square foot, an average concentration of about 25 micromicrocuries of strontium 90 per gram of calcium would be available to grains and grasses. Using a soil/plant fractionation of 1.4 and plant/milk fractionation of 7, the average worldwide concentration of strontium 90 in milk would be about 25 micromicrocuries per gram of calcium, and in vegetation about 18 micromicrocuries per gram of calcium. Assuming that 80 percent of the worldwide dietary calcium comes from milk products—as is true for the average American diet (20)—the predicted concentration of strontium 90 in the diet would be about 5 micromicrocuries per gram of calcium. This in turn would yield a predicted value of 0.6 micromicrocuries per gram of calcium for the average man when equilibrated with the fallout that existed at the end of 1955. The major uncertainties in the calculation are the source of calcium in the average world diet, the average calcium content of the soil, root depth, and possible direct foliar uptake.

In terms of hazard to man, there are two problems to be considered: (i) the average value for the world population and (ii) possible maximal concentrations. Locations near atomic test sites are not included in these considerations.

With regard to the strontium 90 burden of the population of the world in the fall of 1955, it can now be said that this is reasonably well known (0.12 micromicrocurie per gram of calcium) and that this burden is very small compared with the maximum permissible concentration (one-ten-thousandths).

The matter of predicting the maximum average human burden that will ultimately be occasioned from atomic explosions through the fall of 1956 involves several factors. The average burden will be determined by the average strontium 90/calcium ratio in the diet at equilibrium. Thus, if the fall 1955 concentrations in the diet were maintained, the average human being at all ages would reach a maximum of about 0.6 micromicrocurie of strontium 90 per gram of calcium. This would be the result of a worldwide fallout of about 8 millicuries per square mile by the end of 1955 (1). From the end of 1955 to the fall of 1956, another 2 millicuries per square mile would appear worldwide from stratospheric fallout. An additional 8 millicuries per square mile has fallen out since 1955 from high-yield weapons that have deposited all of their debris in the Northern Hemisphere (5). Libby (5) states: "In fact, we estimate at the present time that the total stratospheric reservoir, counting all sources, is about the same as it was 2 years ago—that is, 12 millicuries of strontium 90 per square mile, or the equivalent of 24 megatons of fission products calculated as a uniform worldwide distribution." Taking decay into account, a total quantity of 26 millicuries per square mile would be available in the United States for equilibration with the human skeleton by 1970. Thus, from explosions that have already occurred, the average human bone in the United States should contain about 2 micromicrocuries of strontium 90 per gram of calcium by 1970, whereas the worldwide average concentration should be about 1.3. This will have been the result of about 50 megatons of fission. On this basis, 35,000 megatons of fission would be required to bring the average concentration in the world's population up to the maximum permissible concentration.

The most important problem lies with individual variation. By direct experiment, it has been found that the distribution curve is quite sharp (fig. 3 and table 2). Although food grown in restricted areas of low available calcium content could have 10 to 100 times the mean, it is clear that the general mixing of food sources in the diet of an urban population would make it impossible for most people of the world to exceed the average concentration of strontium 90 by more than a factor of 10.

The theoretical estimation of the maximum concentration of strontium 90 that some individual or a small group of individuals might receive at long distances from atomic test sites is a complex problem involving a number of parameters that are at present subject to large uncertainties. The highest concentrations will be found in isolated individuals who obtain their total food supply from a restricted area that has very low available calcium in the soil (21).

SUMMARY

The worldwide average strontium 90 content of man was about 0.12 micromicrocurie per gram of calcium (one ten-thousandths of the maximum permissible concentration) in the fall of 1955. A few values as high as 10 times the average have been obtained.

The value is in accord with the predicted value based on fallout measurements and fractionation through the soil-plant-milk-human chain.

With the present burden of strontium 90, this average level should rise to 1 to 2 micromicrocuries of strontium 90 per gram of calcium by 1970.

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7. This research was initiated at the suggestion of W. F. Libby. The encouragement, support and criticism of W. D. Claus, F. Western, R. A. Dudley, and D. L. Worf, of the Division of Biology and Medicine and M. Eisenbud, J. Harley, and I. Whitney, of the New York Operations Office, are much appreciated. We also wish to acknowledge the scientific contributions of H. L. Volchok, of Isotopes, Inc., and W. S. Broecker and K. K. Turekian, of this laboratory. Laboratory assistance was provided at various times by J. E. Gaetjen, E. Hitchcock, E. Hodges, P. Kluft, A. Long, R. Lupton, R. Janes, E. Peets, and R. Slakter.
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Strontium 90 in Man

The strontium 90 data on the detention of fission fallout in man, as reported by J. L. Kulp, W. R. Eckleman, and A. R. Schulert (Science 125, 219 (1957)), provide the first sound basis for evaluating the biological hazards from Sr–90 on a quantitative scale. While the authors present excellent data, their conclusions are open to serious criticisms, a number of which are itemized in the following paragraphs.

(1) The biological hazard of Sr–90 is most important in human beings born since the Castle series of nuclear tests (beginning with the March 1, 1954, explosion). Thus the pertinent age group, for measurements made in 1955, is 0 to 1.5 years. It is somewhat misleading to present Sr–90 bone retention data as an average value for ages 10 to 80. This is especially significant since infants show a much higher uptake of strontium than do adults.

(2) Statistics on Sr–90 retention in infants are too limited to permit careful evaluation of the biological hazard. Futhermore, it is again misleading to speak of averages for strontium retention in infants. Our concern should be with the fraction of infants who get, say, 10 times the average value, for we certainly do not wish to consider the radiation risk on a total basis.

(3) The authors assume that their data reflect the establishment of an equilibrium condition in fallout and uptake of Sr–90. About a year elapsed between stratospheric injection of the fission debris and the time of bone measurement. The bomb-to-bone sequence is an intricate one involving the possibility of hold-up in the food chains. For example, dairy cattle fed on stored herbage would contribute less Sr–90 to dairy products than those on open range.

(4) In the case of stillbirths, Sr–90 data may be distorted by nonnutritional calcium prescribed for many pregnant women.

(5) The authors continue the Atomic Energy Commission practice of reporting Sr–90 concentrations in terms of maximum permissible concentration (MPC) which are strictly meant to apply only to healthy, occupationally exposed adults. The MPC for children, to be consistent with the recommendations of the National Commission of Radiological Protection ought to be one-twentieth the occupational MPC.

(6) Values for MPC have been revised downward steadily during the past two decades as more knowledge of the ultimate biological effect of skeletally retained radioelements has accumulated. In view of the greater radiosensitivity of infants to nuclear radiation, the global exposure involved, and the lifetime irradiation periods, it may well be that the appropriate MPC for evaluating the global Sr–90 hazard should be one one-hundredth the occupational value. The MPC for Sr–90 is based on comparison to the radium MPC, which, in turn, hinges on our experience with radium poisoning in human beings. Practically no data are available for radium retention in children and, in addition, very few radium-retention studies on human beings have been carried out over a period of 40 or 50 years.

(7) In projecting their estimates of Sr–90 retention through 1970, the authors make no allowance for additional nuclear tests. In view of the fact that the British will test weapons in the megaton range within a few months and the Soviets may overcome their continental proving ground limitation so that a Castle series of tests may be undertaken shortly, it seems naive to assume a vacuum in testing from 1956 to 1970.

In addition to these seven points, one should consider the role of concentration factors in fallout, the selectivity of global fallout, the possibility of different fallout patterns for bomb debris injected into the stratosphere at points other than the United States and U. S. S. R. test sites as well as the influence of different substrata on fallout phenomenology. Nor should one neglect the possibility of ecological upset owing to concentration of radioelements in nature.

Any meaningful evaluation of the Sr–90 hazard must seek to assess the risk of excessive radiation exposure to the most radio-sensitive groups of the total population. Because of the global nature of the fallout, the problem of risk-evaluation should be undertaken on an international basis. No governmental group within the United States should undertake to assume or calculate risks for peoples of foreign lands. The United Nations has established a committee to in-

vestigate the biological effects of nuclear fallout, and it is to be hoped that technical reports will be forthcoming soon. Then attention may be focused on weighing the probable risks of future bomb tests, and it may be possible to fix a limit to the annual testing of nuclear weapons to keep stratospheric pollution within safe limits.

It is salutary that the Atomic Energy Commission has sponsored such high-quality scientific research and even more hopeful that phases of this work are emerging from the classified category. Independent analyses of the problem, such as those appearing in the Bulletin of the Atomic Scientists, now rest on a more solid foundation of fact that was heretofore possible.

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Although we share Ralph Lapp's concern for the seriousness of the Sr–90 problem for the world population, we wish to dissent from some of his interpretations of our data.

(1) We quite agree that momentarily the biological hazard is greatest for young children. We do not see how a discussion of the average concentration of Sr–90 is indults is misleading when we give all the individual data, as well as the averages of 10-year-age intervals, and definitely conclude that "children have 3 to 4 times more strontium 90 per gram of calcium * * * than adults."

(2) We did not conclude that the present data permit "careful evaluation of the biological hazard" in children. In fact, we made it clear that many more data are urgently needed. The important statistical quantities, of course, are the mean and the standard deviation.

(3) We clearly pointed out that the present situation does not represent equilibrium but that reasonable predictions can be made of what the equilibrium situation may be. By examining the steps in the bomb-to-bone chain, we were able to conclude that the quantity of Sr–90 in human bone is approximately that predicted from our knowledge of the total fallout and the fractionation factors in the chain. Actually, the time scale of importance is on the order of a year, and in this period the milk appears to be fairly well equilibrated with the soil. When milk is the major source of calcium in the diet of young children, the children will likewise approach a transient equilibrium.

(4) The data on stillborns did not involve the average predicted for ultimate equilibrium.

(5) We most emphatically did not present our data in maximum permissible concentrations for several reasons, not the least of which is the current debate among competent medical scientists on what this value should be. We reported all our data in absolute units of micromicrocuries of Sr–90 per gram of calcium. We discussed the data relative to the one official Sr–90 level existing at the time we wrote the article—that is, the maximum permissible concentration for industrial workers stated in the National Bureau of Standards Handbook No. 52.

(6) The setting of the maximum permissible concentration is not in our sphere of scientific competence. This was not one of our conclusions.

(7) We could have calculated the average concentration of Sr–90 in man in 1970 either by using the known number of atomic tests to date or by assuming some unknown arbitrary number. We chose the former and clearly stated our assumption. The point here was to show what will ultimately get into man from a known quantity of debris produced. It was not our intention to calculate how much might be present in man by 1970 assuming some grave political situation.

It is hoped that current experimental work in this laboratory and elsewhere will make it possible to provide information on some of the other problems which Lapp and others have raised. Although there will remain much area of debate, new data to be published shortly will place some further limits on the area of speculation. In reporting the laboratory data on this controversial and globally important subject, we have tried, and will continue to try, to present it as objectively as possible, so that the scientist-citizen such as Lapp may discuss the sociopolitical problem in as well informed a manner as possible.

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STRONTIUM CONTENT OF HUMAN BONES

The refinements of analytic techniques for trace elements, together with a growing interest in the distribution of trace elements in human tissues, has resulted in a continued addition of information on these elements to the biochemical literature. The present paper (1) reports strontium analyses on 277 human bones from a worldwide sampling.

Hodges et al. (2), investigating the strontium content of human bones by an emission-spectrographic technique, found for a limited sampling an average of 220 parts per million for bone ash. Tipton (3) has found a lower value for bones (120 parts per million), also using an emission-spectrographic technique.

The present paper reports results indicating that there are marked regional differences. It is possible that the aforementioned discrepancy may be explained on this basis. It is also possible that regional effects may be confounded with systematic errors in the analytic data. The investigation reported here is an attempt to resolve these uncertainties and to explore the significance of the data as related to biochemistry and geochemistry.

The bone samples were analyzed (4) as ashes that had been prepared at 800° C. for 12 to 24 hours. The individual bone samples were received from the contributors in various forms of preparation or in different stages of desiccation; hence, there can be no significance attached to the loss on ashing. It was felt that the most significant number that could be determined was the ratio of strontium to calcium, inasmuch as this would avoid any of the ambiguities of varying histories of preparation or variations owning to density of bone mineral.

For values above 0.35 (percent Sr/percent Ca times 10^3) the coefficient of variation is about 9. The specimens were run in duplicate, giving a standard error of ± 6.5 percent. On the 95-percent confidence level, this means an error of ± 13 percent for values of 0.35 (percent Sr/percent Ca times 10^3) and higher. Values below 0.35 (percent Sr/percent Ca times 10^3) have a higher error.

The study reported here (5) indicates agreement with Hodges, et al, (2), in that no effect of bone type on the ratio of strontium to calcium was observed. In addition, there is no correlation with age or sex. The main source of variation appears to be related to regional influences, because, as table 1 indicates, there are marked differences in the means of several geographic areas. From table 1 it can also be seen that the standard deviation varies for different regional samples. Where large variances occur, they may either be attributed to a more mobile population or to regions with diverse geologic, and consequently geochemical, environments. In some areas the ethnic diet may play an important role.

TABLE 1.—*Summary of data on regional variations in the strontium of human bones*

Region	Number of specimens	Average (percent Sr/percent Ca) $\times 10^3$	Standard deviation	Range
Colorado	28	0.61	0.27	0.34-1.35
Texas	12	.54	.16	.25- .80
England	3	.67	.55	.30-1.50
Switzerland	7	.35	.15	.11- .55
Cologne	21	.36	.11	.21- .68
Bonn	15	.35	.12	.17- .52
Denmark	2	.89	.23	.73-1.05
Italy	9	.71	.14	.49- .88
Venezuela	47	.60	.24	.31-1.15
Chile	47	.62	.16	.20- .94
Brazil	6	1.33	.33	1.09-1.98
Puerto Rico	5	.62	.30	.37-1.05
Vancouver	21	.50	.15	.33- .75
China	19	.67	.16	.42-1.00
Japan	5	.70	.14	.58- .93
India	29	.69	.28	.34-1.50
Liberia	1	1.25		

It has been established by Turekian and Kulp (6) that there are marked regional differences in the strontium content of almost all types of rocks composing the geologic realm. Waters draining from these rocks and plants growing in these areas would taken on the strontium complexion of the locale. This has been demonstrated for a large number of elements and is a useful tool of geochemical prospecting. These regional differences are the most likely reasons for the variations in the strontium content of human bones from one area to the next.

It may be possible to check this conclusion by examining human bone tissue from a known high Sr/Ca area. The Waukesha (Wisconsin) water supply (7) has been found to have 30 to 50 ppm strontium and 50 to 90 ppm calcium. Hence, the region should have a very high Sr/Ca ratio. It would be interesting to check bones from such a locale.

A histogram of the strontium content of the 277 human bone specimens analyzed (fig. 1) approximates a normal distribution. If the 277 observations can be considered a large enough sampling of the world, then it is possible to arrive at an average ratio of strontium to calcium in human bones. This average is 0.60 (percent Sr/percent $Ca \times 10^3$). If it is converted to parts per million strontium on the assumption of pure calcium phosphate ash, this value is equivalent to 234 ppm. This compares closely with the value of Hodges et al. (2), but, considering the marked regional variations, it may only be fortuitous.

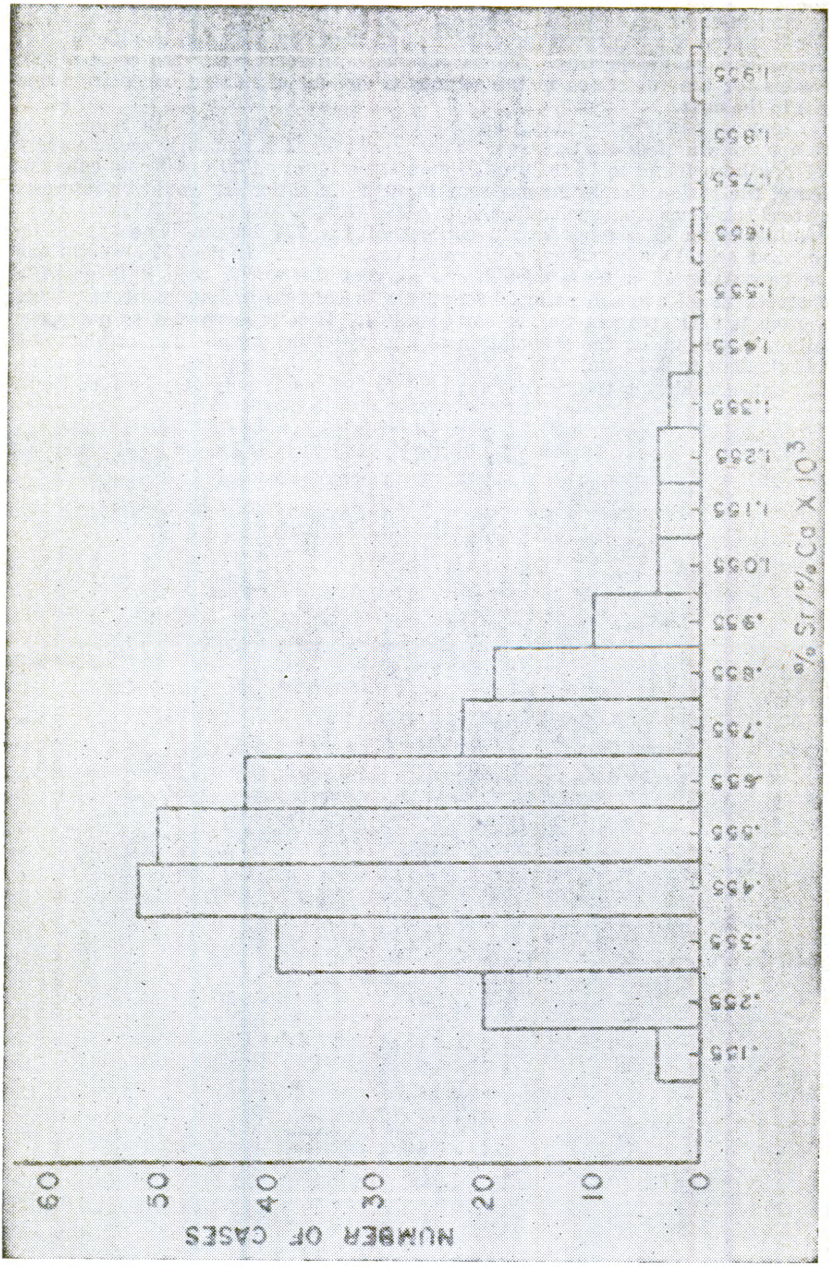

FIGURE 1. Histogram of Sr/Ca ratios for 277 human bone specimens.

Turekian and Kulp (6) have demonstrated that the average (percent Sr/percent Ca$\times 10^3$) ratio in igneous rocks ranges from 6.5 to 23 and in sedimentary rocks from 1.5 to 6; hence, it is evident that the human body markedly discriminates against strontium relative to calcium when compared to the natural environment. This has been documented by Alexander et al. (8) in the case of several small laboratory animal specimens. The mineral apatite occurring in nature, which compares in part to some portions of the skeleton, is reported by Noll (9) to have from 0.1 to 4.5 (percent Sr/percent Ca$\times 10^3$). Hence, the vital effect of the organism is seen operative in the differentiation of strontium and calcium in human bones. Schulert (10) of the Lamont Observatory has demonstrated the existence of such differentiations using Sr–85 and Ca–45.

It is conceivable, considering the foregoing information, that human bones that have started in the direction of fossilization should possibly show a time-dependent increase in the strontium content upon burial. Hence, it is possible that this may be used in a manner similar to the fluorine method for relative chronologies. This aspect of the study awaits further exploration.

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J. LAURENCE KULP,
Lamont Geological Observatory,
Columbia University, Palisades, N. Y.

REFERENCES AND NOTES

1. Lamont Geological Observatory Contribution No. 205. This research was supported by the biophysics branch, Biology and Medicine Division, U. S. Atomic Energy Commission. We wish to acknowledge the assistance of C. Steffens.
2. R. M. Hodges et al., J. Biol. Chem. 185, 519 (1950).
3. Personal communication.
4. Strontium-free calcium phosphate was prepared by ion-exchange separation. Standards for the spectrographic analyses were then made by mixing this strontium-free calcium phosphate and calcium phosphate with a known strontium content. All measurements were made using the Jarrell-Ash Ebert grating spectrograph and the Jarrell-Ash 2100 microphotometer. The critical constants are as follows: approximately 5 mg of bone ash was weighed into necked deep crated 3/16-in. electrodes (National Carbon Co. No. 4000) and arced at 16.5 amp to completion. A glass filter, to absorb second-order interference lines, was placed before the 30μ slit. A rotating sector allowed the transmission of 35 percent of the light. Kodak SA No. 1 film was used and D–19 developer at 3 min and 20° C. The Sr 4607 line was used as the analysis line and Ca 4579 was the monitor line.
5. Additional data on individual samples may be obtained from us.
6. K. K. Turekian and J. L. Kulp, Goechim. et Cosmochim. Acta, in press.
7. U. S. Geol. Survey Water Supply Paper No. 1299 (1954), p. 624.
8. G. V. Alexander, R. E. Nusbaum, N. S. MacDonald, J. Biol. Chem. 218, 911 (1956).
9. W. Noll, Chem. Erde 8, 507 (1934).
10. Personal communication.

Representative HOLIFIELD. The committee will stand in recess until 2 o'clock.

(Whereupon, at 12:20 p. m., the committee recessed, to reconvene at 2 p. m., of the same day.)

AFTERNOON SESSION

Senator ANDERSON. The committee will be in order. The first witness this afternoon will be Dr. William F. Neuman, of the University of Rochester School of Medicine and Dentistry. I am happy you are here today, Dr. Neuman.

STATEMENT OF DR. WILLIAM F. NEUMAN, SCHOOL OF MEDICINE AND DENTISTRY, UNIVERSITY OF ROCHESTER [1]

Dr. NEUMAN. It was my impression that I was to discuss discrimination factors in the human food chain. Attesting to the importance of these discrimination factors, many previous witnesses have found it necessary to discuss them and to use them in their calculations. Because of this previous discussion, and because the hearings are falling behind schedule, I shall be very brief.

I would like to show that the discrimination factors are very important and permit us to relate human bone levels of Sr–90 to the rate of weapons testing.

Let me, at the outset, underscore the remarks of Dr. Kulp to the effect that there is agreement in the scientific community. We are all agreed that present levels of Sr–90 in human bone are quite low and that the associated hazards are quite negligible. It is predicting the future that we, as scientists, part company.

Let me also stress that the survey data given by Drs. Kulp and Eisenbud, though extremely important and valuable, are of limited use in making predictions. These data are inextricably bound to past conditions: testing rates, types of weapons exploded, and so forth. Furthermore, to extrapolate from these data, one must make some guess, some assumption, regarding the fraction of the final equilibrium that human bone has currently achieved. This assumption is quite uncertain.

For purpose of predicting, I submit that the most reliable data from which to extrapolate are those related to the natural strontium in our biosphere. After all, we have been at equilibrium with natural strontium for many generations. Dr. Kulp and Eisenbud have had occasion to refer to these data themselves.

The beauty of using natural strontium as our model is this: Only two assumptions are necessary, first, that strontium 90 behaves like natural strontium (which is not an assumption at all) and, second, that strontium 90 formed in nuclear explosions will show complete mixing with natural strontium and follow the same distribution pattern. One might question this assumption. It is somewhat optimistic, tending to minimize the final evaluation of potential hazards.

The subject of discrimination factors is based on a classic principle enunciated by the famed French physiologist, Claude Bernard—the internal milieu. The basic idea was this: Life originated in the sea. As new evolutionary forms appeared, they were at a disadvantage if they found the composition of the sea not to their liking. They died out. By the time there evolved larger forms, which could leave the sea, these animals could not divorce themselves from their chemical heritage. Their body cells required a fluid environment very much like that of the mother fluid, that ancient sea.

[1] Date and place of birth: 1919 in Petoskey, Mich. Education: Michigan State College, 1936; University of Rochester, Ph. D., 1944. Work history: Member of the staff of Rochester University since 1944; associate professor of pharmacology in 1950, associate professor of biochemistry in 1955. Associated with atomic-energy project at university (chief of biochemistry section). Eli Lilly award in biological chemistry in 1955. Radiological monitor at Bikini bomb tests; representative of AEC to visit European installations in 1952; scientific adviser to State Department at Geneva Conference on Atoms for Peace in 1955. Fields of interest: Bone chemistry and bone metabolism, matabolism of fission products, radiation effects. (Submitted by Witness.)

Within us, today, special cells, special organ systems are constantly at work maintaining the body fluids at this ideal composition, despite wide variations in intake resulting from whim or fortune.

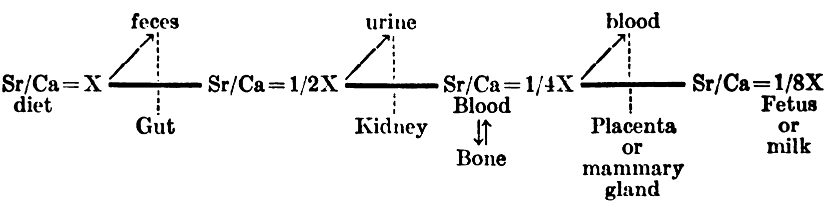
These special organs select and conserve those substances which are needed, eliminate those which are not needed or unwanted. However, these specific selective and regulatory processes are based on natural law, not on magic. They are not perfect. Just as dispensing machines can be made to operate with a slug having a shape, weight and properties similar to a genuine coin, so also can the body be "fooled" when presented with a chemical counterfeit.

Natural strontium is the chemical counterfeit of calcium, an alkaline earth which is required for the maintenance of health and well-being, as we have learned from television, a requirement which we never outgrow. Actually, natural strontium is an excellent counterfeit. It is so good, it can be considered legal tender in many body processes. Consequently, it is relatively nontoxic and harmless. Because of its widespread occurrence in soils and rocks, strontium is found in our drinking water, in our food, and, of course, in our bodies. Like calcium, about 99 percent of the strontium is found in bone.

No counterfeit is perfect, however, and there are slight differences in properties between strontium and calcium which permit our regulating systems to select calcium preferentially, or to discriminate against the less desirable strontium.

Where are the sites at which these discriminations occur? They occur in those organs which are charged with the responsibility of maintaining normal calcium levels in our internal milieu: the portal of entry, the gut; the exit, the kidney; and in two organs specially concerned with assisting the developing infant to maintain its internal milieu, the placenta and the mammary gland. As a crude but very useful approximation we can say that at each of these organs, calcium is preferred 2 to 1. Stated another way, the ratio Sr/Ca presented to the organ is reduced by half in passing the organ system.

Thus:



Given a certain strontium-calcium ratio in the diet—we will call it X—the strontium ratio on reaching the blood will become one-half X, in passing through the gut, with some strontium preferentially appearing in the feces. Upon reaching the blood, it passes the kidney and again we have a preferential reabsorption of calcium to the exclusion of some of the strontium. In this case, the strontium appears in the urine preferentially, which leaves a factor of approximately one-fourth X. The circulating fluids reach the bone areas. In the bone, we know of no real discrimination. Therefore, we can say, as a rough approximation, bone levels will be in strontium content about

one-quarter that of the diet. In the developing embryo, the placental barrier will reduce this further so that the lightly mineralized bone of the infant will be only about one-eighth of the Sr/Ca ratio of the mother's diet. Similarly, milk will exhibit a ratio only about one-eighth that of the diet. This is a crude illustration of how these processes interact within the animal.

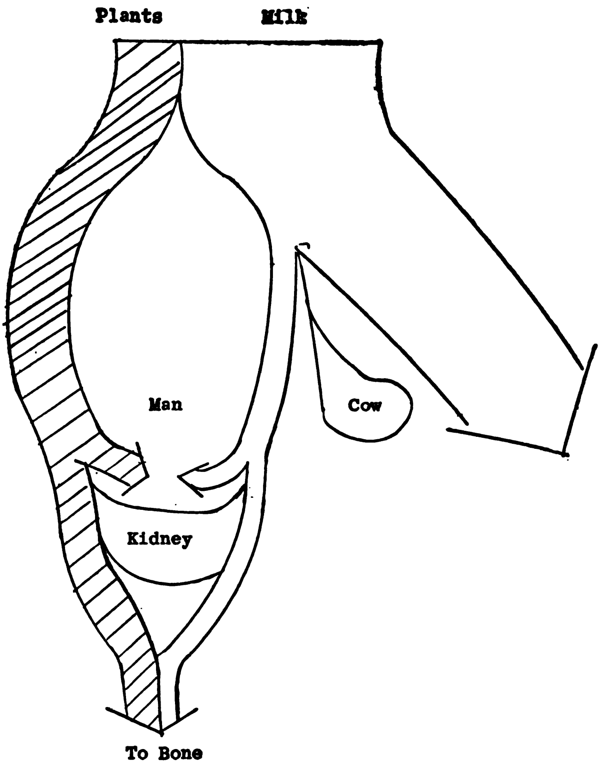
I should point out however, that the discrimination which occurs in the gut is subject to some influence. If the diet is primarily milk, as it is in this country, the discrimination by the gut seems to be much less. It is nearly wiped out. Therefore, we find that, without this favorable discrimination, the person on a milk diet will put down a bone which exhibits a strontium to calcium ration about one-half that of the diet.

Now I would like to go over again something that was discussed by Dr. Kulp this morning. In traveling from plants to human bone we have, really, two alternative paths. One, in which the calcium from the plants goes through the cow and is subjected to the favorable discrimination factors of the cow; one, in which calcium of the plants reaches the diet directly without passing through favorable discrimination factors. Doctor Kulp stressed the fact that most of the calcium comes through the milk chain. This is true, but if we use the discrimination factors best known today, we find that, in terms of strontium, most of the strontium may come through the plant side of the food chain. The reason for this is the favorable discrimination against strontium in the milk chain.

I will try to illustrate this with a drawing but because of my limited art talent, this may be less clear than what I have already said.

We can take a band as coming from plants as being about one-fourth the total strontium-calcium supply and a band three times as wide from the milk side. If we pass through the cow seven-eighths of the

Overall Chain



The Origin of Bone Strontium.

strontium will be discriminated against, and only one-eighth of the strontium then, will reach the human.

Although we start with only one-fourth of the strontium from the plant side, all of this reaches the human with no discrimination along the way. In terms of dietary supply of strontium, more is coming from the plant side than from the milk chain. If we then put in the favorable discrimination of about one-half to one-fourth for the human, we find that the overall ratio in bone is reduced to about one-sixth to one-eighth of the plant levels on the basis of our crude example.

I say the value one-sixth. That is very rough. It is based on the crude approximations made in our original model. What is the proper number to use? How reliable is it? And finally, what does it mean? I think at the present time the best figure to use is one-eighth. This number is a number proposed by Dr. John Lautit, representing the British point of view. It is the mean value of a discussion at which most of the experts in this country were present last April 23. At the present time, this value seems to me to be quite well established—certainly within a factor of 2. The overall discrimination I cannot believe would be less than one-fourth or greater than one-sixteenth.

This value is really quite good. It is based on four general types of experiments. The administration of labeled strontium to animals and to man to determine the metabolism pattern and distribution in the animal and so on. The second kind of experiment involved the simultaneous administration of radioactive strontium and radioactive calcium to animals and man for a direct comparison of these two elements. It has come also from the chronic lifetime feeding of animals of diets containing fixed ratios of radioactive strontium to normal calcium.

Finally, and most importantly, it comes also from the direct determination of natural strontium-calcium ratios in the bones of man and animals and their diets. I consider this area to be much more reliable than many other of the elements of indecision with which we will be faced next week.

Mr. RAMEY. Has this value been as refined all the way through or did you start out with a higher one and gradually work to this as a result of better measurements?

Dr. NEUMAN. If you are asking me personally, may I say that I have been very lucky. I have not had to change my numbers. My original estimates of what this number might be were rather tenuous because much of the data were not yet in. I find myself really very lucky in that the mean of my original estimates and that final agreed upon differ by only about 16 percent. Others were not quite so fortunate. We find that many made estimates originally that were quite favorable. I think now we are agreeing more and more that the factor 8 is probably the best figure to use. We may have to revise this. But I do not believe it will be revised more than by a factor of 2.

This value represents discrimination in the chain from plants to bone only. What about the possibility of discrimination in going from ground to plant? I am frankly quite uncertain and I am willing to stand corrected. From the data I have been able to find and study there seems to be no adequate basis on which to decide at present, and I recommend heartily that no effective discrimination be assumed but with the clear and important qualification that this assumption must be continuously scrutinized and changed as soon as new data permit.

I shall take, then the overall discrimination process from ground to human bone as totaling one-eighth.

Mr. RAMEY. Does that differ from Dr. Kulp's testimony this morning?

Dr. NEUMAN. Dr. Kulp used the same factor for milk that I did, but I believe he used an overall factor of 14 and used the milk side only. I am not sure.

Is Dr. Kulp here?

(No response.)

Dr. NEUMAN. I believe he used the factor of 14, which is going down the milk side, ignoring that which comes from the plants directly. That is a little optimistic in my opinion.

What is the importance of this number in our present considerations? It seems to me that this figure permits us to calculate very simply the rate at which fission products can be released by the various testing programs. Only two bits of information and one decision

are needed. We need to know the variation of the strontium to calcium ratio in human bone. We need to know the variation in the pattern of strontium 90 fallout and evaluate it and we thus, third, make a decision regarding the maximum permissible allowable, some kind of concentration to be permitted in human bone.

This morning Mr. Anderson asked a number of questions which would relate predictions of bone level to test rate. All of the calculations that were prepared in advance were on the basis that no more strontium 90 was being produced.

I would like to go quite the other way around and calculate the permissible production of fission products based on a given human bone level.

The assumption I would like to make is: When we have loaded our biosphere with the permissible amount of strontium 90, the amount of testing that can be done, the amount of strontium 90 that can be produced, will be equal to that which decays in any given year. This will maintain us at the steady, loaded level. This would amount to 2½ percent per year.

Therefore, if we calculate the maximal tolerable biosphere load we automatically have calculated the permissible production of strontium 90 in any given year.

This is such a simple equation that I am frankly embarrassed. These are supposed to be very difficult matters, but I find in the final analysis, that the calculation does not involve calculus but rather only arithmetic.

Thus:

2.5 percent tolerable ground levels = Annual test rate

$$\begin{matrix}\text{Toler.}\\ \text{gnd.}\\ \text{levels}\end{matrix} = \text{MPC}\cdot\begin{matrix}\text{Correction}\\ \text{for}\\ \text{variation}\\ \text{in}\\ \text{population}\end{matrix}\cdot\frac{1}{\begin{matrix}\text{Discrimination}\\ \text{factor}\end{matrix}}\cdot\begin{matrix}\text{Correction}\\ \text{stratospheric}\\ \text{holdup}\end{matrix}\cdot\begin{matrix}\text{Correction}\\ \text{for}\\ \text{nonuniform}\\ \text{fallout}\end{matrix}$$

where

$$\text{TGL} = \text{MPC}\times\frac{1}{V_1}\times 8\times 1.2\times\frac{1}{V_2}$$

We must, to begin, decide on what will be the maximum permissible concentration to be tolerated in the human bones of a large population. without reaching that decision, because this obviously will be discussed next week, I would like to continue with the calculation. We must correct this number by a variation. It is obvious that we can't have the general population averaging the maximum level. If we did this, half the population would be over the maximum. If we are deciding on a maximum level, then it is a maximum level and people should not exceed it. So the maximum permissible concentration must be revised downward by a number which evaluates the variability between individuals. I will discuss this in just a moment.

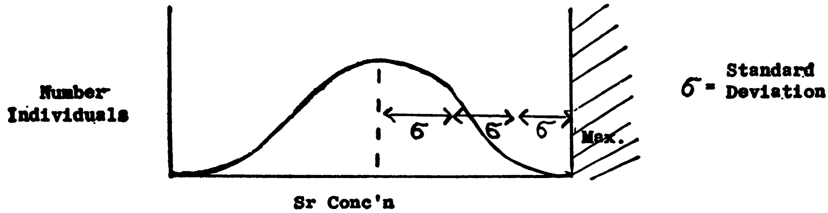
Now we find that we can tolerate more strontium 90 in our biosphere because of the favorable discrimination against it in food chain. We can, therefore, multiply by our overall average discrimination factor of 8.

We must consider the matter of stratospheric holdup. Obviously, if some of the strontium, in reaching the food chain, decays in its path through the stratosphere, this is all to the good and we can therefore

correct our value upward, depending on the amount of decay that occurs between the site of detonation and its entry into the food chain. I believe a factor of 1.2 is a reasonable number. This is because 20 percent of the strontium decays before it gets to the ground. This is perhaps more than necessary but it is such a small correction that it seems hardly worth discussing.

Then, again, we must correct for variations in fallout. It is my understanding that rather large segments of the population are subjected to rather large variations—a factor of 3 perhaps—in the variation of fallout.

I would like to evaluate V–1 and V–2. I think we can evaluate V–1, which is a variation among individuals. I think we can evaluate it quite well. Dr. Libby quite recently has described and summarized much of the available data in his speech of April 26. He concluded that the variation could be described statistically as having a standard deviation from the mean of about one-third.



What does this mean? It means that, if the population shows a normal distribution pattern, some individuals have only a little and some have a lot, but most of the people have about the average. We can describe this population in terms of the variation seen and this statistical description is the standard deviation.

If the variation in strontium 90 found in human bone does show a standard deviation which is about a third of the total average, then a value twice that average will be three standard deviations from the mean. Only a very few individuals will exceed a value twice the mean. We can say that, if the strontium data showed a normal distribution with a standard deviation of a third, and if we put a factor of 2 here for V_1, we will have only a fraction of the percent of the people exceeding the maximum permissible level. Unfortunately, I believe that the distribution is not normal. The data shown by Dr. Kulp this morning showed a skewed distribution. The natural strontium data shows a skewed distribution. It is difficult to evaluate this at the present time, but, to me, this means that the factor V_1 should be larger than 2 and probably less than 3.

I am not competent to attempt to evaluate more than crudely the variations in fallout patterns. The fallout seems to be, according to the concensus of testimony thus far, varying by at least a factor of 3. Mixing of food supplies, it is true, will dilute this effect. At the moment, for want of a better number, I will put in 3 with a stipulation that someone more competent who wishes to change that number may do so.

I think the importance of discrimination factors are now quite clear. In the equation calculating the total permissible burden for our bio-

sphere, the only number of any consequence, which permits us to live in a biosphere more contaminated than we ourselves hope to be, is the discrimination factor of 8. If we are permitted, as more data come in, to raise this discrimination factor, we automatically are permitted to raise the permissible testing rate. If as more data come in we must lower this number, then we must also lower our permissible testing rate.

I think only for purposes of illustration we might substitute these numbers and get an overall figure, since such predictions were made this morning.

Among the various MPC's that have been proposed, the number 100 was mentioned quite frequently. Actually, the National Academy of Science's report gives 2 numbers—100 and 50. The British have said that when children's bones reach a level of 10 we should worry.

Mr. Ramey. Are those sunshine units?

Dr. Neuman. These are all sunshine units or micromicrocuries of strontium 90 per gram of calcium. Therefore, we at the moment, until the discussion next week clarifies this issue, are permitted, I suppose, to choose one of these numbers.

I like 50 because this, in my opinion, would about double natural background. However, it doesn't matter. You can put the 100 in and get a number twice as big at the end.

Putting in the 50 sunshine units, just for illustration, times one-half—I will use one-half here and one-third here and 1.2 and 8. I believe that comes out to be 1.6 times 50, or 80 sunshine units on the ground as the tolerable maximal ground level.

Then the testing rate becomes 2½ percent of this—the decay rate—and that would be 2 sunshine units of annual production.

From Dr. Libby's conversion table, 1 megaton of fission equivalent is equal to 0.9 sunshine units, or we have a test rate then of approximately 2.2 megatons fission equivalent per year.

Senator Anderson. Doctor, I think I have an idea where you get the 2½ percent. Is that based on a 28-year half life?

Dr. Neuman. That is correct. If we have our maximum level on the ground and attain that level, then we can replace it at the rate at which it decays, which is 2½ percent a year.

Senator Anderson. I do not know how to express the 2.2 megaton fission. You could have a lot more power shot off—bigger bangs than that—if you had part of the process fusion.

Dr. Neuman. Yes.

Senator Anderson. When you say 2.2 megatons of fission, if you had a type of weapon that was almost entirely a fission weapon and each one of them had 20 kilotons, would you be able to shoot off 200 of them, or rather 100 of them or 110 of them, without raising the permissible level?

Dr. Neuman. The answer is implicit in this assumption of a uniform distribution throughout the world. If you shot off, as you say, 20 of the 100-kiloton devices, you would have a very nonuniform distribution, and you would have, I should think, a part of the world that would be well over its maximum tolerable level and the rest of the world well under it.

Senator Anderson. That is what I wanted to get from you.

Dr. Neuman. If you switch from one kind of detonation to another, you also change the V_2, the variation of the fallout. By the

proposed explosions that you suggest, V_2 would have to be considered much larger than the value 3 I have used.

Senator ANDERSON. In the case where we are using the Nevada proving range, the fissionable material or these products are not going to be spread equally around the world. The North American Continent will get a larger share of them, and following the wind pattern, I suppose, the area running in the northern half of the United States would get somewhat more than its share.

Dr. NEUMAN. That is correct.

Senator ANDERSON. Therefore, we would not shoot 20 100-kiloton bombs or 100 20-kiloton bombs because that would give a much greater result in the United States since you are figuring on worldwide distribution.

Dr. NEUMAN. That is correct.

Senator ANDERSON. Have you ever tried to calculate what might be a fair amount of discharge into the atmosphere taking into consideration the fact that the British and the Russians and ourselves are all doing it?

Dr. NEUMAN. I have avoided this because I don't know what the Russians have shot off. I don't even know right now what the British shot off.

Senator ANDERSON. I am not trying to involve you, so let me put it this way. I think you know that the Government of the United States through its Atomic Energy Commission has frequently announced that the Russians have exploded a certain type of device, and along with that there has generally been an announcement that sometimes is confined to the people entitled to know the capacity of that particular shot. So that the Atomic Energy Commission at least knows and probably a fair assumption is that the Joint Committee knows and the military authorities know how many kilotons each device they shoot is, plus or minus a small factor of error, and knowing that and what the British are doing and what we are doing, it would be possible to calculate pretty easily what seems to be safe.

Dr. NEUMAN. I think it is fair to say that, if we hope to choose a maximum permissible concentration of 50 micromicrocuries per gram, we have already achieved a biosphere that is fairly close to giving such a level at equilibrium.

Senator ANDERSON. We have already achieved it.

Dr. NEUMAN. I should so expect. You will recall my figures are not really very different from those of Kulp, who gave as an average number 24 micromicrocuries per gram. That is an average figure. I have already reduced the maximum permissible concentration by virtue of variation to an average level of about 25 micromicrocuries. On this basis he would have loaded bone.

Senator ANDERSON. He was going to do that at the end of 60 years. However, he was assuming a static level of testing.

Dr. NEUMAN. That is right.

Senator ANDERSON. Whereas we are in a rapidly ascending curve.

Dr. NEUMAN. He was also using a more favorable discrimination factor than I am using.

Senator ANDERSON. Yes.

Dr. NEUMAN. This is where we really get down to cases, I believe.

Mr. RAMEY. How would your figure work out if you used 100?

Dr. NEUMAN. It would exactly double the permissible rate to about 4.4 megatons per year. In terms of big bombs such as the Castle test, this figure would be one Castle test every 7 or 8 years and no other testing. If you use the maximum permissible concentration of 100 sunshine units, it would be 1 Castle test every 3½ years with no other testing, or 1 Castle test every 5 years with some additional small-weapon testing. I am uncertain about these things, but this prediction is reasonable and I am using the most reliable figures at my disposal. I think it is quite clear that we are not yet in serious trouble, but I think it is also clear that the discussions which you have scheduled could not have come at a more appropriate time—now, rather than later.

Mr. RAMEY. We were going to ask Dr. Eisenbud if he would have any comments to make on this.

Mr. EISENBUD. I believe, Mr. Chairman, that Dr. Neuman has to be complimented for introducing a new and stimulating approach which I think we will all be thinking about for the next several days. The comments, of course, must be centered on the validity of this discrimination factor of 8.

In my own approach this morning, I preferred to avoid this by going directly to milk, since we have reason to believe that the milk comes into fairly rapid equilibrium with the soil. One can thereby avoid the uncertainties involved in attempting to apply a discrimination factor in going from soil to milk and take the measurements directly. I will admit that the weakness of my approach is that I have to assume that the milk does come into rapid equilibrium with soil. This assumption I make on the basis of the evidence as it stands but it is open to some discussion.

Beyond this, I think I might welcome the opportunity later on to comment again after I have had an opportunity to give this more thought.

Senator ANDERSON. Thank you. I understand that Dr. Kulp may be on his way back and we might have some comments from him. These are at least some figures we can take a look at.

Dr. NEUMAN. May I make one more comment?

Senator ANDERSON. Yes.

Dr. NEUMAN. I must emphasize two points: (*a*) First, the testing rate, as calculated, is that which can be maintained indefinitely. Obviously, until maximum tolerable ground levels have been reached, the test rate can exceed this equilibrium value.

(*b*) Second, I am not too concerned whether the equilibrium test rate is 2, 4, or even 6 megatons fission equivalent per year. The principle I wish to establish is that a given maximum permissible concentration automatically fixes the maximum test rate and of the maximum permissible concentrations currently under discussion, none permit a test rate that is very high. The number of megatons that can be exploded is so small, some kind of international control of the production and release of fission products is inevitable.

I also want to say that even after some serious discussion, I do not think we will be off by more than a factor of 2 anyway. As I understand it, the discrimination figure of 14 was used by Kulp in his estimate. This is less than a factor of 2 from what I am using. So no matter how you slice this particular batch of data you come out with a testing rate of the order of a few megatons a year. It might be 7,

or 6, and it might be less than 2. It is not a great big number, and I think we all agree on that.

I am certainly willing to revise this as time goes on. I only wanted to get a target figure out here in the open because it is so difficult to talk in terms, "if we stop testing." We are not stopping tests. This gives us a figure of what kind of testing will result in what kind of a level in human bone.

Senator ANDERSON. I want to say to you, Dr. Neuman, I find it extremely interesting testimony, particularly since so many people keep saying to us, it is very easy to get this thing in equilibrium. All you have to do is to see that the amount of fission material you put in the atmosphere is not any greater than the amount of strontium that just naturally deteriorates.

When you ask them how much that is, they say, "Well," and that is the end of the answer. At least I have a figure here for the first time in my life.

Thank you very much. I found it most stimulating and interesting. I think it offers a possibility we will have additional discussion from other people on it.

Before Colonel Hartgering begins his presentation, I would like to insert in the record the statement by Dr. MacDonald and an article from Science magazine entitled "Strontium 90 Hazard: Relationship Between Maximum Permissible Concentration and Population Mean" by W. O. Caster, of the department of physiological chemistry, University of Minnesota:

(The material referred to follows:)

EXPERIMENTAL CONCLUSIONS PERTINENT TO CERTAIN ITEMS ON THE AGENDA OF THE FORTHCOMING HEARINGS OF THE JOINT CONGRESSIONAL COMMITTEE ON ATOMIC ENERGY ON THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN, MAY 27, 1957, WASHINGTON, D. C.

By N. S. MacDonald, Ph.D., chief, bone metabolism section, pharmacology and toxicology division, atomic-energy project, University of California at Los Angeles

Based on data from experiments performed by the bone metabolism section of the atomic-energy project, University of California at Los Angeles. Much of this work could not have been accomplished without the cooperative efforts of Dr. R. E. Nusbaum and Mr. G. V. Alexander, of the spectroscopy section of the project.

The scope of these conclusions and opinions would have been greatly broadened had the many contributions to the subjects by other authors been included. However, instead of constituting a critical résumé of the whole field, this document reviews only those pertinent investigations for which I can speak at first hand. The paragraph hearings are numbered according to the hearings outline.

POSSIBILITIES FOR WATER TREATMENT

Samples of raw and tap water were obtained from 50 cities located throughout the United States. Analyses for stable, nonradioactive Ca, Sr, and Mg were made by an emission spectrographic technique. Conventional municipal water-treatment practices involving coagulation, filtration, and softening with lime-soda ash, phosphates, or ion exchangers in some cases remove as much as 75 percent of the nonradioactive Sr normally present in the raw water of some cities. However, if the level of Sr-90 activity were more than $20\times10^{-6}\mu c$ per ml., even a 90-percent removal of dissolved $Sr+Sr^{90}$ would fail to reduce the

contamination below the maximum permissible for drinking water. In such a situation, the usual single-stage precipitation would be inadequate.[1]

THE CALCIUM MODEL AS A BASIS FOR PREDICTING SR-90 BEHAVIOR

1. Similarities and differences in behavior

The *qualitative* similarities in metabolic behavior of Ca and Sr are well documented by dozens of workers over the years. However, quantitative differences in absorption, excretion, and skeletal retention do exist.

(*a*) Double tracer studies with Ca-45 and Sr-90 on rabbits have indicated that the kidneys discriminate between Ca and Sr in the blood, with the result that *more* of an intravenous dose of Sr is excreted in the urine than is true for a dose of Ca.[2]

(*b*) Spectrographic analyses for the nonradioactive Ca and Sr always present in foods and bones have shown, both for animals and for man, that the Sr to Ca ratio of their bones is always *less* than the Sr to Ca ratio of their diet.[3] This is the result of the Ca-Sr discriminatory aspect of at least two processes operating in the chain, food to bone. These processes, where discrimination is most effective, appear to be gastrointestinal absorption and urinary excretion.

2. Influence of amount of calcium in soil, diet

The *quantity* of Sr swallowed each day does not alter the magnitude of the ratio (Sr/Ca) bone÷(Sr/Ca) diet. This implies that a permanent increase of the Sr-90: Ca value of a daily diet would result in an increase of the Sr-90: Ca value in the skeleton. Conversely, a decrease in dietary intake of Sr-90 would probably lead to a *slow* drop in skeletal Sr-90 contamination. There is apparently no upper limit to the amount of Sr which can be deposited in the skeleton.[3][4] There does seem to be an upper limit to the amount of yttrium (and presumably the rare earths) which bone can take up.[5]

The quantity of Ca swallowed *along with* a dose of Sr-90 does affect the amount of Sr-90 which ends up in the skeleton. There is a clear-cut reduction of Sr-90 bone burden as the Ca content of the Sr-90 contaminated meal is increased. (This is not true if the Ca is in the form of milk. Milk tends to *increase* the absorption of an oral dose of Sr-90.) Unfortunately, addition of larger and larger amounts of Ca do not result in *equally* smaller and smaller bone burdens of Sr-90. Addition of phosphate, sulfate, or any of a number of other substances tested, to the Sr-90 contaminated meal also reduces the resulting bone burden, probably because of a precipitating action in the gut and stomach.[6][7]

It is probable that supplementation of a meal with calcium phosphate or sulfate tablets would somewhat reduce the absorption and retention of any Sr-90 in the meal. However, the nutritional and toxicological aspects particular in infants, of any such *long-continued elevation* of dietary Ca and/or phosphate demand very thorough investigation.

In regard to the hypothetical situation where, in an extreme emergency, it might become necessary to eat 1 or 2 Sr-90 contaminated meals, a short, prior period of starvation does not necessarily make the meal more dangerous than if eating had been normal up to that time (rats). Also, consumption of large quantities of milk along with the usual solid diet, either for 2 weeks prior to a single, oral Sr-90 dose or for 2 weeks immediately after dosage, fails to alter the bone burden. Apparently, if the body is approximately in normal calcium balance, large "emergency" oral intakes of Ca have little effect on bone burden, unless the Ca is present in the gastrointestinal tract *at the same time* as the Sr.-90.[8]

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[1] Strontium and Calcium in Municipal Water Supplies. J. Am. Water Works Assoc., 46, 643 (1954), Alexander, G. V., Nusbaum, R. E., and MacDonald, N. S.

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[4] The Skeletal Deposition of Nonradioactive Strontium. J. Biol. Chem. 188, 137 (1951), MacDonald, Nusbaum, Stearns, Ezmirlian, and Spain.

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[7] The Effects of Calcium and Phosphate in Foods on Radiostrontium Accumulation. J. Nutrition 57, 555 (1955), MacDonald, N. S., Spain, Ezmirlian, and Rounds.

[8] Quarterly Progress Report, UCLA 386, p. 54, 1957, MacDonald, N. S.

May I venture the following opinions?

(*a*) The intentional enrichment with uncontaminated Ca of the population diet on a routine, permanent or semipermanent basis, with the intent of reducing the Sr–90/Ca ratio for human intake, is impractical and ineffectual. In particular, it might even be dangerous so to enrich cow's milk destined for bottle-fed infants.

(*b*) On the other hand, it is essential to ensure that the daily Ca intake of the population, and particularly growing children, is maintained at an *adequate* level *prior* to an anticipated exposure to suddenly elevated Sr–90 contamination of foodstuffs. This follows from evidence in the literature that animals whose diet has been inadequate in Ca over a prolonged period will absorb more of an oral dose of Ca (and presumably Sr–90) than animals whose nutrition has been adequate.

(*c*) If, after a Sr–90 accident, it is learned that Sr–90 has been inadvertently swallowed or inhaled (the sputum becomes contaminated and is swallowed), oral administration of epsom salts ($MgSO_4$), sodium sulfate, or calcium phosphate, within the first hour, should decrease its absorption.[9] Milk should *not* be given—contrary to lead-poisoning procedure. We have preliminary evidence, in rabbits, that *immediate massive* infusions of sodium chloride or calcium gluconate may be of some slight benefit.

DEPOSITION IN MAN—VARIATIONS OF SR–90 LEVEL

For situations of daily ingestion of Sr–90 over "sufficiently long" periods of time, the Sr–90/Ca ratio should become the same in all parts of the skeleton; that is, the distribution should become uniform, on a gross scale. In 1949, we analyzed skull bone, rib, vertebra and femur specimens from each of 21 humans, aged 5 months, 75 years, for stable, nonradioactive Sr, by emission spectroscopy. In addition, 12 cadavers preserved since 1914 were also analyzed. Within any individual there were no significant differences in Sr content between the various bones, although the Sr level of all these specimens from one individual may have been high or low with respect to the average for all individuals studied. The 1914 values were not significantly different from the 1949 values, suggesting perhaps that no major change in dietary Sr/Ca ratio occurred during this span of years.[10] What constitutes a "sufficiently long" time for this attainment of diet-bone equilibrium remains to be determined. It may be much longer for adults than for children.

As to the ultimate site of residence for Sr or Sr–90 within the skeleton, X-ray diffraction evidence indicates that these atoms finally become incorporated into the internal structure of the tiny crystals of bone mineral, possibly occupying lattice positions which normally would be filled by Ca atoms.[11] This suggests that effective removal of Sr–90 which has been in the bones for more than a short time is possible only by dissolution (resorption) of the bone mineral itself. Although such resorption and reformation of new bone is going on to some extent all the time in our bones, to hasten these processes artificially to any useful degree, is almost impossible and medically very hazardous to attempt.

PREDICTED OCCURRENCE FROM WEAPONS TESTS HELD PRIOR TO 1957

For a number of years we have studied the relationships of the stable, nonradioactive, natural Sr which has always been present in tiny amounts in all our food and in all our bones, in order to set up a model for *predicting* the behavior of the Sr–90 which is now entering our lives. Recently, samples of soils; human foods such as milk, vegetables, fruit and eggs; feeds for cows and hens; and human ribs have been collected from six widely separated regions of the United States, by Mr. G. V. Alexander and Dr. R. E. Nusbaum. They are now analysing these materials for nonradioactive Sr and Ca. The study is not complete, but one of the tentative results is that the value for

$$(\text{Sr/Ca})\ \text{cow milk} \div (\text{Sr/Ca})\ \text{cow feed} = 0.13$$

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[9] Diminishing the Skeletal Retention of Ingested Radiostrontium by Use of Chemical Agents. MacDonald, N. S., in "Therapy of Radioelement Poisoning," ANL–5584, Argonne National Laboratory, p. 83 (1956).

[10] The Strontium Content of Human Bones. J. Biol. Chem. 185, 519 (1950), Hodges, MacDonald, Stearns, Ezmirlian, and Spain.

[11] The Ultimate Site of Skeletal Deposition of Strontium and Lead. J. Biol. Chem. 189, 387 (1951). MacDonald, Ezmirlian, Spain, and McArthur.

Now to arrive at a value quantitatively expressing the fate of Sr as it goes from our food to our bones, it is necessary to know what fraction of our total Ca intake comes from dairy products, what fraction from vegetables, from cereal grains, eggs, etc. This follows from the fact that Sr "travels" along with Ca in the biosphere, so that, aside from *short term* exposures to early fallout material containing particulate Sr–90, inhalation of dusts, etc., our long-term, prolonged, worldwide exposures to Sr–90 will come from the same dietary sources as our Ca. From published data, we estimate that about 72 percent of the total Ca intake for the whole United States population comes from milk and derived products and about 28 percent from vegetables, cereals, fruits, and eggs. The results on the aforementioned samples which have been analysed thus far, yield the following result:

$$(Sr/Ca) \text{ human bone} \div (Sr/Ca) \text{ average diet} = 0.14$$

Using this nonradioactive, natural strontium relationship as a model for predicting Sr–90 behavior, we feel justified in saying that, at *equilibrium* on a *fixed level* of Sr–90 contamination, the average Sr–90 activity per gram of Ca in the bones of the whole population will, in time, attain a value of about 0.14 or one-seventh that of the diet. This number is subject to adjustment, since the work is not yet completed.

Application of the natural, nonradioactive Sr model to levels of Sr–90 contamination now existing.

The following values have been taken from the report on "Sr–90 in Man," by J. L. Kulp, et al., which appeared in Science, February 8, 1957, 125, page 219 (1957):

(*a*) Average worldwide content of total body burden in man equals 0.12 micro-microcuries per gram of skeletal calcium.

(*b*) Young children have 3 to 4 times the concentration mentioned above.

(*c*) In 1955, the average diet for the human population in the United States had a burden of about 7 micro-microcuries of Sr–90 per gram of calcium.

Using our factor from natural Sr for diet to bone of 0.14, if this level of 7 $\mu\mu c$ remained fixed, we would predict that the attainable average bone burden would be (0.14) (7)=1 $\mu\mu c$ of Sr–90 per gram of Ca for the whole population. The burden actually observed was 0.12 suggesting that the bones of humans are not yet in equilibrium with the present dietary level of contamination. There is no *direct* information, derived from actual experiments, to tell us how much time will be required for the population to reach this equilibrium.

None of the work presently underway here (UCLA) gives data directly pertinent to establishment of a maximum permissible concentration (MPC) for Sr^{90} in humans. The National Committee on Radiation Protection (National Bureau of Standards Handbook 52) has proposed a total body burden for adult individuals of 1 microcurie, which is approximately an MPC of 1000 micromicrocuries per gram of skeletal Ca. However, when dealing with whole populations, exposures are likely to be distributed approximately according to a normal, statistical distribution curve. Therefore, if an average MPC of 1000 $\mu\mu c$ per gm of Ca were adopted for that population, and conditions so arranged that the *average* exposure did not exceed this, a considerable number of individuals would incur an exposure of 10 times this average. Therefore, it has been suggested that the MPC be lowered to 100 $\mu\mu c$ per gm of Ca, for controlling a populationwide contamination hazard.

Using this lower MPC, the *present* worldwide average of Sr^{90} in humans is .12 or about one eight hundred and fiftieth the population MPC. The present burden in children is one three-hundredth to one two-hundredth the population MPC. The populationwide average *equilibrium* burden to be reached some time in the future, if the present dietary level remains unchanged, will be one one-hundredth the population MPC.

Put in another way, assuming an average daily intake in the United States of 1 gram of Ca per day, members of the population could "safely" swallow $100 \div 0.14$ or 714 micromicrocuries of Sr^{90} in their daily diet throughout a lifetime. This radioactivity amount to about $714 \times 10^{-6} \times 3.7 \times 10^{4} = 26$ disintegrations of Sr^{90} per second. The present level of dietary intake is reported to be 7 micromicrocuries or one one-hundredth this estimate of the tolerable amount (with so many uncertainties, the quotation marks need for further comment).

One further speculation may be made on the basis of our natural Sr data. We know that the cow discriminates against the natural Sr in her feed during the overall process, feed to milk. That is (Sr/Ca) milk $\div$ (Sr/Ca) feed=0.13. This probably applies to Sr^{90} also. Therefore, one can estimate the Sr^{90} getting

into the human diet via milk products, by monitoring only the cow feed. This obviates the overwhelming task of monitoring all the multitudinous dairy products destined for human consumption. All such products should have a Sr^{90}/Ca concentration of about one-seventh to one-eighth that of the cow feed, unless intentionally enriched by addition of uncontaminated mineral Ca.

[Reprinted from Science magazine, June 28, 1957]

Strontium 90 Hazard: Relationship Between Maximum Permissible Concentration and Population Mean

Recent discussions of radiation fallout (1, 2) have related the population mean Sr–90 body burden to the maximum permissible concentration, or MPC. Libby (1) has introduced the "MPC unit" (1 uc of Sr–90 per kilogram of calcium) and has used it to express concentrations of Sr–90 in milk, plants, soil, and so forth. The direct comparison of any mean with MPC, of course, implies that the two different terms are expressed in comparable units. Unfortunately, this is not true. The MPC unit represents the maximum permissible concentration of Sr–90 within the body that may be considered safe for any individual—and the individual is assumed (3) to be a professional isotope worker, probably male, 45 or more years of age, who is exposed to the isotope only under rigidly controlled laboratory conditions. A population mean, on the other hand, does not represent a maximum value, but rather a value which, by definition, will be exceeded by 50 percent of a population.

How much of a quantitative difference results from this inequality of units of expression? One example may suffice. In discussing the Sr–90 burden in a general human population, at least three additional factors must be taken into consideration: S, a safety factor; C, an allowance for children; and H, a heterogeneity factor. Thus,

$$MPC_{pop.} = \frac{MPC_{oc.}}{SCH}$$

where $MPC_{pop.}$ is the maximum permissible concentration that would be safe for a population mean, and $MPC_{oc.}$ is the maximum permissible concentration for occupational isotope workers (1 μc of Sr–90 per kilogram of calcium, 3).

Because of the uncertainty in the figures for maximum permissible concentration, it is usually suggested that members of the general public should not be exposed to more than one-tenth of the radiation hazard that is permitted for occupational workers (4). The value of S is therefore taken to be 10.

Kulp, Eckelmann, and Schulert (2) showed that children (0 to 4 years old) accumulate Sr–90 more rapidly than do adults (40 to 60 age group). The concentration of Sr–90 in the bones of children averaged 4 to 5 times that found in the general population (and 10 percent of the children exceeded this figure by a factor of (twentyfold or more). Added to this differential accumulation factor is the factor of the increased vulnerability of this age group. This factor is difficult to evaluate without direct experiments. It can be pointed out, however, that children in this age group have over twice the expected life span ahead of them in which to develop neoplastic or bone calcification changes in response to radiation exposure. For the moment, C is taken to be 5 x 2, or 10.

Within the 10 to 80 age group, there is a substantial variation in body burden of Sr–90 for people in one area, who are presumably exposed to the same environmental Sr–90 concentration. This may be related to differences in food habits, idiosyncrasies of calcium metabolism, and other factors. From the data of Kulp, Eckelmann, and Schulert (2) one can estimate that 6.8 percent of this population group will have a body burden of Sr–90 which exceeds by at least fivefold the population mean value for that age and area, 1 percent will exceed its population mean by about tenfold, and 0.2 percent will exceed its own population mean by more than fiftyfold . It is evident, therefore, that H must exceed 10. Combining these terms, one would estimate that:

$$MPC_{pop.} \leq \frac{1\ \mu c\ \text{Sr–90/kg Ca}}{10 \times 10 \times 10} = 1\ m\mu c\ \text{Sr–90/kg Ca}$$

A comparison of this figure with estimates (1, 2) of the Sr–90 concentration in man leads to the conclusion that the Sr–90 burden in the general population in 1955 was at least 10 percent of the $MPC_{pop.}$, and that if the predictions (1, 2) concerning the next 10 to 15 years are correct, the population mean value will shortly reach and exceed the maximum level compatible with public health and

safety. This view gives a very different picture of the probable safety situation from the one obtained by a direct comparison of MPC_{oc}. and the population mean (1, 2) and points to the hazards of introducing novel units of expression without first considering their fundamental nature.

Meanwhile, if any simple safety measures are available, advantage should be taken of these without delay. Since some 80 percent of the dietary calcium (and thus Sr–90) that enters the body comes from milk and milk products (5) it would seem possible to decrease the intake of Sr–90 by declacifying milk. Such a procedure is reportedly effective (6). The technology and economics of this process are essentially those used currently in the production of low-sodium milk (7) and in a process for soft-curd milk (8). Milk calcium could then be replaced by calcium derived from ancient sources (limestone, for example) which would have a much lower Sr–90 content. This procedure would not seriously disturb the calcium balance in the general population, and it could provide an immediate four- to five-fold reduction in the intake of Sr–90.

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University of Minnesota, Minneapolis.

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8. H. E. Otting, Ind. Eng. Chem. 41, 457 (1949).

Senator ANDERSON. Lt. Col. James B. Hartgering, Walter Reed Hospital, is our next witness and we are very glad to have you, Colonel.

STATEMENTS OF LT. COL. JAMES B. HARTGERING,[2] MC; ACCOMPANIED BY LT. ARIEL G. SCHRODT, MSC, WALTER REED ARMY INSTITUTE OF RESEARCH, WALTER REED ARMY MEDICAL CENTER

Colonel HARTGERING. The Walter Reed Army Institute of Research has been engaged in measuring the worldwide fallout from nuclear tests since 1954. Following the radiation exposure of American military personnel to the March 1, 1954, detonation in the Pacific area, Walter Reed Army Institute of Research personnel had the opportunity of participating in their medical evaluation at Tripler Army

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Hospital in Hawaii. As a part of the evaluation, urine specimens were analyzed for radioactive fission products. Routine control specimens were collected from residents of Honolulu and Washington, D. C., and it was determined that these contained about the same amount of iodine 131 activity as urine collected from the servicemen exposed to local fallout in the Marshall Islands.

Prior to 1955 a number of scientists in the United States as well as other countries had measured nuclear debris using air filters, gummed paper, and soil sampling techniques. Professor Libby and others had measured the strontium content of human bones and a few human fetuses. Extrapolations were made estimating the expected levels of fission products in contemporary man, but many technical uncertainties prevented other than gross figures. No direct data was available for the accumulation of specific isotopes in the average individual as he goes about his daily routine.

It appeared to us that the uncertainties of extrapolation of physical data to man were so great, and the problem of such fundamental importance, that direct measurements of large numbers of men should be attempted. The fact that United States military personnel are stationed throughout the world provides an ideal sampling network.

It is known from animal and limited human data that many fission products are excreted by the kidneys and appear in the urine of exposed individuals in amounts that can be predicted, provided the excretion rate for the individual isotope being studied is known. Therefore, it was decided that the measurement of radionuclides present in 24-hour urine specimens could result in direct evidence of the amount and type of activity present in man.

EXPERIMENTAL DESIGN

In January 1955, with the cooperation of active duty military personnel stationed as indicated in figures 1 and 2, a sampling network was selected. At each United States station, 10 individuals participated and at overseas stations, 5 individuals. Twenty-four-hour urine specimens (1–2 liters) were collected by each individual in the United States once a week for 14 weeks during Operation Teapot and shipped by air to the Walter Reed Army Institute of Research. Foreign stations contributed on a reduced schedule. In excess of 2,100 specimens from the 17 United States stations, and 500 from the 15 overseas stations, were received and processed. Isotopes of iodine, strontium, and cesium were separated radiochemically and the levels of activity determined by low-level beta and gamma counting techniques.

Foreign Collection Stations, 1955

Tropical Research Medical Laboratory, Puerto Rico; United States Air Force Hospital, Wiesbaden Germany; United States Air Force Hospital, Thule, Greenland; American Embassy, Australia; Fort Richardson, Alaska; 406th Medical General Laboratory, Tokyo, Japan; United States Mission, Cochabamba, Bolivia; United States Embassy, Pretoria, Union of South Africa; United States Army Mission, Rio de Janerio, Brazil; United States Air Force Dispensary, Tyo, Japan; United States Air Force Hospital, Luzon, Philippine Islands; Burtonwood Air Force Base, Warrington, England; Tripler Army Hospital, Hawaii; Army Medical Unit, Kuala Lumpur, Malaya; Kagnew Station, Asmara, Eritrea.

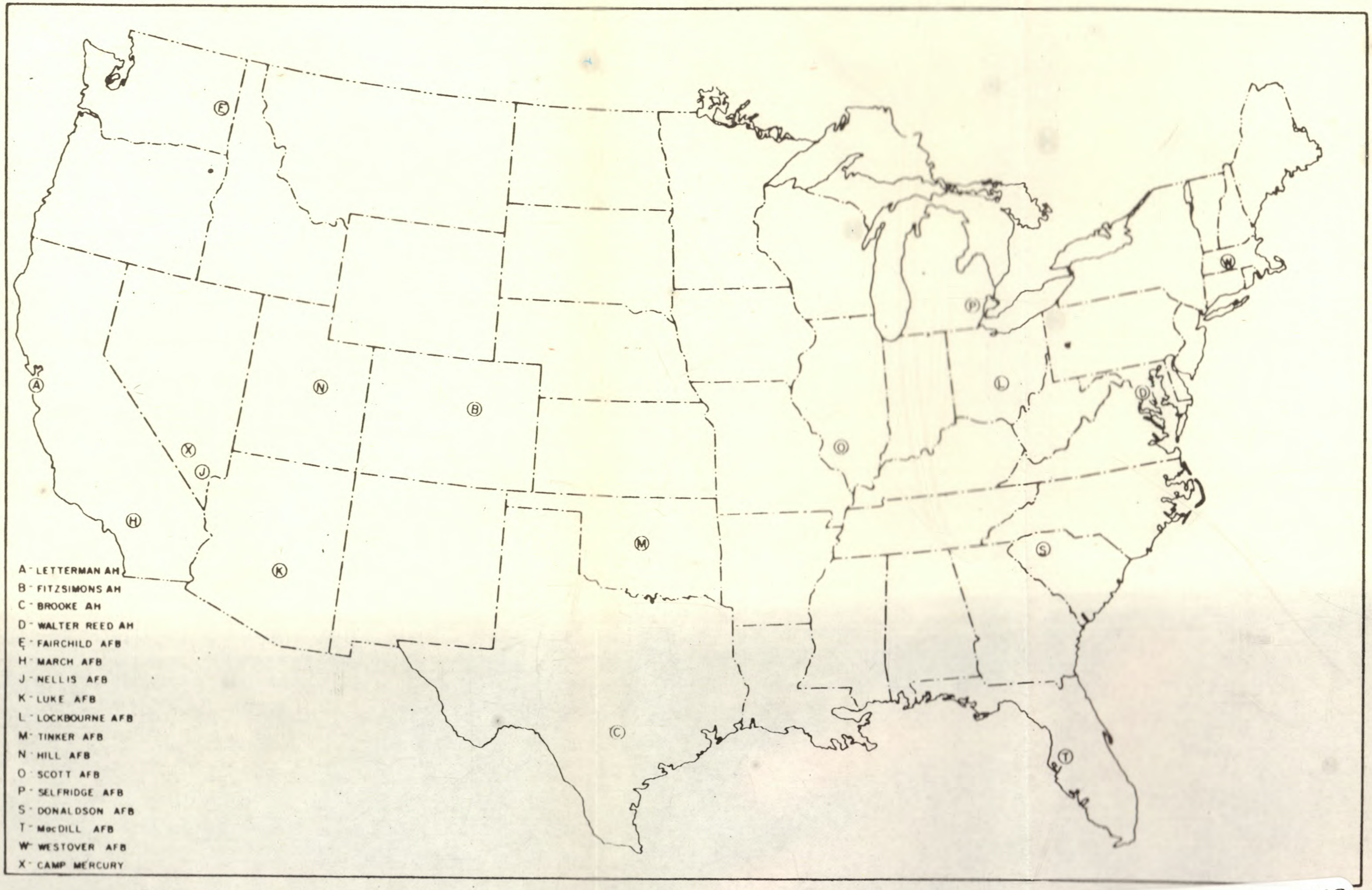

FIG I MAP OF US COLLECTION STATIONS

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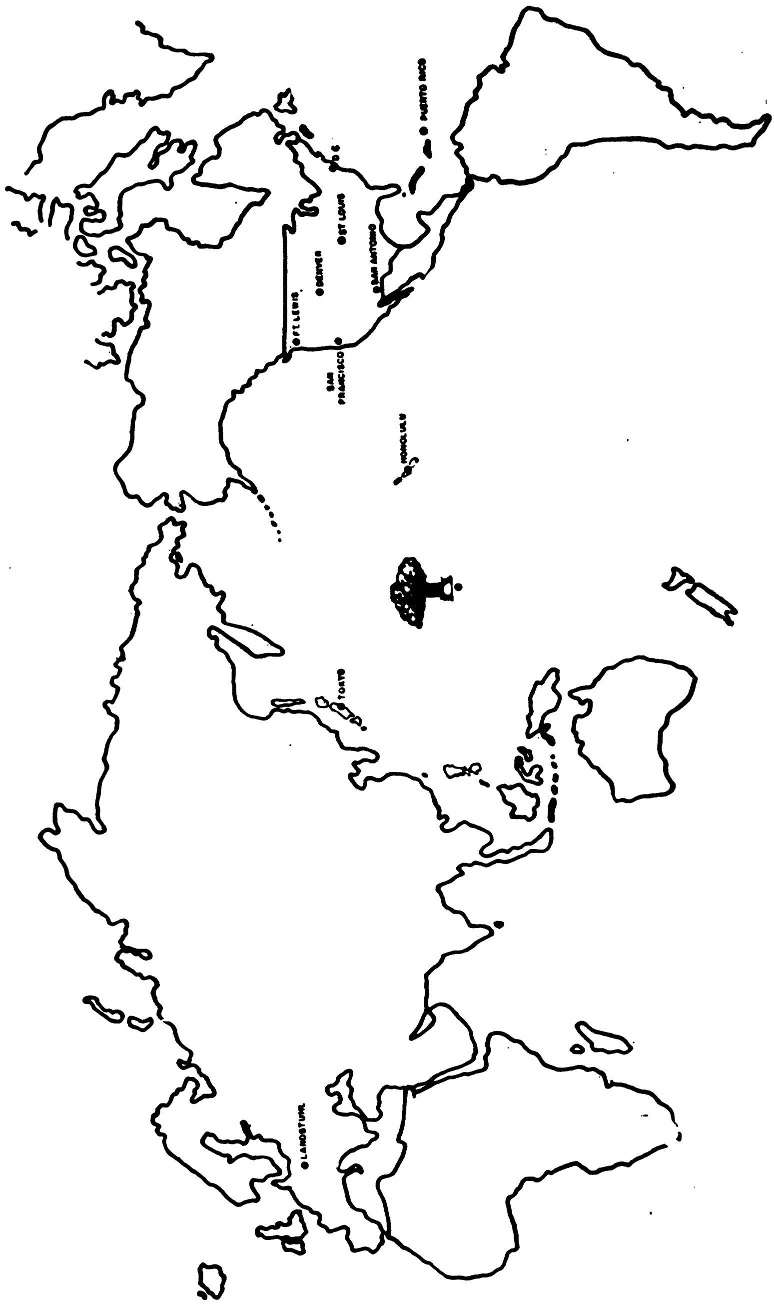
In March 1956, prior to Operation Redwing, a different network (fig. 3) was established after consulting Dr. L. Machta of the United States Weather Bureau. The number of stations was necessarily reduced because the proposed radiochemical procedures were expanded to include, in addition to the isotopes mentioned above, specific determinations for the presence or absence of zirconium 95, ruthenium 106 and whatever other nuclides might be unexpectedly present. Again, in excess of 2,000 specimens were received and processed.

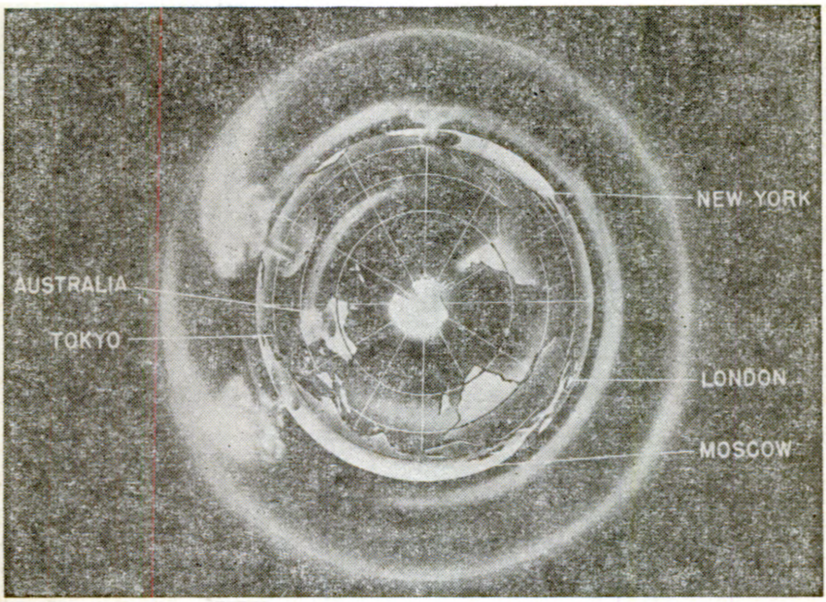


COLLECTION STATIONS

RESULTS

Figure 4 is a schematic representation of the tropospheric and stratospheric distribution of radioactivity following nuclear detonations. Since in this study we are measuring man directly, the values obtained represent the total fallout accumulated in and available for assimilation into the human system at the time of sampling, irrespective of its source. In view of the long estimates for stratospheric storage time (half time of 7–10 years) and the short physical half life of iodine (8 days), it is perhaps apparent that much of the activity measured in the urine specimens was tropospheric in origin.

FIGURE 4



Figures 5 to 13 are examples of the positive data obtained. Iodine values represent the average activity of the 10 or so individual specimens submitted from the specific station on the date noted. Values are corrected for decay to the time of collection. The values among individuals at the same station differed by as much as a factor of 10, but this is within the expected biological variation. The cesium and strontium values were determined by pooling specimens from one or more weekly collections at a given station. Pooling was required as the expected activity was so low as not to be otherwise detectable, even with the very sensitive low level counting equipment at the Walter Reed Army Institute of Research. Strontium values are available only for the first 6 weeks of the Redwing (April–September 1956) study. The remaining samples are currently being processed. Cerium 144 is known to be present in the 1956 specimens, but the actual amounts are as yet undetermined. Detailed radiochemistry failed to reveal the presence of either zirconium or ruthenium.

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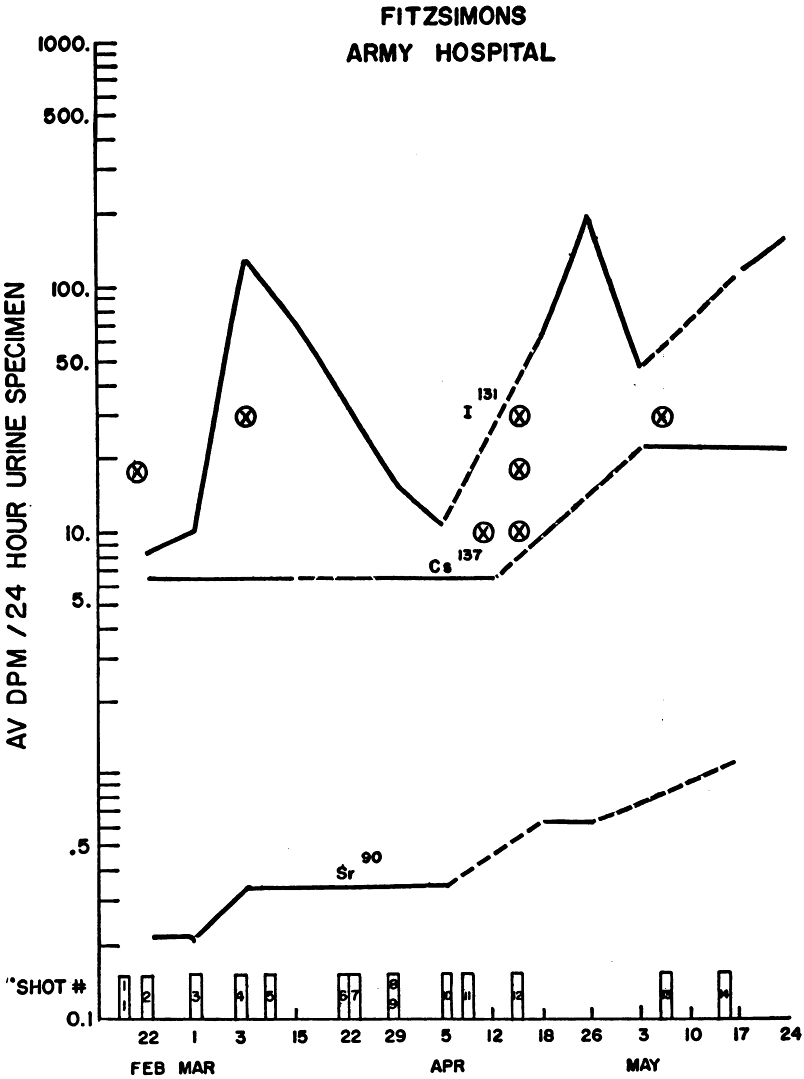
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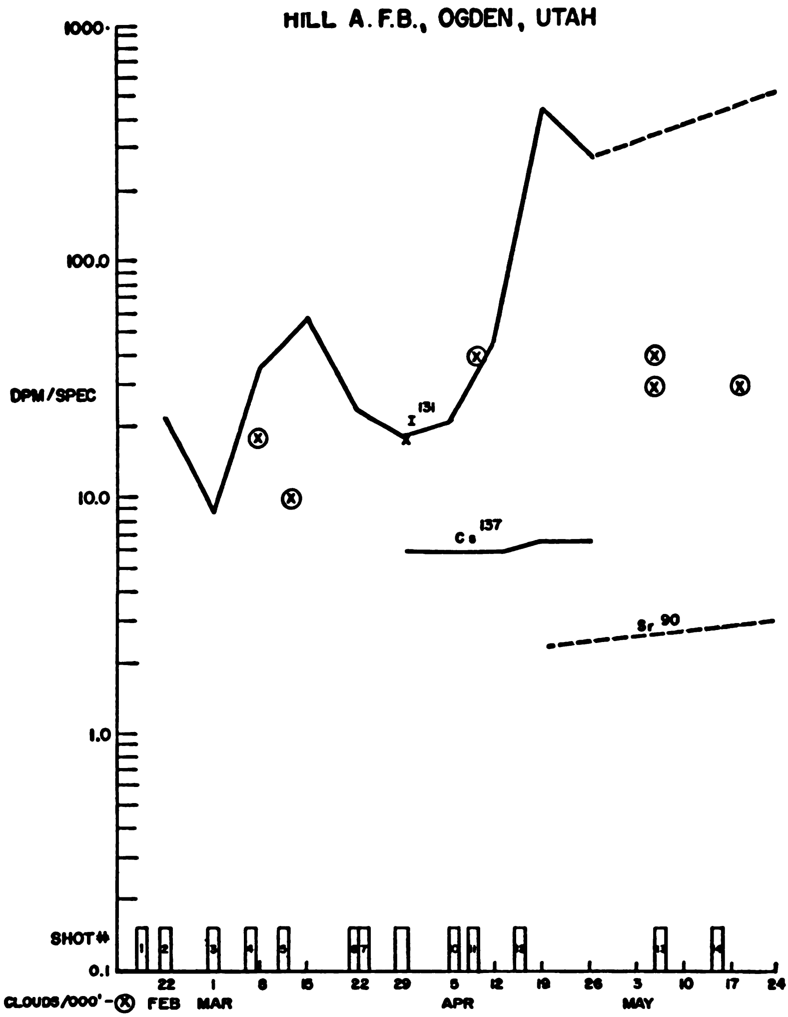
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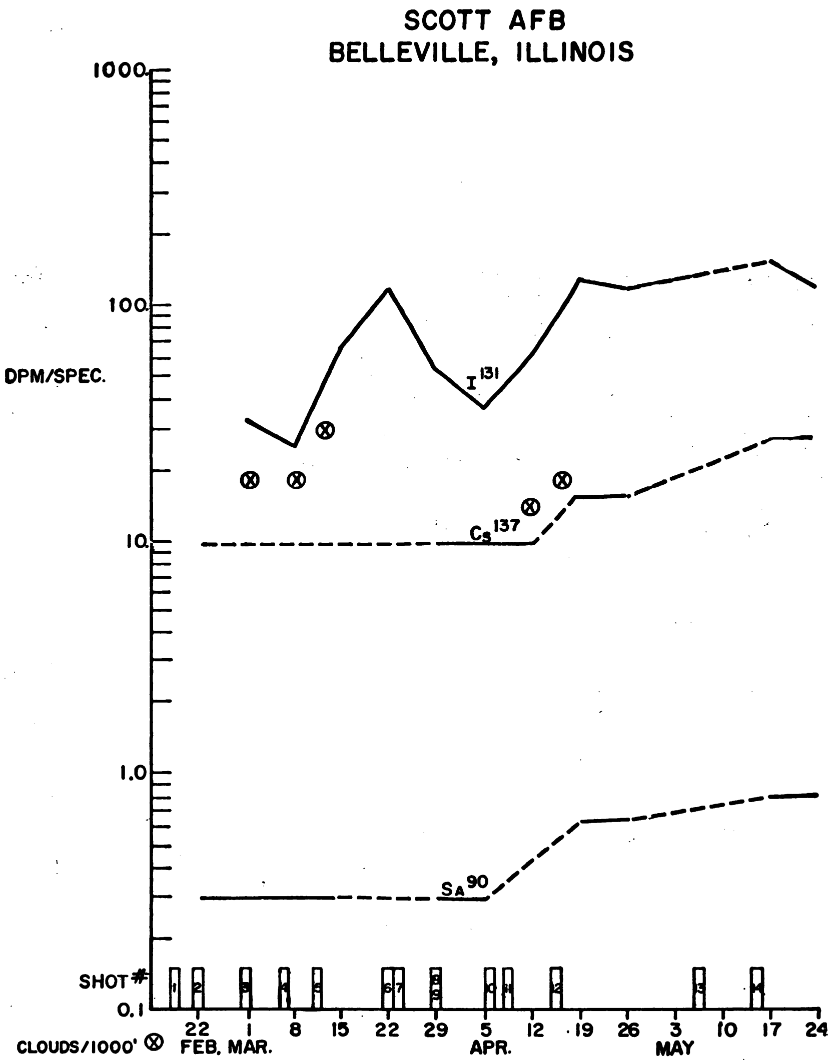
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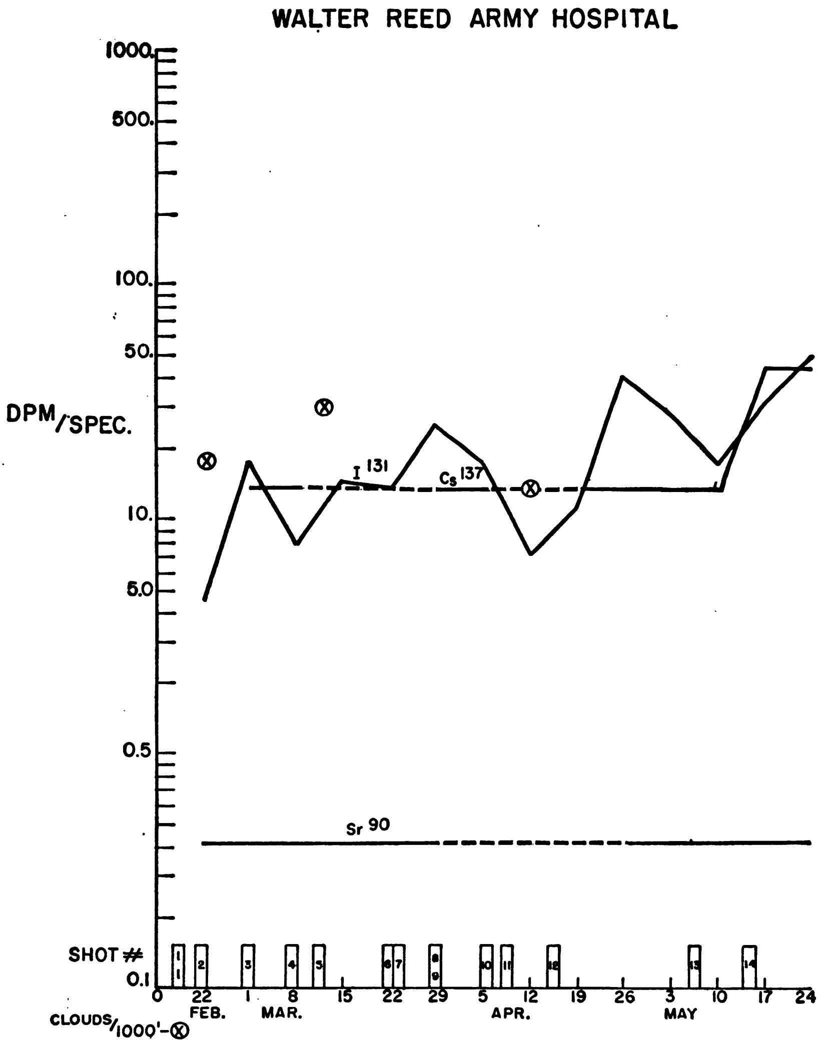

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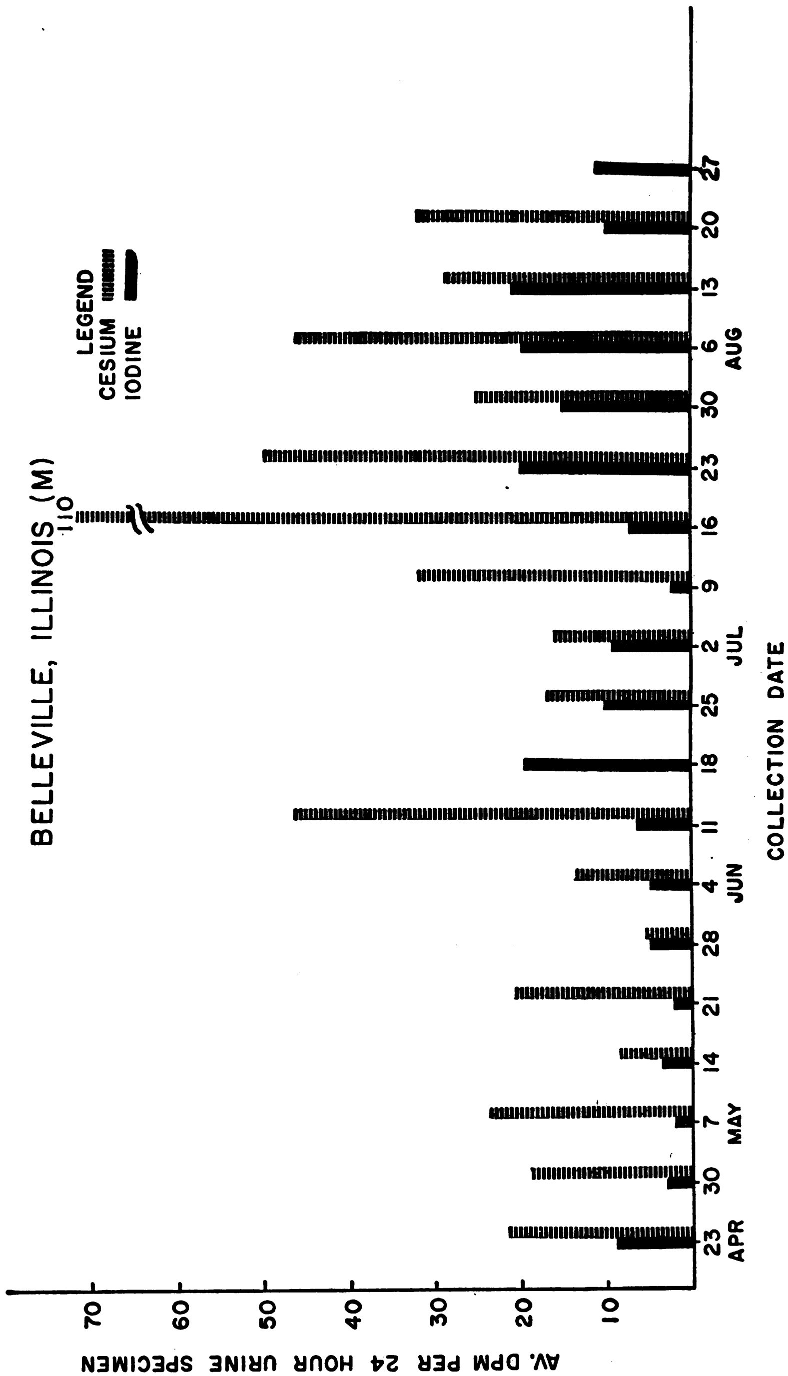
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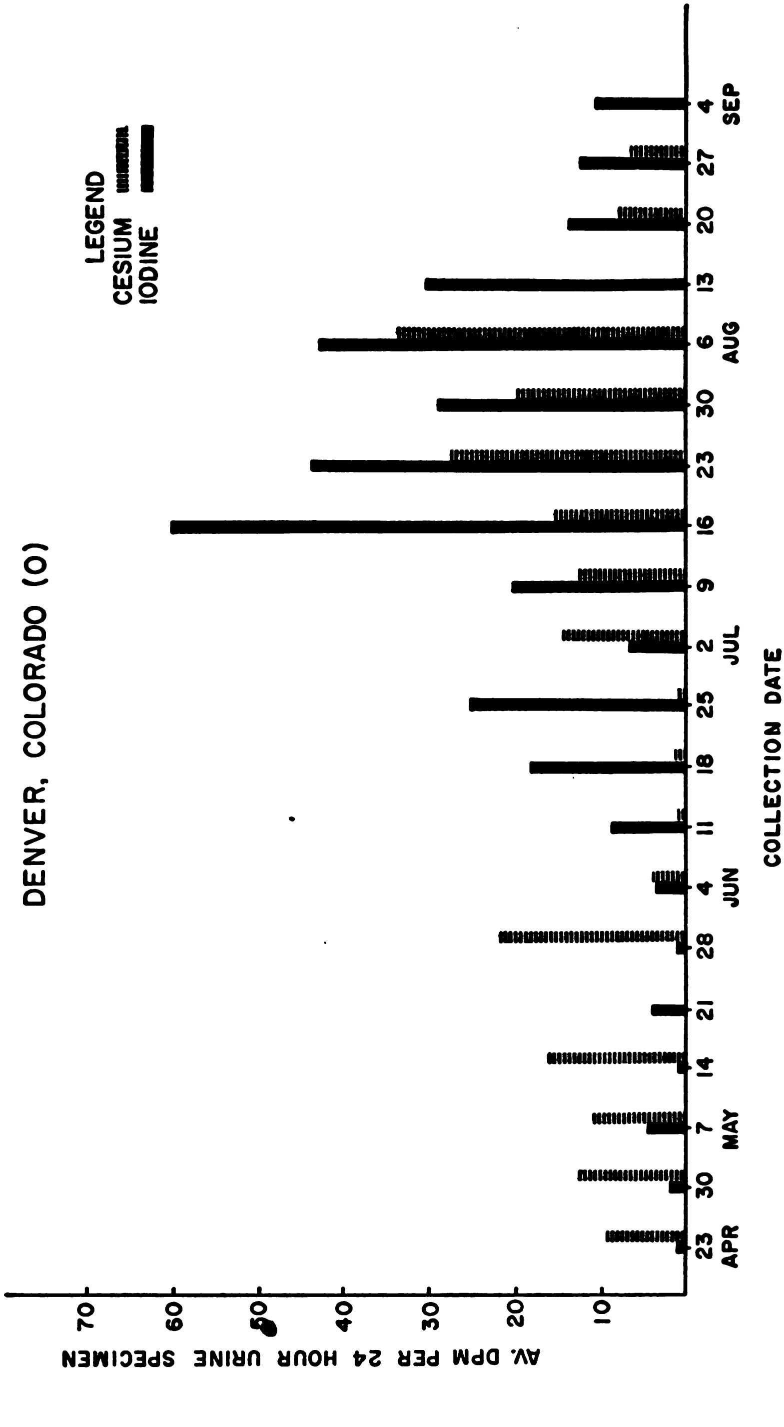
COLLECTION DATE


DENVER, COLORADO (O)
LEGEND
CESIUM
IODINE
AV. DPM PER 24 HOUR URINE SPECIMEN
10
20
30
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70
APR 23
30
MAY 7
14
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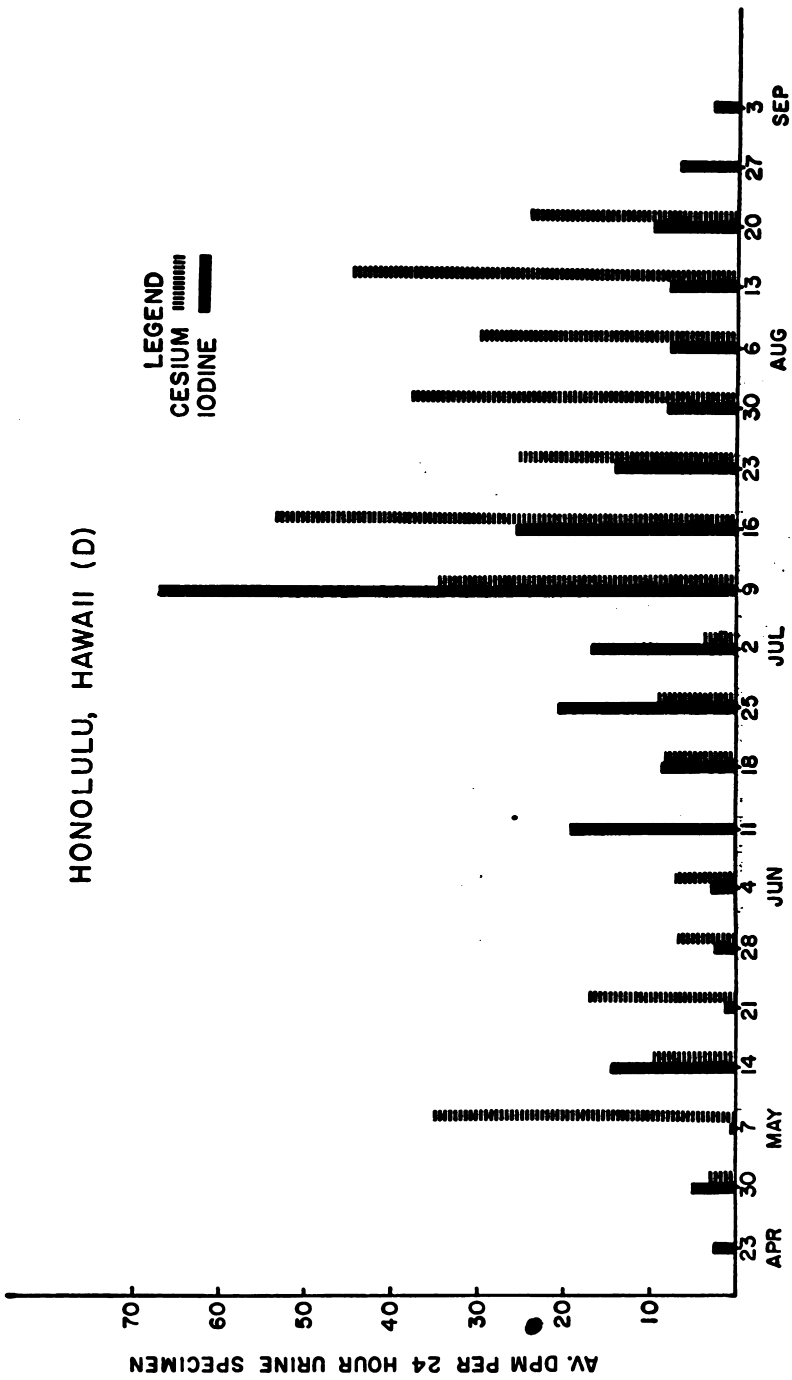
HONOLULU, HAWAII (D)
LEGEND
CESIUM
IODINE
AV. DPM PER 24 HOUR URINE SPECIMEN
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70
COLLECTION DATE
APR
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MAY
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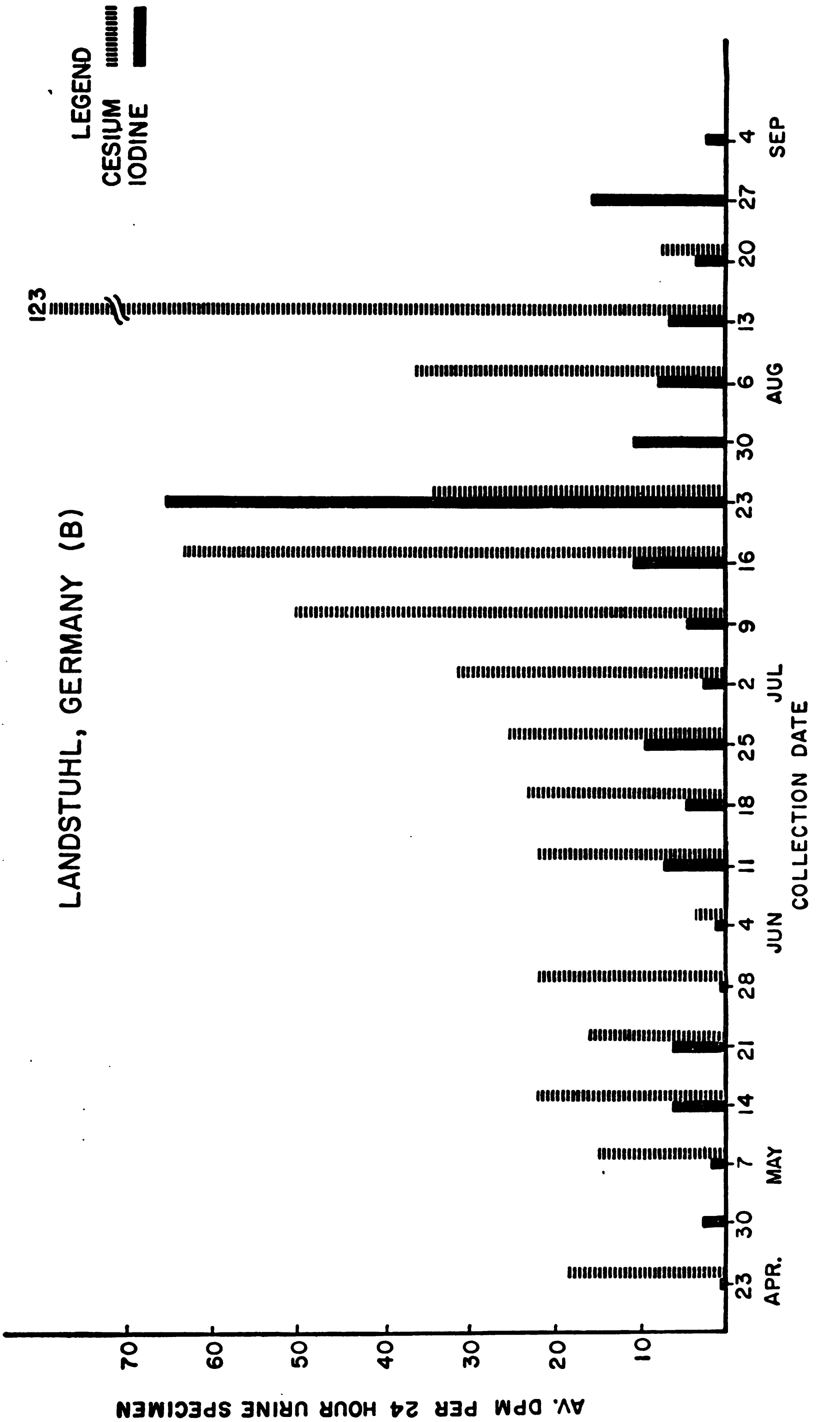
LANDSTUHL, GERMANY (B)
LEGEND
CESIUM
IODINE
123
AV. DPM PER 24 HOUR URINE SPECIMEN
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50
60
70
COLLECTION DATE
APR. 23
30
MAY 7
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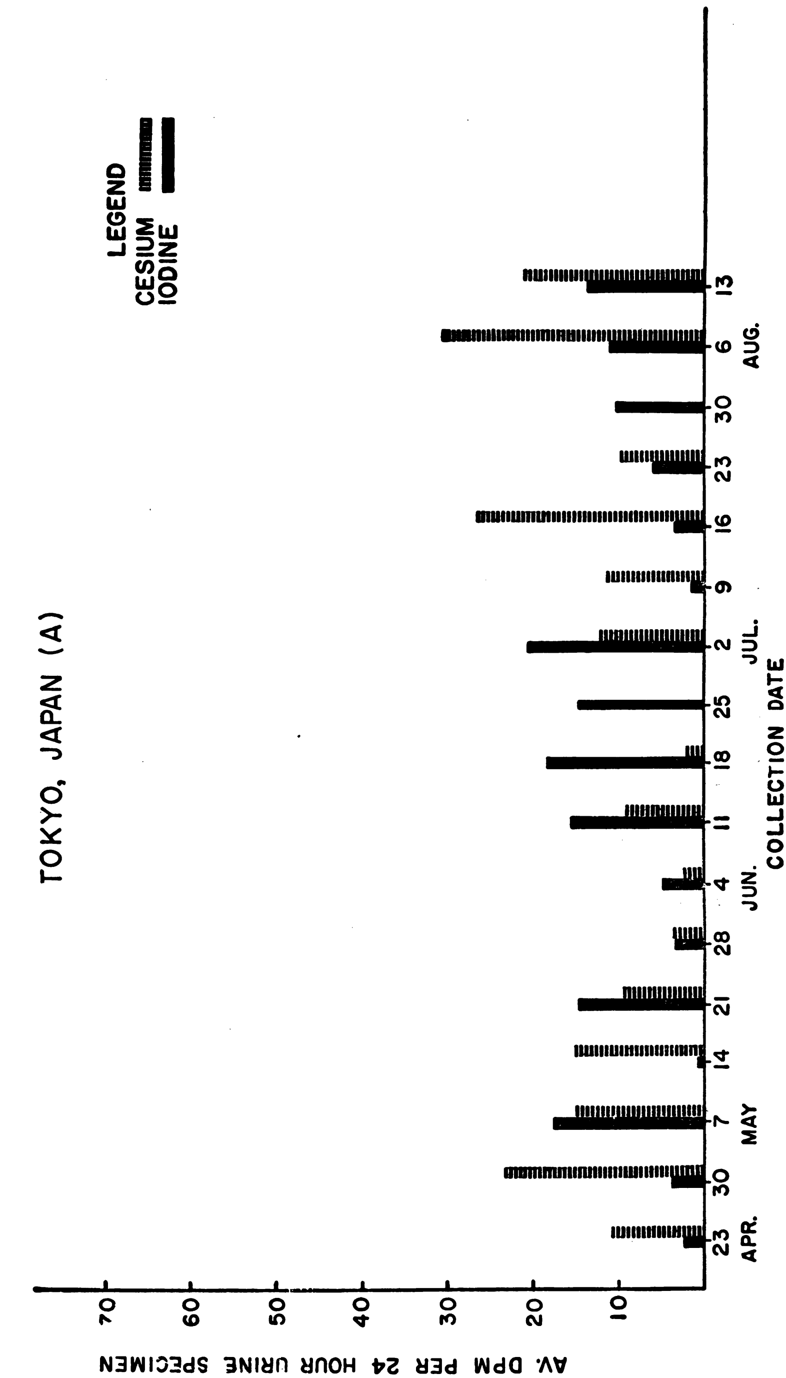
SEP 4


TOKYO, JAPAN (A)
LEGEND
CESIUM
IODINE
AV. DPM PER 24 HOUR URINE SPECIMEN
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APR.
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COLLECTION DATE

The generally rising levels of specific activity during each test series are noted in most of the figures. In addition certain peak values are apparent, particularly for iodine and cesium. These rises, in general, correspond to the presence of a specific radioactive airmass over or near the sampling station. Airmass trajectories were obtained from Dr. L. Machta and are represented by (X's) at the appropriate altitude as measured in thousands of feet. The correlation between abrupt peaks of activity in man and the presence of an active airmass is quite good. Further, negative urine data obtained during Operation Teapot, at other stations not shown in the figures, such as Letterman Army Hospital, San Francisco, and Westover Air Force Base, Mass., helps to strengthen this correlation. In our opinion the peaks, particularly for iodine, but also for cesium and perhaps strontium, are due to inhalation of the radionuclides and do not reflect transmission through the food cycle. No doubt the plant animal-food cycle also contributes to man's radioactivity, but at early times we would suggest that inhalation may be the primary entry route for certain isotopes.

Utilizing known average excretion rates for iodine and cesium in man, our urinary values can be converted to estimated body burdens for Operation Teapot (1955). The maximum average iodine thyroid values were obtained from Ogden, Utah, and were 11×10^{-4} microcuries per individual. For Redwing (1956) the maximum was at Denver, Colyo., where the average thyroid burden was 13×10^{-5} microcuries. It is emphasized that these are maximum values and represent respectively 1/10,000 and 1/100,000 of the amounts given as tracer doses to patients in radioscope clinics throughout our country.

The maximum cesium body burden for Redwing was obtained from Washington, D. C., and was 3.4×10^{-3} microcuries. This may be compared to the proposed tolerance value of 30 microcuries.

Available strontium excretion values for general locations are noted in figure 14. The urinary excretion rate for man can only be estimated at the present time, but a conservative figure would indicate a body burden of the order of 1 to 5×10^{-3} microcuries as compared to the general population tolerance of 0.1 microcurie.

CONCLUSIONS AND RECOMMENDATIONS

The data obtained in this program by measuring in excess of 4,600 24-hour specimens indicates that several fission products are readily detectable in man. Estimated body burdens for individual isotopes were several orders of magnitude below currently acceptable tolerance limits. Incidently, our values agree, in general, with those measured by other laboratories and by other methods.

Strontium values

Area	Year 1955	Spec. No.	DPM/spec.
Southern Hemisphere		129	0.07
North Atlantic		126	.34
North Pacific		160	.46
CONUS all stations	Mar. 8	109	.32

Collection site	Year 1956	Spec. No.	DPM/spec.
Zama, Japan	April–May	59	0.53
Landstuhl, Germany	do	63	.17
Puerto Rico	do	58	.88
Honolulu, T. H.	do	60	.60
San Francisco, Calif.	do	60	.35
Belleville, Ill.	do	57	1.86
Denver, Colo.	do	61	.30

This is to our knowledge the first example of such an extensive sampling program in the field of medicine and establishes the fact that the levels of the various fission products can be readily determined in the world's population by directly measuring man himself. This method avoids any uncertainties inherent in extrapolations from purely physical or animal data to man, and should provide an additional assurance that, in fact, the nature of current radioactive fallout is understood in considerable detail. Further, we believe that the methodology used in this program could, if desired, be expanded and employed as a continuing positive check on the status of worldwide fallout. If indeed, the radioactivity in man began to increase in any part of the world, it could be recognized immediately and appropriate precautionary measures taken before significant levels of activity were reached.

As is often the case in the study of medicine, however, a considerable amount of painstaking research still needs to be completed before we will be in complete control of the atom and its possible effects on man. The obvious gaps in our medical knowledge today are in the following three areas:

(*a*) The requirement for a more complete understanding of the kinetics (uptake, distribution in the body, and excretion rates) of the isotopes now known to enter the body.

(*b*) The somatic effects of the combination of the several isotopes on man. Current tolerances are established for individual isotopes, and it is assumed that the effects are merely additive, but detailed information is not yet available.

(*c*) A long-term continuing evaluation of the actual effects of these isotopes on man so that factual tolerance values can be established.

As research continues in these and other areas and the details become more completely understood, it is entirely possible that, in addition to physical methods, certain preventive medical measures may become available which will reduce the possible accumulation of even low levels of isotopes in man. I would like to point out that preventive research in nuclear medicine is really just beginning at the present time and a considerable degree of optimism is, in my opinion, justified.

Mr. Ramey. Does your work mean that individuals then pick up strontium by breathing from the air?

Colonel Hartgering. That is right.

Mr. RAMEY. As well as from food and milk, and so on?

Colonel HARTGERING. We feel that our data shows this without question, sir.

Mr. RAMEY. Is the amount they pick up significant in relation to these other amounts?

Colonel HARTGERING. This is a very difficult question to answer because it depends on the location of the individual. For an individual who is working for the Department of Defense or the Atomic Energy Commission at the test site, the amount that he picks up during the test period is significant. For the individual several thousand miles away this is just a part of it, and maybe it is a small fraction of the total that he will pick up over the next period of time. In our opinion the peaks particularly are due to the inhalation and do not reflect transmission through the food cycle. No doubt the food cycle is important and also contributes to man's radioactivity at late times.

Mr. RAMEY. Doctor, are the methods that you have used such that they are readily adaptable to monitoring strontium in bone?

Colonel HARTGERING. The methods we used are the same as Dr. Kulp uses in measuring the amounts in bone. We start out with several gallons of liquid urine and reduce it to a small volume and do essentially the same chemical separation and measuring procedures that he uses.

Senator ANDERSON. Thank you very much. This has been a very interesting discussion. I certainly commend you on the work that is being done in the field. It is very encouraging.

Now we come back to where we should have started this afternoon had we been able to stay on schedule: Dr. Langham and Dr. Anderson of Los Alamos Scientific Laboratory.

STATEMENT OF DR. WRIGHT LANGHAM,[1] ACCOMPANIED BY DR. E. C. ANDERSON,[2] LOS ALAMOS SCIENTIFIC LABORATORY

(A comprehensive report by Dr. Langham and Dr. Anderson on the hazards of strontium 90 appears on p. 1348.)

Dr. LANGHAM. The remarks that I plan to make, I would like to limit to a consideration of the long-term fallout problem in preference to getting involved in the discussion of what might be the result of

[1] Received a bachelor of science degree in science from the Oklahoma A. and M. College at Goodwell, Okla., in 1934, and a master's degree from the Oklahoma A. and M. College at Stillwater, Okla., in 1935. From 1935–37 he held a position of research chemist with the Agricultural Experimental Station at the Oklahoma A. and M. College at Goodwell. From 1937–38 he did graduate work at Iowa State College, Ames, Iowa, working in the field of organic chemistry. From 1939–41 he returned to the Agricultural Experimental Station at Goodwell, Okla., as acting experiment station director. In 1941 he entered the University of Colorado, from which he was awarded a doctor of philosophy degree in biochemistry with a minor in biology in 1943. In the same year he joined the staff of the Manhattan district's metallurgical laboratory at the University of Chicago, where he worked on the chemistry of plutonium. In 1944 he was transferred to the Los Alamos Scientific Laboratory, Los Alamos, N. Mex., to study the toxocology of plutonium and to develop methods of diagnosing exposure of personnel to this material. In 1946 he became the group leader of the biological and medical research group of the laboratory. Since then he has investigated a number of major problems concerning the biological and medical aspects of atomic energy development. These include rates of retention and excretion of internally deposited radioactive isotopes, toxicology, and physiology of tritium in man, relative biological effects of ionizing radiations of different types and energies, including bomb gamma rays and neutrons, incapacitating effects of massive doses of radiation, hazards from accidental noncritical detonation of atomic weapons, and the biological and medical implications of worldwide radioactive fallout. He is at present group leader of the biomedical research group and assistant division leader (for research) of the Los Alamos Scientific Laboratory's Health Division, and is an associate professor of biochemistry with the University of California at Los Angeles. He is a member of the following committees and organizations: Subcommittee on Internal Tolerances of the International

shorter-term fallout in the event we are actually involved in a nuclear war.

If we look over the fission products and the radioactive debris that results from weapons detonations, we find we can immediately eliminate the majority of the fission products as a long-term hazard on the basis of their physical half-life, their biological half-life, or the degree to which they enter into the ecological cycle.

Further inspection of these results indicate that there are really only three which might be of serious consideration. One is plutonium 239. It has not been mentioned in this meeting perhaps, but every time a bomb has been exploded it has probably made about as much plutonium as was burned up in the detonation. Therefore in these atomic clouds we do have plutonium.

We have only two other isotopes of appreciably long half-life which have a possibility of being hazardous. Those are strontium 90 and cesium 137. Let us dispose of the plutonium problem first.

Plutonium is a manmade element, at least in any great abundance. It is not taken up by plants and the discrimination factor is going from soils to plants for plutonium is a matter of 10,000. Plutonium is extremely poorly absorbed from the gut. The discrimination in this case is another factor of ten to the fifth or a hundred thousand.

Then we can immediately say that the concentration of plutonium in the bones of the individuals will be only of the order of one-one hundred millionth of what it is in the soil.

Knowing the amounts of plutonium that are made in these detonations, it is immediately possible to eliminate plutonium for any further consideration in the long-term fallout problem.

Let us now come to cesium 137. When a bomb is detonated the fission products form at various stages after the detonation. Cesium 137 and strontium 90 are formed with a halftime of about 3 minutes, which means they are formed late in the history of the fireball. Consequently, they are not trapped as much as are other isotopes in the heavy debris which falls out locally.

Since they are formed at approximately the same times in the history of the fireball, cesium and strontium tend to go together. So where we find one, we will find the other; in approximately the ratio they occur in the fission mixture, which is almost 1 to 1.

Then every time we have a millicurie per square mile of strontium 90, we will have a millicurie per square mile of cesium 137 deposited.

In order to evaluate the cesium 137 hazard, let us refer to natural potassium. The body contains approximately 150 grams of potassium. This is vital to life. Potassium, however, is radioactive, having in it

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Footnote continued from preceding page.

Commission for Radiological Protection; Subcommittee on Internal Radiation Tolerances of the National Committee for Radiation Protection; Subcommittee on Incineration of Radioactive Wastes of the National Commission for Radiation Protection; Subcommittee (Chairman) on the Relative Biological Effects of Ionizing Radiation of the National Commission for Radiation Protection; Subcommittee on Toxicity of Internal Emitters of the National Academy of Sciences and National Research Council; the American Association for the Advancement of Science; Radiation Research Society; Health Physics Society; Sigma Xi Honorary Scientific Research Society. (Submitted by witness.)

[2] Date and place of birth: August 23, 1920, Rock Island, Ill. Education: Bachelor of arts, Augustana College and Theological Seminary, 1942; doctor of philosophy (chemistry), Chicago, 1949. Work history: Assistant analyst, chemistry metallurgical laboratory, Chicago, 1942–44; Los Alamos Scientific Laboratory, 1944–46; Member: Staff biophysics, 49—; Rask-Orsted fellow, Copenhagen, 1951–52; Chemical Society, natural radiocarbon liquid scintillation counter; low-level radio activity measurements; neutrino detection; nuclear radiation dosimetry. (Submitted by Atomic Energy Commission.)

the isotope potassium 40. The number of gamma disintegrations occurring from potassium in the human body is approximately 450 per second.

In other words, from the natural potassium in our bodies, we are absorbing 450 gamma disintegrations per second. The number of beta disintegrations are approximately 9 times higher.

So therefore, we are getting somewhere around 9 times 450, which is about 4,000 beta disintegrations per second from potassium.

All of this is equivalent to approximately a tenth of a microcurie of activity. The amount of activity, taking into consideration the absorption factors in tissue, will result in our receiving a radiation dose of approximately 20 milliroentgens per year. This constitutes approximately 20 percent of our natural background of 100 milliroentgens per year.

When we measure, using the human counter at Los Alamos and the crystal counter at the Argonne Laboratory, the cesium to potassium ratio in people, we find that this ratio is approximately 0.05, meaning, then, that the amount of radiation that we receive from the cesium in the bodies of people as a result of weapons testing so far, is about one-twentieth of what we receive from the natural potassium background level.

The present concentration, then, of cesium 137 in people is contributing approximately 1 milliroentgen per year. It is of interest, of course, to everyone to consider what about the genetic dose. Since it is pretty well established that we have had to and will continue to live with background, the question then becomes one of whether cesium 137 concentrates in the gonads imposing on the population a genetic dose out of proportion to the 1 milliroentgen per year delivered to the total body?

Analysis of the gonads of animals injected with cesium 137 show there is no concentration above normal in these organs. The principal site of concentration of cesium 137 is the same as that of potassium—the muscle. So we might expect muscular people to have a little more cesium than do fat people. We might expect men to have more than women and variation in cesium 137 over the country is but not more than a factor approximately 2 or 3 at most. The dose that we receive to our gonads as a result of cesium 137 fallout is, therefore, approximately equal to the dose that we receive to the rest of the body. We can say with confidence that cesium 137 is increasing our gonad dose by 1 milliroentgen per year, which is again approximately only 1 percent of the radiation dose our gonads receive from natural background.

This, then, definitely says that it is hard to imagine cesium 137 being either a genetic threat or a somatic threat, unless we can say that continued weapons testing or fallout from the stratosphere is going to increase this level by manyfold. Cesium 137 when it falls upon the soil becomes tightly bound and associated with the soil colloids and cannot be leached off very readily and consequently is not taken up by plants except perhaps to the extent of 1 percent. So at most the concentration in the plants we eat will be only about 1 percent of the concentration in the soil.

Another question, then, might be, is it possible that this material, as is the case with strontium 90, concentrates in the bone or in some

tissue from which it has a very slow biological turnover time. Using human subjects we have measured the turnover time of cesium 137. There measurements show that it concentrates in the muscle and the muscle constitutes a major portion of the body mass. Therefore, it is not concentrating a great amount of radiation in a small volume of tissue.

When we measure the cesium 137 turnover time in people, we find that half of it is disappearing every 110 to every 140 days. Then there is no long-term highly concentrating mechanism in the body. We can assume that if weapons tests continued at the present rate, the cesium 137 level in people, which is in reality a measure of the amount of cesium that is falling out in any given period of time and not the total integrated amount of cesium, will remain essentially constant with a constant weapons test rate.

If we accelerate the test rate, we will increase the level of cesium 137 in people by a proportionate amount to the amount of fission that is involved in the weapons tests.

Remember, we are not talking about megatons of explosive yield in this problem. We are talking about the megaton equivalents of fission products put into the biosphere. We can explode bombs with a greater thermonuclear component (and no greater fission component), thereby increasing greatly the yield of weapons fired, and not increase greatly the amount of fission products added to the biosphere. It is my understanding that Dr. Neuman's number of 2.2 megatons per year is referring to fission yield injected into the biosphere, and nct yield of total explosive force fired.

Mr. RAMEY. When you say "we," you mean the United States. That would not necessarily mean some other country that was just learning how to make bombs?

Dr. LANGHAM. Anybody who is firing atomic weapons is being referred to, or should be, in terms of the amount of fission products that he puts into the biosphere, not the total yield of his weapon.

Mr. RAMEY. I understand that. I meant in relation to getting more explosive yield with less fission through some mechanism.

Dr. LANGHAM. Getting more explosive yield through less fission is in all probability what any country will be doing.

Senator ANDERSON. We did not do it the first year we started our work. Did we not wait a few years and gradually develop into this and therefore do you not think other countries not as advanced may do the same thing we did?

Dr. LANGHAM. We have a pretty good idea that the British and Russians are already doing the same as we did. In other words, the thermonuclear yield or the thermonuclear weapons tested both by the Russians and British are an attempt to get more yield which involves less fission.

Senator ANDERSON. I think we would all agree to that. It is a fusion type to a great degree.

Dr. LANGHAM. It doesn't matter whose weapon it is. The thing that is of critical importance is not the total yield of the weapon from the point of view of the problem we are discussing, but how much fission they had to employ in order to get that yield.

Senator ANDERSON. You did not think Dr. Neuman did not understand that, did you?

Dr. LANGHAM. I do not know whether he did or not. He said megatons of bombs.

Senator ANDERSON. No; he did not. You were not following him very closely. He had exactly the same definition you used.

Dr. LANGHAM. That is fine. What I want to say is that this does not mean—and I want to be sure that the audience understands—that 2.2 megaton yield is the limit to the amount of testing that we can do. It is 2.2 megatons of fission yield injected into the biosphere.

Senator ANDERSON. I though Dr. Neuman made it clear, and I am glad you did also.

Dr. LANGHAM. Then I am sorry. This, then, will bring us to the point where I think we can say that the cesium 137 problem is certainly and will remain secondary to the strontium 90 problem. This makes, of course, the strontium 90 problem considerably more interesting and focusses attention in the cesium 137 work on using cesium 137 to study the mechanisms whereby radioactive fission products are distributed in the biosphere. It is for that reason that studies in this direction certainly should continue. It is very possible that cesium 137, or what we learn by cesium 137 until it hits the ground and begins to enter the ecological cycle, can tell us a great deal of what happens to strontium 90 before it hits the ground.

Cesium 137 is much easier to measure than strontium 90. So we may be able to collect considerably more data on strontium 90 by inference to cesium 137 work.

We have just received a phone call from Dr. Rose in Chicago who asked us to read something into the statement that he entered into the record with regard to the cesium 137 problem. Dr. Marinelli and Dr. Rose have just measured the cesium 137 content in a number of people from South America. Their result indicates that the average found in those people was about 14 micromicrocuries of cesium per gram of potassium, and that compares to the measured value in the United States of 34; 14 versus 34. This signifies, then, that if strontium and cesium do indeed go together, then there is a factor of 2 difference between the fallout in South America and the fallout in the United States. This would indicate again what you have heard voiced many times today, that despite the fact that one might think of great variations over the face of the earth, these variations do seem to be confined to factors of 2 and 3. This is a very important point.

As far as I am concerned, this is what we have to say with regard to the cesium 137 problem. I would like to mention the strontium problem insofar as we see it and perhaps add a little more to what has already been said, with regard to equilibrium levels, and its relation to the present test rate.

First I would like to put down the numbers that have been quoted variously, and some of them here today, for the estimated strontium 90 level in the bones of people at the point of maximum fallout which will be in about 1970 or 1975, and really show you how these values agree.

Dr. Libby in his recent speech has estimated that the level would be from 1.7 to 2.5 micromicrocuries of strontium 90 per gram of calcium at equilibrium, assuming no more weapons tests. This estimate was made on the basis of ecological data, assuming a discrimination factor

of from about 20 to about 30, somewhat in disagreement with the discrimination factor you have heard expressed by Dr. Neuman.

Senator ANDERSON. May I ask, is that a comparable figure to the figure of 8 which he was using?

Dr. LANGHAM. That is right.

Senator ANDERSON. 20 to 34?

Dr. LANGHAM. 20 to 30.

Dr. Kulp in his recent article—and I am talking now about the United States of America and the upper northern section, the section that is supposed to have the most fallout—made an estimate of approximately 2 $\mu\mu c$ per gram of calcium, again based on ecological considerations.

We have made estimates at Los Alamos on the basis of ecological factors also. The average equilibrium level for the United States we come out with as 3.1. We have taken Dr. Kulp's bone data and made a correction that we think is justified. I have not discussed it with him. On that basis we come out with 3.2 for the equilibrium level for the upper United States.

Dr. Eisenbud in his consideration this morning, talking about New York, estimated an equilibrium level of 4.1. We have also derived a value from milk data by merely taking not the average for New York but the average for Chicago, New York, and other milksheds, and we obtain a value of 3.5.

I think it is amazing that we do get agreements this close together when these are derived by different means. The thing perhaps we should be considering is the average for the population belt of the world, because the major portion of the fallout is occurring in that area which has the major number of inhabitants. If we do this, we can estimate for the area between 10° N. and 60° N. latitude, approximately 2.5 micromicrocuries of strontium 90 per gram of bone calcium. These are average values for the area.

Senator ANDERSON. Are all these figures based on no more testing?

Dr. LANGHAM. These are all based on no more testing; yes.

Senator ANDERSON. I don't understand why you start on that assumption.

Dr. LANGHAM. You must start on that assumption and it is the first step in going on to trying to estimate what will happen later. This is the reason.

We can make estimates for the entire world and obtain an average of the world population. Dr. Kulp has made such an estimate. For the entire world he has estimated a value of about 1.3. We have estimated this value by two different methods—by considering ecological factors and by using Dr. Kulp's bone data—and when we do, we come out with a value of 1.7, again amazingly good agreement.

Senator ANDERSON. Doctor, does that not also point up how accurate your observation was a minute ago? You were talking about cesium and the study that had been made in Latin America and you said Latin America it was half of what it was in North America. You said this is also very similar to the strontium figure and the strontium figure would show that, because you have a figure of 2.5 here and something like 1.3 there which is certainly comparable to the figures you had a moment ago.

Dr. LANGHAM. Thank you. I didn't notice that myself.

Senator ANDERSON. Is it accidental or does it seem to work out that way?

Dr. LANGHAM. I think as our data get better and better we are going to find these things beginning to pull together. I can remember when the arguments here were that this may be as high as 50. Now we have it narrowed down so that the disagreement is at most a factor of 2. Remember, I am talking about averages over a specific area, and there is a certain finite probability that any person or that a person or a few persons in any proscribed area will run as much as 2 to 3 times these values and I am now talking about the very factor that Dr. Neuman was introducing, his V–1 factor that he put into his equation.

On this basis, then, we can do a little more with the data as far as what does it mean with regard to present and future tests.

Let me now talk in terms of the population average of 2.5. I am talking now about the area of 10 north to 60 north latitude. Present levels are only about one four-hundredth of the workers permissible value if we want to assume that the maximum permissible level that we will permit the population in this area to reach on an average is 1,000 micromicrocuries per gram of calcium, the occupational tolerance—remember, Dr. Kulp did this some time ago and got criticized for it because it is as inferred that he was recommending we let the population do this, he was not so inferring but was merely trying to tie the values to something that we have accepted and we have accepted that the maximum permissible level for the working or occupied population shall be 1,000 micromicrocuries per gram of calcium. It is pretty well established insofar as the national and international conferences on radiation protection are concerned, that if we are including a large segment of population of nonworking people, then we should lower this by a factor of 10, which puts it to 100 micromicrocuries per gram.

Dr. Neuman has preferred to use 50 here, because of the statement that was made in the National Research Council and National Academy of Science report, in which it was merely said it may be advisable in the case of children to lower this even further.

We can now set up a simple proportionality between strontium 90 equilibrium megatons of fission to date as Dr. Kulp did, in which he took Dr. Libby's data on stratospheric storage and fallout, and he said the present situation we face must be the result of the injection of approximately 50 megatons of fission yield into the biosphere.

If this be true we set up a simple proportionality and say that 2.5 is to 50 megatons as x is to 1,000 or 100 micromicrocuries per gram of calcium, and we can calculate the number of megatons of fission weapons we would have to fire all at one time (and let them fall out at one time) in order to bring the average of the population belt up to these various levels.

When one does this, he comes out with 20,000 megatons of fission products injected into the biosphere would bring the population to an average level of 1,000 micromicrocuries per gram of bone calcium.

Two thousand megatons exploded all at once (and letting it fall out all at once) would bring the average up to 100 micrimicrocuries per gram of calcium.

Now we get into the areas of uncertainty. The things that are most important, far more important than factors of 2 difference in distribution or factors of 2 difference in the equilibrium level in people. For example, we have right here a disagreement of a factor of 10 depending on whether we use the occupational or nonoccupational permissible level. If we want to use Dr. Neuman's level of 50, we have then a disagreement of a factor of 20.

Then our major source of disagreement is on what do we dare let the level in the population reach on the average. Let us do something more with this. Let us say that a factor of 3 is enough to cover the difference in distribution and the discrimination factor. Then we would say we would want to divide this number by 3 (20,000) which would give us roughly 7,000, and in this case (2,000) would give us roughly 700, which would say we could explode 700 megatons before we would take a chance of bringing many people up to the nonoccupational exposure.

Let us assume that the tail on the stable strontium distribution curve that Dr. Kulp has presented, in which he said that it was skewed distribution. Let us take the attitude of the ultraconservative and say because this tail exists and we do not have a normal distribution we had better apply a factor of greater than 3. Some people have done this. They have said the bone data themselves, show a spread of a factor of 10. Let us introduce a factor of 10. That would mean we would have to divide the 7,000 by 10, which would give us 700, and if we divide the 700 by 10 we are now down to 70 megatons. In other words, there is on this basis at the present time a factor of about 200 to 300 disagreement between the ultraconservative and the person who dares be somewhat radical. Where does this variation lie?

A factor of 10 lies in this point where some person might say it does not hurt to let people reach 1,000 micromicrocuries, another says 100 and another says even lower.

There is another factor of 10 in what we assume for the spread in non-homogenity of distribution of this material in people and soil.

Then if we multiply 10 by 10 we have a factor of 100. So 100 out of our 200 or 300 disagreement comes in 2 points, which points out immediately, the important things for us to do.

Senator ANDERSON. Doctor, would you excuse me just a second. I do not follow you for a second there. When you have 25 over 3 you come out with 7,000. When you have 25 over 10, why don't you come out with 2,000?

Dr. LANGHAM. You are right. That is what I should come out with, 2,000 and 200. In other words, 2,000 and 200 instead of 700 and 70.

I think my statement was that the most important things for us to do is to work on what really should be the average maximum permissible level that we dare allow a large segment of the population to reach.

The second thing that we should do is continue the type of work that Dr. Kulp is doing, and others, and study by all means the distribution of strontium 90 in soils, the distribution of strontium 90 in bones, and anything which will give us a cue as to the nonuniformity aspect of the problem.

Do we divide by 10: do we divide by 3; or don't we divide at all?

The Atomic Energy Commission's research program, that of the

Division of Biology and Medicine, is pointed in this direction. What does this mean in terms of future tests?

This, as I said, was a consideration based on no more weapons testing and then firing a number of megatons and making the assumption that they all came down at once and they were all injected into the biosphere at once.

If we continue to test weapons at the present rate, and this is something that I know has worried Senator Anderson a great deal, we cannot establish yet what the increasing rate of weapons testing is. In Castle—and I do not mean weapons testing, I meant to say the rate of injection of fission products into the biosphere. It does not correlate necessarily with megatons of weapons exploded.

In Castle we exploded a lot. In Redwing we exploded quite a number of megatons of yield but less fission products were injected.

So it is hard to say that we do have any kind of an increasing rate in weapons testing which will allow us to say what we will go to if we continue to accelerate at our present rate. But we can say this: If our present situation is the result of 50 megatons injected into the biosphere and this has occurred over 5 years, then we are roughly contributing on an average about 10 megatons per year to the biosphere. If we continue at this rate, there are two numbers that one can use. Only two estimates have been made, one from the British and one by Dr. Libby.

Dr. Libby has stated that if we continue at our present rate, we will reach equilibrium at about 8 times the level that we would be at if we stopped immediately.

For the population belt we said it was 2.5 micromicrocuries per gram of calcium. If we take 8 times that, that gives us roughly 20 micromicrocuries per gram of calcium will be the average level in the people in the population belt if Dr. Libby's factor of 8 is right and if we continue to test by injecting 10 megatons per year into the biosphere.

This is not in disagreement with somebody who said 24 this morning.

Mr. RAMEY. That was Dr. Kulp.

Dr. LANGHAM. If you want to consider the United States, the United States is somewhat higher. In other words, here we would have certainly as much as 3. So 3 times 8 equals 24 which was the figure Dr. Kulp quoted for the United States.

There is one other figure. Let us enlarge this still further. If this is the average and we say we can let a factor of 3 take care of nonhomogenities, then there may be a few people who will be 3 times the average in the United States, or 72 micromicrocuries, which is getting up close to the recommended maximum level of 100.

If we want to be even more conservative and introduce a factor of 10, then we must multiply 24 by 10 which gives us 240 which is a factor of 2 above what has been specified as the maximum permissible level.

Again we must emphasize the necessity of our settling this idea of what is the area of uncertainty. This can only be done by statistical treatment of large numbers of samples.

The British come up with a somewhat more conservative number. On the basis of their soil and air and water measurements, they have said that if the present rate of testing continues, Britain will have 200 millicuries of strontium 90 per kilometer squared, which is roughly

500 millicuries per square mile, which is 900 micromicrocuries per gram of calcium in the soil. If we assume a discrimination factor of 10 which I like better than Dr. Neuman's factor of 8—but I think you will agree the difference is not great—then this would mean according to the British figures the average in the population of Britain would reach 90 micromicrocuries per gram of calcium indefinitely or in about 100 years of testing which is almost our maximum level of 100.

Now if we multiply that by 3 in order to allow for nonhomogenity of distribution, we have a segment of population that could conceivably go as high as 270. Then immediately we get to the most critical point, and that is: What does this mean in terms of risk to the population of the United States and to the population of the world? This is a subject about which I feel very keenly, but I know that the committee has lined up the finest experts in the country on this subject, so I think probably this is a good place for me to stop unless you want to drag it out of me by questions.

Senator ANDERSON. No; I only want to know if I understand this at the end. Is that comparable to the 100 safe figure that was being used?

Dr. LANGHAM. We have 2 bases. We have Dr. Libby's factor of 8, which gives us a level of about 24 micromicrocuries at equilibrium if we continue testing. That is on the average. So we can multiply it by 3, assuming that there is a factor of 3 spread. That would give us 72. This would then mean that there was a finite probability that a few people might go this high when the average is 24. Our maximum permissible level for larger areas of population is 100 micromicrocuries. So we can see if we continue testing weapons at our present rate, or rather injecting fission products into the biosphere at the present rate, then we would level off at about one-fourth of the maximum permissible level on the average.

There is a possibility that some people would be almost there.

If we take the British values based on soil deposition in Britain and use their factor—instead of saying 8, they really say we will reach equilibrium between 12 and 14 times, and there is a disagreement between the British and Dr. Libby—then what corresponds to the 24 in the United States goes up to 90 in Britain. What corresponds to the 72 in the United States goes up to 270 in Britain.

Senator ANDERSON. Is that 270 to be compared with the 100 which is the safe level?

Dr. LANGHAM. Yes. This is to say that the average on the basis of the value of 90 would be below the safe level if you want to call 100 safe.

Senator ANDERSON. But those people who are above the average would be right at the level.

Dr. LANGHAM. And these people with 270 would be a factor of almost 2 above this level.

Representative HOLIFIELD. Almost 3.

Dr. LANGHAM. Yes.

Senator ANDERSON. How many years would it take to achieve that?

Dr. LANGHAM. According to the British it would be reached in 100 years and according to Dr. Libby in about 50. Which of these levels you may select depends entirely on the crucial biological point, which is, is it leukemic and bone sarcoma response to radiation dose linear with dose, or is it a threshold? If it is a threshold, we have to

look at this (100 micromicrocuries) as the maximum permissible level. If it is a nonthreshold response we may look at this as an average level and try to decide what the risk is averaged over the entire population or averaged over any segment of the population.

What a nonthreshold response essentially says is that for every increment increase in dose there is an equal increment increase in effect and theoretically there is no maximum permissible level. There is an extremely small probability that any amount of radiation, the amount we wear on our wristwatches or the amount that we get from our natural potassium is going to harm somebody.

So the whole point of which of these numbers we can accept will depend upon our making a value judgment how much is atomic energy worth in cases of leukemia and bone cancer on a probability basis, averaged over the entire population or a certain segment thereof.

Senator ANDERSON. Thank you very much. I can say from personal acquaintance I know how long and hard you have worked in this field and I am very grateful to you for your testimony.

The next witness is Dr. Anderson.

Dr. ANDERSON. Mr. Chairman, I have nothing to add to the formal statement Dr. Langham made. I was in attendance only to answer questions.

Senator ANDERSON. Before we proceed with a discussion period with our several witnesses, there are several things that I would like to insert in the record at this point. First a statement by Wright H. Langham and Ernest C. Anderson. Next an article from Science Magazine, by Ernest C. Anderson, Robert L. Schuch, William R. Fisher, and Wright Langham, and finally a statement by L. D. Marinelli and J. E. Rose of the Argonne National Laboratory.

(The material referred to follows:)

SR-90 AND CS-137 IN RELATION TO THE PROBLEM OF WORLDWIDE RADIOACTIVE FALLOUT

By Wright H. Langham and Ernest C. Anderson, Los Alamos Scientific Laboratory, University of California, Los Alamos, N. Mex.

Although a number of isotopes are present in the fission mixture, the fallout of Sr-90 from weapons testing programs is the principal concern. Sr-90 is the most important isotope because of its similarity to calcium, long physical and biological half-time and high relative fission yield. These factors lead to high incorporation in the biosphere and a long residence time in bone. General contamination will result in the bones eventually reaching an equilibrium state with the Sr-90 in the biosphere.

Accepting Libby's postulation of three types of fallout (local, tropospheric, and stratospheric), levels as of the fall of 1956 were about 25 mc./mi.^{2} for the upper midwestern and northeastern sections of the United States, 16 mc./mi.^{2} for the section between 50° N. and 10° S. latitude, and about 4 mc./mi.^{2} for the rest of the world. These general values are variable, depending upon local rainfall and other meteorological patterns.

The observed levels of Sr-90 in bones of various ages are in good agreement with those calculated on the basis of a simple model of skeletal growth, remodeling and exchange. Using the data of Kulp for adults and children normalized to this model, an average equilibrium value of 3 μc. Sr-90/g. Ca is calculated for about 1975. Estimation of the equilibrium value from ecological discrimination factors suggests approximately the same average level. The normal spread of values for stable strontium and Sr-90 in human bones and for Cs-137 in people suggests that there is a very low probability that many people will show levels more than three times the average. On the basis of an equilibrium concentration of 3 μc. Sr-90/g. Ca resulting from detonations to date, about 18,000 megatons of fission could be injected at once into the biosphere before the average value would equal the maximum permissible level of 1,000 μc./g. Ca (the MPL for

industrial workers), and 1,800 megatons could be injected before reaching an average of 100 μc./g. Ca (the MPL for large areas of the population).

The above approach to the problem suggests (assuming no more weapons tests) that the average equilibrium level from weapons already tested may be about 3 percent of the MPL for the general population with a spread of from 1 to 9 percent. In terms of lifetime bone dose, these values are from 1/400 to 1/2,800 of the minimum dose from Ra 226, which has produced nonpathological bone changes. The biological significance of present and future predicted levels and whether average values may be applied to the general population depends on whether such chronic responses as bone sarcoma, leukemia, etc., to Sr–90 deposition are threshold or nonthreshold phenomena.

Estimates as to the number of megatons of fission that may be injected into the biosphere before Sr–90 becomes a serious health hazard to the general population vary by a factor of about 200. It is this variation in opinion that is responsible for much of the public confusion. Two factors that contribute a major portion of this wide uncertainty are:

1. The heterogeneity as to distribution of Sr–90 uptake in the skeleton as a function of diet and geographic location; and
2. Lack of information as to actual leukemogenic and tumorogenic response of man as a function of radiation dose.

Increased research effort to narrow the uncertainties in these two factors would seem to be desirable.

Measurements of present levels of Cs–137 in people indicate that it is of little significance in the potential hazard of radioactive fallout from weapons testing programs. Because of the chemical similarity of cesium and potassium, it is convenient to report cesium levels as Cs/K ratios. Potassium is an essential body constitutent and is itself naturally radioactive. The normal body potassium contribute 20 mr/year of the total natural yearly radiation dose of 100 mr. The present average Cs–137/K–40 total disintegration ratio is about 0.05. Taking into consideration their respective energies, the radiation dose from present levels of Cs–137 is only one-twentieth of that from natural K–40, or about 1 mr/year. This is about 1 percent of the average total natural radiation dose. The amount of Cs–137 now present in the population of the United States averages 0.006 μc., which is less than one-thousandth of the value given in the Recommendations of the International Commission for Radiological Protection as the maximum permissible level for the general population.

The short biological half-time of Cs–137 and its unavailability from soils will ensure that the levels in people will not continue to rise in the manner of Sr–90. The cesium levels will follow the rate of fallout and not integrated total accumulation.

Since Cs–137 does not show unusual concentration in the gonads, present levels in people will contribute only about 1 mr./year, or about 1 percent of the natural background level, to the genetic radiation dose.

The study of the distribution of Cs–137 should be continued to furnish information on fallout phenomena and to provide a measure of the rate of fallout and the amount of stratospheric storage, since this information might make considerable contribution to the solution of the Sr–90 problem.

[Reprinted from Science magazine, June 28, 1957]

Radioactivity of People and Foods

Ernest C. Anderson, Robert L. Schuch, William R. Fisher, Wright Langham[1]

The problems of widespread, low-level radioactive contamination from nuclear weapons testing have been increasingly before the public during the past year. The principal concern is the fallout and entry into the biosphere of strontium 90. There is general agreement that present levels of strontium 90 in foodstuffs and in the human body are far below the most conservative permissible amounts; however, the human burden of strontium 90 may be expected to rise as a result of deposition of stratospheric debris from weapons already (and subsequently to be) tested. Predictions based on conservative assumptions indicate that there remains a considerable margin of safety. If the rate

[1] Dr. Anderson, Mr. Schuch, and Dr. Langham are members of the biomedical research group at Los Alamos Scientific Laboratory, University of California, Los Alamos, N. Mex. Mr. Fisher, a former member of the group, is now at the University of Colorado School of Medicine, Denver.

of weapons testing continues to increase, however, this margin may eventually disappear.

Although the permissible levels contain inherent safety factors, it is essential that close attention be devoted to all aspects of the fallout problem during the next several years. Only in this way can advance notice of the possible approach to permissible levels be obtained and assurance given that they will not be exceeded inadvertently. Recent reports of the National Academy of Sciences-National Research Council Committee on the Biological Effects of Atomic Radiation (1) support the importance of systematic measurements of general levels of radioactivity in order that information on the rate of accumulation of extraneous radioactivities may be obtained while the latter are still below natural levels.

Large-scale production of nuclear power will create problems of a similar nature. A 100-megawatt (heat) reactor will, in one year of operation, produce the same quantity of long-lived fission products as the detonation of a 1-megaton fission bomb. The estimate of the United States nuclear power production rate by 1975 is 20,000 to 40,000 megawatts, and the United Kingdom expects to be producing 6,000 megawatts by 1965. Reactor-produced fission products constitute a much less immediate problem than those from a bomb test, since more control can be exercised over their immediate fate, but disposal of the fission products must eventually be made.

If disposal is to be simple enough to make nuclear power economically competitive, dispersal by natural means such as ocean burial or other means may have to be resorted to. This will increase the possibility that reactor-produced fission products may ultimately enter the food cycle and reach man. The basic problems of permissible body burdens and distribution mechanisms in the biosphere, therefore, are similar for bomb and reactor debris, and information gathered in the study of the former problems should prove valuable in the latter.

An extensive survey of strontium 90 levels (Project Sunshine) has been underway for several years, and the results have been reported by Libby (2–4) and by Kulp (5). Because strontium 90 and is daughter yttrium 90 emit only beta rays, analysis requires time-consuming and destructive chemical separations. Detailed studies of the temporal and spatial distribution of long-range fallout would be easier if they could be based on a gamma-emitting nuclide. The discovery of the presence of the fission product cesium 137 in human beings and in foodstuffs by Miller and Marinelli (6) provides a possibility of such an approach.

Similarity of the decay chains of the fission products of mass 90 and mass 137 indicates that distribution of cesium 137 and strontium 90 in bomb debris will be similar:

$$\text{Kr–90} \xrightarrow{\text{33 sec}} \text{Rb–90} \xrightarrow{\text{2.7 min}}$$

$$\text{Sr–90} \xrightarrow{\text{28 yr}} \text{Y–90} \xrightarrow{\text{64 hr}} \text{Zr–90 (stable)}$$

$$\text{I–137} \xrightarrow{\text{19 sec}} \text{Xe–137} \xrightarrow{\text{3.4 min}} \text{Cs–137} \xrightarrow{\text{27 yr}}$$

$$\text{Ba–137m} \xrightarrow{\text{2.6 min}} \text{Ba–137 (stable)}$$

Both nuclides have two gaseous or volatile predecessors with appreciable half-lives. Strontium 90 and cesium 137 are formed at relatively late times after bomb detonation and are not proportionally included in the larger and more refractory particles which fall out locally. Stratospheric storage and distant deposition will be high for both nuclides, and their ratio in distant fallout should be approximately that calculated from the known fission yields. Once strontium 90 enters the biosphere, its behavior becomes very complex. Its concentrations along the ecologic chain change slowly and reflect a summation of all past fallout. In addition, it enters plants both through the soil (in some relationship with available calcium) and by foliate absorption from direct fallout.

One very important and difficult problem is to determine the fraction of strontium 90 entering the ecologic chain by way of these routes. Cecium 137, however, is apparently poorly taken up from the soil by plants (7) and its biological half-times (8) are comparatively short (140 days in man (9) and 20 days in the cow (10)). These factors suggest that cesium in people and in milk and other foodstuffs may be a direct and relatively simple measure of fallout rate. One should be able, therefore, to make a direct determination of fallout rate as a

function of geographic location and time, as well as of changes in stratospheric storage following test operations, by measuring cesium 137 in biological materials. Cesium 137 measurements on soils might provide a more convenient method than strontium 90 measurements for estimating integrated fallout.

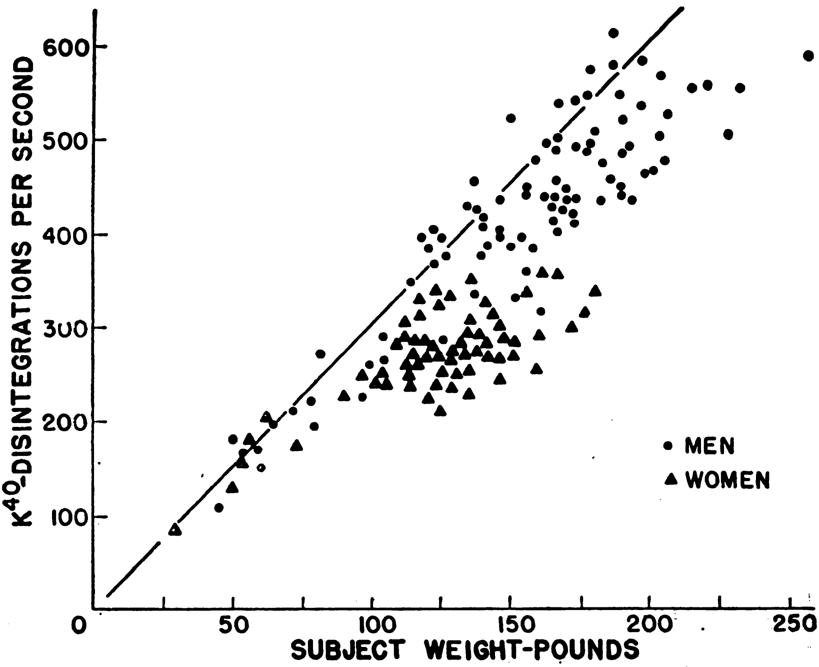


FIGURE 1.—Potassium 40 gamma activity in people as a function of gross body weight.

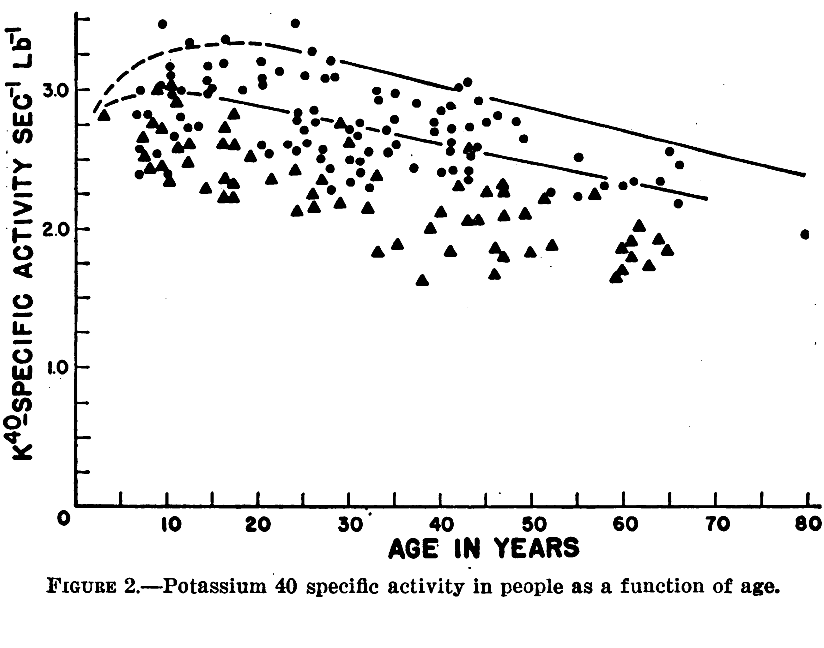


FIGURE 2.—Potassium 40 specific activity in people as a function of age.

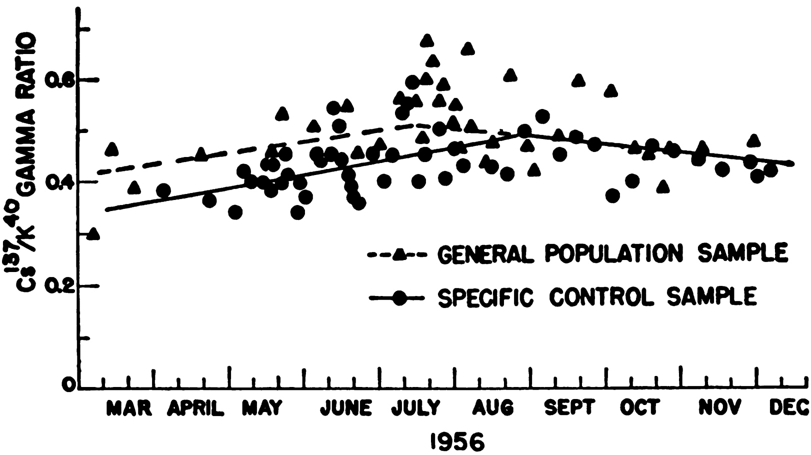


FIGURE 3.—Cesium 137/potassium 40 gamma ratio in people during 1956.

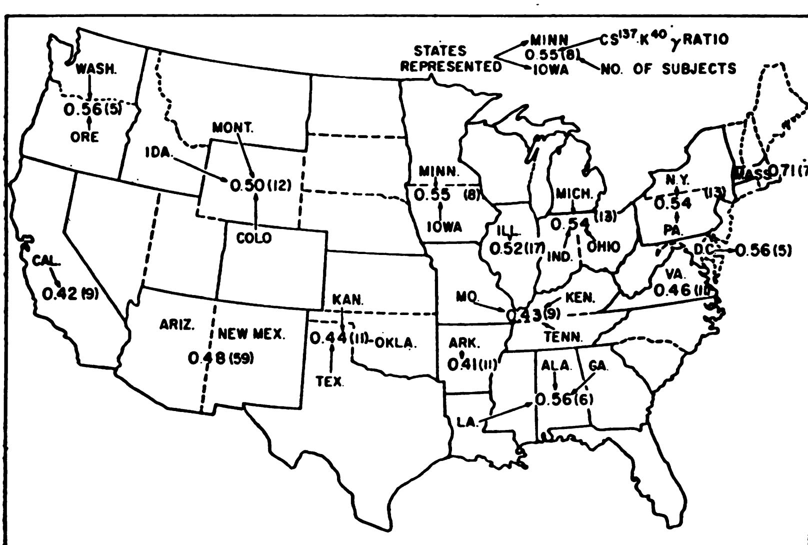


FIGURE 4.—Geographic distribution of cesium/potassium ratios in people.

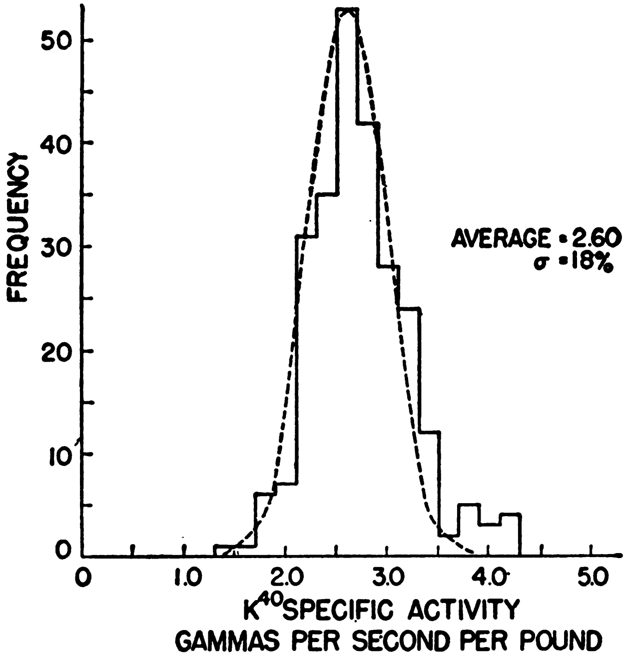


FIGURE 5.—Frequency distribution of potassium 40 specific activity.

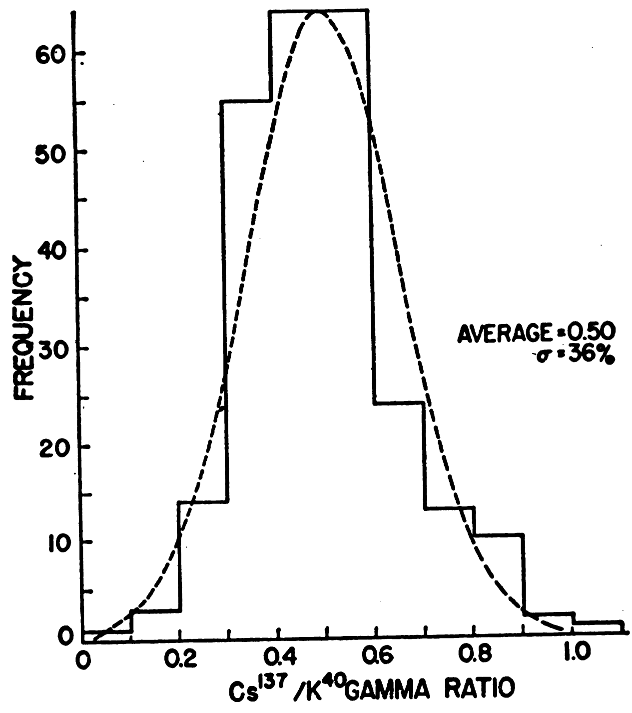


FIGURE 6.—Frequency distribution of cesium/potassium ratio.

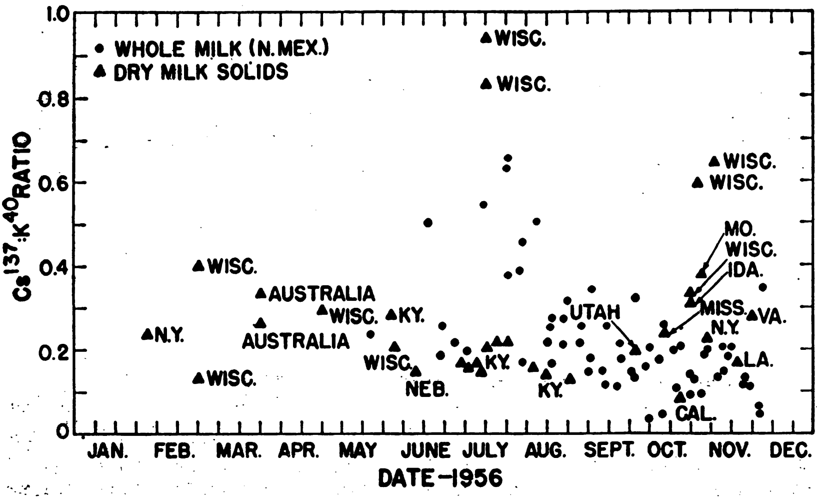
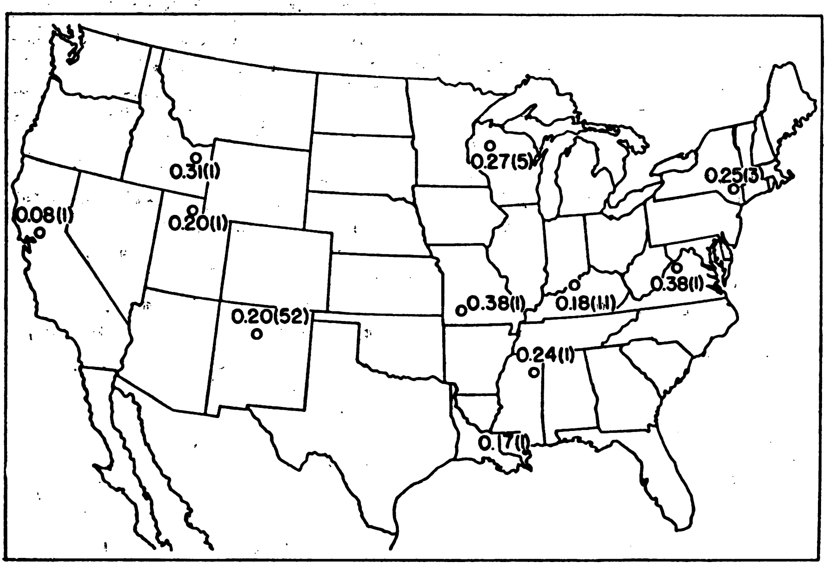


FIGURE 7.—Cesium 137/potassium 40 gamma ratio in milk during 1956.



Cs137:K40 GAMMA RATIO IN MILK
SPRING AND FALL, 1956
UNITED STATES

FIGURE 8.—Geographic distribution of cesium/potassium in milk.

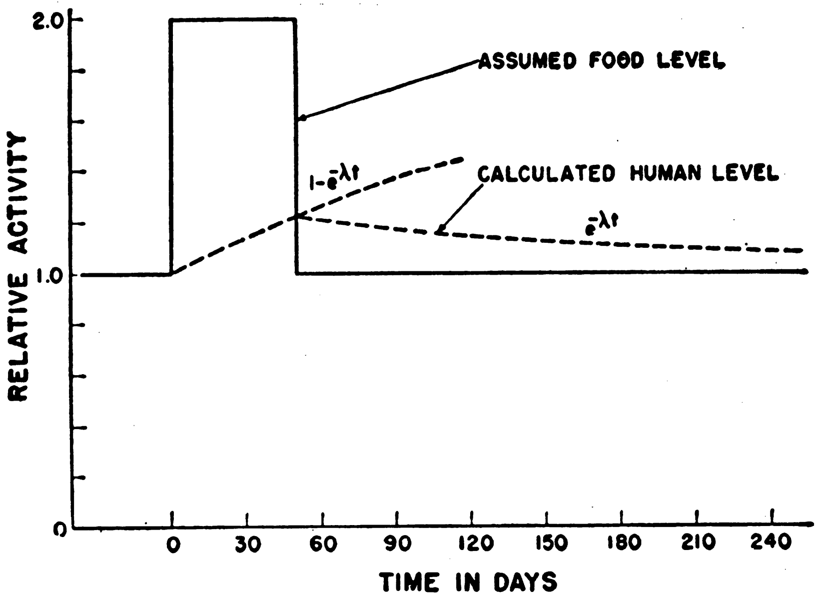


FIGURE 9.—Calculated effect of increased cesium in diet on human level.

Cesium 137 and strontium 90 also are similar in that they are soluble and closely related to potassium and calcium, respectively, which are normal base exchange cations in soil and essential constituents of living matter. In this they differ from other high-yield fission products such as zirconium-niobium 95, ruthenium-rhodium 106, and cerium 144, which have been observed in rug dirt by Miller and Marinelli at Argonne National Laboratory (11) but which are apparently not ecologically concentrated and have not been detected in the general population and in foodstuffs.

POTASSIUM 40 AND CESIUM 137 IN PEOPLE AND FOODSTUFFS

After the announcement by Miller and Marinelli of the presence of cesium 137 in people (6), an intensive program of study of this nuclide in people and in foodstuffs was begun at the Los Alamos Scientific Laboratory. Some 1,500 measurements were made; preliminary results have been reported previously (12). This article (13) summarizes the data collected during 1956. A compilation of all the primary data is being prepared as an unclassified laboratory report which will include detailed analyses of procedures, sources of error, and other information.

Measurements were made with the Los Alamos "human counter" (14), a large liquid scintillation detector that is capable of counting gamma rays from human subjects and from samples of foodstuffs up to several hundred pounds in weight with 100 percent geometrical efficiency. Although the energy resolution of this detector is quite limited compared with that of a sodium iodide (thallium) crystal, it is adequate to permit the simultaneous determination of the cesium 137 (0.661 Mev.) and potassium 40 (1.46 Mev.) gamma rays. Its ultimate sensitivity is 0.0005 microcurie of gramma activity (20 disintegrations per second) for only 100 seconds of counting time. If a 100-kilogram sample is counted, this corresponds to a specific activity of 5 times 10^{-15} curies per gram, which is far below the natural radioactivity of most materials. The natural potassium 40 radioactivity of man (about 0.013 microcurie as gamma rays) can be measured to a precision of better than 5 percent in less than 2 minutes. The cesium 137 determination has a precision of 0.001 microcurie for the same counting time.

Potassium 40 in people.—The average potassium content of the adult male is estimated to be about 133 grams (6, 15, 16) (0.19 percent of gross body

weight of the standard man), which is equivalent to about 400 potassium 40 gamma disintegrations per second.

Figure 1 gives the natural potassium 40 gamma activity of 164 representative subjects, 81 of which were reported earlier (17), plotted against gross body weight. These data show a pronounced scatter of the points to the right of a limiting line and a definite difference between males and females. Correlation of potassium 40 activity with the fat-free body weight of a select group of these subjects indicated the amount of fat to be the principal factor causing variation in apparent potassium content of the body (17). The total body potassium expressed as percentage of gross body weight will show considerable variation, therefore, depending on sex, age, weight, body type and physical activity, but it can be accurately calculated from a determination of total body water.

In figure 2 the specific activity of potassium 40 (gamma disintegrations per second and pound) is plotted against subject age. These data confirm the general decrease of potassium with age reported by Sievert (16). The solid lines indicate the probable upper limits for uncontaminated male and female subjects, respectively. Not enough children have been measured for us to be certain of the trend below age 15. The dashed lines, therefore, are estimates over this region. Deviation from these curves is an indication of possible surface contamination of individuals during periods of local fallout, since only 0.002 microcurie of hard gamma contamination is sufficient to raise the average adult from the lower to the upper limit of the specific activity distribution.

Cesium 137 in people.—Libby (2) adopted the procedure of reporting strontium 90 results as strontium 90/calcium ratios because of the metabolic similarity of strontium and calcium and to facilitate the comparison of different types of materials. Our cesium 137 results are reported as cesium 137/potassium 40 ratios for similar reasons. The principal differences in the biological behavior of the two elements can be accounted for in terms of the appropriate biological half-times. The ratios are reported as the ratio of cesium 137/potassium 40 gamma disintegrations (18).

Figure 3 summarizes the measurements of cesium 137/potassium 40 ratios in people for 1956. The triangles represent results in people from various parts of the United States (the distribution is indicated on the map, fig. 4). Each point is an average for 10 to 20 persons, and the range of values before averaging was 0.1 to 0.9. The circles are averages of measurements on a local control group of 10 laboratory personnel. The scattered high values during the period from June to September are probably the result of surface contamination from tropospheric fallout during Operation Redwing. That they were caused by surface contamination was indicated by their sudden rise and fall, by abnormally high apparent potassium 40 values during the same period, and by the fact that these high apparent potassium 40 values were reduced to normal after bathing in those cases in which remeasurement was possible.

Because of this evidence of external contamination, a line through the more reproducible lower limit of the distribution is regarded as representing the trend of internal activity. The data from the two groups agree in that they indicate a slight rise during the spring followed by a slow decline during the fall. The control group was apparently somewhat lower in the spring, but in the fall the two groups were indistinguishable.

General 1956 averages of cesium 137/potassium 40 ratios for people from various States are presented in figure 4. The results are surprisingly uniform in view of the sizable variations among individuals from the same State. Uncertainty in the averages due to small sample size precludes any deduction of fine structure until more data are available. Within the range 0.5 ± 0.2, the cesium 137/potassium 40 ratio is essentially uniform over the United States, except during periods of tropospheric fallout.

The frequency distribution of potassium 40 and cesium 137 in the population sample is essentially normal. The potassium 40 frequency curve is given in figure 5 as a histogram with a normal error curve fitted to it. The standard deviation of the normal curve is 18 percent. The subsidiary peak outside the normal curve is caused by surface contamination during periods of tropospheric fallout.

Figure 6 shows the corresponding frequency curve of the cesium 137 data for the same population sample. Distribution is again normal, but the width is twice as great as that of potassium 40, the standard deviation being 36 percent. The smaller deviation of the potassium 40 data probably reflects control of the potassium 40 level of the body by a homeostatic mechanism that is not highly de-

pendent on intake. The cesium 137 burden, however, may vary with the dietary habits of the subject and the concentration of cesium 137 in his foodstuffs.

Libby (19) has shown that other trace elements, such as stable strontium, strontium 90 and radium 226, show normal frequency distribution curves with deviations comparable to that observed for cesium 137.

The abnormal subsidiary peak shown in the potassium 40 distribution curve is not present in the cesium data. This indicates merely that the surface contamination distorting the potassium 40 level was present in the cesium 137 channel to a proportional extent and left the cesium/potassium ratio unaffected.

Cesium 137 in milk and other foodstuffs.—Figure 7 summarizes the measurements of cesium 137/potassium 40 ratios in milk during 1956. A peak in cesium 137 activity during July, presumably woing to tropospheric fallout from Operation Redwing, is clearly visible in Wisconsin and New Mexico samples, but is absent from Kentucky milk. This observation is consistent with the path of the cloud as estimated in the United States Public Health Service air sampling network. A peak in the activity in Wisconsin milk in October is indicated also; it may be the result of a foreign test.

Data on geographic distribution of the cesium 137/potassium 40 ratio in milk are as yet scanty, but are summarized in figure 8. As with the measurements of people, one concludes that distribution is essentially uniform within the limits of the data. The uniformity, of course, applies only to the periods in which tropospheric clouds are not present. It is interesting that the two Australian milk samples (fig. 7) are in agreement with the general United States average, lending support to the assumption that the general levels are derived from the stratospheric reservoir. A sample of American dry milk produced in 1942 showed no detectable cesium 137, the cesium 137/potassium 40 ratio being less than 0.02.

Some preliminary measurements of cesium 137 in foodstuffs other than milk are given in table 1. During the spring of 1956, beef and lamb showed a ratio comparable to that of people but considerably higher than similar samples collected in the winter of 1956–57. During both periods, beef and lamb consistently ran higher than pork, which might be expected from the differences in grazing and feeding habits. One sample of dried blood collected in April 1952 showed a ratio less than one-third that of samples collected during the winter of 1956–57.

DISCUSSION

Measurements of present levels of cesium 137 in people indicate that it is of little significance in the potential hazard of radioactive fallout from weapons testing programs. The present average cesium 137/potassium 40 total distintegration ratio is about 0.05. Taking into consideration their respective energies, the radiation dose from present levels of cesium 137 is only one-twentieth of that from natural potassium 40, or about 1 milliroentgen per year. This is about 1 percent of the average total natural radiation dose and less than 10^{-3} of the dose of cesium 137 given in the recommendations of the International Commission for Radiological Protection as the maximum permissible level for the general population (20). Interest in cesium 137, therefore, centers on its potential usefulness in the study of fallout mechanisms.

A rough quantitative comparison of the present average strontium 90 and cesium 137 levels in people is of interest. According to Libby (3), the strontium 90 level in children is about 0.001 microcurie. A fractionation factor of about 10 against strontium between primary fallout and human bone is indicated by the stable strontium data (21) (that is to say, the strontium/calcium ratio in soil is 10 times the strontium/calcium ratio in bone), but cesium can be assumed to be quantitatively absorbed by both cow and man. Although strontium will continue to accumulate because of its long biological half-time, the effective accumulation time for cesium will be limited to some 200 days. If stratospheric fallout is assumed to have begun with Operation Castle (1954), strontium 90 has been accumulating for some 2 years, and this factor will cause it to exceed cesium 137 by $2\times365/200$, or 3.6. Finally, the relative activity yield in the fission process is 1.27 in favor of cesium (assuming fission yields of 0.0510 and 0.0620 for the mass 90 and mass 137 chains and half lives of 27.7 years for strontium 90 and 26.6 years for cesium 137). The overall factor is then $10\times1.27/3.6$, or about 3 for cesium 137, and the estimated level based on a strontium level of 0.001 microcurie is 0.003 microcurie. Considering the crudity of the several approximations, this is in surprisingly good agreement with the observed average of 0.005 microcurie.

TABLE 1.—*Radioactivity in foodstuffs*

Sample	K-40 specific activity (disintegration/sec. lb.)	Cs-137/K-40 ratio	Sample	K-40 specific activity (disintegration/sec. lb.)	Cs-137/K-40 ratio
Meat, spring 1956:			Flour, spring 1956:		
Beef rounds	3.84	0.53	High-altitude wheat (Colorado)	1.30	0.09
Lamb, dressed carcass	3.83	.81	Bleached, enriched (A)	1.70	.32
Pork, fresh hams	3.52	.30	Bleached, enriched (B)	1.49	.27
Pork, loins	3.23	.19	Whole wheat, graham	7.00	.11
Meat, winter 1956–57:			Potatoes, spring 1956:		
Beef, sirloins	2.75	.15	Colorado	7.82	<.06
Lamb, dressed carcass	3.67	.16	Idaho	6.52	<.06
Pork, loins	3.55	.10	Vegetables, spring 1956:		
Pork, loins	3.26	.07	Lettuce	2.34	<.07
Dried blood:			Cabbage	3.20	.12
Illinois, Apr. 1952	9.20	<.07	Carrots	6.82	<.03
California, winter 1956–57	7.30	.25	Fruits, spring 1956:		
Minnesota, winter 1956–57	5.40	.25	Tomatoes	3.81	.03
Texas, winter 1956–57	5.90	.18	Oranges	2.10	.38
			Grapefruit	3.30	.25
			Watermelon	3.75	<.03
			Coffee, spring 1956	30.00	<.06

TABLE 2.—*Calculated cesium 137 intake based on per capita food consumption. Diet was based on Consumption of Food in the United States, supplement for 1954 (22)*

Source	Consumption (lb./mo.)	Cs-137 concentration (mμc./100 lb.)	Cs-137 intake (mμc./mo.)
Dairy products (as dry-milk solids)	5.8	14	0.81
Meats	11.4	3.3	.38
Flour and cereal products	13.0	1.0	.13
Vegetables	16.8	(1)	?
Citrus fruits	3.2	2.4	.21
Potatoes	8.8	(1)	?
Total			1.5

1 Not detected.

Measurements of cesium 137/potassium 40 ratios in milk during 1956 (fig. 7) indicated peak activities resulting from periods of tropospheric fallout. The relative effect of such increases in foodstuffs on the cesium 137 level in people can be estimated from the simple model shown in fig. 9. A step function change in the foodstuff level will be followed by a $(1-e^{-\lambda t})$ change in the population level (where λ is the biological elimination rate), and a new equilibrium value will be reached only after an elapsed time of the order of 1 year. If the foodstuffs return to their previous value before equilibrium is attained, the population level will cease rising and will decay back to its previous value with a halftime corresponding to the biological elimination rate.

This model can be applied to the situation during July and August, when the level of cesium 137 in milk rose by about a factor of 3. Since not enough data are available to define completely the shape of the peak, and since milk values are used as representative of all foodstuffs, the actual peak can be replaced with a step function of the same approximate area. This gives a rise of about 2 times "normal" for a period of 50 days. In this case, the maximum rise in the population level, predicted on the basis of the model in figure 9, is 20 percent. Using the average value of the cesium 137/potassium 40 ratio for the control subjects in the spring of 1956 (fig. 3) of 0.4, their calculated ratio 6 months later is 0.5. The observed summer maximum average was 0.48, in agreement with the model.

An estimate of the biological halftime of cesium 137 in the chronically exposed case was obtained by counting a large urine sample representing 52 man-days of excretion. The sample showed 408 disintegrations per second of potassium 40

(136 grams of potassium) and 40 disintegrations per second of cesium 137. Assuming an average body burden of 0.005 microcurie of cesium 137 for the 6 subjects who contributed urine samples, the excretion rate is 0.004 per day, which corresponds to a halftime of some 180 days if the excretion is exponential and entirely urinary. If fecal excretion is 25 percent of urinary, the halftime would be 145 days. This is in agreement with the biological halftime of 140 days observed on volunteers who ingested 1 microcurie of radiocesium (9).

Using Bureau of Agriculture statistics for food consumption per capita in the United States (22) and our preliminary values for the average cesium 137 content of foodstuffs, the dietary intake of cesium 137 can be estimated (table 2). On the basis of these data, it appears that milk contributes about 50 percent and meat about 25 percent of the cesium 137 found in the body. The excretion rate of cesium 137 can also be estimated from these intake data. This method is only an approximation because of uncertainties in diet and in the average cesium 137 level in the various dietary components. According to the data in table 2, the turnover rate is of the order of 1.5 millimicrocuries per month, compared with the observed value of 0.6 millimicrocurie. Part of the discrepancy may result from using retail weights in computing the diet with no allowance for wastage and loss of minerals in cooking, but the principal source of error is probably the inadequacy of our knowledge about cesium in foodstuffs. For comparison, a similar computation was made for potassium (table 3). The calculated potassium intake is about 3 grams per day, while the observed urinary excretion was 2.6 grams per day. Elkinton and Danowski (23) reported potassium turnover as falling in the range of 2 to 6 grams per day.

TABLE 3.—*Calculated potassium intake based on per capita food consumption*

Source	Consumption (lb./mo.)	Potassium	
		Content (g./lb.)	Intake (g./mo.)
Dairy products	5.8	6.0	35
Meats	11.4	1.2	14
Flour and cereal	13.0	.5	6
Vegetables	16.8	1.0	17
Citrus fruits	3.2	1.0	3
Potatoes	8.8	2.0	18
Total			93

While the spring 1956 average value for the cesium 137/potassium 40 ratio in milk was 0.25, the average in people for the corresponding period was 0.4. This difference may be explained on the basis of the longer holdup time of cesium in the body as compared with potassium. If q_{cesium} is the amount of cesium in the average daily diet, and $q_{potassium}$ is the corresponding amount of potassium, then $q_{cesium}/q_{potassium}$ is the cesium/potassium ratio for the average diet. The milk ratio can be used since it is the most important single factor and is the only one known with any accuracy. The equilibrium amounts of cesium and potassium in the body, on the basis of the simplest model, will be given by the product of $q\tau$ for each element, where τ is the mean life of the element in the body (24). For cesium, τ has been determined to be 200 days; τ for potassium can be estimated from our data on the potassium content of normal urine as about 58 days. Thereforce, cesium should be concentrated relative to potassium by a factor of 200/58, or 3.4. If the average diet ratio is 0.23, the predicted ratio in people is about 0.8. This is too high by a factor of 2.

Libby (4) has estimated stratospheric injection by Operation Redwing at about 6 megatons of fission products in addition to the 18 megatons left from the previous operations. This would imply a 30-percent increase in the fallout rate from the stratospheric (worldwide) component after the tropospheric component is gone. A comparison of the spring and autumn milk averages indicates no detectable increase in the fallout rate. The spring sampling was inadequate; hence there is considerable uncertainty about the proper average. However, it would appear that, if anything, the cesium levels in the fall were lower. This may be a seasonal variation resulting from the change from pasture to hay feeding of the dairy herds, which would conceal possible small increases.

SUMMARY

Measurements of the cesium 137 content of people and of foodstuffs indicate that this nuclide is unlikely to be a decisive factor in the long-term hazards from weapons testing and reactor waste disposal. The amount of cesium 137 now present in the population of the United States averages 0.006 microcurie and shows no marked dependence on geographic location. The average radiation dose received from cesium 137 is one-twentieth of that received from natural radiopotassium and 1 percent of the average total dose from all natural sources. Because of the short biological half-life of cesium of about 140 days, it does not accumulate in the body as does strontium 90. The study of the distribution of cesium 137 is being continued to furnish information on the mechanisms of the fallout process and provide a measure of the rate of fallout and of stratospheric storage.

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13. This work was performed under the auspices of the U. S. Atomic Energy Commission. We are grateful to a number of persons who assisted in various phases of this program. We are particularly indebted to L. D. Marinelli and C. E. Miller of Argonne National Laboratory for helpful discussion and for their generosity in making measurements of interest to us on their crystal counter. Their assistance aided materially in the initial development of this program. J. W. Ballow supplied most of the foodstuff samples, and R. J. Remaley of the American Dry Milk Institute has been very helpful in the procurement of samples of dry milk. H. E. Gilbert of the Los Alamos Scientific Laboratory set up the punched card system used for electronic data processing.
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24. $\tau = 1/\lambda = t\frac{1}{2}/0.693$, where τ is the mean or average time the nuclide remains in the body, λ is the elimination rate, and $t\frac{1}{2}$ is the time necessary to remove half the body burden.

STATEMENT SUBMITTED TO THE JOINT COMMITTEE ON ATOMIC ENERGY BY L. D. MARINELLI [1] AND J. E. ROSE,[2] RADIOLOGICAL PHYSICS DIVISION, ARGONNE NATIONAL LABORATORY, LEMONT, ILL.

TOPIC IX. OCCURRENCE OF CS-137 IN THE ATMOSPHERE, BIOSPHERE, AND ITS UPTAKE AND BEHAVIOR IN MAN

The fission product Cs-137 is produced with a yield of about 6 percent and it has a half life of about 27 years. The general characteristics of its distribution and behavior in mammals, as reported by several authors (1-4), indicates only a partial qualitative similarity to potassium. Important from our standpoint is the fact that cesium is excreted by humans at a rate lower than potassium. This leads to a Cs/K ratio in vivo which is from 2 to 3 times the ratio in the ingested food.

Because of its gamma-ray emission, Cs-137 can be measured in the living animal and in bulk material without recourse to lengthy chemical analysis.

To make these measurements, it is necessary to shield both instrument and subject from the radiation emitted by ordinary building materials. This is done by performing the tests in an 8 by 8 by 6 foot room with 8-inch steel walls, weighing 60 tons. This room consists of a bolted frame of angle beams upon which one-quarter inch plates of 12 to 26 inches width are placed in staggered sequence on all sides in order to avoid continuous cracks in the walls. The side plates are held in place by clamping them together between the frame and appropriately placed angle irons.

Gamma-ray radiation emitted by the subject impinges on an 8 inch by 4 inch NaI crystal; the electrons liberated therein produce scintillations which are amplified by a photomultiplier tube and registered, according to their sizes, by a 256-channel analyzer. From the scintillation spectrum it is possible to identify the energy of the gamma radiation (hence the radioelement responsible for it) an dits intensity (hence the amount of material involved). Presently this apparatus has a sensitivity greater than 10^{-9} curies of the gamma emitters under discussion in the intact human subject.

In the summer of 1955, at the Argonne National Laboratory, measurements of the total body gamma-ray activity of members of our staff, visitors from various parts of the country and from overseas, local medical students, etc. (5), disclosed the presence of this radioelement in all of the test subjects. Since then, continual tests on a group of 12 people, has shown an increase in the human burden by a factor of about 2 up to the spring of 1956, and a constant value thereafter, corresponding to about 3.2×10^{-11}C of Cs-137 per gram of potassium (fig. 1). Contrasted to the findings for Sr-90, children do not exhibit high concentration per unit weight.

No correlation between Cs-137 content and geographic origin of the subject was noted (table I). On the other hand, the dependence on the dietary habits of the individual (fig. 2) became evident after a study of the Cs-137 content of food and water. These revealed that bovine meats, milk and milk products constitute the main routes of intake (fig. 3). Subsequent confirmation of these findings on larger representative samples of people and foodstuffs have been obtained at the Los Alamos Scientific Laboratory (6). The observations to date

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are consistent with the concepts of (*a*) stratospheric storage, (*b*) constant deposition on grazing lands, (*c*) uptake by cattle, and (*d*) transmittal to man.

Other relatively abundant and long-lived fission products, i. e., Ce–144—Pr–144 (290 day), Zr–45—Nb–95 (63.3 day), and Ru–106—Rh–106 (1 year), easily detectable by our technique in laboratory air, dust, sweepings from house carpets (fig. 4) and soil (7) are not present in the intact mammal in measurable quantities. These findings are consistent with previous observations on their low intestinal absorption following oral intake by laboratory animals (3).

In its present concentration, Cs–137 contributes on the average less than 0.3 mrad to the yearly dose of over 150 mrads which a human being is reported to absorb from natural sources of radiation (fig. 1).

Because of its relatively short life in the cow and of its reputed unavailability to the roots of some plants,[8] the concentration of this radioelement in milk is likely to serve as an excellent indicator of average rate of fallout over milk sheds. Since we can measure directly its presence in the living human we need not rely on theoretical predictions as to the possible individual variations under various conditions. Thus, only a factor of 6 separates the lowest values found in oriental subjects (whose diets are practically devoid of cattle products) to the highest found in the United States of America in an individual on a milk diet.

Pertinent to this discussion and to item X of the agenda are our recent findings on some inhabitants of the Marshall Islands which were measured in our facility by Dr. C. E. Miller. The scintillation spectra are shown in figure 5, and the body contents are included in table I. It should be noted that subject No. 10 is a control living in Majuro Island which did not experience unusual fallout. The next four subjects were inhabitants of Rongelap removed more or less permanently from that island to Majuro Island because of heavy fallout. Their content of Cs–137 is about 2 or 3 times that of the average United States citizen. The reason for this cannot be stated at this time but consumption of coconuts (reputed to acquire Cs) may be implied. The presence of Zn–65 in their body is due to contamination of seafood.

The highest contents of both Cs–137 and Zn–65 were found in subjects Nos. 5 and 18 who were removed temporarily from the island of Uterek because of heavy fallout and returned there after appropriate decay of the external radiation. It is obvious that they represent burdens likely to be acquired by living in zones of relatively high levels of contamination. Yet, despite these circumstances the increased dose rate of radiation to which they are exposed is only a fraction of the normal background of 100 to 160 mrads per year.

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(NOTE.—See middle of p. 745 for a remark concerning this statement.)

TABLE I.—*Gamma ray activity of human beings*

Country	Subject	Date	Cesium 137			Zinc 65		Natural Potassium
			μμc/gK	mμc/man	mrads/yr.	mμc/man	mrads/yr.	mrads/yr.
United States	Average	1956	34.0	4.8	0.29			Average value for all humans 25–40.
England	T	May 16, 1956	33.0	4.7	.28			
Do	R	July 13, 1956	35.0	4.9	.29			
France	J	Sept. 21, 1956	33.0	4.6	.27			
Denmark	F	Oct. 30, 1956	26.0	3.7	.22			
Sweden	N	Nov. 29, 1956	32.0	4.5	.27			
Australia	P	Mar. 27, 1957	50.0	7.0	.42			
India	Vo	Dec. 18, 1957	18.9	2.6	.16			
Do	Va	do	20.8	2.9	.17			
Japan	S	July 26, 1956	24.5	3.4	.20	3.2	0.02	
Indonesia	S	Aug. 10, 1956	13.9	2.0	.12	2.1	.01	
Do	M	do	8.5	1.2	.07			
Marshall Islands	10	Apr. 5, 1957	65.0	9.1	.55	30.0	.19	
	6	do	69.0	9.7	.58	73.0	.46	
	9	do	73.0	10.0	.61	30.0	.19	
	4	do	79.0	11.0	.67	30.0	.19	
	7	do	95.0	13.0	.80	62.0	.39	
	5	do	1,600.0	230.0	14.0	480.0	3.0	
	8	do	2,700.0	380.0	23.0	230.0	1.5	

Source: NBS Handbook 52—Maximum Permissible Levels: Zn-65=430 μcs; Cs-137=90 μcs.

FIGURE 1

Cs^{137} TRENDS IN HUMANS

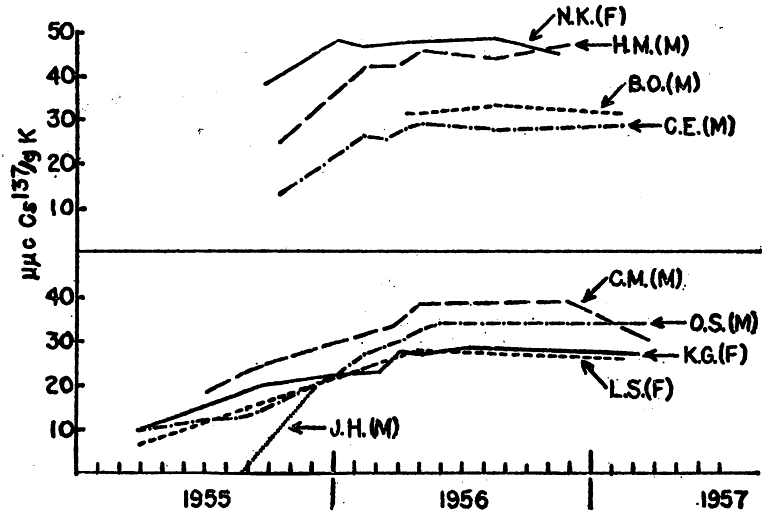


FIGURE 2

GAMMA RAY SPECTRA of FOUR NORMAL UNEXPOSED HUMANS

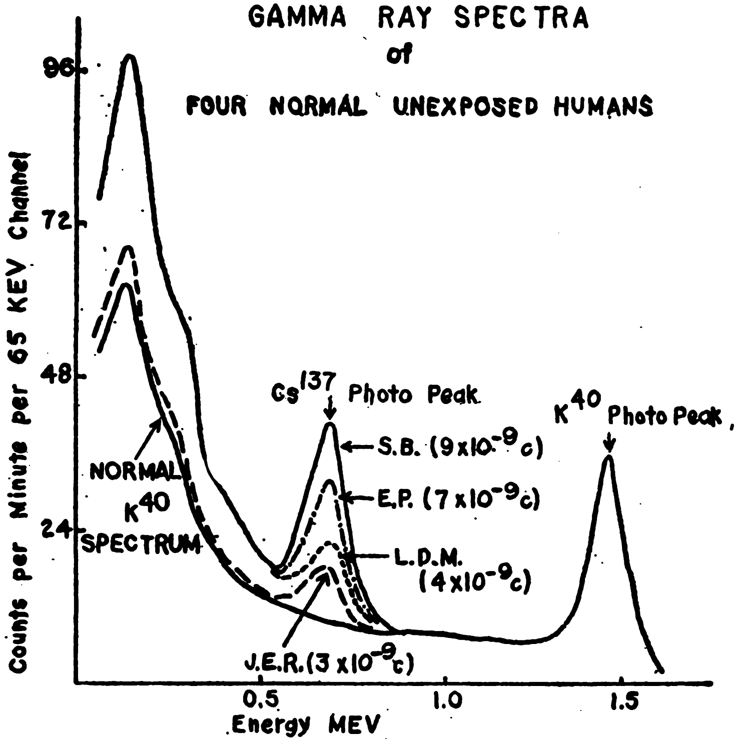


FIGURE 3

GAMMA RAY SPECTRA of TOBACCO and MILK

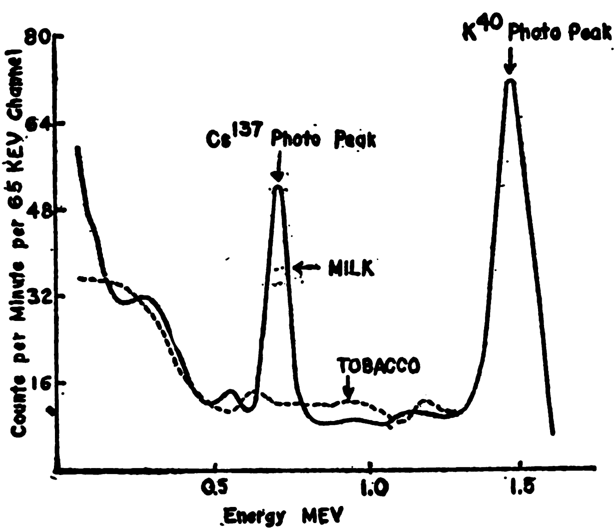


FIGURE 4

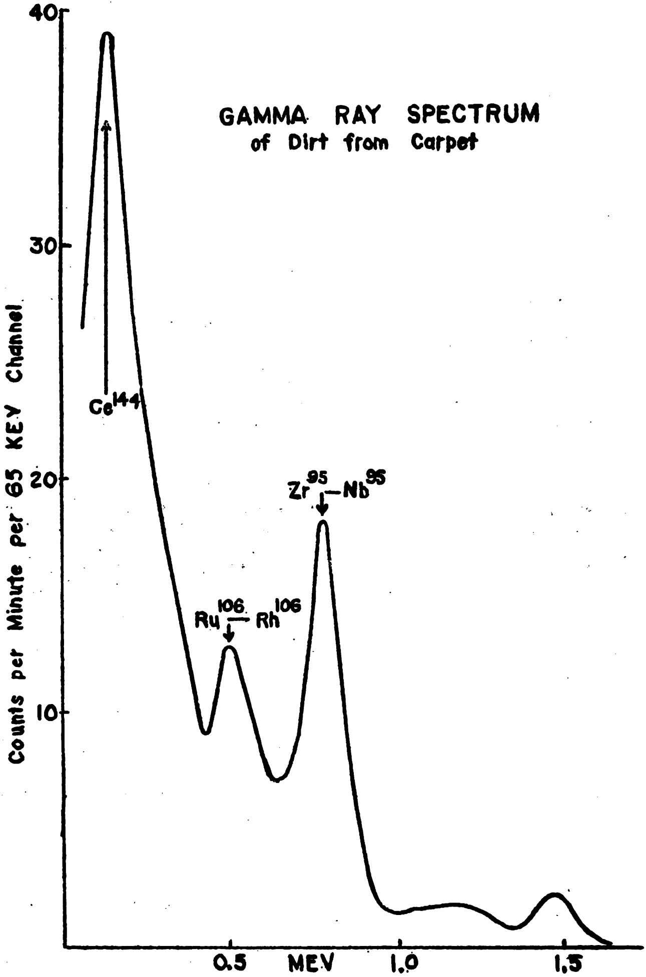
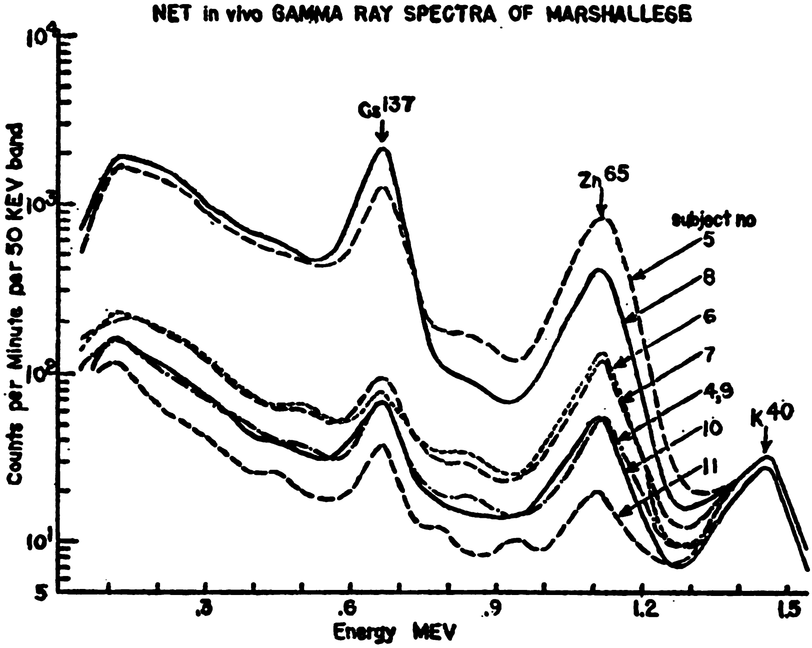

GAMMA RAY SPECTRUM
of Dirt from Carpet
Counts per Minute per 65 KEV Channel
40
30
20
10
Ce144
Ru106 — Rh106
Zr95 — Nb95
0.5
MEV
1.0
1.5

FIGURE 5

NET in vivo GAMMA RAY SPECTRA OF MARSHALLESE



Senator ANDERSON. Dr. Kulp, Dr. Eisenbud, and Dr. Neuman, do you and Colonel Hartgering want to get into some questions here, back and forth, that would be helpful to all of us? Dr. Langham, we would like to have you in it also.

Mr. Neuman, do you want to kick off on any comments you may have on the afternoon presentation?

DISCUSSION BY DR. J. L. KULP, MERRIL EISENBUD, DR. WILLIAM F. NEUMAN, DR. WRIGHT LANGHAM, AND COL. JAMES B. HARTGERING

Dr. NEUMAN. I would rather sandbag, if I may.

Mr. RAMEY. It might be desirable if Dr. Neuman could sort of state his case. Some of the members were not here and Dr. Kulp was not here at the time either.

Dr. NEUMAN. As a brief summary, I think it best to say that, in my opinion, the very best evaluation of future levels of bone are those calculated from our equilibrium data on natural strontium because this involves only one assumption; strontium behaves like strontium.

It is also my opinion that the natural strontium data in England and the bulk of the experimental data available in this country indicate that the overall discrimination from ground to bone is about a factor of 8. With this number, one has a fixed relationship between ground level and bone level. If we choose a certain maximum level to be permitted in human bone, we automatically fix a maximum level that can be permitted on the ground. With this number one can calculate the maximum rate at which testing can produce fission

products. The testing rate can be 2½ percent of the permissible ground levels.

Using a rather arbitrary set of numbers, the maximum permissible level of 50 sunshine units, which is one of the several levels that have been suggested, the test rate comes out to be a rather small number of the order of 2 megatons of fission equivalent per year.

I think that is a fair summary.

Senator ANDERSON. I think what perplexes a great many of us is that Dr. Langham wrote down a great number of figures and they all were pretty much in agreement—2.5, 3.1 at Los Alamos, 4.1 from Dr. Eisenbud, 3.5 from the milksheds—those figures are very close, very accurate, and apparently we have come to some sort of agreement.

Do you agree that it is going to take a hundred years of testing to get to the point where there is some danger to it if we continue at just the present level?

Dr. NEUMAN. I don't see how anyone could make a prediction concerning how long it would take because one has to assume a test rate.

Senator ANDERSON. I tried to say at the present level of putting fission products into the atmosphere. I am trying to get some fixed term if I can use it. If that is not a correct one I am sure Dr. Langham can give me a better one.

Dr. NEUMAN. I am not clear what present level of testing has been assumed. I take it to be based on measurements up to the fall of 1956.

Dr. LANGHAM. It has averaged roughly 10 megatons for 5 years.

This does not include anything that the Russians and British have done in the last few weeks.

Senator ANDERSON. I think that is useful. Can we all use that same figure and say we will assume, regardless of whether it is accurate or not—let us assume it is accurate, and I think it is probably very close—that we have been putting 10 megatons of fission products into the atmosphere each year for the last 5 years. Suppose we continue at that clip for the next 20 years, Dr. Neuman would you think that we had reached the maximum permissible level after 25 years or something of that nature?

Dr. NEUMAN. I should think it would be of the order of 25 years. Actually it would be a million years before you achieve true equilibrium. But for practical purposes, 25 years would be a reasonable number. It depends upon the MPC.

Mr. EISENBUD. 50 would probably be best.

Dr. NEUMAN. I agree, if the MPC is taken as 100 sunshine units.

Senator ANDERSON. Dr. Kulp, what do you think.

Dr. KULP. I don't think I am competent to say when there is a hazard. This is what you must get into next week. As far as the levels are concerned, it looks like there is only about a factor of 2 or 3 difference in the way Dr. Neuman and I calculated this thing. He used 50 instead of 100 as the safe level, and he used a factor of 10 from the soil to bone.

Dr. NEUMAN. Eight.

Dr. KULP. Yes; eight. I did not base it on the soil because I do not believe that the strontium 90 is ever going to be homogeneously distributed all the way through the soil, which is the condition you must have to get his 8 and 10 figure. This is based essentially on dividing

the milk value by 2. I end up with a practical soil to bone value of closer to 20. You can take your choice on these two things. That is the way they are derived. It depends on whether or not the strontium 90 becomes homogenized throughout the whole earth column.

Senator ANDERSON. Dr. Eisenbud.

Mr. EISENBUD. Mr. Chairman, I would like to supplement the remarks I made earlier today. I predicted a maximum of 5 micromicrocuries per gram from the tests to date. If we assume that the testing rate is going to continue as it has in the last 5 years, then the maximum instead of being 5 would be about 40, sometime after the turn of this century. If I superimpose a factor of 3 that Dr. Neuman used in order to allow for individual biological variation—and I am not sure this is necessary—then I arrive at a figure of 120 micromicrocuries per gram. This suggests that if we are going to continue testing until the year 2020 and if we are going to limit the maximum to Dr. Neuman's figure of 50 micromicrocuries per gram, then we are going to have to reduce the testing rate to one-third of the rate for the past 6 years.

Senator ANDERSON. You now say, if I can try to tell you what is in the minds of some people, that if we can continue the test to the year 2020 and if these other factors continue, and I am not trying to bind anybody to them, then we will have to cut down the testing by a factor of 3. That brings it down to 3 megatons of fission products to be discharged each year. We say if we are going to continue as we are now going, when will we reach an extremely dangerous position? Will that be 40 or 30 years?

Mr. EISENBUD. I was going to get to that, Mr. Chairman, and summarize by saying that at the rate information is accumulating we will have the answer long before the hazard develops. We are talking in terms of hazards developing at the present rate in terms of tens of years. We are accumulating significant information from month to month. Whether or not this factor of 3 should be included I think is something that will be known in the next year or so. We will not know everything we would like to know in the next few years, but we will learn enough to eliminate many uncertainties in calculations of the type Dr. Neuman has described.

Senator ANDERSON. Doctor Kulp.

Dr. KULP. I should like to emphasize that I agree completely with Dr. Langham's analysis that the big uncertainty is in the biological hazard where you are going to set the level. This factor of uncertainty of 3 in the distribution should be very firmly fixed within another year at the rate at which data is coming in. This is just a matter of getting an adequate number of samples at the right locations and measurements betwen the bone analysis and the anlaysis of the total fallout that Dr. Eisenbud's group is doing. That will not be important with regard to uncertainty a year hence.

Senator ANDERSON. Dr. Langham or Colonel Hartgering, do you have a comment there?

Colonel HARTGERING. I guess I am the only physician in the group. I don't like to leave the impression that we will know what the somatic relation will be in man. You do not get that in a couple of years. The bone samples and soil samples are coming in rapidly. The type of work I discussed we are doing at Reed. This is obviously continuing, but the somatic effects on man and what this means in terms of injury to him is not going to be available in the next year or two.

Senator ANDERSON. Suppose we should get to another assumption now. Suppose we should say that since other countries are developing weapons that we now raise this rate from 10 megatons of fission products a year to 20 megatons. Would that accelerate pretty rapidly in the time when we might reach these dangerously high levels or what we might assume to be?

Dr. Langham, would you care to comment?

Dr. LANGHAM. I think this borders on something that is more in your field than in ours. What it really amounts to is this: About 10 megatons per year total testing by everybody seems to be just about as much as one would assume we should allow.

Senator ANDERSON. That is a very important and very fine statement. That is one of the things we were trying to get in this hearing, and I am so happy to have it from you, because I know you have spent a great deal of time on it. I am going to try to find out how many people agree with you in a short time. Regardless of agreement, the fact that we bring a figure out on the table and talk about it I think is extremely important because it does indicate your belief, and I think the belief of a great many others that if 10 megatons of fission products is about what we should put in the atmosphere each year, then maybe there is some need to find out what the other people in the world are doing and see if we can hold at that figure.

As you know, I strongly support the idea of constantly improving and testing the devices that we have. I have been very happy at the work at both Los Alamos and Livermore in making these tests possible with as little disturbance to our strontium 90 pictures as possible. Therefore, I am very happy to have this figure from you. You were going to say some more but I did want to break in to say that this is one of the things many people have been hoping to hear from somebody in authority.

Dr. LANGHAM. The implications of this are, of course, that we are no longer the only people testing weapons, which leads automatically to the idea that what one really needs is some type of international agreement with regard to an allocation of fission products that can be injected. Such an arrangement would not place a limit on the total megatons tested but the amount of fission that could be injected into the atmosphere.

This would lead to an encouragement of people to build cleaner weapons or to explode them under conditions which would allow less material to go into the biosphere. The only way one could monitor this would be to get an agreement whereby they would put a tracer on each bomb which would allow anyone in the world to sample their cloud and tell what fraction of the cloud they tested. Then we could monitor how much each country was putting into the biosphere.

The only thing is that in so doing we would gain a lot of information about their bombs which would, of course, mean that it would be hard to get an agreement on such a point.

Senator ANDERSON. Thank you very much. Dr. Eisenbud, we would be glad to have you comment on this upper limit that we have tentatively placed on what we can do. It is a very interesting figure.

Mr. EISENBUD. I think, to use a colloquialism, "We are in the right ball park."

I am also impressed with the fact that the proposed limit is approximately of the same order of magnitude as they assumed the past test-

ing rate. I note that we are talking about reaching the so-called maximum permissible limit, not in terms of months or years but in decades. This suggests that one can feel comfortable that the emergency is not here. We are talking about a hazard that may develop many years from now. We will have ample time to study the problem.

Senator ANDERSON. Dr. Neuman, I would be very happy if you would comment on Dr. Langham's number a moment ago. I hope this is not embarrassing to you.

Dr. NEUMAN. I should say, at the outset, that I am amazed. I felt very lonely up there when I was giving my testimony. Now I find we really are not so different in our predictions after we used our pencils.

Senator ANDERSON. I think that is true generally of scientists when they get together in discussions. That is one of the reasons of having many scientific discussions. That is one of the reasons why the chairman of this committee and the chairman of the subcommittee were anxious to get scientists in here to testify in a fine scientific fashion.

Dr. NEUMAN. It was not very long ago for lack of data that we were swinging rather wildly. The data that have come in recently have sharpened the arena of debate to the point where, as I indicated at the outset, I did not feel we were in trouble, that the present levels are indeed low. This has been stressed many times. It probably needs no further emphasis.

On the other hand, the rates of permissible testing have now narrowed down to somewhere between 2 and 10 megatons per year of fission products released. I think the most important aspect is not whether it is 2, 4, 6, or 10, but the fact that it is a small and finite number, one small enough that we can right now envision an international agreement, as Dr. Langham has indicated, and at the same time, as Merril Eisenbud has indicated, that we can approach this without a scare—or fear—psychology, that there is sufficient time to draw sane and careful agreements embodying necessary precautions so that these agreements can be enthusiastically endorsed. We are really uniquely in a good position knowledgewise to affect an effective ban.

Senator ANDERSON. Thank you. Can 1 of the 5 of you help me on a matter? The statement was made that it might take a hundred years to reach a high level—a hundred years of steady testing—and yet the strontium 90 has a half life of 28 years. How do I try to relate those two things?

Dr. Langham, can you help me?

Dr. LANGHAM. What this means, sir, is that if strontium 90 is decaying with a half time of 28 years, then in about a hundred years we will have shot—or if you want to take Dr. Libby's number, about 50 years—into the biosphere enough strontium 90 so that it is decaying at the same rate we are testing. We will be at equilibrium and we can test for the next 2,000 years supposedly and it would not increase a great deal more.

Senator ANDERSON. I think you explained that to me once before but I wanted to get it in this record.

Dr. Kulp, do you have any comments?

Dr. KULP. I think the only point I would like to add is that in considering this 2 and 10 that we are still shooting at this 100 number. Taking the radical view for just a minute, I wonder if we are fair in multiplying our values times 3, to be very safe, and comparing this

with a hundred when already we have dropped this to a hundred for an average general population. I think the radical might still say we might be allowed 30. The conservative might very well say that 2 would be about 200 times too much. But this will come out next week.

Chairman DURHAM. Doctor, what is that 100 based on? Is that based on medical statistics or what is it based on? Did you just pull the 100 out of the air?

Dr. KULP. I think Dr. Langham should answer that.

Dr. LANGHAM. You were asking about the 100 micromicrocuries and what is it based on?

Chairman DURHAM. Yes.

Dr. LANGHAM. The way it was derived is as follows: We have had a certain amount of experience with radium in people. We think we have a good idea that one-tenth of a microcurie of radium fixed in the bone for essentially one's lifetime will do him no harm. Therefore, we have picked that amount of strontium 90 which would give the worker the same amount of radiation that was given to these people who had a tenth of a microcurie of radium.

So we have said for our occupational tolerance we will take that amount of strontium which is equal to the maximum permissible level of radium. For the world population or for the unoccupied people in this particular pursuit, we will take one-tenth of it. One-tenth of this value which has been selected in comparison with radium is equivalent to the 100 micromicrocuries per gram of calcium in the bone.

So it is based on comparison with radium and our experience in the radium industry. It assumes, sir, that the response to radiation is a threshhold effect. Whether or not radiation is a threshhold effect is not clearly yet established. In fact, the general tendency is to look at long-term changes following radiation damage, including genetics, as being the linear type and not the S-shaped curve I drew on the board. It turns out that 100 micromicrocuries of strontium on the average is about equal to the natural background that we get to the bone.

You can go on from his. As Dr. Lewis has just done in a recent excellent article in Science, you can tie this to the leukemia incidence in the world and you can go ahead and make postulations which will allow you to postulate how many cases of leukemia this may mean distributed throughout the world population. But in order to make that comparison you must assume that you know the effect of radiation or that the increase in radiation damage is linear with increasing dose, and not a threshold response.

Next week is going to be a very interesting session because it is primarily to bring out these very points.

Senator ANDERSON. Very well said, and a fine statement. I thank you for it.

Are there other observations that anyone wants to make?

If not, I would like to ask you if you would sort of document these "chalk talks" as much as you can to us so when we select certain portions for reproduction we may be able to add to it a little bit the bases upon which these decisions were reached, just as you have done here.

May I therefore announce that the hearings will resume on Monday, June 3, and continue through Friday, June 7. June 11 and 12 have

been reserved for testimony from scientific experts who wish to appear but who are not taking part in the first 2 weeks' discussions. The Monday meeting will be in this room.

Just in closing, may I say it has been a real privilege to have such a fine panel for this work today.

Chairman DURHAM. I would like to add my appreciation, too. It certainly has been a fine discussion.

Senator ANDERSON. Thank you very much.

We are adjourned.

(Thereupon, at 4:15 p. m., Wednesday, May 29, 1957, the hearing was recessed, to reconvene at 10 a. m., Monday, June 3, 1957.)

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

MONDAY, JUNE 3, 1957

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION
OF THE JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The special subcommittee met, pursuant to recess, at 10:10 a. m., in the caucus room, Senate Office Building, Hon. Chet Holifield, chairman of the subcommittee presiding.

Present: Representatives Holifield, Durham (chairman of the Joint Committee), Price, Dempsey, Van Zandt; Senators Anderson, Hickenlooper, and Bricker.

Also present: Professional staff members: James T. Ramey, executive director; George E. Brown, Jr., Hal Hollister, staff technical adviser, and Paul C. Tompkins, consultant.

Representative HOLIFIELD. The committee will be in order, please.

The hearings have covered up to now sections I through IX of the outline. This includes background information on the nature of radioactivity, the production of fallout by the detonation of weapons, its transport in the atmosphere, and its deposition and uptake in animals and man. We spent a good deal of time on local and delayed fallout and began our investigation of the main culprit, strontium 90. Many subjects that initially appeared to represent marked disagreement have developed into subjects on which there is general agreement when put into perspective.

(*a*) The radioactivity from fission products is considerably more dangerous than the radioactivity induced in the environment by neutrons. Furthermore, a radioactivity "clean" weapon device is apparently not possible.

(*b*) The way radioactive materials are introduced into the biosphere is subject to wide variation with air detonations favoring wide dispersion and surface detonations favoring local fallout. For estimating how much material is injected into the stratosphere the consensus figure is about 50 percent, although RAND and others feel that 20 percent is a better figure for detonations over land. The distribution of worldwide fallout appears to be nonuniform. Variation from the average by more than threefold, as far as long-range deposition is concerned, does not appear to happen.

(*c*) The depositions from fallout to date in the biosphere including uptake in man is apparently low—approximately 10 percent—when compared to natural radioactivity. The significance of this fact has yet to be discussed.

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(*d*) Discrimination between calcium and strontium in biological processes does occur.

(*e*) The rate at which knowledge of the food chain processes is accumulating leads to confidence that serious trouble from fission products can be foreseen in advance.

(*f*) On Wednesday, May 29, there was a concensus of qualified witnesses that fallout was hazardous to a degree, and that some limitation on the injection of fission products from all sources into the atmosphere was desirable. There were differences of opinion as to this limit, with the highest fallout limit being placed at the rate of 10 megatons of fission yield per year.

(*g*) The critical scientific issue in the strontium 90 controversy was identified as the determination of whether or not the dose-effect relationship is linear or has a value below which it produces no effect, that is, a "threshold." This subject is to be covered this week, starting this morning.

This is a brief summary of the testimony as prepared by the scientific members of our staff.

There was some confusion in the testimony regarding the units of measurement for radiation, and we have changed the order of witnesses this morning. We are going to have Mr. Lauriston Taylor, who is head of the Atomic and Radiation Physics Division of the National Bureau of Standards, testify this morning on this particular subject.

Mr. Taylor, will you please come forward and take the chair on the right?

Mr. TAYLOR. May I use the podium?

Representative HOLIFIELD. Yes, you may use the podium.

STATEMENT OF LAURISTON S. TAYLOR, CHIEF, ATOMIC AND RADIATION PHYSICS DIVISION, NATIONAL BUREAU OF STANDARDS [1]

Mr. TAYLOR. Mr. Chairman, thank you very much for the privilege of appearing before you.

I am the head of the Atomic and Radiation Physics Division of the Bureau of Standards.

Representative HOLIFIELD. Thank you, sir.

Mr. TAYLOR. The material that I have to cover this morning is rather extensive, if one goes into detail. In order to save time, I would like, with your permission, to insert some dozen and a half

[1] Date and place of birth: June 1, 1902, Brooklyn, N. Y. Education: Stevens Institute of Technology and Cornell University. He joined the staff of the National Bureau of Standards in 1927. With the exception of the periods 1943–46, when he served overseas with the 9th Air Force as Chief of Operations Research Section, and in 1948, when he was loaned for 1 year to the AEC as Chief of the Biophysics Branch, Dr. Taylor has been engaged in the NBS research and development programs in radiation physics. Chairman of the International Commission on Radiological Protection from 1937 to 1950, adviser on the United States Delegation to the International Conference on the Peaceful Uses of Atomic Energy at Geneva, Switzerland, in 1955. Chairman, National Committee on Radiation Protection and Measurements; consultant, Department of Defense Weapons Systems Evaluation Group; member of the American Medical Association's council on physical medicine; National Research Council's committee on nuclear science, and the subcommittee on radiobiology; the National Academy of Science's advisory committee on civil defense, etc. Gold Medal of Radiological Society of North America for leadership in field of radiation protection on national and international scale; Janeway Medal of American Radium Society for contributions toward development of international standards for radiation protection and radiation units; Sylvanus Thompson Medal from British Institute of Radiology; Bronze Star Medal (1945), Medal of Freedom (1946) for services for the Air Force. (Submitted by the Department of Commerce.)

pieces of material into the record, and also, if it is agreeable to you, I will refer to these by number as I go along. I have supplied the stenographer with a list of these, and this will save the time of having to read each title fully.

Representative HOLIFIELD. Without objection, that will be the order of the committee.

Mr. TAYLOR. Thank you.

The main purpose of this part of the discussion is to give some general background for our radiation units, and the standards of radiation protection, and to indicate some of the areas of certainty or uncertainty, and some of the areas where we know things with reasonable certainty. At the same time, I think it would be worth while in this connection to indicate some of the areas of research needs that will be fairly obvious from my discussion.

Until about 1928, any protective measures that we may have had were purely on a guesswork basis, and the reason for this was that until 1928 we had no acceptable unit of radiation dose. In 1928 there was international agreement on a unit of dose known as the roentgen. Once this unit had been decided upon, it was then possible to go forward with the establishment of quantitative radiation dose units for protection purposes.

Now, the roentgen was the only unit of radiation dose until the early 1940's. In fact, it was the only unit for which there was any use, because, for all practical purposes, we had only to deal with radiation from radium and X-rays, and the unit "roentgen" applies only to gamma rays and X-rays.

So in that early period, the formative period of our radiation protection philosophy, we dealt entirely with the roentgen as the unit. It is still a completely acceptable and useful unit of exposure dose, with certain limitations. However, one should not speak of the dose from all radiations in the terms of roentgens. This, unfortunately, has been somewhat the practice, at least in public writing. I think the scientists in general keep these matters straight.

Because we had other types of radiation to worry about beginning in the forty's, particularly large quantities of beta rays, neutrons, alpha rays, and so on, it was necessary to have another unit which could measure all of these radiations in equal terms, and for that purpose in 1953 the International Commission on Radiological Units adopted another unit known as the rad.

The "rad" is a unit of absorbed dose. It tells you how much energy has been imparted to tissue from any of the radiations in question. It is still a difficult quantity to measure directly. A considerable amount of further developmental work on this is certainly required, and I will come back to some of that later.

When we are talking about dose, there is another term that enters our thinking, and this is known as "dose rate," and dose rate is, as its name implies a rate of giving dose.

Perhaps to give an example to illustrate this, let us consider a dose of aspirin. One tablet of aspirin could be called dose, and when you have taken this 1 tablet you have had a dose of 1 tablet of aspirin. Now, a doctor could tell you that he wanted you to take a dose of eight tablets. If you take all of these eight tablets at once, you are probably going to be in some sort of medical difficulty. So you distribute them,

and he will tell you to take these 8 tablets at the rate of 1 per hour or 1 every 2 hours, thereby introducing into the dose the time concept. Certainly, in the case of aspirin, it makes a lot of difference whether you take the whole eight tablets of aspirin at once, or whether you distribute them over a day, or over a week.

This time distribution may or may not be important, depending upon the circumstances, when we are talking about radiation.

There is another unit that has been referred to in previous testimony called the "rep." This was a unit that was invented for practical purposes during the Manhattan District days, and was designed to establish some relationship between the dose that is given by different kinds of radiations. The difficulty today with the rep is that it has been variously redefined many times to the point where nobody is really certain as to what is meant when he speaks of the rep.

The value for the rep of 93 ergs per gram—that is, the energy that is absorbed by 1 rep of radiation—was mentioned in earlier testimony. All I can say is that 93 ergs per gram is not considered to be a good value for the rep, and the hope is that this term will gradually be discontinued.

There is another term that we will hear a great deal about, I suspect, in the next 2 or 3 days, known as the RBE, or the relative biological effectiveness of radiation. The RBE is a factor that is used to equate the biological effects of different kinds of radiation, such as alpha, beta, gamma rays, X-rays, neutrons, and so on. These act differently on tissue because of their physical characteristics, and it is not readily possible to compare the effect of these different radiations without some kind of a comparison factor. There is considerable uncertainty about the values of the RBE for many radiations, and this will come out in later testimony.

The relative biological effectiveness of two different radiations depend on a great many factors. It depends on the particular organ in the body that you are investigating, the kind of effect in that organ, the absorbed dose that is given to that organ, the rate of delivery of the dose, the way the dose is fractionated between exposures, the pH and oxygen content of the tissue, the temperature, and other factors. At the present time the RBE is known only roughly in some of the areas that must be regarded as quite important. Here is an area where a great deal more research is certainly called for.

I might remark at this point that the National Committee on Radiation Protection in this country, and about which I will tell you more later, has a subcommittee working actively on the whole question of RBE. This program is under the chairmanship of Dr. Wright Langham, who testified before you the other day, and is being supported in part by the Atomic Energy Commission in its program at Los Alamos.

In this program they are presently compiling and correlating a vast amount of data that is pertinent to this question. We hope they will develop the specific areas of need, and thereby direct attention to them, so that the necessary research and development can be carried out to fill any gaps.

I might point out at this time that our lack of knowledge of the RBE, the relative biological effectiveness of the different radiations, is not as far as we are aware, introducing what one might call danger factors in the setting of our permissible exposure levels. It is possible

to make some kind of upper and lower estimates, of the RBE values, and where there is any doubt the higher value is chosen. This is erring on the side of providing more protection than your basic numbers might initially indicate.

On the other hand, it is important to take advantage of the proper values of the RBE when you know them, because, if you use values that are too low, it makes protection very costly and this, in turn, reacts unfavorably on the development of nuclear energy programs.

There is still another unit that I will use, and others will use considerably as time goes on, and this is known as the rem.

The "rem" is what we would call a unit of RBE dose. It essentially includes all of the factors that we know about dosimetry. It includes the RBE, and the absorbed dose in rads; and if you want to talk in completely general terms about radiation, you can refer to radiation dose in rems for all radiations, and for all conditions, bearing in mind that where you have an uncertainty, for example, in RBE's you also have an uncertainty in RBE dose.

Another unit that has been talked about is the curie. Now the curie is not a unit of dose, it is a unit of quantity of radioactive material. It essentially tells you how much material you have, and it is related to the number of radioactive disintegrations that occur in a given time in a radioactive material. It is really more comparable, you might say, speaking loosely, to a unit of weight. In fact, for radium, 1 curie is equal to 1 gram of radium.

One other unit that has been talked about is the sunshine unit, it is based on the maximum permissible body burden of strontium. Where we have set for occupational purposes a permissible body burden of one microcurie of strontium, this means one microcurie of strontium for the whole body. Since there are approximately a thousand grams of calcium in the whole body, so it means one microcurie of strontium 90 per thousand grams of calcium. When you go through the arithmetic, this would turn out to be a thousand micromicrocuries of strontium per gram of calcium.

Now the sunshine unit, I believe, relates primarily to the body burden for people not occupationally exposed. Those levels are considered to be one-tenth of the levels for occupational exposure. So one sunshine unit would then be one one-hundredth of the average permissible body burden for the general population.

A word as to the status of the units. On the whole I think it can be said that they are fairly well in hand.

Representative HOLIFIELD. Excuse me, Doctor; was there any scientific reason why the word "sunshine" was used in this instance?

Mr. TAYLOR. The term "sunshine" itself?

Representative HOLIFIELD. Yes.

Mr. TAYLOR. There may well be, but I am not aware of it.

Senator ANDERSON. Did it not grow out of the fact it was called "Project Sunshine"?

Mr. TAYLOR. I suspect this is the source of the name; yes. I do not think there is any scientific reason.

Representative HOLIFIELD. The word "sunshine" has a cheery note to it, and I was just wondering if we were allowing, let us say, propaganda to creep into our scientific terminology. Why did you not put it "happy" units, or something like that?

Senator ANDERSON. I do think it should be said that he not only did not have anything to do with the naming of it, but never heard of it until he came to these hearings. Was it not the fact that the whole project making this study was given the name "Sunshine Project"?

Mr. TAYLOR. Yes.

Senator ANDERSON. Just as we have "Operation Plumbob" going on. They have put out Project Sunshine as the most enlightened and happiest look on radiation damage.

Mr. TAYLOR. If I might divert just one moment, when I was with the Atomic Energy Commission myself for a year some years ago, we started a problem closely related to this, and we called it "Project Gabriel"—you know, Gabriel blow your horn. That had kind of a sinister sound to it, and it may be that they wanted to undo a little of that.

I think I was about to point out——

Representative HOLIFIELD. I did not know but what maybe this might be a tranquilizing pill.

Mr. TAYLOR. I obviously have had no close connection with this particular project, as you can see.

Getting back to the question of units there has just been published within the last month or so a very complete report on radiation units by the International Commission on Radiological Units, and this I will submit as exhibit 2 of my accompanying material.

With regard to units generally, I might say that at the present time there are standards for all of the basic units. These have been established in various laboratories and are now maintained at the National Bureau of Standards as part of their normal program. However, there are many areas where further research is needed, particularly in the area of absorbed dose.

There are two things needed here. One is some basic development on the standards and instrumentation. This is the type of work that might be done at the Bureau of Standards, or some other comparable laboratory.

I might point out that one of the real difficulties in making progress with this is our inability to get the suitable people to work in a Government laboratory because of the competitive salaries offered by the airplane industry, and so on. This is a real handicap.

There is also a great deal of work needed in clinical and medical institutions, and biological laboratories, on this same question. Work of this sort is going forward, but much more is needed.

On the RBE itself, this, of course, depends upon the absorbed dose studies, and, as I mentioned, there is a great deal of work going on in this area in the Atomic Energy Commission laboratories, as well as others.

Now, in setting of the permissible dose from sources of radiation external to the body, we have to make some basic assumptions. Until 1947, the standards that we used were based almost solely on the effect of the radiation on the individual. In 1947, we began to obtain or have made available to us some more quantitative information on the genetic effects of radiation, and on the effects of radiation on expected life spans (exhibit 3).

At that time, in 1947, while there was this evidence, it was not considered to be sufficiently quantitative to be used in a quantative readjustment of our permissible exposure levels.

In 1952 some tentative levels for the average population dose were set by the International Commission on Radiological Protection, mainly for genetic reasons. The actual promulgation of these recommendations was held up because even then it was felt that the information was not sufficiently quantitative, and there was still time to await better information.

It is rather interesting, in this connection, to notice that the recommendations that were discussed in 1952 are essentially the same as those that were recommended by the International Commission itself in the spring of 1956, and then later by the National Academy of Sciences in its report of last June.

In this connection, I would like to introduce in the record exhibits 4, 5, and 6.

The recommendations of the International Commission, which I will not go into in detail, give 1 or 2 important numbers.

One was that for occupational exposure a person should not receive more than 50 r. e. m. up to age 30, and 50 r. e. m. per decade, or every 10 years thereafter. This is equivalent to an average of about 5 r. e. m. per year. The difficulty, however, is that one cannot work very comfortably to an average that is geared to a yearly accumulation in this way. And particularly for legal purposes there is a great deal of difficulty in interpreting what is meant by a 5 r. e.m. per year average.

Accordingly, the National Committee on Radiation Protection in this country introduced a concept which it hopes will serve to straighten this out somewhat. They have said that the maximum permissible dose for occupational purposes for an individual shall not exceed five times the number of years of age of that person over 18. Eighteen is the age at which persons are allowed to begin radiation work.

You can write that MPD=5 (N–18) rems, where N is his age.

This means that if a person starts to work at age 18, in his first year he can receive 5 rem, and so on.

This material has been described in some detail in quite a number of places. I will introduce one for the record here as exhibit 7. I will return to that in just a moment.

Senator ANDERSON. Does that mean a person has got to be 60 years old to take 42 times 5 rems, or 210 rems?

Mr. TAYLOR. Yes, sir. I have a chart that will show that.

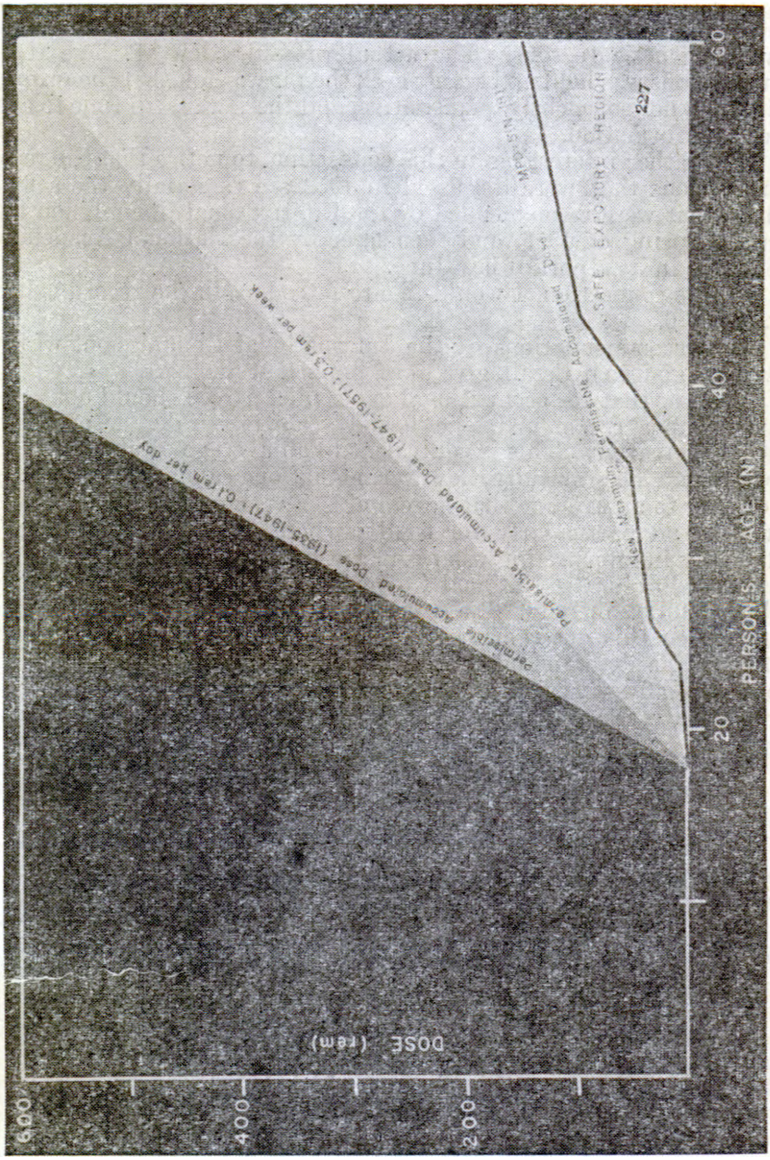
Senator ANDERSON. Is not that a pretty fair sized dose?

Mr. TAYLOR. Yes, but let me show you what is meant by that, if I may, in just one moment.

In connection with this change, which is a lowering of the level we have had up until now, we have still retained tentatively the allowance of 0.3 rem per week, or 15 rem per year, provided that the age-prorated dosages as given by the formula is not exceeded.

I have a chart with me that was prepared for another purpose, but which I think will illustrate the point.

EXHIBIT 1


Permissible Accumulated Dose (1935-1947): 0.1 rem per day
Permissible Accumulated Dose (1947-1957): 0.3 rem per week
New Maximum Permissible Accumulated Dose
SAFE EXPOSURE REGION
227
DOSE (rem)
200
400
600
PERSON'S AGE (N)
20
40
60

Can you see the lower part of it? Do not pay any attention to the upper part.

Here [indicating] we have plotted the dose in rems, 200, 400, 600 rems as a function of the person's age—age 20, 40, and 60.

Now, in the upper region, indicated by this magenta color, is indicated the dose that a person could have received as a function of age under the permissible dose limits that prevailed from the period of 1935 when we had our first dose limits, to 1947, when they were revised.

According to this, a person could receive about 600 rem by age 38. If you extrapolated this up to 60, it would perhaps be twelve or fifteen hundred rem.

Now, 1947——

Representative HOLIFIELD. Who set that particular chart? Who set that? Was that the Radiation Association of Scientists?

Mr. TAYLOR. Well, yes. This particular level of 0.1 rem per day was set in this country by the National Committee on Radiation Protection in 1935 on the basis of the information that we had available to us then. The year before this, in 1934, the International Commission had set a level of 0.2 rem per day which was twice as high as the United States value. This country lowered it to one-tenth of a roentgen per week.

Representative VAN ZANDT. What is the estimated dose count at age 60?

Mr. TAYLOR. It looks like something of the order of maybe 1,200 rem.

Senator ANDERSON. You say this one time there had been a much sharper rise, and therefore a much larger dose indicated prior to 1935?

Mr. TAYLOR. The first permissible dose in quantitative units was set in 1934 by the International Commission of Radiological Protection at two-tenths of a rem per day, and that would have been a curve coming up substantially steeper than the one I have indicated.

Senator ANDERSON. Then this was changed in 1935?

Mr. TAYLOR. This was changed in 1935 by the United States committee.

Senator ANDERSON. And is substantially the same now as it was then?

Mr. TAYLOR: No, sir. I am coming to that.

Senator ANDERSON. You said it had been changed in 1947?

Mr. TAYLOR. Right. In 1947 we began to have, as I mentioned, more information available about genetic effects, longevity effects, and the effects of small doses of radiation. And so in 1947 we lowered the permissible dose from one-tenth rem per day (or six-tenths rem per week), to three-tenths per week. In other words, it was cut in half.

That would have meant, on the basis of the old permissible dose, that at age 60 a person could have received, if he had taken the maximum, something in the order of slightly over 600 rem.

Representative VAN ZANDT. Was that for the 12-year period from 1935 to 1947?

Mr. TAYLOR. Until 1956 or 1957 there was no limitation that was related to age as such. This is a new concept.

Representative VAN ZANDT. What I am trying to develop is why did they make this reduction in dosage?

Mr. TAYLOR. In 1947?

Representative VAN ZANDT. Yes.

Mr. TAYLOR. Prior to Mahattan District days we had very little quantitative evidence to go on. A great deal of biological work was done by the Manhattan District Project in order to protect their own people, and this turned out to be very valuable and useful information with relation to maximum permissible exposures. So it was lowered in 1947 because of better information, because we found that our technological advances would make it possible, and because it just looked like good sense.

Now, in 1956, the International Commission again recommended a further lowering of the dose, and this is represented by the lower part of the graph, the white part on the graph here. This is the dose in relation to age, and according to this, by age 60 a person would be allowed to accumulate approximately 200 roentgens. These are whole body exposures, I am talking about.

Representative HOLIFIELD. That is accumulative?

Mr. TAYLOR. That is accumulated. He can be allowed to accumulate up to approximately 200 roentgens by age 60. This means working from age 18 to 60, or 42 years.

Representative HOLIFIELD. Now you said that you have a new concept of permissible radiation dose in relation to age. Why do you have a difference in the age group? Why do you have a different allowable dose? Does it have to do with the somatic cells and the reproductive cells?

Mr. TAYLOR. It has to do with both the somatic cells and the reproductive cells; yes. Primarily, I would say the reason has to do with the genetic problem. Once you have done this for the genetic problems and introduced the kind of levels necessary for genetic protection, if you speak of it that way, you might just as well continue and provide the improved somatic protection at the older age.

Representative VAN ZANDT. Doctor Taylor, based on experience, have you any figures to indicate the number of people that were actually affected and the number of deaths that resulted from exposures to radiation?

Mr. TAYLOR. Well, I was going to say later, but I might as well say right now, that there is no tangible evidence that I know of, of anybody working under these limits, the 1947 limits, or the new limits, who have been harmed by the radiation levels they were exposed to. In other words, I know of no causative relationship between damage that might develop in an individual, and the radiation to which we were exposed at any of these levels we have been talking about; in fact, even under some of the less conservative levels prior to 1935.

Representative HOLIFIELD. The fact you do not know of any specific cases does not necessarily prove that there has not been damage, and you would not state there has not been damage?

Mr. TAYLOR. I certainly would not. It does, however, indicate the likelihood of damage is quite small. And this is another point that I will come to later. The probability is so small that it requires rather fine statistical techniques, in order to really determine whether there is any effect or not.

Representative HOLIFIELD. But there must be some concern or the scientists would not have been continuously reducing the permissible level there?

Mr. TAYLOR. This is correct. Even back in the early thirties, when we knew very little about this problem, nobody who really thought about the question would say that promiscuous exposure to radiation was harmless. We have always felt it was desirous to limit as far as possible the unnecessary radiation exposure.

Representative HOLIFIELD. I assume we will have a great deal of testimony on this point from other witnesses.

Mr. TAYLOR. I am sure you will; yes.

Senator ANDERSON. I noticed in newspapers of Sunday, May 26, there was a story from Science Service that quoted Dr. W. L. Russell, principal geneticist at the Oak Ridge National Laboratory, in which he said that "Offspring from a man exposed to such radiation will have their lives shortened on the average of 20 days for each unit of the radiation their father has received."

Will you relate that to that chart? Does that mean, going from the 200 level up to the old 600 level, that they would have their lives reduced 400 times 20 days?

Mr. TAYLOR. If his figures are correct.

Senator ANDERSON. I am suggesting we assume the figures are correct. This was from the proceedings of the National Academy of Sciences, and I assume they have given some thought to it.

Mr. TAYLOR. Yes.

Senator ANDERSON. But he said that a man's life would be shortened from 5 to 35 days for each unit of radiation received by his father, and the father's life, of course, would also be shortened. But I am trying to relate it to that chart. Does it mean, going from 200 up to 600, which was the old limit and now brought down to 200—had that been left alone, and assuming Dr. Russell from Oak Ridge and his figures are correct, does that mean their life would be shortened 400 times 20 days?

Mr. TAYLOR. Yes; I think, assuming the figures were correct, this would be what you would expect.

Representative HOLIFIELD. The Chair might say that Dr. Russell will be a witness tomorrow.

Mr. TAYLOR. Right. I would much rather let Dr. Russell discuss this particular question.

Senator ANDERSON. Well, I would like to have you comment on what he said.

Mr. TAYLOR. If the figures are correct—and I am not willing to admit personally that they have been amply proven, although there is a lot of evidence pointing in this direction—then I think you have to accept the sort of life shortening you have just mentioned.

Senator ANDERSON. Something like 20 years, 22 years?

Mr. TAYLOR. What was the figure you gave—20 days per roentgen?

Senator ANDERSON. Twenty days per roentgen. I do not know whether it is per roentgen. It says "each unit of radiation." You were talking about different kinds of units. I do not know whether it refers to rems, curies, sunshine units, or what. I assume it is some kind of a unit.

Mr. TAYLOR. At 20 days per man-rems, you would multiply 20 by 200, and that would be 4,000 days, would it not?

Senator ANDERSON. I am taking the spread from 200, now regarded as safe, up to the 600 they regarded as safe only a few years ago, and

you get 8,000 days or 22 years. Is not that a pretty substantial shortening of life?

Mr. TAYLOR. It is pretty substantial. I do not believe it.

Senator ANDERSON. Well, we are trying to get information here adduced by scientists, and the National Academy of Scientists is a reasonably good body, is it not?

Mr. TAYLOR. I am not in the best position, sir, to discuss that particular point, but I would say this: That I do not believe there is any evidence of a shortening of life span by that many days on the part of people who have been living within these higher permissible exposure levels. If there is such information, I am unaware of it.

Senator ANDERSON. Yes. I was only taking the colors you had in the chart there, and was trying to see what Dr. Russell's statement meant. We will ask him also, but I wondered if it related to your chart. That is all.

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. To what extend does industry live within these standards of exposure are are they required to live within these limitations?

Mr. TAYLOR. Well, industry has for the last 10 or 12 years been required to live within the limitation covered by the center area down, and by and large they have done this very well. Actually, the Atomic Energy Commission operations have, for safety's sake, been doing better than that, and they have in fact been living within the white area for some considerable time. So that these levels we are talking about now, while they have been reduced by a factor of 3, are probably not going to raise the hackles of the Atomic Energy Commission at all.

Representative VAN ZANDT. It is my understanding when industry is licensed to handle atomic material, they have to then meet the requirements laid down either by the National Bureau of Standards or the Atomic Energy Commission. Is that right?

Mr. TAYLOR. This is correct. And the license requirement calls for living within or below the center region. At some time they may decide to accept the new recommendations and work within the white region.

Representative VAN ZANDT. Do you have any information on the experience of the Navy as far as the *Nautilus* and the *Seawolf* are concerned?

Mr. TAYLOR. As to the exposure of the people?

Representative VAN ZANDT. Yes.

Mr. TAYLOR. I do not have any quantitative information. I will tell you this, though, from personal experience: I made a 24-hour trip on the *Nautilus* about a year ago, and I took onto the ship with me a very sensitive pocket dosimeter, which laid behind the reactor. I could hardly read any radiation, and I should guess that would be the hottest spot, or 1 of the 2 hottest spots on the ship.

Representative VAN ZANDT. Does your experience go back to Arco, Idaho, when the reactor initially went critical?

Mr. TAYLOR. I have no experience with that particular situation, and no knowledge of it.

Representative VAN ZANDT. Then your experience is with the reactor on board the *Nautilus?*

Mr. TAYLOR. Purely a small personal experience.

Representative VAN ZANDT. Do you know of any naval personnel that may have been exposed to a dose beyond the limits that you mention?

Mr. TAYLOR. No, sir; I do not.

I am, understand, getting back to the *Nautilus* for a moment, that you are just about as safe from radiation in the *Nautilus* as you are in the average environment.

Senator ANDERSON. Let me ask you a question. You have me curious now. I went aboard the *Nautilus*, carrying a dosimeter. It registered an increase in radiation. Am I to be regarded as more suspect, as a more rapid carrier?

Mr. TAYLOR. You probably had a hotter wristwatch on than I had. I do not know, sir. As I say, what I did was not done for the purpose of making a quantitive measurement, but just done for a lark.

Representative HOLIFIELD. Of course, it is true they have a full-time doctor on the *Nautilus*, and they have taken every precaution known to science to protect the crew and the people on there from radiation; and this is not to be compared with free radioactive particles that are floating around in the atmosphere.

Mr. TAYLOR. This is quite true. This, of course, also applies to large Commission operations. It is one of the reasons why you allow occupational exposures to be higher than nonoccupational exposures, the individuals who are so exposed, are so well watched over.

Representative HOLIFIELD. You have a continual screening of the air to remove any dust particles in the *Nautilus*, and any solids that might be in the atmosphere, certainly, which gives a purer air to the inhabitants of the *Nautilus* than you would get outside where the free dust is blowing.

Mr. TAYLOR. This is probably correct.

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. One more question.

About a week ago, the newspapers carried stories concerning a resident of Texas who had been exposed to radiation, and the stories stated he was completely isolated by not only his fellow employees, but by his neighbors who refused to associate with him. Are you familiar with that situation?

Mr. TAYLOR. No, sir. I know there was some situation down there, but I was away at the time that occurred, and I only know about it secondhand.

Senator BRICKER. Mr. Chairman?

Representative HOLIFIELD. Senator Bricker.

Senator BRICKER. I am a bit mystified as to how you got this 20-day figure or 22-year figure as the shortening of the life period as a result of radiation, since there is no human experimentation possible from which to come to that conclusion. Could it be that you have had animal experimentation from which you deduce that conclusion?

Mr. TAYLOR. You are asking me, sir, questions that I am not in the best position to answer. Dr. Russell is in the room at the present time if you would like to have him answer this.

Representative HOLIFIELD. This is quite complicated, and I think when Dr. Russell comes to the stand, we will go into it in some detail.

Mr. TAYLOR. I think this would be the better way to do it.

Representative HOLIFIELD. I think you had better proceed at this time with your statement.

Mr. TAYLOR. I have talked thus far about occupational exposures to radiation, and I should next point out that for nonoccupational exposure we have set levels that are one-tenth of those for occupational exposure. This was done several years ago for the first time—I think 1953—partly for genetic reasons and partly for somatic reasons. It is also partly a matter of ethics to keep this dose as low as possible, because here you are exposing people where they are in no position to control their own environment.

In connection with these low nonoccupational exposures, it is desirable, when possible, to take advantage of occupancy factors and exposure factors; in other words, the fraction of the time that a person may be exposed or not exposed. There is some evidence that some of these lowered values, these one-tenth values, are going to make for too severe restrictions in certain limited areas, and there is a possibility that they may be slightly raised in the future.

We come, next, to the question of population dose, and here the recommendation has been made that the permissible exposure to the population as a whole should not exceed 14 million man-rems per million of population up to age 30, and one-third of this value per decade thereafter. This value might be changed from 14 million to 15 million, simply to make the arithmetic easier, because we really do not know enough about the problem to know the difference between 14 and 15 anyway.

This means that persons in a population group of, say, a million people can receive an average dose per person of 14 million man-rems, including background radiation. This also implies that some persons may receive subsbtantially more than 14 million man-rems, provided others receive substantially less. This is an average population dose.

I have a chart (exhibit 8, slide 1) that would be of some interest in providing some perspective as to the sources of radiation with which we are dealing these days.

This is a chart that indicates in a very rough way the dose distribution in the population for people up to age 30.

Of this, the background will provide a dose of about 4 million man-rems per million of population.

Representative HOLIFIELD. Is that the natural background?

Mr. TAYLOR. That is the natural background; yes, sir.

There is slight uncertainty in all of these figures, but that is close enough. This, of course, applies to a hundred percent of the population.

Then we have from medical usage, exposures which have been estimated at somewhere between 1 and 5 million man-rems per million population. The figure 1 is the European figure, and the figure 5 is the American figure, and I suspect that the true figure, if it is ever found, will be somewhere between the two.

Representative VAN ZANDT. Is that where you use radiation for medical purposes?

Mr. TAYLOR. That is correct. This you think of as applying to 100 percent of the million people we are talking about. Actually some will get no radiation, and others will get more.

Representative VAN ZANDT. Dr. Taylor, is that the amount that the individual will be allowed until he reaches 30 years of age; is that correct?

Mr. TAYLOR. This is what they are getting now, as far as we can judge from present practices.

Representative VAN ZANDT. Spread over a period of 30 years?

Mr. TAYLOR. Yes.

Representative VAN ZANDT. Suppose they take the dose all at once.

Mr. TAYLOR. Within reasonable limits, this might be all right. There may be extreme cases where it would not be all right.

The total allowable amount we are going to talk about is this 14 million man-rems per million population.

Now, our occupational exposure at the new levels of 5 man-rems per year average would put into the population 150,000 man-rems as compared with 4 million and 5 million man-rems from medical exposure. This, however, as you see, applies only to a third of 1 percent of the population, which is an estimate that we think is reasonable for some years to come.

Then we allow in the environs of a plant, one-tenth of the occupational levels, and that would contribute another 150,000 man-rems to the population dose. And we have just made a guesstimate that that might apply to 1 percent of the population. To give you some idea of the reasonableness of this estimate, that would be something of the order of 16 million people living in the immediate environs of radiation sources, which I suspect is a very high figure for today.

Finally, we have last, "Fallout" for which I have allotted 200,000 man-rems per million of population. I think the National Academy figure here would have come to 100,000. Other estimates have been higher, but still you see it is quite small.

If you total these up, the things we know about, and the fair guesses, this comes to about 9½ million man-rems. This means that if you allow 14 million total that we have a balance of about 4 million man-rems that can be used up in other ways. Some of this will be used up in radioactive devices in the homes. You might use some of this in medical exposure, you might use some of this in fallout, and so on. But there is still a little room for motion here, as far as our uses of radiation are concerned.

Representative VAN ZANDT. Dr. Taylor, are these figures based on the radiation in the atmosphere today plus what will be in the atmosphere, say 5, 10, or 15 years from now?

Mr. TAYLOR. No, sir. These are based on the situation today.

Representative VAN ZANDT. Today?

Mr. TAYLOR. Right. Yes.

I am sorry. I skipped one point in connection with this previous discussion which I would like to return to, if I may, for just a moment. This has to do with working within the white region under our new levels.

So long as a person's dose accumulates at such a rate that he never exceeds the line at the top of the white area, he can acquire his dose at varying rates. If he starts work late in life, say at age 35, he can accept his dose at the rate of 15 man-rems per year, until he begins

to get up close to the top, at which time prudence would say he should taper off a bit. This is one of the nice features about this concept, because it gives the plant enormous latitudes in designing their safety operations. This is discussed in item 7 of the exhibit material.

Representative HOLIFIELD. You will submit both charts for the record, will you not?

Mr. TAYLOR. Yes, sir; and with reference to the other chart I showed, I would refer you to item 8 of the material.

Now, these standards that I have been talking about apply to the whole body, the lens of the eyes, and the gonads. They apply to all kinds of radiation, but obviously we need a great deal more information than we now have with regard to the RBE for the different radiations, and for the different organs.

Representative VAN ZANDT. May I ask a question at this point?

Mr. TAYLOR. Yes, sir.

Representative VAN ZANDT. Mr. Taylor, suppose an individual takes the limit of the dose up to the age of 30, then he leaves the industry, and comes back at age 50, and starts all over again. What has happened to the dose he took prior to and up to the time he was 30 years of age?

Mr. TAYLOR. There would probably have been some recovery from that dose, but for control purposes, you will assume there has been no recovery.

Representative VAN ZANDT. In other words, you assume that some radiation is still in his body?

Mr. TAYLOR. That is what you would assume. He would then between age 30 and 50 have been out of radiation work for 20 years. For each of those 20 years, he would have built up an allowance of 5 man-rems, or a hundred man-rems total. He can then take 15 man-rems per year until he begins to get up close to that, and then he would begin to flatten off again.

Representative HOLIFIELD. Doctor, we are going to have to move along. We are running a little behind schedule, and we have two other witnesses this morning. Are you in a position now to begin your summary?

Mr. TAYLOR. Well, I can do that.

Representative HOLIFIELD. How many more items had you planned on discussing?

Mr. TAYLOR. There were several. There has been a lot of side discussion I had not counted on.

Representative HOLIFIELD. I know there has. It is not your fault. Go ahead.

Mr. TAYLOR. I will touch very quickly on some of these points.

Representative HOLIFIELD. I understand you are going to submit for the record a complete treatment of each one of these subjects.

Mr. TAYLOR. This is correct; yes.

In dealing with the exposure of the body to different kinds of radiation, whether it be external sources of radiation or internal sources of radiation, you would have to treat each source and each body organ separately, the dose to those organs, the RBE for that dose, and so on. This again is an extremely complicated problem, and

one about which we have relatively little quantitative knowledge at the present time.

The new recommendations that I have been referring to and that will make for a lowering of the accumulated dose will not apply in this same way to internal emitters. Most of the permissible dose levels for internal emitters will remain at the values that they are currently. This is because only partial body exposure is involved, and, therefore, you do not need to go into the same extensive lowering of internal dose that you do for external dose.

This, incidentally, is going to somewhat simplify, or at least not make more complicated the atomic energy operations as they are now being carried out.

With relation to strontium 90, the permissible dose of strontium 90 in the body is related to the knowledge that we have of the effects of radium in the body, and primarily in the bone. You cannot compare these exactly because radium is an alpha emitter, and strontium is a beta emitter. In the process of arriving at a permissible dose for strontium various factors of uncertainty have been resolved in such a way that I believe I am correct in saying the permissible levels for strontium may be as much as 15 times more conservative than the levels we have for other radioactive isotopes. This will undoubtedly be discussed by one of the later speakers.

Without going into any discussion, I will introduce into the record discussion of the various international and national committee activties by introducing items 9 through 15 of the supporting material.

In connection with the question of permissible dose standards, there are, as I have indicated, a large number of variables, and a large number of uncertainties. We do not have now, and probably will not have for a long time, any quantitative evaluation of the risks involved. What somebody has to do, is to evaluate the risk due to radiation against the benefits from the use of radiation, and the risk due to other things that affect our normal life.

In this connection I frequently feel compelled to say that this question of radiation safety and permissible dosage standards is not a subject for which there is a clean and simple answer. The whole question of setting radiation exposure limits depends on physics and biology. It depends enormously on ethics and morality, and on an enormous amount good judgment and good wisdom on the part of the people who are responsible for setting them. It is by no means a clean-cut quantitative physical problem.

I was asked specifically to comment on the possible effects of the use of different protection standards by different nations, as to whether this might introduce some kind of misunderstanding between nations. This is fortunately an academic question at the present time.

In this country, the States and the Federal Government use the recommendations of the National Committee on Radiation Protection. As far as I am aware, all countries in the world, including the Iron Curtain countries, use the recommendations of the International Commission on Radiological Protection. These are in exact agreement with those that are used in this country. Perhaps they could not help but be because of the close relationship between the United States national and the international committees.

A problem does exist, at the present time, with regard to both national and international bodies working on these questions. This problem had to do with the plethora of organizations that are setting up various kinds of protection groups and protection studies. The manpower that is knowledgeable in this area is very limited, and the same people are called on repeatedly, wearing different hats, to engage in different and overlapping activities. To avoid this, the international commissions are presently setting up formal working relationships with various other international organizations—WHO, ILO, UNESCO, ISO, and so on—about a dozen organizations. These have their own special interests, but by common consent they now look to the two international commissions for their basic guidance.

One of the big question marks in the future is whether the new International Atomic Energy Agency will do this also. It is to be hoped that they will.

I think at this point, sir, I will stop. I have gone way over the time that was originally allotted me.

Representative HOLIFIELD. I am sure that your presentation has been very valuable to us, and I know that the documentation which you told me of previous to the meeting will be additionally valuable. You did take up in this documentation the comparison of strontium 90, the 100 permissible dose as against the 1,000?

Mr. TAYLOR. The sunshine unit?

Representative HOLIFIELD. Yes.

Mr. TAYLOR. No, sir; that is not in my documents. I have no documents on that, but I am sure that will be documented from other sources in the course of the hearing.

Representative HOLIFIELD. Thank you very much, Mr. Taylor.

(Mr. Taylor's full statement with exhibits follows:)

STATEMENT OF DR. LAURISTON S. TAYLOR, CHIEF, ATOMIC AND RADIATION PHYSICS DIVISION, NATIONAL BUREAU OF STANDARDS

The main purpose of this part of the discussion before your committee is to give some background in the matter of radiation protection standards, and to indicate areas of uncertainty and reasonable certainty in this field. Mention will also be made of some specific areas of research that should have more emphasis.

RADIATION UNITS AND STANDARDS

Until about 1928 any protective measures that we had were on an almost purely guess-work basis because of lack of radiation units and standards. The adoption of the roentgen in 1928 was the first step leading to the first quantitative radiation protection standards 6 years later. The roentgen was the only radiation unit until the early 1930's and applied only to X- and gamma-rays. Of course until the early forties' we had essentially no other radiations to worry about. The roentgen is still an important and useful unit of exposure dose within its limitations.

Recognizing the need for a universal unit of dose, the International Commission on Radiological Units and Measurements, in 1953, adopted the rad, a unit of absorbed dose that applies to all radiations. The rad measures the energy imparted to matter by the ionizing particles, per unit mass of irradiated material at the place of interest, and is equal to 100 ergs per gram. However, it is still difficult to measure a rad directly and on this much more developmental research is needed.

Dose rate is a commonly used term. As its name implies it is the rate of administering a dose. In many radiation effects the rate of administering the dose can have an important influence on the ultimate biological effect. It

may be compared with a dose of aspirin. You might say that one tablet was one dose, or that eight tablets was one dose. In the latter case it makes a great deal of difference whether the prescription says to take the eight tablets all at once, or eight per hour, or eight per day. The results upon the individual might be quite different under the various circumstances just noted.

The rep is a unit that has been mentioned several times in previous testimony. This unit was invented during the early Manhattan District days for equating various radiation effects of different kinds of radiation. One of the difficulties with the rep at present is the fact that it has been redefined at various times with the result that one is never exactly sure as to what is meant by the term. In view of present-day knowledge it is certainly not 93 ergs per gram as mentioned by one of the earlier speakers. It is our hope that the use of the rep will be completely discontinued.

The RBE, standing for relative biological effectiveness, is a very important term, and is used in equating the biological effects of various kinds of radiation, such as alpha, beta, gamma, and X-rays, neutrons, etc. All values of RBE are related to an assumed value of RBE=1 for X-rays of the energy more commonly used therapeutically. The RBE depends upon many factors, such as the kind of radiation, body organ, kind of effect caused, absorbed dose, absorbed dose-rate, dose fractionation, pH, oxygen tension, temperature, etc. Because of its dependence on all of these factors, it is not too surprising that our present-day knowledge of RBE values is not all that it should be. For alpha particles, the RBE may vary between 2 and 10, depending upon the interpretation of various experiments.

At the present time the RBE is known only roughly in many important areas and much more research is needed in the study of this factor. The National Committee on Radiation Protection (NCRP) has a subcommittee working actively on this whole question under the direction of Dr. Wright Langham of Los Alamos. At present the subcommittee is compiling and correlating a vast amount of data and are developing specific areas where further research is needed. It should be pointed out that our lack of knowledge of the RBE is probably not introducing a danger factor in the setting of our permissible dose levels. We can make some reasonable estimates of the upper and lower limits of the RBE for particular circumstances, and where there is any uncertainty a higher or more conservative value is chosen. On the other hand, use of too high values for the RBE can cause us to be over-protecting ourselves, at some considerable cost.

Another radiation unit is the rem, a unit of RBE dose, which essentially includes all of the factors entering into the biological dose as well as they may be known. An RBE dose expressed in rems is equal to the product of the absorbed dose in rads and the RBE. Thus, RBE dose in rem=Σ[absorbed dose (rads) x RBE].

The Curie is not a unit of dose but a unit of an amount of radioactive material. It is related to the number of disintegrations that occur per second in radioactive material and this, in turn, specifies the amount. It may be compared with a unit of mass and, in fact, one curie is equivalent to 1 gram of radium

Another unit used in connection with Sr–90 problems is known as the "sunshine unit." This is related to the maximum permissible average body burden of Sr–90 in the population outside of controlled areas. One sunshine unit is equal to one-hundredth of the average permissible body burden for the general population. If one considers that the maximum permissible body burden for radiation workers is one micro-curie of Sr–90, then the permissible body burden for persons outside of controlled areas would be 0.1 microcuri of Sr–90. Since the body contains about 1,000 grams of calcium this body burden would amount to 0.1 microcuries of Sr–90 per thousand grams of calcium. This reduces to 100 $\mu\mu$curies of Sr–90 per gram of calcium. One sunshine unit is one-hundredth of this quantity.

STATUS OF UNITS

On the whole it is considered that our situation with regard to radiation units is fairly well in hand, as exemplified by the latest report of the International Commission on Radiological Units and Measurements in 1957.

Exhibit 2. National Bureau of Standards Handbook 62, Report of the International Commission on Radiological Units and Measurements (ICRU) Pages 5–7, 1956

I. Quantities, Units, and Symbols

1. DEFINITIONS OF QUANTITIES AND UNITS [1]

1.1.[2] Absorbed dose of any ionizing radiation is the energy imparted to matter by ionizing particles per unit mass of irradiated material at the place of interest.

1.2. The unit of absorbed dose is the rad. 1 rad is 100 ergs/g.

1.3. Integral absorbed dose in a certain region is the energy imparted to matter by ionizing particles in that region.

1.4. The unit of integral absorbed dose is the gram rad. 1 gram rad is 100 ergs.

1.5.[2] Absorbed dose rate is the absorbed dose per unit time.

1.6. The unit of absorbed dose rate is the rad per unit time.

1.7. Exposure dose of X- or gamma radiation at a certain place is a measure of the radiation that is based upon its ability to produce ionization.

1.8. The unit of exposure dose of X- or gamma radiation is the roentgen (r). One roentgen is an exposure dose of X- or gamma radiation such that the associated corpuscular emission per 0.001293 g of air produces, in air, ions carrying 1 electrostatic unit of quantity of electricity of either sign.

1.9. Exposure dose rate is the exposure dose per unit time.

1.10. The unit of exposure dose rate is the roentgen per unit time.

1.11. Intensity of radiation (radiant energy flux density) at a given place is the energy per unit time entering a small sphere of unit cross-sectional area centered at that place.

1.12. The unit of intensity of radiation may be erg per square centimeter second, or watt per square centimeter.

1.13. The unit of quantity of radioactive material, evaluated according to its radioactivity, is the curie (c). One curie is a quantity of a radioactive nuclide in which the number of disintegrations per second is 3.700×10^{10}.

1.14. Specific gamma-ray emission (specific gamma-ray output) of a radioactive nuclide is the exposure dose rate produced by the unfiltered gamma rays from a point source of a defined quantity of that nuclide at a defined distance.

1.15. The unit of specific gamma-ray emission is the roentgen per millicurie hour (r/mch) at 1 cm.

1.16. Linear energy transfer (LET) is the linear-rate of loss of energy (locally absorbed) by an ionizing particle traversing a material medium.

1.17. Linear energy transfer may be conveniently expressed in kilo electron volts per micron (kev/μ).

1.18. Mass stopping power is the loss of energy per unit mass per unit area by an ionizing particle traversing a material medium.

1.19. Mass stopping power may be conveniently expressed in kilo electron volts per milligram per square centimeter (kev cm^2/mg).

Notes on the following definitions

The numbering corresponds to the paragraphs above.

1.1 Absorbed dose.

(a) In the definition of absorbed dose "energy imparted to matter" means energy retained by matter and made locally available at the place of interest. The absorbed dose includes all energy absorbed per gram of any material under consideration. Thus it includes the energy of nuclear collisions as well as that of electronic collisions.

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[1] Symbols and nomenclature. There are numerous national and international bodies that have reached varying degrees of acceptance of the use of symbols and units for physical quantities. However, there is no universal acceptance of any one set of recommendations. It is suggested that each country modify the symbols used herein, in accordance with its own practices. Thus one may write: kev, keV, or Kev; ^{14}C or C^{14}; rad per unit time, rad per time, or rad divided by time; rad/sec, rad/s, or rad's^{-7}; etc. The most generally accepted system of symbols and units may be that contained in Document UIP 6 (1955) prepared by the International Union of Pure and Applied Physics. These are in fairly close agreement with the recommendations of the International Standardization Organization project ISO/TC 12, the Conférence Générale de Poids et Mesures, Union Internationale de Chimie Pure et Appliquée, and the International Electrotechnical Committee. No effort is being made in the present report to conform to the standards recommended by the above organizations.

[2] See notes following these definitions.

(b) Because the rad does not specify the medium, the medium should be stated unless it is clearly implied. For example it is convenient to use the term "tissue rad," which corresponds to a rad at the point of interest in soft human tissue.

(c) Absorbed dose may be estimated by application of the cavity relation whereby the energy imparted to a solid per unit mass, $(\Delta E/\Delta m)=E_m$, is related to the ionization per unit mass of gas, $(\Delta J/\Delta m)=J_m$, by the equation

$$E_m=J_m W s_m, \qquad (1)$$

where s_m is the ratio of the mass stopping power of the material to that of the gas.

(d) Whenever ionization methods are used for the estimation of absorbed dose, the observed ionization must be multiplied by a quantity W equal to the average energy expended by ionizing particles in the production of an ion pair in the gas.

The value of W for X- and gamma radiation probably lies between 33 and 35 ev for air. It is recommended that the value of $W=34$ ev be used for calculations involving X- and gamma radiation of quantum energy greater than 20 kev. Section 5 contains the currently recommended values of W and s_m as needed in X- and gamma radiation and neutron dosimetry.

1.5 Absorbed dose rate.

It should be pointed out that there are special situations when the absorbed dose rate should be expressed more explicitly. For example: when the absorbed dose rate is not constant during the time of irradiation, it may be desirable to specify also the instantaneous absorbed dose rate. It is recognized that the term "instantaneous" may not always be sufficiently explicit and that perhaps a statement should be added referring specifically to an absorbed does rate of pulsed radiation averaged over a single pulse.

1.7 Exposure dose of X- or gamma radiation.

(a) The translation of "exposure dose" into German is "Bestrahlungsdosis," into French "dose d'exposition," and into Spanish "dosis de exposición."

(b) Although the definition of exposure dose was purposely stated in loose terms, a more physically specific definition might be as follows: "the exposure dose is measured by the ion charge, ΔQ, of either sign, produced in air by the secondary electrons, which are produced by X- or gamma radiation in a small mass, Δm, of air divided by Δm." Note that according to the above definition, ΔQ is *not* the charge measured in Δm. However, under electronic equilibrium conditions, the charge produced in Δm is numerically equal to ΔQ (see sections 1.8 (a), 4.2.a, and 5.2).

(c) The wording of this definition leaves open the possibility of later defining exposure dose for radiations other than X- or gamma rays.

1.8. The roentgen.

(a) According to the definition, a dose of one roentgen is obtained at a point if the high-speed electrons generated in 0.001293 g of dry air at that point produce along their track 1 esu of ions of either sign. Accurate measurements in roentgens are not obtained by actually measuring these ions. Instead the concept of electronic equilibrium is used so that one can measure the ionization per 0.001293 g of air. According to this concept the ionization produced outside of a small mass, m, by high-speed electrons generated inside of m is compensated by ionization produced inside m by electrons generated outside of m (Fano, 1954).

(b) The corpuscular emission shall not include contributions due to secondary X- or gamma radiation produced in the quantity of air in which the corpuscular (electron) emission referred to is generated. This follows from a consideration of the actual definition of the roentgen and the concept of electronic equilibrium.

(c) It becomes increasingly difficult (because of electronic equilibrium limitations) to determine the exposure dose in roentgens as the quantum energy of the X- or gamma radiation approaches very high values. For practical purposes, 3 Mev is arbitrarily regarded as the useful upper limit of the energy range over which the roentgen should be used.

1.9. Exposure dose rate.

(a) Exposure dose rate can be used to specify a field of irradiation or the output from an X- or gamma-radiation source up to 3 Mev.

(b) For quantum energies above 3 Mev, the ICRU at present is not in a position to make a firm recommendation on the specification of output from a radiation source. One may use either intensity, or the absorbed dose rate at

the peak of the buildup curve in a phantom under specified conditions; the latter may be derived from ionization measurements.

(c) It should be pointed out that there are special situations when the exposure dose rate should be expressed more explicitly. For example, when the exposure dose rate is not constant during the time of irradiation, it may be desirable to specify also the instantaneous exposure dose rate. It is recognized that the term "instantaneous" may not always be sufficiently explicit and that perhaps a statement should be added referring specifically to an exposure dose rate of pulsed radiation averaged over a single pulse.

2. RECOGNIZED SYMBOLS

2.1 RBE (relative biological effectiveness). RBE is used to compare the effectiveness of absorbed dose of radiation delivered in different ways. It has been commonly represented by the symbol η. It signifies that m rads delivered by a particular irradiation procedure produces a biological response identical with that produced by $m\eta$ rads delivered by a different procedure.

The statement that "the RBE of α radiation relative to γ radiation is 10" signifies that m rads of α radiation produces a particular biological response in the same degree as $10m$ rads of γ radiation. This statement may be further summarized as $\eta\frac{\alpha}{\gamma}=10$.

The concept of RBE has a limited usefulness because the biological effectiveness of any radiation depends on many factors. Thus the RBE of two radiations cannot in general be expressed by a single factor but varies with many subsidiary factors, such as the type and degree of biological damage (and hence with the absorbed dose), the absorbed dose rate, the fractionation, the oxygen tension, the pH, and the temperature.

2.2. RBE dose is equal numerically to the product of the dose in rads and an agreed conventional value of the RBE with respect to a particular form of radiation effect. The standard of comparison is X- or gamma radiation having a LET in water of 3 kev/μ delivered at a rate of about 10 rad/min.

2.3. The unit of RBE dose is the rem. It has the same adherent looseness as the RBE and in addition assumes conventional and not necessarily measured values of RBE. It is therefore recommended that its use be restricted to statements relating to radiation protection. For example, the statement might be made:

> The permissible weekly whole body RBE dose is 0.3 rem regardless of the type of radiation to which a person is exposed.

Should occasion arise when results have been evaluated with other than agreed conventional values of RBE, the values used should be clearly stated.

In the case of mixed radiations the RBE dose is assumed to be equal to the sum of the products of the absorbed dose of each radiation and its RBE:

RBE dose in rems=Σ[(absorbed dose in rads)$\times$RBE].

(Mr. Taylor's statement—continued.)

Standards of all of the basic physical units have been established and are maintained at the National Bureau of Standrds. However, further research is needed in certain areas.

More research and development is needed on the units and standards of absorbed dose (rad). The physical aspect of this problem should be carried out at the National Bureau of Standards, but here there is a severe difficulty, through the inability to secure properly qualified personnel at salaries that can be paid by the Government. In addition to this, considerable research on clinical applications of the absorbed dose concept should be developed in medical and biological institutions.

As noted above more research is needed on the RBE. This, in turn, depends upon the studies of absorbed dose and absorbed dose standards. The biological aspects of this work are already being carried out by the Atomic Energy Commission and various biomedical groups, but more effort is need.

STANDARDS OF PERMISSIBLE DOSE FROM SOURCES OF RADIATION EXTERNAL TO THE BODY

Until 1947 standards of permissible dose were based almost solely upon the radiation effects on the individual. There was an awareness that this might not

be sufficient but as already noted quantitative evidence and other effects was lacking. In 1947 considerable new evidence had been developed, as a result of studies in the Manhattan District project. These gave strong indications that genetic and longevity effects should be considered but again lack of a sufficient evidence at that time prevented their quantitative use. The effects, if any, were either marginal or were of such a long-range value that their true assessment could be attained only by critical statistical analysis. At the time we were not yet ready to go forward with this.

EXHIBIT 3. NATIONAL BUREAU OF STANDARDS HANDBOOK 50, PERMISSIBLE DOSE FROM EXTERNAL SOURCES OF IONIZING RADIATION, SECTION 3.9 AND 3.10, PAGES 17–20, SEPTEMBER 1954

3.9. GENETIC EFFECTS

Ionizing radiations are capable of producing changes in indivdual genes and chromosomes in all nucleated body cells. The subsequent manifestations of these primary effects (when sufficiently marked) are generally deleterious to the individual in his lifetime and to future generations when they occur in the germ cells. It has been shown experimentally that genetic changes can be produced with low doses of radiation. The frequency of occurrence increases linearly with the dose in the case of gene mutations and is independent of the duration of the exposure. In the case of chromosome breaks with subsequent abnormal union of some fragments (e. g., translocation) the frequency of occurrence depends also on the dosage rate, within certain limits. It is evident that wheher an individual is particularly susceptible or not, some injury of this type is unavoidable. Some cells in his body, including some germ cells, will be genetically altered. However, *genetic changes of the same kind occur spontaneously* and one is not dealing with a mysterious injury of an entirely new type. The main point is to control exposure in such a way that the eventual manifestation of genetic injury is not too large in comparison with the occurrence of spontaneous genetic abnormalities. Insofar as the welfare of the race is concerned (i. e., future generations) gene mutations with inconspicuous manifestations play the most important part. The controlling factor is then the number of undesirable genes (both spontaneous and radiation-induced ones) present in the general population in which intermarriage occurs. It is, therefore, immaterial in this case whether in one generation the undesirable genes are present largely in a few individuals or are distributed throughout the population in correspondingly smaller number per individual. Accordingly, the amount of radiation received by the gonads of one individual up to the time of conception of the last child in his family, can be very large without noticeably damaging the population as a whole—provided that only a very small fraction of the whole population is exposed to this extent. Under present conditions and for some time to come, genetic damage to the population as a whole in future generations is not a limiting factor in setting up a permissible level for occupational exposure to ionizing radiation. For other reasons the level must be considerably lower than might be set on the above grounds. However, it is well to bear in mind that this factor assumes greater importance as the percentage of the population exposed to radiation increases. Moreover, it should be realized that any amount of radiation received by the gonads of even a few individuals before the end of their reproductive period is likely to add to the number of undesirable genes present in the population. While the majority of these genes may have no recognizable effects for a number of generations, practically all are potentially bound to result eventually in undesirable conditions.

Considering now genetic damage manifestable in the lifetime of the individual or in the first-generation offspring, it is obviously necessary to limit the exposure of *every* individual. Chromosomal damage in somatic cells may be responsible, at least in part, for radiation injuries that become evident in the lifetime of the exposed individual. Very little is known about this (which in essence has to do with the mechanism of the action of radiation), but a great deal is known about the observable effects themselves. For purposes of protection it is sufficient to choose a level of exposure that will effectively prevent the occurrence of the injurious effects no matter how they are produced. Genetic changes manifestable in the first-generation offspring are of concern to the exposed individual, since his well-being depends in no small degree on psychological factors in his family life. Sterility, stillbirths, and abnormal children may be produced by overexposure of radiation. Most of the information on these effects has been obtained from animal experiments, but it may be taken for granted that the same effects

occur in man. However, practical experience indicates that undesirable effects of this nature, if present, have not been so marked as to attract attention, in the case of radiologists and technicians who have been occupationally exposed to radiation—sometimes excessively, as shown by other more obvious injuries. It should be noted in this connection that sterility, stillbirths, and abnormal children occur in nature spontaneously or for reasons in which exposure to radiation plays no part. In any particular instance, it is therefore extremely difficult to attribute any such effect to radiation.

3.10. EFFECT ON LIFESPAN

Experiments performed with laboratory animals (chiefly mice and rats) show that exposure to radiation *in sufficient amounts* shortens the average lifespan. This has been found to be true under a variety of different conditions of irradiation, including daily exposures and single treatments. In all these experiments survival curves of the irradiated animals are compared with survival curves of a control group. Because there is always considerable biological variability, small differences in survival curves may occur in the control groups themselves. Hence small differences caused by exposure to radiation are obscured and cannot be considered significant. In order to establish small differences it is necessary to use very large numbers of animals (of the order of thousands rather than dozens) and to take many precautions. Because the number of animals used in such experiments has been too small, it has been customary to extrapolate to smaller doses the results obtained with doses so large that significant differences could be established. Following this procedure it may be shown that an appreciable shortening of the lifespan occurs in mice and rats exposed daily to doses of X-rays in the neighborhood of 0.1 r. Whether this extrapolation is justified or not cannot be decided at the present time. Experimental data on lifespan obtained with other laboratory animals are quite fragmentary and extrapolation to low daily doses is even more uncertain. No quantitative information is available in the case of man. Because the possibility of a shortening of the lifespan in man by small daily doses cannot be excluded, the available experimental data may be assumed to indicate the desirability of lowering the permissible daily dose for lifetime exposure of the whole body to penetrating radiation.

Essentially the same situation exists in connection with the interpretation of other gross effects produced by continued exposure of the whole body to penetrating radiation. Small effects are difficult to determine accurately unless very large numbers of experimental and control animals are used.

(Mr. Taylor's statement—continued.)

In 1952 the International Commission on Radiological Protection arrived at tentative agreement regarding an average population dose that would take into consideration the genetics hazard. However, actual promulgation of any recommendations were held up because it was not considered that the urgency was very great and that, in fact, it would be wiser to await better information. It is interesting to note that the tentative agreements of 1952 were in close accord wih those later recommended by the ICRP in April 1956 and later by the National Academy of Sciences in the same year.

EXHIBIT 4. NATIONAL BUREAU OF STANDARDS TECHNICAL NEWS BULLETIN, MEETINGS ON RADIATION UNITS AND PROTECTION, VOLUME 40, NO. 7, JULY 1956

MEETINGS ON RADIATION UNITS AND PROTECTION

Important decisions relating to the measurement and safe use of radiations were announced at the triennial meetings of the International Commission on Radiological Units and the International Commission on Radiological Protection, both of which were held April 2–11, 1956, in Geneva, Switzerland.[1] In a move to achieve greater international uniformity of X-ray measurements, the ICRU initiated a project in which basic equipment, to be developed by the Bureau, will be loaned to countries that lack primary standards of their own. The principal outcome of ICRP deliberations, on the other hand, was a revised set

[1] The reports of the two commissions are expected to be published in the late fall of 1956. The ICRU report will appear in the form of an NBS Handbook and the ICRP report will be published as a supplement to the British Journal of Radiology.

of recommendations for permissible levels of ionizing radiation to which human beings may be exposed. Another highlight of the ICRU meetings was a discussion of problems raised in translating measurements made in roentgens to the newer unit of absorbed dose, the *rad.*

UNITS COMMISSION

The ICRU has been in active existence since it was established by the first International Congress of Radiology in 1925. Its major objective is the establishment of units of radiation dosage for use in medicine and biology, and in this field it has set the world pattern for over 30 years. At the same time, it deals with special problems of radiation measurement in both clinical and biological applications, and establishes the necessary nomenclature and descriptive procedures in regard to radiation doses.

Problems facing the ICRU have become increasingly complex as radiation energies have moved to higher and higher values, and as other ionizing radiations requiring measurement, such as neutrons, have been introduced. An important step in advancing the common base for all radiation measurements was taken in 1953 with the introduction of an energy unit of measurement called the *rad.* The rad, equal to 100 ergs per gram of irradiated matter, for the first time expresses radiation dose directly in terms of basic physical units. Determination of dose in terms of the older unit, the roentgen, involves only ionization measurements, and the transition to the technique of energy measurement has been difficult. Much of the recent meeting was therefore devoted to developing background material needed to make energy measurements more practical under clinical and experimental conditions. The forthcoming report of the Commission will contain not only this extensive body of material, but also the physical and numerical factors needed to convert ionization measurements made with conventional instruments to energy measurements applicable to radiation absorption in tissue.

It was brought out at the ICRU meeting that few countries have primary standards of X-ray dosage with the requisite range and accuracy. Though in the recent or distant past, intercomparisons of primary standards have been made between the United States, Great Britain, Sweden, France, Germany, and Canada, a great many other countries that use X-rays and gamma rays from radioactive nuclides are without the benefits of central national laboratories. To improve this situation, the Commission recommended that a secondary X-ray standard be developed and constructed which can be loaned to other countries for the calibration of the working standards which in turn are used to control radiation measurements in their hospitals and research institutions.

The National Bureau of Standards was asked to undertake this program and has agreed to do so. The plan is for the Bureau to construct a suitable cavity ionization chamber which can be calibrated over a wide energy range against the NBS primary standards. To control the measurements of the ionization chamber, a standard capacitor will also be provided. The Bureau will also supply a standard diaphragm to be used in intercomparing free-air standards in those laboratories having them.

PROTECTION COMMISSION

Established in 1928, the ICRP has had the responsibility for setting the working levels of ionization radiation to which all persons may safely be exposed. When the Commission's work began, there was little thought that its findings and recommendations would play so important a part in the international picture of radiation use. It was fortunate, indeed, that its first recommendations on permissible exposure, made in 1934, were able to provide the guidelines for protection.

Work of the ICRP has been closely paralleled by that of the United States National Committee on Radiation Protection (NCRP), which was formed in 1929. The two groups have substantial overlapping of membership on their subcommittees which, in turn, are formed along similar lines. In fact, a part of the present international recommendations on radiological protection follows the pattern set by the NCRP.

The 1934 recommendations on permissible exposures were based mainly on the possible harm to the individual working with X-rays. Virtually no thought was given to possible genetic effects. However, as a result of the intensified study of this question during the Manhattan district days, and new evidence which indi-

cated the possibility of both genetic effects and long-range effects on the individual, a review of these questions was made by both the national and international commissions. In 1948, the NCRP recommended that the permissible levels of radiation exposure be reduced by a factor of about two. Thus the permissible exposure of 0.1 roentgen per day was lowered to 0.3 roentgen per week. The same basic figure was subsequently adopted by the ICRP in 1950.

Since 1950 it has become increasingly clear that larger fractions of the population will be exposed to radiation from both medical and atomic-energy sources. It therefore, became paramount to reassess the whole protection problem, and to take into account the newer evidence on possible long-range genetic effects and possible shortening of individual lifespan due to radiation exposure. The ICRP took up these problems at Stockholm in 1952, and its latest recommendations again provide for a lowering of the permissible exposure.

It is proposed that a lowering be achieved without changing the basic level of 0.3 roentgen per week. If this maximum level were maintained indefinitely, it would mean an exposure of about 15 roentgens a year, or between 400 and 600 roentgens per working lifetime. The commission has agreed that exposure of a substantial fraction of the population to this much radiation is undesirable. Accordingly, while adhering to the permissible exposure of 0.3 roentgen per week, the latest recommendations of the ICRP state that it is inadvisable for an individual to receive more than 50 roentgens up to his age of 30, 100 roentgens up to age 40, and 200 roentgens up to age 60. This means that individuals who are exposed to radiation at the maximum level will be allowed to work only one-third of the time. In effect, the normal daily working level is reduced by a factor of about 3.

This will not be as difficult to carry out as might at first appear, since most of the large atomic energy and other radiation establishments in this country are already operating at exposure levels considerably below even the new reduced amount. It is rare for workers, even today, to be exposed to more than about one-fifth of the present permissible amount.

The new recommendations will, however, introduce a penalty on those installations that insist upon exposing their workers to the maximum permissible weekly amount. Under the new rules, such workers may be exposed to this level for only one-third of their working time, the penalty thus taking the form of intermittent and hence uneconomic use of personnel. The inducement will therefore, be strong for such installations to improve their protection facilities to the point where the maximum radiation exposure will not exceed the newly recommended average levels.

NBS PARTICIPATION IN THE MEETINGS

The participation of the National Bureau of Standards in the international protection program dates from 1928, the year in which the ICRP was formed. Since then, the Bureau's radiation research program has grown steadily in variety and scope. It now includes the gathering of basic radiation data with the 50-million-volt betatron and 180-million-volt synchrotron, theoretical studies on radiation, penetration, and diffusion, development of instruments, for detecting and measuring radiation, and publication of handbooks on methods of safe handling of X-rays and radioactive nuclides. An accelerated program also has been started on the study of protection against neutrons.

The Bureau's participation, together with other representatives from the United States in the work of the two international commissions was markedly increased as a result of the recent meetings. In the case of the Bureau, this appears not only in its preparation of a portable X-ray standard, but in the parts assigned to the following NBS staff members in the work of the commissions. L. S. Taylor, chief of the NBS division of atomic and radiation physics, was renominated for an additional 3-year term as chairman of the ICRU and as a member of the ICRP. H. O. Wykoff, chief of the radiation physics laboratory, continues as member of the ICRP committee on protection against X-rays generated by voltages up to 3 million volts. He has for several years been chairman of the corresponding subcommittee of the NCRP. Dr. Wykoff was also nominated as a member of the ICRU and chairman of its committee on standards and measurements of radiological exposure.

W. B. Mann, chief of the radioactivity laboratory, was nominated cochairman of the ICRU committee on standards and measurement of radioactivity. H. F. Attix, member of the X-ray laboratory staff, was named to the ICRU committee on standards and measurement of absorbed dose. Mrs. S. Raskin, staff

member of the radiation physics laboratory, attended the meetings as technical secretary to the chairman. H. W. Koch, chief of the betatron laboratory, continues on the ICRP committee on protection against X-rays above 3 million volts, beta rays, gamma rays, and heavy particles, including neutrons and protons. S. W. Smith, chief of the radiological equipment laboratory, was nominated as a new member of the ICRU committee on standards and methods of measurements of characteristic data of radiological equipment and materials used in diagnostic and therapeutic radiology.

EXHIBIT 5. COMPTES RENDUS, PAGES 3134–37, JUNE 1956

(Translation from French)

Radiobiology:[1] An indispensable reduction of the tolerated dosage recommended by the International Commission for Radiological Protection. Note by M. Pierre-Octave Robert, presented by M. Jacques Trefouël.

The recommended tolerance dosage; i. e., maximum permissible concentration (suggested) by the 1953 Congress amounts to a cumulative annual dosage of 15 roentgens and to a cumulative dosage of 450 roentgens.

The annual dosage is a dosage which would be genetically dangerous in 30 years; the 30-year dosage would be 50 percent fatal (if absorbed in 24 hours) and in 30 years would cause a shortening of life by 3 to 8 years.

At a time when France and the entire world will see a considerable increase in the number of nuclear centers, the personnel protection problem becomes one of the greatest importance.

A. Maximum permissible concentration:

(*a*) The maximum permissible concentration formerly allowed was 0.1 roentgen per working day.[2]

(*b*) At the London Congress of 1950 and the Copenhagen Congress of 1953 the International Commission for Radiological Protection reduced the maximum permissible tolerance to 0.3 roentgen per week in the open air, or to 0.06 roentgen per working day.

B. The cumulative annual dose and the cumulative annual dosage for a generation of 30 years.

We shall determine these dosages—assuming that the tolerated dosage is reached every day.

We recognize that in nuclear study centers, that this tolerated dosage is only rarely reached, although it may be accidentally exceeded.

On the other hand, however, in the neighborhood of atomic piles and experimental nuclear reactors, one must consider the personnel who control the operations; the chemists and physicians who do the work can be exposed daily to a dosage which they incorrectly believe to be a safe dosage.

This is why we have felt it ncessary to draw up the following table:

Cumulative dosage							
		per year of			for 30 years		
daily	weekly	50 weeks	48 weeks	40 weeks	50 weeks	48 weeks	40 weeks
0.06r	0.3r	15r	14.4r	12r	450r	432r	360r

C. What do these cumulative dosages represent?

1. The cumulative dosage in 30 years, on the order of 400 roentgens, represents a dosage which would be 50 percent fatal if it were absorbed in 24 hours in a spot contaminated by an atomic bomb. The same cumulative dosage over 40 years represents a 100 percent mortal dosage under the same conditions.

2. For comparative purposes, we may say that for a period of 30 years the natural sources—the soil and cosmic radiation—supply a cumulative dosage of 3 roentgens at sea level. By way of exception, the inhabitants of Tibet, where the cosmic radiation is more intense, absorb a cumulative dosage of 5 roentgens per 30-year generation.

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[1] From Académie des Sciences, séance du 25 uin 1956, pp. 3134–3137.

[2] "r" concerns a quantity of X-rays or gamma rays; for general radiation (X, γ, α, β, neutrons) "rem" is used.

8. Comparison with experimental explosions of atomic bombs. The United States Atomic Energy Commission has established that the total quantity of radiation absorbed by Americans from all atomic explosions was—on February 15, 1955—0.1 roentgen, this for a total of 50 bombs exploded over a 4-year period. Finally, we may state that if atomic tests were to continue for another 30 years at the same rate, an average American would absorb approximately 0.75 roentgen in addition. What does this 0.75 roentgen represent in comparison with 400 roentgen of accumulated tolerance.

D. The genetic effects.

Several English and American documents give accounts of the work of Dr. H. J. Muller, a prominent geneticist, who believes that a doubling of the mutation rate of genes from generation to generation might bring about disastrous results. Dr. H. J. Muller believes that the received dosage must be limited to a quarter of the dosage which doubles the mutation rate. The AEC laboratory at Oak Ridge has determined the doubling dosage for mice. Experiments carried out lead to the conclusion that this dosage is 50 roentgens.

Extrapolating for human beings was believed possible, despite the uncertainties which this extrapolation causes by the fact of variations from one genus to another. Dr. H. J. Muller thus arrives at a maximum dosage per generation of 12.5 roentgens. This dosage is exactly the size of the dosage which would be absorbed in a year by a worker who absorbs the maximum permissible concentration daily. What would be the effects at the end of 30 years?

E. Effects on longevity.

It is certain that estimates of this sort can only be rather indefinite, as they are based on rather controversial statistical methods.

Statistical studies based on mortality estimates (due to all possible causes) have been carried out by Dr. Hardin Jones and Dr. K. Z. Morgan at Berkeley and Oak Ridge, respectively; in one case for an exposed population, and in the other for an unexposed population. They give results which agree very nearly on a factor of 2.

The result of this estimate is that if the dosage of radioactivity is given uniformly over a long period, that the expected length of life is shortened in the first case by 3 days per roentgen and in the other by 6 days per roentgen.

By elementary calculations, one can obtain—from a figure of 450 roentgens for the cumulative dosage for 30 years—a shortening from 1,350 to 2,700 days, and for the dosage of 360 roentgens a shortening of from 1,080 to 2,160 days, hence a definite shortening of life by 3 to 8 years from the dosages cited above (exactly 2.97 and 7.5 years—the uncertainty of the base figures has led us to round off to 3 years and 8 years).

We may note that these figures are relative to a very weak dosage of radiation such as those produced by experimental explosions.

The mortality percentage increases much faster than the dosage.

CONCLUSION

The maximum permissible concentration figuring in the recommendations of the International Commission for Radiological Protection is much too large, and it may represent, for personnel working permanently in nuclear centers and nuclear study centers, a serious danger.

It is certainly such possibilities as this which have caused the Harwell Study Center to take protective measures which limit the dosages received by workers at the center to 0.25 roentgens annually, a dosage which—as Sir John Cockroft states—is one-sixtieth of the internationally permitted concentration.

Our standpoint seems to be well justified, for at the last meeting of the International Commission for Radiological Protection at Geneva, the following suggestions were made:

A. For workers:

1. That the weekly dosage of 0.3 roentgen remain unchanged.
2. Protection services must see to it that the cumulative dosage does not exceed 5 roentgens annually.

B. For the population:

Here a more prudent position has been taken; the dosage has been reduced by one-tenth or 0.03 roentgen.

It is felt that this security factor is sufficient for sparse populations near (nuclear) centers.

For greater population densities, the dosage may be reduced to 0.2 or 0.3 roentgen annually.

Finally, the greatest preoccupation, of course, is for the consequences for the mass of the population.

Nevertheless, we believe that the dangers which threaten researchers cannot be neglected.

These projects cannot be discussed except at the forthcoming Mexican Congress, on July 25 next, and even this is not certain, considering the lively discussions which took place at Geneva.

A reduction of the annual maximum permissible tolerance appears indispensable and a decision in this sense must be energetically pursued.

EXHIBIT 6. REPORT OF MEETING OF ICRP, GENEVA 1956, STAFF TALK AT NBS, JUNE 1956

(Transcription of the tape recording of talk given to the Atomic and Radiation Physics Division staff by L. S. Taylor on June 1, 1956. This is an unedited discussion and is not for publication)

This is a brief review of some meetings which took place in Geneva last month and attended by a number of the members of our staff. These were the meetings of the International Commission on Radiological Protection, and the International Commission on Radiological Units and a meeting with a few members from each of the two Commissions with the World Health Organization.

The International Units Commission was established under the auspices of the First International Congress of Radiology, 1925. The Commission has been responsible for the development of the units used in radiation measurement. It also deals with problems of radiation measurement and standardization.

The Protection Commission was formed under the same auspices in 1928, and has been in continual operation ever since. I think it could be said quite properly that the Internatioanl Protection Commission has been responsible for the setting of the permissible exposure levels that have been used over the world, for some time now of the order of 25 or 30 years. The problem of protection, of course, is becoming more and more complicated as time goes on and I will review briefly a little of the background leading up to some changes that have been introduced by the International Commission meeting this past April.

The first specific permissible exposure levels were set by the ICRP in 1934, and at that time the level was given as two-tenths roentgen per day, whole body exposure. In the United States in about 1935 we lowered the level to one-tenth roentgen per day. Then in 1947 the National Committee on Radiation Protection which is the responsible body in this country for the establishment of safe working levels for radiation, reviewed a good deal of information that had become available during the Manhattan District days and again lowered the permissible exposure to three-tenths of a roentgen (or three-tenths of a rem) per week. In 1950, the general principles of this lowered permissible exposure were adopted by the International Commission and in 1953, recommendations similar to those of the United States national committee were reaffirmed and considerably extended.

The basic philosophy regarding radiation protection for individuals remains essentially the same with one important modification resulting from our 1953 meetings. This new modification has been discussed at some length for several years by the International Committee as well as the NCRP in this country, but was never put forward actively because here we have felt for some time that there has not been sufficiently definitive data leading to a quantitative lowering of the permissible exposure levels. Now, there are various factors that enter into the philosophy of permissible exposure or permissible dose. One is the question of detectable injury to the individual, and it has been this primary characteristic that has formed the basis for permissible exposure levels under occupational conditions. The present permissible occupational exposure of three-tenths rem per week is based primarily on consideration of a level which will not produce detectable injury to an individual during his lifetime.

Now, there has been nothing very much said in the setting of these levels about other problems such as genetic effects. They have, nevertheless, been considered—not simply passed by. Another problem that is basic to the question of permissible exposure is the short- or long-range genetic effects and I will return to these in a moment. There is also the question of the shortening of the life span. All of these questions have been considered at considerable length by the National Committee on Radiation Protection in this country as

well as by the International Commission. I will not go into any great detail on the genetic problem because I have a brief discussion on that at one of our staff meetings here just before we went to the Geneva meeting. I will, however, remind you that any radiation exposure of the genetic reproductive system will produce irreversible damage, and this can be passed on from one generation to another. Most of the experience in this field is based on animal experiments; there is very little information on man although some is gradually coming out of the Hiroshima-Nagasaki studies that are being made. There is very little doubt as to the basic fact that genetic damage is irreversible but I should like to emphasize that any quantitative information on man is extremely scant and nebulous. The really important factor in genetic damage is not so much the single individual as the whole population. The thing that really counts is the amount of radiation that is absorbed by the whole population or at least that part of the population which one expects normal crossbreeding.

Now, in 1952, the International Committee met in Stockholm for the primary purpose of discussing the genetic aspects of radiation exposure. There were several geneticists, among other people, there and at this time we considered the problem of exposure of whole population groups. At that time the estimates for exposures per individual, averaged over the whole population, ranged from 3 to 20 roentgens. There was one school of geneticists that set 3 and one that set 20 and there were some others in between. This appeared to represent to some extent the split of uncertainty among the geneticists themselves. In any case, suppose you were to take a value of 10 roentgens per person averaged over the whole population. This would mean you would sum up the total radiation exposure of the whole population, divide it by the number in the population; if this exceeded 10 roentgens, the geneticists believed we would have an unacceptable amount of genetic injury. At the Stockholm meeting in 1952, it was agreed that the present rates of exposure that we were working with were acceptable and the problem was not critical. This was reaffirmed with some minor reservations at our meetings last April. It was also recognized that with problems of fallout, widespread expansion of nuclear power, waste disposal, etc., time might begin to run out on us before too long. In spite of this, the International Commission felt that we would not be justified in setting a permissible average exposure level per person for the whole population.

In the last few years, there has been increasing evidence regarding the effect of radiation exposure on the expected life span of individuals. This was also considered in 1952 in Stockholm and again in 1956 in Geneva but again it was felt that the information for the most part was quite limited and did not provide grounds for a quantitative lowering or limitation of the permissible exposure levels. As a matter of fact, there is very little or maybe no serious effect of such radiation exposure with regard to life span insofar as small exposures to small areas of the body are concerned. For example, heavy overexposure of the hand is not going to have any serious effect on life span. On the other hand, where you expose the whole body to radiation some effect on the life span may be expected—a lowering of the life span. This type of effect is not very well understood, and it is not cumulative in the ordinary sense of genetic effects. I think the general belief is that radiation exposure somehow or other prematurely ages or makes one a little bit more susceptible to disease. These things of course, all contribute to an average shortening of life span.

It is rather interesting that there is actually some evidence that for very small dose rates to the whole body there might be some lengthening of the life span. This is, of course, based on animal experiments. However, I certainly would not recommend people living in a light atmosphere of radiation in the hope of living longer. Also, the effect of radiation with regard to life span is much greater for rather large, acute doses as compared with small chronic doses. Some of the best information on this question has been obtained from a study of leukemia among radiologists. It has been found that in comparing physicians who are radiologists and physicians who are carrying on normal medical practice, there is roughly a 10 percent shortening of the life span over the number of people who have been examined. This has to be treated very carefully, however, because the people who have been examined are the older radiologists; the younger ones are still living so you can't tell anything about their life span. The other radiologists have undoubtedly been exposed to many, many, many times the quantity of radiation that we feel is safe today and consequently this 10 percent figure as related to our present allowable exposures might have little significance. It has been agreed by the National Committee and by the Inter-

national Committee that the evidence is not sufficiently conclusive in this respect to warrant a quantitative lowering of the permissible exposure.

In spite of this lack of quantitative evidence, the problem was very seriously considered again with regard to making some lowering of the permissible exposure. I must say that I feel that the commission bowed a little bit to public clamor. There is an enormous amount of public pressure with regard to radiation exposure and I almost have the feeling that this limitation, which I will come to in a few minutes, may be regarded in some measure as slightly political. I don't mean political in the Government political sense, but political in the sense of being a good policy even though the foundation for it is moderately limited. Now in this whole problem of deciding upon a permissible radiation exposure level, we have to consider the sources of radiation. For example, at the present time, we have the fallout problem. This is one that is very much in people's minds. It was decided in a discussion of this that the increased radiation exposure to the population, as a whole, from fallout from tests made in this country and Russia is probably insignificant at the present time. This does not mean that it might not become significant later but certainly at the present time it is not to be seriously worried about in comparison with other exposures.

We then have occupational exposure in industry. By and large this is well-controlled exposure. The industries are very keen to insure that their working people are not overexposed and here we have a rather good radiation control program on the whole. Most of the large atomic energy plants, large industrial plants, and our own laboratory here already operate and have for many years operated at levels that are very much below the permissible level which we recommend as being safe. This problem may become more critical with the more widespread development of nuclear power.

A great deal depends on how we solve some of our waste problems and some of our normal exposure problems. As I mentioned a couple of months ago, I feel rather strongly that one of the serious limitations in the development of nuclear power will probably have to do with the efficiency with which we can control the radioactive waste disposal.

In the medical field we have another large source of radiation exposure. In some respects, this may be regarded as more critical than the others and in large areas much less well controlled, certainly, than in industry. This, however, varies. For example, in this country at the present time, we have about 3,000 radiologists using X-ray equipment and by and large these persons use X-rays with a reasonable degree of intelligence. They have some knowledge as to how much radiation they are using and they know something about protection so that we do not have to worry too much about the radiologists. On the other hand, we have another group including general practitioners, of which there about 15,000, using X-rays in this country. These people have very little knowledge about radiation protection and radiation dosage and in addition most of the working conditions that they have to live with are such as to militate against good protection practice. Even more serious than this is about an equal number totaling some 15,000, I believe, of osteopaths and chiropractors who are using radiation equipment. They very probably know substantially less about the problem than the general practitioner. Last but not least there are about 65,000 dentists who are using X-rays. It is possible for all of these people to provide adequate protection for themselves and for their patients just by applying principles that are already well known and adequately covered in recommendations in either the National Committee on Radiation Protection in this country or the International Commission over the world.

There has been a lot of difficulty in some local areas because people have learned about the effect of radiation exposure on life span. I have received several letters indicating that people are refusing to have dental examinations made because it exposes them sufficiently to shorten their life span by say 1 week. This is probably pretty silly but it does indicate something of what is happening in the public mind. In the first place, even an unreasonably large dental exposure is for the most part pretty well limited to the head. It is very different, as far as life span is concerned, from a whole body exposure. It is true the whole body gets some exposure but it is quite small. One would wonder as to whether we should be more concerned about a nebulous week's extension of one's life, on the one hand, or dying of some kind of an infection, on the other hand. In any case, there is a great deal to be done in the way of improving protection in these fields.

So much for the backbround and the need with regard to protection. I will come next to the primary change that was made in the protection recommendations in Geneva.

As I have mentioned, and I assume most of you know, the present basic level of exposure for the whole body is 3/10 rem per week for both internal and external exposure. This weekly limit has not been changed. As far as the individual is concerned, this is still regarded as a completely safe exposure. On the other hand, we have recognized to some degree these other effects, namely genetic effects and shortening of life span. As a result we have put a further limitation on this weekly exposure. Now if you allow the 3/10 rem per week, this would be equal to roughly 15 rems per year. If you consider that a person is going to work from age 20 to age 60, or 40 years, this would add up to something of the order of 600 rems for 40 years. If people actually worked to this level for a whole body exposure, there is no doubt, or very little doubt, that there might be some effect, however difficult to ascertain, on their life span. Certainly, if anything approaching this amount of radiation were received by the gonads during the reproductive period of a person's life, this would mean a very large exposure as far as genetics are concerned. To provide some further control on exposure we left the basic weekly figure the same but added a decade limitation such that up to age 30 we will allow a total exposure of 50 rems; up to age 40, 100 rems; and up to age 60, 200 rems.

The new figures of 200 rems compares with 600 rems at present so that insofar as continued exposure to radiation is concerned, we have in effect lowered the exposure limit by a factor of 3. Thus if someone wants to work at the full level of three-tenths rem per week, but not indefinitely, no harm is likely to result to him personally and not too much to his progeny. But he can only do this for a third of his working lifetime. If a person is employed continually in radiation work, this essentially means that the shielding has be so designed as to provide 3 times as much protection as we have been accustomed to using up to the present time. Thus while our basic level remains the same, in effect this amounts to a reducing of the working level by a factor of 3.

This is at best a lumping together of a lot of fairly uncertain factors and finally coming out with about as well-educated a guess as we know how to make. In some place or other, you certainly have to draw a line between no radiation exposure at all, which is what the geneticists would prefer, and what is reasonable and compatible with the advantages of radiation. These new limits will not, as far as we know, have any serious effect on our atomic energy industry. As far as we know, the atomic energy plants in this country are already operating at levels that are at least this low and in many cases lower. The principle bind may be in some of the industrial operations where management has, for the sake of economy, provided the minimum protection to stay within the law as it were. It will also probably crowd some of the medical setups.

Another problem that was discussed at some length by the National Committee and the International commission numerous times in the past, has to do with a setting up of a national or international registry of radiation exposure. In theory this looks like a nice idea. From the straight scientific point of view, a good registry of radiation exposure would have an enormous value in providing us later on with information on some of the long-range effects we are interested in. It was discussed again in great detail in Geneva and finally it was decided that for practical reasons it just could not be recommended. I might point out that in some countries like England, New Zealand, and Sweden, they have systems there which might make it possible to set up a registry of radiation exposure. Probably New Zealand and Sweden are the two closest to this because they already have some degree of governmental control over the use of all radiation equipment. However, in a country of this size, the task seems almost impossibly large.

Also, there is another difficulty certainly at the present time and probably until we have some strict control over radiation usage. One of the important sources of radiation exposure is that applied by general practitioners. Here we cannot expect for many, many years, at best, to have any really good idea of what exposures these people are using. They do not know themselves. They have no techniques by which you can really estimate the exposure given in a GI examination. The exposure might be 10r for a good operator; it might be 200r for a careless operator, and as long as you have information that is sloppy at source, there is no use in setting up a careful system for recording it.

In the matter of permissible concentrations of radioactice materials in air, the body, and water, the International Commission set up a substantial list of

about a hundred in 1953. To this there will be added about 70 more radio nuclides for which we now can give some reasonably good information on permissible concentrations. The new levels for the International Commission will be the same as those that have already been tentatively adopted by the National Committee in this country. For internal exposure, the base figure of three-tenths rem per week remains the same, for individual organs. There are, however, some special conditions for which lower exposure levels have been recommended. For example, where the genetic system constitutes the critical organ for a particular isotope or group of isotopes, the permissible exposure level will be one-tenth rem per week, or a third of the present value. If the whole body is the critical organ for internal emitters, it will also be one-tenth rem per week. The body burdens of radioactive materials are still based on the figure we have used for many years, namely, an accumulation of one microgram of radium in the body.

With regard to the International Commission, the membership has remained about the same; there were two new people added to the Main Commission. The subcommittees have remained about the same although there have been a few people added to those. Incidentally, a Russian was added to the membership of one of the committees operating under the Main International Commission. There has been a new committee set up to deal with problems of waste disposal. This is under the chairmanship of Dr. Straub, a member of the Public Health Service who until recently has been working at Oak Ridge.

With regard to our own relationships with the protection program, I have been continued as a member of the Main Commission. Wyckoff continues as a member of the X-Ray Protection Subcommittee, and Koch continues as a member of the High Energy Particles Subcommittee.

(Mr. Taylor's statement—continued.)

One of the recommendations made by the International Commission on Radiological Protection in 1956 was the dose limitation for occupational workers of 50 rems up to age 30, and 50 rems per decade thereafter. This averages to about 5 rems per year, but in plant operations and design, strict adherence to a 5-rems-per-year average is extremely difficult. In addition there was the fear that this yearly average figure might be enacted into law and for this reason the NCRP sought an interpretation that would allow more latitude in this use.

A suitable means for accomplishing this appeared to be the establishment of a permissible occupational exposure based on a total accumulated dose in relation to the age of an individual.

EXHIBIT 7. NATIONAL BUREAU OF STANDARDS TECHNICAL NEWS BULLETIN, MAXIMUM PERMISSIBLE EXPOSURES OF RADIATION TO MAN, VOLUME 41, PAGES 17–19, FEBRUARY 1957

MAXIMUM PERMISSIBLE RADIATION EXPOSURES TO MAN

A PRELIMINARY STATEMENT OF THE NATIONAL COMMITTEE ON RADIATION AND MEASUREMENT

The accompanying preliminary statement of the National Committee on Radiation Protection and Measurement (NCRP) presents in brief form the essential changes introduced by its new recommendations on the safe limits of radiation exposure. In making the new recommendations, the Committee reviewed its past recommendations in the light of increased knowledge about the long-range effects of radiation exposure on the genetic makeup and life expectancy of man.

The NCRP, an advisory group of experts in various phases of the radiation field, is sponsored by the National Bureau of Standards. The Committee is made up of representatives from the following organizations: American College of Radiology, American Dental Association, American Industrial Hygiene Association, American Medical Association, American Radium Society, American Roentgen Ray Society, International Association of Government Labor Officials, National Bureau of Standards, National Electrical Manufacturers Association, Radiological Society of North America, United States Air Force, United States

Army, United States Atomic Energy Commission, United States Navy, and United States Public Health Service. The reports of the Committee are published in the NBS Handbook series.

Because the new recommendations of the NCRP will affect material contained in many of its handbooks, revisions of the latter will be undertaken at an early date. Until these revisions are completed and ready for publication, an effort will be made to prepare for each handbook a simplified statement of the changes needed to comply with the new recommendations. These summary statements will be released to the technical journals for publication as they are completed, and will be used with the handbooks now in stock. The full and detailed changes will be incorporated in the revised editions of the handbooks.

Since the publication of NBS Handbook 59 on Permissible Dose From External Sources of Ionizing Radiation, the National Committee on Radiation Protection and Measurement (NCRP) has continued the study and review of its recommendations,[1] particularly with respect to genetic effects and the possible shortening of average life expectancy due to radiation exposure of a larger fraction of the population. The NCRP proposals resulting from these studies had an important influence on the decisions reached by the International Commission on Radiological Protection (ICRP) in Geneva in April 1956,[2] which resulted in a general lowering of the maximum permissible accumulated dose (MPD for occupational radiation exposures, as well as for exposures of the population as a whole. These changes are in accord with the informal agreements reached by the ICRP in Stockholm in 1952.

The NCRP has now agreed upon the formulation of revised recommendations on maximum permissible doses which integrate the national and international views for practical application. The Committee is pleased to note that the findings of the ICRP are reinforced by the important information and data provided in the subsequent reports of the National Academy of Sciences and the British Medical Research Council.

The changes in the accumulated MPD are not the results of positive evidence of damage due to use of the earlier permissible dose levels, but rather are based on the desire to bring the MPD into accord with the trends of scientific opinions; it is recognized that there are still many uncertainties in the available data and information. Consideration has also been given to the probability of a large future increase in radiation uses. In spite of the trends, it is believed that the risk involved in delaying the activation of these recommendations is very small, if not negligible. Conditions in existing installations should be modified to meet the new recommendations as soon as practicable, and the new MPD limits should be used in the design and planning of future apparatus and installations. Because of the impact of these changes and the time required to modify existing equipment and installations, it is recommended on the basis of present knowledge that a conversion period of not more than 5 years be adopted, within which time all necessary modifications should be completed.

DEFINITIONS

For the purposes of this preliminary statement, the following tentative definitions are given:

Controlled area.—A defined area in which the occupational exposure of personnel to radiation or to radioactive material is under the supervision of a radia-

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[1] The recommendations of the NCRP are regularly published in the NBS Handbook series, copies of which may be purchased from the Superintendent of Documents, Government Printing Office, Washington 25, D. C. The following handbooks containing NCRP recommendations are currently available at the indicated prices per copy: H42, Safe Handling of Radioactive Isotopes, 20 cents; H48, Control and Removal of Radioactive Contamination in Laboratories, 15 cents; H49, Recomemndations for Waste Disposal of Phosphorus 32 and Iodine 131 for Medical Users, 15 Cents; H51, Radiological Monitoring Methods and Instruments, 20 cents; H52, Maximum Permissible Amounts of Radioisotopes in the Human Body and Maximum Permissible Concentrations in Air and Water, 25 cents; H53, Recommendations for the Disposal of Carbon 14 Wastes, 15 cents; H54, Protection Against Radiations From Radium, Cobalt 60, and Cesium 137 (revision of H23), 25 cents; H55, Protection Against Betatron-Synchrotron Radiations up to 100 Million Electron Volts, 25 cents; H56, Safe Handling of Cadavers Containing Radioactive Isotopes, 15 cents; H58, Radioactive-Waste Disposal in the Ocean, 20 cents; H59, Permissible Dose From External Sources of Ionizing Radiation, 30 cents; H60, X-ray Protection (revision of H41), 20 cents; H61, Regulation of Radiation Exposure by Legislative Means, 25 cents.

[2] See Meetings on radiation units and protection, NBS Tech. News Bul. 40, 102 (July 1956).

tion safety officer. (This implies that a controlled area is one that requires control of access, occupancy, and working conditions for radiation protection purposes.

Workload.—The output of a radiation machine or a radioactive source integrated over a suitable time and expressed in appropriate units.

Occupancy factor.—The factor by which the workload should be multiplied to correct for the degree or type of occupancy of the area in question.

RBE dose.—RBE stands for relative biological effectiveness. An RBE dose is the dose measured in rems. (This is discussed in the forthcoming report of the International Commission on Radiological Units and Protection.)

MPD RECOMMENDATIONS FOR OCCUPATIONAL CONDITIONS (CONTROLLED AREAS)

1. *Accumulated dose.*—The maximum permissible accumulated dose, in rems, at any age, is equal to 5 times the number of years beyond age 18, provided no annual increment exceeds 15 rems. Thus the accumulated MPD=5 (N−18) rems where N is the age and greater than 18. This applies to all critical organs except the skin, for which the value is double.

2. *Weekly dose.*—The previous permissible weekly whole-body dose of 0.3 rem, and the 13-week dose of 3 rems when the weekly limit is exceeded, are still considered to be the weekly MPD, with the above restriction for accumulated dose.

3. *Emergency dose.*—An accidental or emergency dose of 25 rems to the whole body, occurring only once in the lifetime of the person, shall be assumed to have no effect on the radiation tolerance status of that person. (See National Bureau of Standards Handbook 59.)

4. *Medical dose.*—Radiation exposures resulting from necessary medical and dental procedures shall be assumed to have no effect on the radiation tolerance status of the person concerned.

MPD RECOMMENDATIONS FOR THE WHOLE POPULATION

5. The maximum permissible dose to the gonads for the population of the United States as a whole from all sources of radiation, including medical and other man-made sources, and background, shall not exceed 14 million rems per million of population over the period from conception up to age 30, and one-third that amount in each decade thereafter. Averaging should be done for the population group in which crossbreeding may be expected.

RECOMMENDATIONS FOR INTERNAL EMITTERS

6. In controlled areas, the permissible radiation levels for internal emitters will conform to the general principles outlined above. Where the critical organ is the gonad or the whole body, the maximum permissible concentrations of radionuclides in air and water will be one-third the values heretofore specified for radiation workers. Where single organs other than the gonads are regarded as the critical organ, the present maximum permissible concentrations will continue. For individuals outside of controlled areas, the maximum permissible concentrations should be one-tenth of those for occupational exposures. (Other changes in the maximum permissible concentrations for radionuclides may be introduced because of additional information developed since the publication of National Bureau of Standards Handbook 52.)

DISCUSSION OF REVISED RECOMMENDATIONS

7. The MPD for occupational exposure is based on the absence of detectable injury to the individual. It remains at its present level of 0.3 rem per week for the whole body. Where the dose in any week exceeds this value, a dose of 3 rems in 13 weeks may be accepted. The 13-week period may start at the beginning of the calendar quarter or the beginning of the week during which the permissible weekly dose was exceeded.

8. The rules given in Handbook 59 will be continued for operational and administrative purposes, but some of the rules will be modified by provisions related to an average yearly limitation of occupational exposure to external sources of ionizing radiation of 5 rems to the blood-forming organs, gonads, and lenses of the eyes, and of 10 rems to the skin. The use of "5 rems" in the statement of the revised rules is for the purpose of design and administration.

The critical limitation will be that defined for the total accumulated dose in paragraph 1 above.

9. If a person's occupational exposure is documented or otherwise known with reasonable certainty, he may be permitted to use his reserve exposure in accordance with paragraphs 1 and 2 above. In all other cases, he shall be assumed to have received his maximum accumulated dose as indicated in paragraph 1 above.

10. It is considered that with the current and proposed low levels of occupational exposure, it is presently not necessary to make special allowance for medical exposure in conjunction with occupational exposure. This consideration may later become important. The effects of medical exposures have long been considered by this committee to be the responsibility of the attending physician; it is his responsibility to evaluate medical radiation exposure in relation to the health of the individual. (See National Bureau of Standards Handbook 59.)

11. In the determination of the population dose in the vicinity of radiation sources, proper consideration should be given to occupancy factor and to workload. The exposure of individuals outside of controlled areas may be integrated over periods up to 1 year.

12. While at the moment it is not feasible to determine the average exposure for the population with any reasonable accuracy, the adoption of some figure is necessary for planning purposes. For the immediate future, it may be assumed that the total integrated RBE dose received by all radiation workers will be small in comparison with the integrated RBE dose of the whole population. Furthermore, persons outside of controlled areas, but exposed to radiation from a controlled area, constitute only a small portion of the whole population. Therefore, if this small portion is assumed to receive yearly an average per capita dose of 0.5 rem, the total dose to the whole population from man-made radiations is not likely to exceed 10 million rems per million of population up to age 30. (This assumes a dose of 4 million rems per million of population over this age period from background radiation.)

Dated: January 8, 1957.

(Mr. Taylor's statement continued.)

In accordance with this idea, the NCRP now recommends that the maximum permissible accumulated dose in rems for occupational exposure be 5 times the number of years of age of an individual over 18 (age 18 being the time when an individual is allowed to be engaged in radiation work). This can be expressed by the relationship MPD = 5 (N–18) rems. The old permissible value of 0.3 rem per week or 15 rems per year will be continued, provided the accumulated dose as expressed by the formula above is not exceeded. Also the 25-rem emergency dose will be continued.

The relationship of the new permissible dose levels to the older levels is illustrated in the accompanying figure. (See p. 784.) The upper shaded area is bounded by the permissible dose relationship that existed from 1935 to 1947. according to this, a person would be allowed 0.1 rem per day, and accordingly could accumulate a dose of 600 rems by an age of approximately 38 years.

The center shaded area indicates the accumulated dose that would have been received according to the standards used during the years 1947 to 1957, when the allowance was 0.3 rem per week. On this basis, a person could have received slightly over 600 rems by age 60.

The lower region indicates the limits in accordance with the 1957 recommendations on the basis of which a person would be allowed to receive a maximum of only 210 rems by age 60.

The way in which a person may receive his occupational dose under the new recommendations is quite flexible. So long as his accumulation at a given age remains within the lower white area, he will be complying with the new recommendations. If he works well below these limits for a given period, he may essentially build up a reserve, and then if circumstances demand, he can accept an occupational exposure at the rate up to 15 rems per year so long as he stays under the age-prorated limitation. Furthermore, if an individual having had no previous radiation experience were to start work at, say, age 35, he can be exposed at the level of 15 rems per year for a number of years until he begins to approach his age-prorated limitation. By following this exposure plan, an individual is prevented from receiving his accumulated dose too rapidly, particularly at the younger ages where, for genetic reasons, his dose reception has greater significance. Typical ways of distributing this dose are indicated by the plots

shown in the white area. (It should be pointed out that while this white area is labeled "same exposure region", we do not mean to imply that any radiation exposure is safe, but rather that it complies with the standards.)

For persons receiving nonoccupational exposure in the environs of radiation sources the permissible levels have been set at 0.1 of the values for occupational exposure. Strictly speaking, this should be an average exposure over a large number of individuals, because here again it does not make any difference if a few people receive more than the average, provided that others receive correspondingly less. This is a higher allowable figure than for the population as a whole, but according to present estimates it is not expected in the foreseeable future that more than 1 percent of the population is likely to be exposed to radiation at these levels. In computing the average exposures, adequate allowance should be made for occupancy factors, and factors which take into consideration the fraction of the time that the plant is emitting radiation. Making this type of allowance is difficult legally, as evidenced by the current AEC regulations. On the other hand, too strict a legal interpretation of this philosophy will cause undue hardship on the radiation industry.

For the population as a whole, the NCRP has recommended an average exposure of 14×10^6 rems per million of population up to age 30, and one-third of that amount per decade thereafter. Here again it should be noted that this is an average figure taking into consideration a large population group, and it is not correct to reduce this number to an exposure for a single individual. It might be pointed out that the number 14 may be raised to 15. This is mainly for making the arithmetic easy. There is no significant difference between 14 and 15 as far as our present knowledge is concerned.

It may be instructive to have a rough picture of how the dose to our population will be distributed according to present knowledge. The following table shows this.

Percent of population	Radiation source	Man-rem per million population
100	Background	4,000,000
100	Medical	1–5,000,000
1/8	Occupational	150,000
1	Installation environs	150,000
100	Fallout	200,000
Total		5–9,500,000
Balance		8–4,500,000
Total		14,000,000

Exhibit 8. Current Situation With Regard to Permissible Radiation Exposure Levels, American Nuclear Society, December 1956

ADDRESS GIVEN BEFORE AMERICAN NUCLEAR SOCIETY, DECEMBER 17, 1956

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This discussion will deal primarily with permissible radiation exposure levels in relation to occupational exposure in industry. While these levels are intimately related to general population exposure levels, this phase of the problem will not be discussed in detail.

For the past 25 years this country has based its permissible occupational exposure levels on the recommendations of the National Committee on Radiation Protection and the International Commission on Radiological Protection. In both cases the exposure levels have been based primarily on the effect of radiation exposure upon the individual. Reduced to the simplest terms, this has amounted to a figure of 0.3 rem per week over the period of employment. The current permissible weekly level is based on the premise that a whole body exposure of 0.3 rem per week for an indefinite period of time would not produce any detectable harmful effect on the individual.

It should be pointed out that at the time these levels were set, extensive consideration was given to the effect of this degree of radiation exposure on our genetic system and upon the average expected life span of the occupational ir-

radiated population. Handbook 59, which set forth the proposals of permissible exposure from external radiation, made numerous references to the fact that this basic requirement for permissible exposure might have to be changed in the future. This is typified by the following statement, "As the applications of atomic energy expand and the number of exposed individuals increases, genetic effects will become more important. Accordingly, it may be expected that at some time in the not too distant future a reappraisal of the situation will become necessary. On the basis of present knowledge of the genetic effects of radiation, it may be predicted that any future revision of permissible doses to the gonads of young persons will be downward. This should be borne in mind, and unnecessary exposure to radiation should be avoided at all times." Similarly, the effect of radiation on the average life expectancy was considered. The knowledge of a few years ago was vastly less than today, and even today the question is one in which there is considerable quantitative doubt.

The present permissible exposure levels took into consideration the fact that only a small fraction of the population would be subjected to radiation exposures approaching the maximum permissible levels. No allowance was made for the fact that the great majority of radiation exposures would be considerably below the permissible levels. By neglecting this fact an additional safety factor was automatically introduced. It is undoubtedly estimating on the high side if one ventures to suggest that there may be as many as 500,000 occupational workers in this country. This would be about one-third of 1 percent of our population. Considering occupational exposure alone, in comparison with medical and other sources of exposure, the total exposure to this group of people would be regarded as genetically relatively unimportant.

The report of the National Academy of Sciences on the genetic effects of radiation indicated that an exposure of 1,000 roentgens has produced in the radiological profession a shortening of about 5 years in the average life expectancy. This figure is probably as good as any that can be attained, yet it must be borne in mind that it is based on statistics including most of the early radiologists, who worked under conditions of almost zero protection. Many people believe that the estimated figure of 1,000 r average exposure for these persons is in all probability low and that a more realistic figure may be of the order of 3,000 or 4,000 r. It will be impossible to ever determine what this exposure was.

According to the present levels of 0.3 r per week or 15 r per year, a radiation worker could receive as much as 600 r in 40 years of working lifetime or about 300 r up to the end of his genetically important period. There can be little argument that in the light of present knowledge this amount of exposure may be unreasonably high, yet as far as the individual is concerned, there is no evidence at present that exposures at these rates have caused individual detectable injury. In fact there is no evidence that the higher levels of 100 r per year from 1928 to 1936, or 36 r per year from 1936 to 1947, have resulted in detectable injuries either. Of course one must point out that average shortening of life expectancy might be essentially regarded as an undetermined injury.

Even with the low levels of 0.3 rem per week in us since 1947, most atomic energy plants found that they could operate satisfactorily at levels ranging from one-third to one-tenth of this value without seriously interfering with their operations. It is erring on the safe side, to say that in no large atomic energy plants, in this country, has the average exposure per individual exceeded, or in even most cases approached, 0.1 r per week.

It should also be pointed out that when one considers the problem of radiation effect on life span, one thinks of whole-body exposure. In fact a substantial fraction of the persons receiving occupational exposures in the atomic energy, medical and industrial field have only small portions of their body exposed. Under such circumstances the effect on life expectancy is very much less.

The National Committee on Radiation Protection in 1946, and the International Commission on Radiation Protection in 1950, undertook extensive studies of the whole question of permissible exposure and it was during this period that the present levels were set. In 1952 there was a special meeting of the ICRP, in conjunction with the Joint Committee on Radiobiology of the International Union of Pure and Applied Physics, for the specific purposes of studying the genetic effects of radiation exposure. Participating in this were a number of geneticists from several countries, and the basic problem before the Commission at that time was consideration of an average population dose to the gonads, during the reproductive period. The acceptable values at the time ranged from 3 to 20 rems over and above that due to natural and background radiation.

(These bracket the figure suggested by the National Academy of Sciences report in 1956.) At the time it was considered by the ICRP that the fraction of the population receiving radiation exposure was so small that immediate concern over the total population dose was not necessary. It is believed that many of the members still feel much the same way, but on the other hand there has been and still is clear recognition of the fact that this situation will almost certainly change as our radiation uses throughout the world increases.

At meetings of the ICRP in Geneva in April of this year the genetic problem were again considered and a figure of amount 10 rems of manmade radiation was recommended as a reasonable average per capita dose for the future. It should be emphasized that within our present practices it is not likely that the increase of occupational exposure would contribute very importantly to this total exposure level.

It has been decided by the NCRP on the basis of present evidence that the most important new limitation should be one relative to gonadal exposure to individuals during their reproductive lifetime. For this it was considered that the occupational exposure a dose of about 100 rems up to age 40 would not involve an unacceptable risk. Similarly an exposure of up to 250 rems by age 70 would not be regarded as unacceptable. In the case of exposure received beyond the reproductive age, the further influence of such exposure would in the main be through its effect on expected life span. Operationally, it would seem to be just about as easy to continue beyond age 40, the same permissible levels allowed below age 40.

In these considerations it is clear that the emphasis has shifted from the exposure of the individual to exposure of the population. At the same time it has to some extent shifted from a weekly or monthly exposure level to the exposure accumulated over the individual's lifetime.

TABLE I (DISTRIBUTION OF POPULATION DOSE) (P. 818)

In principle, permissible exposures such as I have just mentioned may be considered as adequate for all occupational and regulatory purposes. Unfortunately in practice this is not possible and as a result it has been necessary to develop a series of derived permissible exposure levels over varying periods of time, and in relation to the age of the individual.

The difficulty up to now in dealing with a single number giving the individuals total permissible lifetime exposure, is introduced because of the fact that the safety *recommendations* of a few years ago have now entered our legal machinery and are now *legal requirements* backed up by varying degrees of enforcement. For the 25 years beginning with the appearance of the first NCRP Handbook in 1931, radiation safety control in this country was on the basis of recommendations backed up by voluntary compliance and good sense. It must be admitted that there were times when neither compliance nor good sense prevailed, but in the great majority of situations it did. A substantial number of legal cases involving overexposure to radiation, were settled one way or the other on the basis of NCRP handbooks without any of these handbooks ever being legally adopted in the form of codes or laws.

The basic scientific aims and information regarding permissibe radiation exposures have been well understood for a number of years. On the other hand, there have been situations where the information was capable of misinterpretation by people who were inexperienced in the field. Partly as a result of this there has been increased tendency on the part of industry and by State and Federal Government to call for strict codification and the development of radiation protection rules wherein a situation is either black or white. Here begins our trouble.

Let us consider as acceptable a total accumulated exposure of 250 rems over a period of 50 years, up to age 70. Now it is obviously impossible, with any means that we now know of, to measure and record this exposure for each individual. Therefore, the wise operator might decide to make his measurements over a period of 10 years, assuring that no employee would receive over 50 rems over a period of 10 years. This is still impractical because we have no instruments that an individual can wear for 10 years to record his dose. Furthermore, there may be occasional unscrupulous employers who may allow an individual to receive his 10-year dose of 50 rems in 3 years and having done this simply fire the man because he is no longer useful in the radiation plant.

For the purposes of design and operation, one might want to integrate the exposure over a shorter period of time, say 1 year, in which case the average

allowable amount would be 5 rems. This is also difficult to control but it is suspected that before too long adequate instrumentation will be obtainable such that this can be done within adequate accuracy limits. At least as far as plant operations are concerned the level of 5 rems per year is a much better figure to work with.

The importance of limiting the exposure to individuals during their child-bearing age has been mentioned. For this purpose the NCRP is recommending a permissible exposure of approximately 60 rems up to age 30, and an additional 50 rems up to age 40.

Now here again it is important to assure adequate distribution of this dose over time. If one considers that 50 percent of the children are born to parents by the time they reach age 30, and 90 percent by the time they reach age 40, it is clearly more important to curtail exposure at the lower end of this age range than at the upper age range. There would be a large difference in the total genetic damage due to an exposure of 50 rems at age 20, as compared to the same exposure at age 30. At the age of 40 this exposure would only affect 10 percent of the genes that might be expected to be transmitted. Consequently there is some wisdom in stating that for operational purposes the average occupational exposure of individuals should not exceed 5 rems per year.

But even here there are difficulties. Suppose that the NCRP were to recommend 5 rems per year for the basic permissible level as compared with the present level of 0.3 rem per week. As soon as this figure is even mentioned it will find its way into various laws and regulations. Everything automatically becomes black or white and any individual who may be so unfortunate as to receive 5,001 rems in any 1 year might think he had legal grounds for seeking redress from his employer. This is, of course, sheer nonsense and yet it is difficult to develop a law in this field in which there is adequate latitude for such reasonableness. The NCRP has worried a great deal about this problem and various suggestions have been put forward.

Consider the cost to a plant operation if a legal limitation of 5 rems per year to the individual worker. Plant management would be foolish to plan their operations in such a way that part or all of its workers would be allowed to take a dose closely approaching 5 rems per year. Experience has demonstrated that surely 1 or 2 people are going to accidentally exceed this and even though we know that a slightly higher exposure is without harm the employee might seek redress. Because of this fear, the plant must plan its operation such that in general they will not exceed, say, 40 or 50 percent of the permissible exposure. This can be very costly and time consuming and could seriously retard the atomic-energy industry.

It has also been suggested that the yearly limit be specified as 5 rems plus or minus 20 percent. This would certainly simplify plant planning and operations, but I am very doubtful as to how and whether this could be dealt with to the satisfaction of our legal authorities. Another suggestion has been to specify a total exposure not exceeding 50 rems in 10 years delivered at an average rate not exceeding 5 rems per year. Here, again, this situation is wide open to abuse or difficulty in legal interpretation. For example, an employer could allow an individual to receive 25 r in each of 2 years and then lay him off under circumstance which would make him unemployable in the radiation industry. Also this would be unacceptable because one should not allow an individual at the beginning of his reproductive age to receive this large dose. If he is to have a large dose, it is much better that he receive this after the conception of his last child and this, of course, changes with age.

Another alternative is to specify the 5 rems per year average but with the further stipulation that no one be allowed to receive in excess of 10 rems in any consecutive 2 years. Even this would allow some abuse, but such a provision would vastly simplify plant operations. For example, if an occupational individual received, say, 6 rems in 1 year, it would be up to the plant to insure that he could receive no more than 4 rems in the following year. If one decides on an average figure of 5 rems per year, the plant will design accordingly. The plant will possibly go further than this, since control over a year is not always easy. It may well, for purposes of simplicity, make its designs so as not to allow the exposures to exceed 0.1 rem per week, which is one-third of the present maximum permissible exposure.

All of the discussion so far has led us to one almost inescapable conclusion—namely, that the maximum permissible exposure levels are going to be reduced to one-third of what they are at the present time. One could then ask why all the fuss about the various exposure levels that have been discussed above?

Except for the exposure limitation for genetic reasons in the 20- to 40-age bracket, all of the levels are so low as to make it virtually impossible to make any distinction between dose delivered over a few months or even a few years.

One could well ask, Why not take the basic figure of 250 rems up to age 70, and leave the rest of it up to the plants—or even do the same for 50 rems per 10 years? The difficulty, however, is that various individuals will interpret the limits in various ways and, unless the permissible dose is integrated over a relatively short period, a given employer might use up all of the employee's permissible exposure, making it impossible for the employee to shift positions or to continue work in his chosen field. The various numbers mentioned above may be included in the new recommendations of the National Committee on Radiation Protection. It must be borne in mind, however, that the permissible levels that are expressed in terms of 1 week, 13 weeks, a year, or even 10 years, are regarded as numbers to be used for design and operational purposes. There is nothing sacred about any of them, and yet failure to introduce such time-dose limitations will encourage the introduction of such a high degree of nonuniformity of permissible exposure in the radiation field as to make employment in radiation work unattractive.

Where a plant or individual can provide adequate assurance that his monitoring and recording methods are adequate, he should be allowed the widest latitude in the use of permissible-exposure levels with his employees. I do not know how this can be worked out legally, but I feel fairly certain that in operations of any substantial size, it would pay the employer to have adequate measuring and recording systems and then be allowed wide latitude in his use of any of the various permissible levels that I have mentioned above.

The main difficulty may occur with the small users of radiation sources where there is transient employment and where the cost of monitoring equipment may be incommensurate with the cost of his operation. For such individuals permissible-exposure levels integrated over 1 week, or, at the most, 3 months, would in all probability be the most feasible.

In conclusion, let us look briefly at the pattern of exposure levels that were agreed upon by the executive committee of the National Committee on Radiation Protection at its meetings in September and December of this year.

The basic figure of 0.3 rem per week, together with the penalty allowance of 3 rems per 13 weeks, presently in use will be continued. So also will be the allowance of 15 rems per year (allowing for 2 weeks' vacation).

These basic figures will, however, be subject to a further limitation to insure that the dose is not accumulated too rapidly, or reaches totals per individual that are not considered acceptable. If we allow an accumulated dose, over 50 years, of about 250 rems up to age 70, this would amount to an average of about 5 rems per year.

Most of the operational problems can be solved by means of a simple formulation that implies the building up of a bank of reserve exposure that may be called upon as needed. The new requirement will be that at any given age of 18 or over, an individual may be allowed to accept a dose such that his total accumulation will not be in excess of five times the number of years over 18.

TABLE II (STATEMENT OF NEW RECOMMENDATIONS) (P. 818)

Thus, at any time, a person's exposure must equal or be less than 5 (N–18) where N is the person's age. This automatically holds down the exposure at the younger ages where it is most critical. It gives leeway at the older ages where it is less critical. Thus, if a person starting work at age 18 receives only 1 rem per year for 5 years, his total will be 5 rems, as compared with 25 rems permitted. He has thus banked a reserve of 20 rems. Should occasion then demand a larger dose, he can take up to 15 rems in the next year and still have a reserve of 5 rems in the bank.

If a person starts radiation work at an older age, he will have automatically built up a reserve and can take larger annual doses—so long as the accumulation remains less than the age prorated maximum. This procedure eliminates the need for the present rule allowing larger exposures for persons over 45.

CURVE ILLUSTRATING USE OF RESERVE EXPOSURE (SEE P. 784)

For design or planning or operational purposes, plants may wish to use average weekly, monthly, or quarterly levels and do their monitoring accordingly. Under the new rule this will be permitted. Where it is desired to keep monitoring and recording procedures to a minimum, it would probably be wise, in many

cases, to operate on a weekly or monthly basis anyway. The choice of procedure will depend largely upon the economics of any particular situation.

Where a person's exposure in prior employment may be unknown or undocumented, it will be presumed to have been the maximum permitted up to his particular age.

It might be pointed out that the levels above need not be modified by the acceptance of one emergency exposure of 25 rems during that person's lifetime.

Also it might be noted that these exposure levels are not modified by any radiation exposure received for medical reasons. On the other hand, it would be prudent for the employer to take any especially large medical exposures into consideration in the assignment of an individual to radiation work.

The rules given in Handbook 59 will be continued but some of them will be modified by provisos related to a yearly limitation of 5 rems to the blood-forming organs, gonads and lenses of the eyes, and to a limitation of 10 rems to the skin.

Permissible radiation levels for internal emitters will conform to the general principles already outlined. Where the critical organ is the gonads or the whole body, the maximum permissible concentrations for air and water will be one-third the present values specified for radiation workers. For persons living in the neighborhood of controlled areas, the maximum permissible concentrations should be further reduced by a factor of 10. Where single organs are regarded as the critical organ, the present concentrations may be continued.

TABLE I.—*Maximum permissible accumulated dose up to age 30=14,000,000 rems per million persons*

Source:	*Rems per million persons*
Natural	4,300,000
Medical	5,000,000
Occupational	150,000
Plant environs	150,000
Fallout	200,000
Total	9,800,000
Balance	4,200,000

TABLE II.—*Basic whole-body maximum permissible dose (occupational)*

1. Three-tenths rems per week or 15 rems per year.
2. Not to exceed at age N an accumulate dose of 5 (N–18) rems.
3. One emergency exposure of 25 rems in a lifetime.
4. MPD not modified by medical exposure.

(Mr. Taylor's statement—continued.)

The standards described above apply to conditions where the whole body, the lens of the eye and the gonads are exposed. Where the blood-forming organs may be the critical organs, these may in some circumstances provide the basic limitation.

The above standards apply to all kinds of radiation but here again we are handicapped in practical applications by lack of adequate knowledge of the RBE.

When dealing with various sources or kinds of radiation incident upon a particular organ, each is treated separately and the dose from each is added.

The rate of delivery of the dose is relatively unimportant so long as we stay within the basic age-prorated limitation described above.

Partial body exposures may be larger depending upon circumstances. For example, where only the hands and feet are exposed, the permissible dose to the skin may be 15 times larger than indicated by the formula above. Similarly, where the skin of the entire body is irradiated by low penetration radiation the permissible levels may be twice those given in the formula. There may be other such variations and other specific organs under certain conditions.

A detailed discussion of this question is given in Handbook 59. Although this handbook was published several years ago most of the arguments contained in it still apply. A revision of the handbook is now in preparation and while this will contain most of the old material, it will also include revisions to take into consideration the new accumulated dose concept.

STANDARDS OF PERMISSIBLE DOSE FROM INTERNAL SOURCES OF RADIATION

The previous discussion has indicated some of the more essential points relative to the maximum permissible dose for external sources of radiation. For sources of radiation inside the body, the protection standards are related to and are consistent with those for external sources. Unfortunately there is relatively little quantitative knowledge of the exact effects of internal radiation on man although a great deal of information has been obtained from animal experiments.

Much of our basis for the standards for internal dose is related to information obtained from the deaths several decades ago of the radium dial painters in New Jersey. This question will undoubtedly be discussed in greater detail by one of the subsequent witnesses. In the case of the radium dial painters there was substantial uncertainty about the kinds and quantities of material that contributed to the dose and the results might be seriously influenced by the presence of large fractions of mesothorium in the dial paint. Even today there is some uncertainty in the proper RBE to use for radium under these circumstances. In using this information as a basis for permissible dose the most conservative extremes have been used, and there is not unreasonable likelihood that our present permissible body burden for radium may, in fact, be unnecessarily low.

Many isotopes tend to go to and localize in certain body organs or tissues. The permissible dose therefore is based on the dose to those organs (see Handbook 52). Where more than one organ may be affected the most critical one is usually chosen. For many isotopes a different base is employed. For pure gamma emitters and some beta emitters the base level is chosen as an amount which would give the organ a dose not in excess of 0.3 rem per week. The results of using this base are not completely consistent with those using the radium base but the difference is not critical. For Sr–90 the radium base is used even though strontium is a beta emitter and radium primarily an alpha emitter. Allowing for this difference in base and the uncertainty in RBE, it is possible that the maximum permissible body burden for Sr–90 may be conservative by a factor as large as 15. It is almost certainly conservative by a factor of 5.

Body burdens are based on the concentration in certain areas where localized damage may originate. In some circumstances you may have maximum body burdens of different isotopes in different organs where there is no interplay between the organ functions. At present there is relatively little knowledge on this. It is an area where much research and statistical study is needed.

EXHIBIT 9. NATIONAL BUREAU OF STANDARDS HANDBOOK 52, MAXIMUM PERMISSIBLE AMOUNTS OF RADIOISOTOPES IN THE HUMAN BODY AND MAXIMUM PERMISSIBLE CONCENTRATIONS IN AIR AND WATER, MARCH 1953

D. FACTORS THAT DETERMINE THE HAZARDS OF RADIOISOTOPES

The factors that determine the hazards of the various radioisotopes are as follows:

1. Quantities available

None of the radioisotopes except those occurring naturally presented problems until the age of high-voltage accelerators, nuclear reactors, and atomic bombs. From the standpoint of common use and quantity available, I–131, P–32, Co–60, Si–90, C–14, S–35, Ca–45, Au–198, Ra–226, Pu–239, and uranium present the major problems of irradiation inside the body.

2. Initial body retention

Large fractions of some elements such as iodine, strontium, and sodium are absorbed when they are taken into the body by any of the several routes and when available in their common chemical forms. In the case of elements like plutonium and uranium, only a small fraction is absorbed in the gastrointestinal tract. Therefore, the greater retention would increase the hazards from the first group as against those of the second. In dealing with the inhalation of radioisotopes, unless information specific to the radioisotope is available, it is assumed in the case of soluble compounds that 25 percent is retained in the lower respiratory tract. From this tract it goes to the blood stream, and a part of this goes to the critical organ within a few days. Fifty percent is held up in the upper respiratory tract and swallowed, so a fraction of that swallowed also reaches the critical organ. In the case of insoluble compounds, it is assumed that 12 percent is retained in the lower respiratory tract, which is usually taken as the critical organ

when considering the inhalation of insoluble compounds. The rest is eliminated by exhalation and swallowing.

3. Fraction going from blood to critical body tissue

Some elements in the blood stream are eliminated rapidly from the body, whereas large fractions of others are deposited in certain body organs.

4. Radiosensitivity of tissue

Some body tissues are more radiosensitive than others. For example, the lymphatic tissue and bone marrow are much more radiosensitive than muscle or nerve tissue. Therefore, in equal concentrations an element like plutonium is more hazardous than uranium because the plutonium concentrates in the most sensitive part of the bone, whereas the uranium goes to other portions of the bone, the kidney, and various other relatively insensitive organs.

5. Size of critical organ

For a given number of microcuries of a radioisotope in a critical organ, it follows that the smaller the organ the greater the concentration and the greater the dose delivered to the critical tissue. Iodine presents a much greater problem than sodium, since iodine is very selectively absorbed in a small body organ, the thyroid gland, whereas sodium is rather uniformly distributed throughout the body. In many cases the radioisotope is deposited in a large organ but localized in a small portion of that organ, so that, in effect, the mass of critical tissue may become very small.

6. Essentiality of the critical organ to the proper function of the body

Some body organs are either not essential to the body function, or, when they are damaged or removed, special steps can be taken to supplement or compensate for their reduced function. It is for this reason that damage to the bone marrow, kidneys, eyes, etc., would represent perhaps a greater hazard than equal tissue damage to the thyroid gland.

7. Biological half-life

Some elements like radium, plutonium, and strontium are deposited in critical body tissue where the rate of turnover is very slow or the biological half-life is many years. These radioisotopes are much more hazardous than the radioisotopes like carbon, sodium, and sulfur (C–14, Na–24, and S–35), which have biological half-lives of a few days or weeks. The principal methods of elimination of radioisotopes from the body are by way of the urine, feces, exhalation, and perspiration. Usually elimination is much more rapid before the radioisotope is translocated from the blood to a more permanent area, such as the bone, than afterward. This time is usually of the order of a few days to a few weeks. After this initial period the elimination rate becomes more nearly exponential, and the application of the term "biological half-life" has real meaning.

8. Radioactive half-lives of intermediate length

The mixture of U–238+U–234+U–235 that occurs in nature does not present much of a radiation hazard (if the radioactive daughter elements are removed); because with the very long, controlling half-life of U–238 of 4.5×10^{9} years, it requires 1.5×10^{6} g. of this uranium isotopic mixture to make up a curie of alpha activity. The maximum permissible amount of this mixture in the body (given in table 3, A) is 0.02 μc. This corresponds to about 0.03 g., and it is unlikely that a person would get this much uranium in his body. If he did, it probably would result in a chemical hazard before the detrimental effects of radiation would show up. The U–233, with a half-life of 1.62×10^{5} years (100 g./curie [1]), and Sr–90, with a half-life of 25 years (6.3×10^{-3} g./curie), are much greater hazards. In the case of Sr–90 in equilibrium with Y–90 the maximum permissible amount of Sr–90 in the body is 1μc., or only 6.3×10^{-9} g., which is about 10^{-13} of the mass of the human body. This concentration is so small and the rate of elimination so low once a maximum permissible amount of S–90 in equilibrium with Y–90 becomes fixed in the bone, that it is then very difficult if not impossible, to make accurate estimates of the amount present. Therefore, if there is exposure to such radioisotopes, every precaution should be taken to minimize the body uptake, and urinalyses should be made frequently so that the amount going into the bone can be estimated from concentrations in the urine

[1] g/curie$=7.66\times10^{-9}At_r$, in which A=atomic weight, and T_r=radioactive half-life of the radioisotope in days.

during the early portion of the period of exposure, when the elimination rate is much higher.

At the other extreme of specific activity, radioisotopes with very short half-lives do not present much of a hazard unless the exposure is maintained by continuous uptake, since the activity of such radioisotopes when deposited in the body soon decays to an insignificant level. As a rule-of-thumb one can remember that the activity is reduced to less than 1 percent after 7 half-lives (2^{-7}= 0.008=0.8 percent). Examples of these short-lived radioisotopes are P-32, with a half-life of 14.3 days (3.5×10^{-6} g./curie), and N-16, with a half-life of 7.35 sec. (10^{-11} g./curie). In general, radioisotopes with intermediate radioactive half-lives of about 5 to 50 years present the greatest hazards, other factors being equal and the danger diminishes for radioisotopes with greater or smaller radioactive half-lives. The most important period of exposure to laboratory personnel is from the age of 20 to 45, because very few younger persons are employed by laboratories that handle radioisotopes, as they are not frequently subject to large internal doses of radioisotopes; and many of the chronic effects of radiation do not manifest themselves until 15 to 25 years after the radiation insult (and 45+25=70 years, which is the average life span). The younger the person who accumulates the radioisotopes in his body the greater the hazard and the more serious the accumulation of intermediate-lived radioisotopes like Pu-239, Ra-226, Sr-90, and Po-210 in the body. It is for this reason that added precautions should be observed not to take into the body radioisotopes like Sr-90 that might be translocated to the fetus; contaminated clothing should not be worn home, where it may present a radiation hazard to young members of the family; and dangerous quantities of radioisotopes should not be discharged into the air or into the public water supplies, where the population as a whole may be exposed. It is generally true also that fast-growing cells of the body are more subject to radiation damage than fully developed cells, and this is a good reason to be more cautious in permitting the accumulation of radioisotopes in young people or in women in the child-bearing age.

9. Energy of the radiation produced by the radioisotope

The radiation hazard associated with a radioisotope deposited in the body is proportional to the average energy of disintegration weighted for the biological effectiveness of the radiation. The total effective energy per disintegration of the Ra-226 plus half[2] the energy of Rn-222 and its alpha-emitting daughter products is 14.5 Mev. The energy per disintegration of Pu-239 is 5.16 Mev, and so (on an energy basis alone) when equal curie amounts of Ra-226 and Pu-239 are deposited in the body, one would expect Ra-226 to be about three times as hazardous as Pu-239. (Actually, it is thought that the fact that Pu-239 is more densely concentrated in the radiosensitive portion of the bone than Ra-226 more than compensates for this energy difference, so that the reverse may very well be true. The maximum permissible amount of Ra-226 (in microcuries) in the body is taken to be about 2.5 times that of Pu-239.) Another interesting comparison is obtained by examining some of the beta- and gamma-emitting radioisotopes. In a comparison of H-3 with Na-24 it is noted in table 4 that the effective energy per disintegration of H-3 is 0.006 Mev, and the effective energy per disintegration of Na-24 is 2.7 Mev. On an energy basis alone the maximum permissible amount of H-3 in the body would be 450 times that of Na-24. In this case both Na-24 and H-3 are assumed to be rather uniformly distributed in a similar matter throughout the body, so that the effective energy per disintegration is the principal factor determining the relative biological damage from these two radioisotopes when deposited in the body. The ratio of the maximum permissible amounts of the two radioisotopes in the body (using values from table 3, B) is approximately inversely proportional to the ratio of the effective energies.

10. Specific ionization and attenuation of energy in tissue

As indicated in table 1 alpha particles are considered to be 20 times as damaging on an energy-absorption basis as beta or gamma radiation because of their high specific ionization. The specific ionization in air of a 1 Mev alpha is about 6×10^4 ion pairs per centimeter path, whereas that of a 1 Mev beta is only 45 ion pairs per centimeter path. It is considered that for most of the gross damaging effects of radiation the concentrated energy loss in tissue produced by

[2] Experiments of R. D. Evans have indicated that about half of the radon escapes from the body.

an alpha particle represents a greater hazard by a factor of 20 than the less dense energy loss in tissue represented by the greater penetration of beta and gamma radiation.

Beta radiation is absorbed mostly in the immediate vicinity of the atoms from which it is emitted, while the attenuation of gamma radiation of the same energy is much slower (e. g., if a beta emitter has a maximum energy of 2 Mev, a negligible fraction of the beta rays has the maximum range in tissue of about 1 cm. In the case of a 2 Mev gamma emitter, only about 3 percent of the gamma-ray energy is absorbed in the 1 cm. of tissue). Hence in a small organ most of the beta radiation emitted in the organ will be absorbed in the organ, whereas gamma energy emitted in the same organ will be absorbed in a much larger volume of tissue or escape from the body altogether. Alpha radiation is even more localized than beta. For example, almost all the energy of the 5.9 Mev alpha from At–211 is absorbed in the thyroid gland, where it localizes. An alpha ray must have an energy of about 7.5 Mev to penetrate the epidermal protective layer of skin about the body, which has a minimum thickness of about 0.07 mm. The range of a 70–key beta ray is about 0.07 mm. of tissue, so only a small fraction of 70 Kev beta rays will penetrate this protective layer. Therefore, hazards from alpha and low-energy beta radiation can be controlled by keeping alpha and low-energy beta sources outside the body.

To give some idea of the complexity of the problem the following factors enter into a determination of the isotope hazard.

1. Quantity available.
2. Initial body retention.
3. Fraction going from blood to critical body tissue.
4. Radiation sensitivity of the tissue.
5. Size of the critical organ.
6. Essentiality of the critical organ to proper function of the body.

Essentiality is difficult to define. The production of carcinogenisis in any organ would, in a sense, define essentiality since this would most likely result in death. On the other hand, irradiation of such organs as the eyes, gonads, or thyroid can produce damage to each without necessarily impairing other body functions at least to the point of causing death.

7. Biological half life.

This includes the physical half life of the radioactive material and the half life of the material itself within the body; in other words the time which it takes for the body to eliminate, by normal processes, 50 percent of the radioactive material that entered it.

8. Radioactive half lives of intermediate length.
9. Energy of radiation produced by the radioisotopes.
10. Ionization density and attenuation of energy and tissue.

Control of the radioactivity in the body is brought about through control of what is allowed to enter the body. Having determined the allowable amount in the whole body or in an organ, the procedure is to work backward to arrive at the concentrations in air, drinking water or food that if taken into the body continuously or intermittently would eventually build up to the allowable amounts. It should be pointed out that the maximum permissible concentrations and body burdens for occupational workers assume continuous intake for the working lifetime of the individual. Where the intake is not continuous larger concentrations may be tolerated.

In arriving at the maximum permissible exposures for man there are no safety factors included as such. Calculations are based on the best biological and clinical information available and are made as rigorous as possible. However, at each stage where any uncertainty exists the most conservative choice of values are chosen. In most cases this introduces a sort of safety factor. One of the largest uncertainties takes into account the individual variations between persons.

In getting the total effect of internal and external radiation on the body, each source is treated separately, and the summation from all sources is made with proper weighting. The organ dose for each source is summed up for the most critical organ. This addition is not entirely clean if more than one organ is involved, which is generally the case for external irradiation. As noted before, these additive effects need much more study but it is believed that present practices for the designation of maximum permissible dose are in the conservative direction.

RECOMMENDATIONS OF THE NATIONAL COMMITTEE ON RADIATION PROTECTION AND MEASUREMENTS

At the present time the National Committee on Radiation Protection and Measurements has prepared a list of maximum permissible concentrations for air, water, and the body for 100 radioisotopes. These are included in table III of Handbook 52. At the present time the committee is working actively on an extension of this list and it is expected that within a few months a new edition of the handbood will list over 300 isotopes. This same information will be used by the International Commission on Radiological Protection.

The maximum permissible concentrations in the revised handbook are, for the most part, not materially different from the old values, even though we have reduced the maximum permissible accumulated dose by a factor of 3. This reduction by a factor of 3 applies only when the whole body or the gonads is the critical organ. This will be important mainly for such radioisotopes as Na-22, Na-24, H-3, C-36, C-37, Br-82, A-37, and some of the noble gases.

The NCRP has recently reaffirmed its previous position that the dose to persons in the environs of radiation sources and outside of controlled areas should be 0.1 of the levels for occupational exposure. On a yearly basis that would amount to 0.5 rem per year. Even though there is some evidence that very young and very old persons may be somewhat more susceptible to radiation effects, no distinction is made between them and persons of intermediate age. This is possible because an average dose of 0.5 rem per year is so low (only a few times background) that many age differentials would average out. Also in this connection the dose in these uncontrolled areas should be averaged over the recipients and should not apply to the single individual. This admittedly has practical legal difficulties but the fact still stands that on the average it will do no harm for some people to exceed this while others have less. Partial body exposures are generally less important than are whole-body exposures, but there may be some important exceptions to this. Again this is an area about which relatively little is known at the present time. If we always consider that we are dealing with whole-body exposures we will continue to err in the safe direction.

EXHIBIT 10. ACUTE LETHALITY OF PARTIAL BODY IN RELATION TO WHOLE-BODY IRRADIATION, H. A. BLAIR, UR-462

THE UNIVERSITY OF ROCHESTER ATOMIC ENERGY PROJECT

By H. A. Blair

ABSTRACT

If to cause acute death from radiation the same amount of lethal substance or injury must be produced, whether the whole body or segments thereof are irradiated, it is shown that the reciprocal of LD_{50} for the whole body, will be equal to the sum of the reciprocals of LD_{50} for the separate segments, all doses being measured in roentgens. Existing data are compatible with this view but they involve irradiation of large segments of the body. For small segments undoubtedly the rule will fail. Some general aspects of partial body irradiation are discussed.

Partial body irradiation is of interest because it is common in occupational and accidental exposures, because it results from the ingestion of most radioactive materials owing to their inhomogeneous distribution and because of its possible use as a technique in studies of the nature of the injurious action of radiation.

One type of experiment which has been performed is determination of LD_{50}—30 days for a given strain of animals for whole body irradiation, for irradiation of a segment of the body and for irradiation of the remainder of the body excluding this segment.

If the segments of the body are A and B, respectively, the whole body is A+B. If the lethal action of the radiation is proportional to the dose and if the same level of lethal effect, Q, is required whether whole body or individual segments are irradiated then

$$Q=AR_A=BR_B=(A+B)R \tag{1}$$

R_A, and R_B and R being the lethal doses in roentgens and A, B, and A+B are now the products of the weights of the segments and the constants of proportionality converting roentgens to lethal effect.

On dividing the two right hand terms of equation 1 by BR_BR

$$1/R = (A/B+1)/R_T$$

but from (1) $$A/BR_B = 1/R_A$$

therefore $$1/R = 1/R_A + 1/R_B \qquad (2)$$

According to this equation and the hypotheses used, the reciprocal of whole body LD_{50} is equal to the sum of the reciprocals of the LD_{50} for irradiations of two segments which together include all of the body.

TABLE 1

Strain	Whole body		Head alone R	Body alone R	Head and body 1/R+1/R
	R	1/R			
Dba	500	0.0020	500	1,265	0.00280
Marsh	570	.00175	1,185	1,018	.00183
C57	550	.00181	1,300	858	.00194
C_3H	492	.00203	1,443	735	.00198

NOTE.—Data by Reinhard et al. (see footnote 1) on determinations of LD_{50}, R, for partial and whole body irradiation in 4 strains of mice. According to the hypotheses used here the reciprocals of cols. 2 and 5 should be equal.

Data by Reinhard et al.[1] giving LD_{50} in four strains of mice for head alone, body alone, and whole body are shown in table 1. It will be seen that agreement with equation 2 is fairly good except for the one instance in which LD_{50} for head and whole body are the same. One of these values is most probably in error on general grounds.

These results are compatible with the view that a given amount of a lethal effect or lethal substance is required to kill and that it is equally effective whether it is produced uniformly in the body or only in a segment thereof.

Equation 1 is compatible with the view, but does not require, that the total number of gram roentgens required to kill are the same whether delivered to the whole body or to a segment only; this because the constants of proportionality in A, B, etc., may, or may not be, proportional to the weights of their respective segments.

Information of this point is supplied by Kereiakes et al.[2] [3] who demonstrated by irradiating rats and mice through a protective grid with equal diameter apertures that the lethal dose in gram roentgens was nearly the same for irradiation of the whole body as for distributed fractions down to about 15 percent of the body. However, when the apertures were reduced in diameter while keeping constant the area exposed, the gram roentgen lethal dose increased. This latter effect was attributed to reduction of effectiveness of the injury by reparative actions occurring across the increasingly larger surface of normal tissue surrounding the irradiated as the apertures become small.

While the experiments just cited show that within fairly wide limits the gram roentgen lethal dose may be the same for whole body and segmental irradiation, when the segments represent all types of tissue, this is not true, according to Swift et al.[4] when the abdomen alone or the remainder of the body alone are irradiated. Their data are shown in table 2.

[1] Reinhard, M. C., A. E. Mirand, H. L. Goltz, and J. G. Hoffman, Mouse-Strain Differences in Response to Radiation, Proc. Soc. Exp. Biol. and Med., 85 : 367–370, 1954.

[2] Kereiakes, J. G., W. H. Parr, J. B. Storer, and A. T. Krebs, Effect of Partial Shielding by Grids on Survival of X-irradiated Rats, Proc. Soc. Exp. Biol. and Med., 86: 153–156, 1954.

[3] Kereiakes, J. G., and T. A. O'Neill, Further Studies of the Effect of Partial Shielding by Grids on Survival of X-irradiated Animals, Army Medical Research Laboratory Report A. M. R. L.–178, 1955.

[4] Swift, M. N., S. T. Taketa, and V. P. Bond, Regionally Fractionated X-irradiation Equivalent in Dose to Total-Body Exposure, Radiation Res., 1 : 241, 1954.

Swift, M. N., S. T. Taketa, and V. P. Bond, Effect of Regionally Fractionated X-irradiation Equivalent in Dose to Acutely Lethal Total Body Exposure, USNRDL–365, 1952.

TABLE 2

	Whole body	Abdomen exposed	Abdomen shielded
R	650–750	1,025	1,950
kg. R	175	134	275
1/R	0.00143	0.00097	0.00051

NOTE.—Data of Swift et al. (See footnote 4.) LD_{50} in roentgens, R, and in kilogram roentgens, kg. R, for whole and partial body irradiation of rats of the Sprague-Dawley strain. Whole body LD_{50} is taken as 700 r in calculating 1/R. Acutally the reciprocal of 676 r is equal to the sum of the reciprocals for the segmental irradiations.

It will be seen that the sum of the reciprocals for partial body irradiations, 0.00148, is essentially equal to, 0.00143, the reciprocal of 700 r the average of the range of values 650 to 750 r given for whole body LD_{50}. Consequently these data conform to equation 2. However, the lethal doses in kilogram roentgens are quite different for the three modes of irradiation. Consequently agreement with equation 2 may be attributed, not to the necessity of equal gram roentgens, for lethality, but rather to the necessity for equal lethal effect or substance. These matters can be discussed most easily by generalizing equations 1 and 2.

Let Q=amount of lethal effect required. Let there be n segments of weight w_1, w_2, etc., requiring, respectively, for lethality the doses r_1, r_2, etc. Then if a_1, a_2, etc., are the constants of proportionality giving the effectiveness of the radiation in producing lethal effect per gram per roentgen in each segment

$$Q=a_1w_1r_1=a_2w_2r_2= \dots a_nw_nr_n=(a_1w_1+a_2w_2+ \dots a_nw_n)r \qquad (3)$$

the last term representing irradiation of all the segments together with lethal dose, r. The sum of the segments may or may not include the whole body.

On dividing the second and last terms by r and $a_1w_1r_1$

$$\frac{1}{r}=\left(\frac{1}{r_1}+\frac{a_2w_2}{a_1w_1r_1}+\dots\frac{a_nw_n}{a_1w_1r_1}\right)$$

But

$$\frac{a_2w_2}{a_1w_1r_1}=\frac{1}{r_2} \text{ etc.} \dots \frac{a_nw_n}{a_1w_1r_1}=\frac{1}{r_n}$$

Therefore

$$\frac{1}{r}=\frac{1}{r_1}+\frac{1}{r_2}+\dots\frac{1}{r_n} \qquad (4)$$

It will be observed that agreement of data with equation 4 imposes no requirement for equality of the products wr, or gram roentgen doses in equation 3, because the a's may be different. In the experiments of Swift et al.,[3] a, for the abdomen is twice as great as that for the remainder of the body. In other words twice as much lethal effect occurs in the abdomen per gram roentgen as in the remainder of the body.

The reason that the data of Kereiakes et al, show a wide range of equality of gram roentgens for lethality is, presumably, that similarly representative samples of all the tissues were usually exposed. When this condition is not fulfilled gram roentgens will vary.

A feature of equation 4 of some interest is its prediction with respect to the lethal dose for any segment in relation to that for the whole body. Let this be the *n*th segment. Then

$$\frac{1}{r_n}=\frac{1}{r}-\left(\frac{1}{r_1}+\frac{1}{r_2}+\frac{1}{r_{n-1}}\right)$$

or

$$\frac{1}{r_n}<\frac{1}{r} \text{ or } r_n>r \qquad (5)$$

That is, the lethal dose in roentgens for irradiation of any segment of the body, no matter how sensitive it may be, is always greater than the lethal dose in roentgens for the whole body.

As a corollary to this the lethal dose will always be increased by shielding part of the body and the increase will be greater the greater the sensitivity of the shielded part.

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It has been implied thus far that the irradiated segments are mutually exclusive and this is necessary if the relations derived are to be obeyed. Furthermore, this condition can be met, by appropriate shielding, in fairly close approximation using external sources. With injected radioactive materials, however, if two varieties are administered alone and then in combination, the regions in which they accumulate may overlap. This is immaterial, however, providing each substance produces its effect independently of the other. It is of interest to consider two such materials.

Using the same notation as before

$$Q = a_1 w_1 r_1 = a_2 w_2 r_2 = \alpha a_1 w_1 r_1 + \beta a_2 w_2 r_2$$

the first terms representing LD_{50} for the two substances separately and the last term representing LD_{50} for the case in which the two materials are given togther in fractions α and β respectively, of the LD_{50} when given alone. Owing to the prolongation of dosage and simultaneous recovery r_1 and r_2 will be effective rather than real doses.

It will be seen that the above relation requires $\alpha + \beta = 1$. It is not to be expected, however, that this relation will be obeyed except for two materials of the same effective half-lives; otherwise their maximal effects will not occur at the same time and consequently will not be wholly additive. On this ground, therefore, it would be expected that $\alpha + \beta$ would usually be greater than 1. It is shown by recent work of Carsten and Noonan [5] however, that recovery from a primary radiation injury as measured by a second dose is faster in the rat when the abdomen and hind limbs alone, rather than the whole body, is irradiated. This suggests the possibility that recovery rate decreases as the irradiated volume increases. If this is true for radioactive materials it permits the possibility that the maximal acute injury from two materials of different tissue distributions acting together and affecting a larger volume of tissue would each be greater than when the materials acted alone. If so $\alpha + \beta$ might be less than 1. This case has been observed by Friedell et al.[6] who found that half LD_{50} of radioactive gold and of phosphorus administered together constituted almost LD_{100}.

It will be seen from these considerations that no simple additive relations are necessary to be expected for the acute lethality of mixed radioactive materials except when they have the same organ distribution and biological half-lives.

COMMENTS

The present analysis is compatible with either hypothesis that radiation produces a toxin which spreads to the whole body or, an injurious effect, confined for the most part, to the volume exposed.

If there is a toxin produced there is no evidence that it is combatted more effectively by nonirradiated than by irradiated tissue. An exception to this may be the spleen. However, its action may be wholly restorative of hemopoiesis rather than detoxifying. In any case spleen shielding is protective much out of proportion to the predictions of the hypotheses used here.

In this regard it is significant that shielding extensive areas of bone marrow does not alter the lethal level of injury in the unshielded segment of the body.

This analysis is compatible with the usual hypothesis that the lethal effects of radiation are in direct proportion to the dose but the proportionality constants are not the same for all tissues.

In the field of partial body irradiation more information is required especially with respect to long term effects. It is known that non-homogeneously deposited internal radioactive emitters cause shortening of life-span but the dose-effect relationships are not obtainable from existing data.

It is indicated by these results that the gram roentgen dose for lethality will tend to be the same for the whole body inhomogenously exposed, homogeneously exposed, and partially exposed. This rule will fail, however, in either direction when the average sensitivity of the exposed tissues is not the same as for the whole body. It will also fail, presumably, for irradiation of segments of the body, not essential to life, such as limb, which, with proper treatment, could be exposed to the extent of necrosis without leading to death of the whole organism.

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[5] Carsten, A. L., and T. R. Noonan, Determination of the Recovery From Lethal Effects of Lower Body Irradiation in Rats, University of Rochester Report UR-445. 1956.

[6] Friedell, H. L., and J. H. Christie, Synergistic Effect of Phosphorus 32 and Colloidal Gold 198 on Survival in Male Albino Rats, Proc. Soc. Exp. Biol. and Med., 76: 207-210, 1951.

(Mr. Taylor's statement—continued.)

A subcommittee of the NCRP is presently working on the gonadal dose from all internal emitters, including those which move relatively quickly through the body. This work is under the direction of Dr. K. Z. Morgan at Oak Ridge, and is an extremely complicated problem, the solution of which will require the most modern machine computation techniques. If this program is successful, it is hoped that the method will be extended to the determination of the skeletal dose which, according to the most recent information, is beginning to look more important, particularly in relation to fallout problems.

PHILOSOPHY AND BACKGROUND OF RADIATION PROTECTION

This part of the discussion will be covered only in outline since much of the supporting information is covered in the accompanying documents.

EXHIBIT 11. PHILOSOPHY UNDERLYING RADIATION PROTECTION, AMERICAN JOURNAL ROENTGENOLOGY, VOLUME 77, PAGE 914, MAY 1957 (MIMEOGRAPH COPY)

ADDRESS BEFORE THE NINTH ANNUAL CONFERENCE ON ELECTRICAL TECHNIQUES IN MEDICINE AND BIOLOGY, NOVEMBER 7, 1956

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In the broad sense, radiation protection is a problem that will touch the lives of most of us and more as time goes on, whether it be in connection with nondestrictive testing, with radiology, or with the production of nuclear power. The uses of radiation-producing machines and the use of radioactive materials will undoubtedly increase as time goes on, and radiation, as such, will become an increasingly important part of our national economy.

The general substance of the discussion [this evening] will have to do with the development of our philosophy of radiation protection. To do this in proper perspective, it will be desirable to review in some detail the early history of radiation protection, and the development of radiation-protection standards.

The term "radiation-protection standards" is used somewhat loosely and yet at the same time there is probably very little real uncertainty as to the broad meaning of the term. One normally thinks of a standard as being something rather firm, rather well understood, inflexible, accurately known, and reproducible. When a standard is mentioned, one immediately thinks of something like the standard meter-bar, carefully locked away in a vault and resistant to the changes of time; or of some accurately measurable quantity, such as the ohm or volt.

However, in the field of radiation protection standards, there are many unknowns and many uncertainties. They involve a great many assumptions that may have to be changed from time to time. In fact it would not be oversimplifying the case to state that our protection standards are essentially protection goals or objectives. Where it is possible to develop numbers to assign to some of the standards, these numbers are really more in the nature of a means to achieve some goal than the goal itself. Even the goal itself is difficult of

definition. It is primarily to determine the limits of radiation exposure to which the individual, or whole population, can be exposed without encountering risks incommensurate with the benefits to be expected from its use. Standards of safety go back directly to the individual who will be exposed to radiation. For this it is necessary to determine how much radiation he can absorb without injury to himself or to his progeny.

Knowing the complication of the human being as an organic structure, and the great difference in sensitivity between individuals, it is very easy to see why such a determination becomes an extremely difficult problem. To point up the difficulties, it may be helpful to review rather briefly some of the early background and philosophy leading to the development of standards of radiation protection. Much has been written on this subject and therefore only the high spots will be touched upon.

Radiation was recognized as a potential hazard to health soon after its discovery. Efforts to understand and curtail radiation exposure to individuals, were not begun seriously until the 1920's. One should bear in mind that it was not until 1928 that the world had a uniform and acceptable unit of radiation dose, namely the roentgen. Consequently, radiation-protection efforts and protection standards were, of necessity, on a purely qualitative basis. Until 1928, most radiation treatments were expressed in terms of fractions of an erythema dose—the amount of radiation that would cause a defined reddening of the skin. This in itself was a very uncertain factor, since it depended upon the energy of the radiation, the time over which it was delivered, the size of the irradiated field, and the amount of backscattering, not to mention the individual's idiosyncrasy with regard to radiation sensitivity. It was not until the midthirties that stray radiation exposure was measured quantitatively, thus making it possible to put radiation-protection standards on a reasonably firm quantitative basis. Interestingly enough, later efforts proved that these early standards were not grossly wrong.

One could venture the suggestion that if there is any basic standard of radiation protection it would be what is now referred to as the maximum permissible dose, or maximum permissible exposure of an individual. By maximum permissible exposure is meant the amount of radiation to which the whole body of an individual can be subjected over the period of his adult lifetime without producing in that individual any detectable harmful effects. For the occupational exposure of an individual to radiation, such a standard might be adequate, but as I will explain in more detail, another basic standard is needed for the exposure of the entire population—one that takes the genetic effects into consideration.

Parenthetically, it should be remarked that the old term "tolerance dose" that prevailed for many years is a complete misnomer. There is no such thing as a "tolerable dose of radiation." No radiation, other than for the treatment of disease, is known to be beneficial to man. Any radiation exposure received by man must be accepted as harmful. Therefore, the *objective should be to keep man's exposure as low as possible and yet, at the same time, not discontinue the use of radiation altogether.*

The big problem is to obtain some quantitative idea, as to the degree of harm that will result from exposure to various amounts of radiation.

As already mentioned, early permissible exposures were expressed in terms of the erythema. This followed as a result of a very few observations made on a very few people who had been overexposed to radiation, under conditions where there was some crude idea as to the amount of radiation involved. As a result of this, numerous proposals were made, and for a time served a useful purpose. For example, Mutscheller proposed as a "tolerable dose," one-hundredths of an erythema dose in 30 days. Others reduced this to one-thousandths of an erythema dose in 3 days. Sievert independently proposed one-tenth of an erythema dose per year, which was not appreciably different from Mutscheller's value.

Various attempts were made to place the tolerance dose on a sound physical basis. Glocker and Kaupp described a tolerance dose as that radiation level which would give just barely visible fluorescence observable in a completely darkened room by dark-adapted eyes. They also described it, as barely visible blackening of a "duplitized" X-ray film after an exposure to radiation of one hour. Mutscheller developed a simple but very inaccurate formula for computing erythema doses, for a given distance from the X-ray tube, for the useful beam. The number of erythema doses was given by the number of milliampere minutes, divided by 25 times the square of the distance.

Knowing as much as we do now about radiation, these standards look pathetic indeed, and yet they marked important milestones leading to our more accurate understanding of the problem. When one realizes that, on the basis of today's knowledge, an erythema dose under given conditions may vary from 270 to 1,000 roentgens over a range of 100 to 1,000 kilovolts, some visualization of the vast uncertainties in the early work might be had.

By the early thirty's such works as just mentioned were correlated and reconciled by the free use of safety factors—or just factors. Germany proposed the first quantitative expression of a permissible dose measured in roentgens, arriving at a figure of 10^{-5} roentgen per second as their so-called tolerance level of radiation. In 1934 it was possible, for the first time, for the International Commission on Radiological Protection to express permissible exposure in terms of roentgens. The value then chosen was two-tenth roentgen per day. In the United States in 1936 a somewhat lower level, namely one-tenth roentgen per day was adopted. This lower value was in part a result of the belief that there was not an adequate safety factor in the international recommendations.

It will be noted that the permissible exposures mentioned above were integrated over varying lengths of time, ranging over a period of 1 second to 1 year. Although precise information on radiation recovery is lacking, it is undoubtedly true that a given dose of some roentgens received in a period of a few minutes is probably more harmful to the individual than the same dose distributed evenly over a year's period of time. Therefore, while the various proposals for permissible exposure appear to reduce numerically to the same quantity, they were not, in fact, biologically equivalent.

Additionally, there is a serious administrative problem involved, when one compares the integration of a dose given over a few seconds or over a year. For example, according to the early German proposals, if at any time an individual is exposed to more than 10^{-5} roentgens in any one second, he would have exceeded his permissible dose rate even though this might only occur once in a year. This, of course, is nonsense. On the other hand, the problem of integrating a dose over a year's time could also present serious difficulties, depending upon the particular technique used. A person might be heavily overexposed during an early period in a year, yet this might not be detected until it was too late. It was through consideration of such reasons as these, that integration over a period of one day was adopted in the mid thirties.

In 1946, the National Committee on Radiation Protection undertook an intensive review of the whole problem of permissible dose. This review was instigated by the fact that, during the Manhattan District days, a tremendous amount of experimental and biological research had been carried out for the purpose of assuring safety to radiation workers; new biological data had become available. It was quickly realized that the value one-tenth roentgen per day, as used in this country, provided only marginal protection. There was increasing evidence, leading the committee to believe that the value should be lowered. At the same time, it was decided to review the question of the period over which the dose would be integrated. For technical, as well as administrative reasons, it appeared that integration over one day was unnecessarily restrictive. Integration over about 1 month appeared to be more reasonable and a compromise was finally reached at one week. The committee arrived at the recommendation of three-tenths roentgen per week as the permissible whole-body exposure to gamma rays and to moderate and medium energy X-rays. This value has since been adopted internationally.

There is one very important fact that I would like to emphasize at this point. From 1928 to 1936, the permissible exposure was one-tenth of an erythema dose or 50 to 100 r per year; from 1936 to 1948 it was 36 r per year; since 1948 it has been 15 r per year. Now, as far as I know, there is not a single case on record where an individual who has maintained his exposure within any of these limits, has developed any detectable injury that can be reasonably ascribed to that radiation exposure. One may ask with some reason, why, if this is such an imposing negative record, has there been a steady lowering of the permissible exposures, and why still further reductions are being considered. The reason for the 1948 change has been well documented in the NCRP report on the permissible dose from external sources of ionizing radiation. This was published as NBS Handbook No. 59.

One point not mentioned there is the fact that through the availability of better technical data required for shielding design, it has now become possible to provide this better shielding at costs that are not unreasonable. In other words, better protection was readily obtainable—why not use it? It might be

remarked that most of the large atomic energy installations have applied further factors of safety of 5–10 to the lower levels introduced in 1948.

Thus far in my discussion, the problem of radiation exposure to the individual has been considered only under essentially occupational conditions. There are a number of other conditions that have to be considered. Radiation effects on human beings may be more or less significant depending upon many factors. For example, there is increasing evidence that irreversible genetic damage may result from exposure of the gonads to any amount of radiation. All such exposure is cumulative. This implies that an exposure that may not produce any harm to the individual himself, may be passed down through the genetic chain to the descendants of this individual. One cannot neglect the possibility that such damage may occur.

I should point out rather quickly that the importance of this problem has been very evident to the National Committee and the International Commission for at least the past 10 years. The problem was clearly recognized by the NCRP in 1946 when it established a new subcommittee on permissible radiation exposure and included 3 geneticists in its membership. In its final report, issued as NBS Handbook 59, there was frequent mention of the problem, although it was felt then that there was insufficient evidence to warrant the establishment of further quantitative limitations on exposure, for purely genetic reasons.

In 1952 the International Commission on Radiological Protection met in Stockholm with a number of genetricists for the specific purpose of considering the genetic problem in relation to the possible exposure of a much larger fraction of the population than had theretofore seemed reasonable. Recognition of the potential hazards of the new nuclear age had been forced upon us.

The impact of radiation exposure on a large homogeneous population group—a few hundred thousand persons—was considered from the point of view of the resultant genetic burden. On the basis of the premises then accepted, the maximum allowable per capita exposure averaged over the population group ranged, from about 3 to 20 rems per person. An average of about 10 rems in addition to background appeared to be a reasonable compromise, but no specific value was officially adopted. One reason for not being specific was the uncertain state of our knowledge of human genetics, and the widespread of opinion among the several geneticists present. Another reason was the belief that the situation would not become sufficiently critical or dangerous within the next 5 or 10 years to warrant the introduction of further limitations on radiation exposure. It should be remarked here that there are still a number of qualified persons who share this belief.

In March of this year the ICRP again took up the genetic question. Again opinion was somewhat divided, but there was clear recognition of substantial improvement in our genetic knowledge—at least of animal genetics. In a very carefully guarded statement they said that "Until general agreement is reached, it is prudent to limit the dose of radiation received by the gametes from all sources additional to the natural background to an amount of the order of the natural bacground in presently inhabited portions of the earth." This would be an amount of the order of 3 to 4 rems in 30 years or roughly 0.1 rem per year. It was further recommended that for radiation workers the exposure be limited to 50 rems accumulated during the employment period up to age 30. If one considers employment of about 10 years, up to age 30, as about average, this would mean an average of 5 rems per year as compared with 15 rems per year, the level in use for the past decade.

Very similar figures were proposed by the National Academy of Sciences in its report issued in June.

In September of this year the National Committee on Radiation Protection met to consider what steps it should take in interpreting these various recommendations for practical use in the radiation field. This was undertaken with some degree of urgency since the National Committee has, for many years, been looked to for the basic recommendations on radiation safety standards in this country. In interpreting the recommendations of the ICRP, it was deemed necessary to introduce numerous qualifications in order to permit reasonable uses of radiation, while at the same time providing reasonable protection not only to the individual but to future generations.

Before going into the details of the NCRP recommendations it should be mentioned that together with genetic considerations, attention was given to the possible effect of radiation exposure upon the average expected life span. Here again, on the basis of animal experiments as well as some limited human data,

there is increasing evidence that either chronic or acute exposure of the whole body to radiation may statistically shorten one's life expectancy. This shortening of life span may amount to as much as 1½ percent per 100 rems of whole body exposure. For partial body exposure the effect would be only a fraction of that for whole body exposure.

Well, these facts, if facts they be, are not such that much comfort can be derived from them. But, they should be considered in proper perspective, and in relation to the many other complicatetd facets that may influence our lives in much the same way.

The National Committee on Radiation Protection felt it was essential to minimize these additional risks—and to some degree controllable—that may be associated with the use of radiation. To this end, it is now proposing a general lowering of the maximum permissible exposure levels for both radiation workers, and, for the general population. These changes are patterned after the 1956 recommendations of the ICRP. Without going into great detail, the new recommendations that are being considered, will be outlined.

First we have defined a controlled area as one under the supervision of a radiation safety officer and one in which personnel can be exposed to radiation or radioactive material. Any person working in a controlled area is a radiation worker.

A summary of the maximum permissible accumulated dose for radiation workers is as follows: The maximum weekly limit for whole body exposure will remain at 0.3 rem per week but will be subject to further limitations. For example there will be a limitation according to age—this is primarily for genetic reasons. Thus:

Up to age 30 the limit will be 50 rems.
Up to age 40 the limit will be 100 rems.
Up to age 50 the limit will be 150 rems.
Up to age 60 the limit will be 200 rems.
Up to age 70 the limit wil be 250 rems.

For design purposes, or for that matter for all practical purposes, these figures indicate that an average exposure of 5 rems per year may be regarded as permissible. On the other hand, the average may be taken over 10 years, provided the decade limits are not exceeded.

Most of the rules given in Handbook 59 will still apply but some will be modified to comply with the average yearly limitations of 5 rems to the blood forming organs, gonads and lens of the eyes, and to a limitation of 10 rems to the skin.

The rule allowing double exposure over age 45 is no longer necessary, but on the other hand, for persons over that age having no radiation history, the weekly limit of 0.3 rem may be taken indefinitely.

Let us consider next the problem in uncontrolled areas. Here we have the situation wherein persons may be subject to radiation without their knowledge or permission. If for no reason other than ethics, their exposure should be substantially less than for a radiation worker. For such persons, the important consideration is their *average* exposure—that is the exposure per individual averaged over the whole population. Thus the exposure of one person may be very much higher than this average, while the exposure of another may be very much lower. This, incidentally, is one of the hardest points to get across to the public. They hear some number like 10 rems bandied about as an average exposure—then they get a dental X-ray which someone says is larger than 10 rems and they think 'they've had it."

Averaging should be done for the population group in which crossbreeding may be expected. While at the moment it is not feasible to determine this average exposure with any reasonable accuracy, the adoption of some figure is necessary for planning purposes.

For the foreseeable future, it may be assumed that the integrated exposure of all radiation workers will be small compared with the integrated exposure of the whole population. Furthermore, persons nonoccupationally exposed in the immediate vicinity of radiation sources constitute only a small portion of the whole population. Therefore, if this latter group is allowed a maximum yearly exposure of 0.5 rem per year, it is not likely that the average per capita exposure to manmade radiations would exceed 6 or 7 rem up to age 30. This, added to the background radiation of 4 rem and medical exposure of 3 rem will make an overall average exposure of 14 rem.

The new recommendations will permit some latitude in the designation of controlled and uncontrolled areas. Within an installation, any space may be designated as a controlled area, and the permissible occupational levels used therein. But, the area and the employees must then be monitored. This leaves to the employee the decision as to whether or not to work there. Matters of economics and employee relations will dictate the decision as to designation of controlled areas.

Permissible radiation levels for internal emitters will conform to the general principles that I have just outlined. Where the critical organ is (*a*) the gonads, (*b*) the whole body, or (*c*) the blood-forming organs, the maximum permissible concentrations for air and water will be one-third the present values specified for radiation workers. For persons outside controlled areas, the maximum permissible concentrations should be further reduced by a factor of 10. Where single organs are regarded as the critical organ, the present maximum permissible concentrations will continue.

Let us return for a few moments to further emphasize the meaning of this average exposure—this figure of 10 rem that has been so thoroughly misunderstood and misused. The genetic limitation on radiation exposure depends upon the total dose to the population—not the dose to any individual. Thus, for every million persons, we will allow in addition to background exposure, a total dose of 10 million rem in their first 30 years, or 14 million rem including background. This million persons includes radiation workers, doctors, dentists, and all other persons whether they receive radiation or not.

Let us see how this 14 million rem is used up. First take medical exposures—according to present practice, this will use up 5 million rem. Next take radiation workers, which may be estimated as not more than one-third percent of the people, or 3,000 per million population. They may receive an average exposure of 50 rem up to age 30—this would use up one-fourth million rem.

Persons living around radiation areas, would not reasonably be expected to exceed 1 percent of the population at the moment. Their radiation allowance is only one-tenth that of radiation workers, or 5 rem in 30 years—this uses up only another quarter million rem. Present background radiation to which all persons are exposed is of the order of 4 rem in 30 years—this will use up 4 million rem.

These figures add up as follows:

	Million rems
Background (now)	4
Medical	5
Occupational	¼
Around plants	¼
Total	9½

Since we can accept a total of 14 million rem per million persons, this leaves a balance—you might say, a reserve—of 4½ million rem, or about half again as much as we are already using. Some of this reserve may eventually go into increases in medical exposure, increase in fallout radiation, increases in atomic power operations, and so on. The way in which we distribute our use of this reserve is a matter to be worked out as time marches on.

So much for the new recommendations of the NCRP.

Now at this point let us think a little about what we have done. Of course it is clear that over a period of many years we have set physical limits to the radiation exposure that man may be allowed to accept. It is also clear that there is an almost total lack of direct experience on the effects of radiation on man. There is a wealth of indirect experience.

Also—and this is a most impressive fact—there are virtually no cases where persons subjected to any of the permissible exposures of radiation have evidenced any detectable harm to themselves.

Let us pause a moment to consider this most unusual situation which has almost no parallel in industrial hygiene. In the well-known situation with silicosis, there was clear evidence as to cause and effect, and positive steps could be taken to minimize its incidence. Similar to radiation, in a sense, is the fact that the disease might first show itself many years after the exposure to silica dust occurred. However, and differing from radiation, once the disease has developed there is a positive relationship to its cause; this is the case with most industrial toxic agents.

One of the most important—if not the most important—aspects of radiation injury is the absence of a clear and definable relationship between the exposure

and the end result. Many of the manifestations of disease that can be ascribed to radiation exposure can equally well develop in persons who have absolutely no radiation exposure history. Considering this, together with the fact that radiation exposure does not excite any of our ordinary sensory organs, makes it loom as a hazard quite distinct from all others. It is primarily for these reasons that such an enormous effort has been expended over the past 30 years on matters of radiation protection. It is surely why, with this particular hazard, we are today relatively so much further ahead than we might be with any other hazard having such a short history. It is the reason why we are doing so much to prevent something from ever happening.

There are perhaps other reasons which exist even though we may not ordinarily think of them as such.

I am reminded of an incident that occurred at a scientific meeting a few years ago. In the course of the discussion of a paper on radiation protection, I was taken to task for having used the phrase, "the science and philosophy of radiation protection." The discussant insisted that the radiation protection problem was straightforward, scientific and clean cut and that there was no argument as to permissible exposure levels and such. I can think of nothing further from the truth, and nearly everything that I've said this evening will bear me out.

The basic problem is even more complicated than just science and philosophy. Here I must tread lightly.

Today, as we consider the total problem of radiation exposure, I feel certain that we are forced to consider not only the philosophical and scientific aspects but also the aspects of morality. I'm not sure where one begins or the other ends.

Consider the most beneficial use of radiation that we can think of—the diagnosis and treatment of disease. No one can question the enormously important benefits that radiation offers in improving our health. To deny ourselves this tool i sto take a step back toward the dark ages. Yet, as we now know, it carries some element of risk—not necessarily to ourselves, but to some unknown, unborn person at some unknown time in the future. How can we balance the choice between our own health—or possibly even our own survival—between this and that unidentifiable life of the future. I suspect the answer is clear, but can we be sure?

For the present, we must preserve our sense of proportion. Through misunderstanding and many confusing public statements, the public does not know what to do. This is becoming more evident daily, as we hear of people refusing to have chest X-rays, or dental examinations. There is no future for that unborn child, if first we die of tuberculosis or infection. Here is where our sense of proportion enters.

For the radiologist or physician or dentist, it is essential that he know more about matters of protection. It is essential that he be trained to perform the necessary examination with the very minimum of exposure to the patient. It is essential that he have equipment designed to minimize or eliminate exposure that serves no useful purpose. It is essential that he know the dose that he administers—at least within some reasonable limits. If he knows this he will almost surely become aware of the protection problem.

For the individual—the prospective patient—it is essential that he place confidence in the physician. The ordinary individual cannot exercise judgment as to the need for radiation diagnosis any more than he can as to the usefulness of a particular prescription. He can, however, avoid unnecessary radiation by not "shopping around for better X-rays", by not using such foolish things as shoe-fitting X-ray machines, and by not bringing unnecessary radioactive devices into his home. Let him keep his sense of proportion.

Let us take the genetic problem. Let us assume that the geneticist is correct in predicting that if we expose the population to an average per capita dose of X rem, we will produce Y genetic deaths at some time within the next Z generations where Z may be any number up to, say, 50. (This is of the order of 1000 or 1500 years.) Let us note that these deaths will be in such forms as failure to conceive, still-births, miscarriages, death below the average lifetime and so on. Let us also note that few if any of these deaths will ever be individually and causatively related to some specific earlier genetic damage caused by radiation.

On the other side of the ledger, let us note that through the use of radiation today we may be able to better our lives either as to material things or as to health.

But even here—strange to say—the problem is complicated by our enormous improvement in health. Many who have faced the problem will question the wisdom of prolonging life to the point of so greatly increasing the number of people in the state of senility. Others will question the wisdom of prolonging,

in misery, the lives of those unfortunates for whom there is no hope in living. The answers to these problems are largely matters of ethics and morality. Of course it must be admitted that through overexposure to radiation we may well augment rather then decrease these numbers.

In any case, with all our knowledge today, we are still dealing with a large number of imponderables. Are we to be morally charged with murder if through our judgment today we allow—or cause—these genetic deaths of the future? Can we leave to the future generations the problem of somehow adjusting themselves to an environment of higher radiation levels? Will the future race be better off or worse off with whatever changes may take place in it? The answer here may lie in whether or not this future race, having knowledge of itself, will consider that he has reached perfection—as apparently we now consider ourselves to have done. This is a matter of opinion.

We pride ourselves today on our greatly increased stature developed over the past few hundred years. For what purpose? It just necessitates a bigger foxhole to keep from being shot by the other guy. A genetic improvement? I sometimes think—facetiously, I'll admit—that we may be in more genetic danger today from automobiles and comic books than from radiation.

But, laying all of these diversions aside, I think our course today is relatively clear. *We must minimize our exposure to radiation. We must maximize the benefits it offers to our health and our material well-being.* These two aims are mutually contradictory as far as we know today. Radiation protection is not only a matter for science. It is a problem of philosophy, and morality, and the utmost wisdom. What is the answer? I don't know. No one does.

(Mr. Taylor's statement—continued.)

I would like at the outset to correct the general impression that radiation hazard is a new problem. It is not. The problem of radiation protection has been under continuous study for nearly 30 years and the setting of permissible dose levels has kept apace with our knowledge and the seriousness of the problem. The present is no exception.

In 1928 the International Commission on Radiological Units and Measurements established the basic radiation units necessary to put protection standards on a quantitative basis. They have revised the units and concepts as necessary to cope with the rapidly changing problems.

EXHIBIT 12, ICRU HISTORY

October 1956

1. FORMATION

The ICRU was formed in 1925 under the auspices of the First International Congress of Radiology, then meeting in London. The single factor having the most bearing on the formation of this Commission was the absence of any units of radiation dosage for use in therapeutic application of radium and X-rays. Up to that time, several units had been proposed and were in use in various countries but there was no international acceptance of anyone of them.

2. MODE OF OPERATION AND HISTORY

In its initial formation, the ICRU consisted of 2 representatives from each of the countries participating in the congresses of radiology. Of these 2 members, 1 was expected to be a physicist and 1 a radiologist. As a matter of fact, about three-quarters of the members were primarily radiologists. Meetings of the ICRU were held during the International Congress. The continuity of the Commission and the arrangement of meetings, agenda, etc., were in the hands of a secretary selected from among the Commission members. The meetings of the Commission were presided over by 1 or 2 honorary chairmen who were usually people of substantial scientific or radiological reputation from the country in which the meeting was held. Occasionally there were also honorary secretaries having no defined responsibilities. In general, these honorary positions were filled by persons having little or no contact with the radiological field and hence were honorary in the strictest sense of the word.

Since there were of the order of 50 countries participating in each International Congress, the rules of the Commission permitted as many as 100 members. As a matter of fact, many countries made no attempt to designate members and the largest number meeting at any one time was 41. With such a large membership, it was extremely difficult to carry out the Commission's work. As

a matter of fact, it usually developed that some 8 to 10 persons carried the load. On the other hand, many countries felt they should have a voice in the matter, with the result that there was an enormous amount of useless discussion at each of the meetings.

Formal minutes of the meetings were not kept. It was considered that the report released by the Commission constituted its findings as well as its minutes.

1925

At the first meeting of the ICRU in London in 1925, some tentative recommendations were made with the idea that they would be considered for action at the next meeting in 1928, at which the full membership would be present.

1928

The second meeting of the ICRU was held in Stockholm in 1928 at which time there were about 40 members present. At this meeting, the Commission adopted the definition of the unit of X-ray dose known as the roentgen. This was an extremely important step forward and for the first time it was made possible to measure radiation in all countries in terms of the same unit. This unit has continued in use to the present time with only minor modifications.

1931

The third meeting of the ICRU was held in Paris in 1931 at which time 39 members were present. Between these meetings, international comparisons of X-ray standards were carried out, one in 1928 by Behnken using a portable thimble chamber, and one in 1931 by Taylor using a portable primary standard. As a result of these intercomparisons, the ICRU in 1931 made some specific recommendations regarding the general characteristics of primary X-ray standards and of the radiation quality ranges over which these standards were to be used. This then marked the first time that there was substantial agreement regarding the basic characteristics of primary radiation standards.

Beginning in 1931, in addition to the members designated by the member countries of the Congress, the Commission also included in its membership representatives designated by each of the recognized national laboratories. At the time, this included the national laboratories of England, Germany, Sweden, and the United States.

At the same time, it became evident that the large size of the ICRU made its deliberations unwieldy. It was therefore proposed that the continuity of program and the direction of the technical discussions be placed in the hands of a small group known as the executive subcommittee. This arrangement was put into effect for the following meeting.

1934

The fourth meeting was held in Zurich in 1934, and was preceded by an informal meeting of the executive subcommittee, then consisting of five persons plus a secretary. At this meeting, it was agreed that the affairs would be managed by the executive subcommittee, with the secretary of the executive subcommittee responsible for the records and continuity between congresses. By this mechanism, the essential technical agreements were reached by the executive subcommittee, and the meeting with the total membership of 33 was mainly for the purpose of accepting and ratifying the previously developed recommendations.

1937

The fifth meeting of the ICRU was held in Chicago in 1937, with 41 members attending. Again at this meeting, the executive subcommittee was in full operation and the affairs were conducted expeditiously. There were no important units recommendations made as a result of the 1934 and 1937 meetings other than those relating to some details in the definition of the roentgen. There was developed at this time the first pattern for a more adequate description of radiation treatment conditions. This was designed to take into consideration all of the many factors necessary for a description of a radiation treatment. These treatment recording recommendations have been continued since that time but have undergone some modification at almost every meeting of the Commission.

Fifteen years lapsed before the next meeting, during which time World War II had been in progress, and many of the original members of the Commission had

either died or been killed. During this interim period Taylor, as secretary of the Commission, had kept abreast of developments in the field preparatory to reviving the Commission's operations.

In reorganizing the Commission, it seemed advisable to change its basis of membership selection in order to avoid the handicap of working with such large numbers. In advance of the fifth congress, discussions were held between L. S. Taylor and W. V. Mayneord and it was agreed that a membership of about 12 would be desirable. With this number, it would no longer be necessary to have an executive subcommittee. This suggestion was put to the past president of the fourth congress and the incoming president of the fifth congress, and was tentatively accepted by them subject to later approval by the International Executive Committee of the Congress. Approval was obtained during the 1950 congress in London.

1950

The reorganized Commission held its sixth meeting in London in 1950. At this meeting a set of rules was developed governing the membership and the work of the ICRU. The rules limited the membership of the Commission to a chairman and 12 additional members, selected for their recognized technical ability without regard to nationality. They also insured a reasonable turnover in membership, yet at the same time provided for adequate continuity of membership.

In its 1950 recommendations, the ICRU recognized for the first time the need for absolute measurements of radiation based on calorimetry or other fundamental techniques. It recognized further that the Commission was not in a position at that time to make any specific recommendations but the way was prepared for introducing improvements at a later time. There was also developed a more completed section on the specification for the description of X-ray treatments.

For the year or 2 prior to the 1950 meeting, the American and British Units Committees had been very active in the development of the concepts of energy measurements as applied to radiological problems. These were widely circulated well in advance of the meetings with the result that at the 1950 meetings, it was relatively easy to bring about substantial agreement between the different viewpoints prevailing. This advance action paved the way for the acceptance of the new energy unit for dose proposed later by the Americans and British.

1952

In 1952, a joint meeting was held in Stockholm between the ICRU, the ICRP, and the UNESCO Joint Committee in Radiobiology. This meeting was primarily for the purpose of discussing the genetic aspects of radiation. While it was agreed that no specific recommendations would be made as a result of these meetings, it is interesting to note that the general findings were substantially the same as those made in 1956 by the ICRP and by the National Academy of Sciences.

1953

The seventh meeting of the ICRU was held in Copenhagen in 1953 with 12 members attending. The most important outcome of this meeting was the introduction of a new basic unit for the measurement of radiation dose. This unit, known as the rad, was designed to place the measurement of dose on the basis of first principles. The reason for this was related to the fact that we were using energies very much higher than was ever dreamed of in the thirties, and for which the roentgen is not always the most suitable unit of measurement. It was clearly recognized during these meetings that much more information needed to be provided before the rad could be regarded as a practical unit. However, between 1953 and 1956, its attractions became evident and it has begun to appear regularly in the radiological literature.

The Commission established two subcommittees to provide more concentrated studies in the fields of X-ray standard and standards of radioactivity. Dr. W. J. Oosterkamp (Netherlands) served as chairman of the Subcommittee on X-ray Standards, and Dr. B. Rajewsky (Germany) as chairman of the Subcommittee on Standards of Radioactivity.

During the 1953 meetings, the Commission held its first symposium at which invited papers were presented on current work in radiation units and measurements. This provided an opportunity to discuss at open meetings the work being done in various countries and the problems that require further investigation.

1955

In 1955, an informal meeting of the ICRU was held in Geneva during the International Conference on the Peaceful Uses of Atomic Energy. This was attended by those members present at the conference; and was mainly in preparation for the formal meeting to be held the following year.

1956

The eighth meeting of the ICRU was held in Geneva in the spring of 1956. This marked the first time that the Commission had met separately from the meetings of its parent organization—the International Congress of Radiology. Meetings were held jointly with the ICRP and extended over a period of 12 days.

Another departure at these meetings was the fact that they were held with the assistance of the World Health Organization. This resulted from contacts made during the preceding few months, when the WHO had indicated its need for technical advice in the field of radiation units and protection. During the course of the meetings, the Commission accepted the invitation of WHO to enter into an official relationship as a nongovernmental participating organization. As a result of this relationship, the ICRU is now recognized by the World Health Organization as its body of technical advisers in the field of radiological units and measurements.

The meetings were attended by the 10 members and the chairman of the main Commission; 11 subcommittee members and 5 subcommittee consultants were also in attendance.

The report of the ICRU developed during the 1956 meetings represents the most complete effort thus far. In addition to some degree of clarification of the different units used in measuring radiation dose, the report will include for the first time a large body of technical data. It also will include extensive discussions and instructions regarding the problems met in introducing the new energy units into medical and biological practice.

It was also agreed that some interim secondary standards of radiation measurement should be developed and made available to any countries requiring calibration of its equipment. The National Bureau of Standards (United States) agreed to undertake the responsibility for the development and construction of this equipment.

The committee structure of the Commission was reorganized and enlarged, and patterned very closely after that developed recently by the National Committee on Radiation Protection and Measurements in the United States. As a result, there are now the following four committees:

Committee I: Standards and Measurement of Radioactivity for Radiological Use. Chairman: W. E. Perry, United Kingdom.

Committee II: Standards and Measurement of Radiological Exposure Dose. Chairman: H. O. Wyckoff, United States.

Committee III: Measurement of Absorbed Dose and Clinical Dosimetry. Chairman: L. H. Gray, United Kingdom.

Committee IV: Standard Methods of Measurement of Characteristic Data of Radiological Equipment and materials. Chairman: B. Combée, Netherlands.

In order to better describe the Commission's scope of activities, it's name was changed at this time to International Commission on Radiological Units and Measurements (ICRU).

During the meetings, 2 half-day symposiums were held for the purpose of presenting and discussing 17 reports on specialized problems in the field of radiation units and measurements. The symposiums were arranged by the ICRU, and through the assistance of the World Health Organization, were held in the U. N. buildings. They were attended by some 75 persons including members of the ICRP and its subcommittees.

For a period of 3½ days following the commission meetings, selected members of the ICRU met with a WHO study group to consider on a worldwide basis how the recommendations of the ICRU might better be implemented. As a result of this study, the WHO will take active steps to disseminate on a worldwide basis the recommendations developed by the ICRU.

1956

An informal meeting of the ICRU was held in Mexico City in the summer of 1956 during the Seventh International Congress of Radiology. Seven members

who were in attendance at the congress met to work on some of the phraseology of the report which had been developed in Geneva.

At the time of writing, preparations are going forward for a joint meeting of the ICRU and ICRP to be held in New York October 31 to November 7, for the purpose of discussing a number of problems to be studied for the U. N. Scientific Committee on the Effects of Atomic Radiation.

Listed below are the formal meetings of the ICRU held since its inception, the number of members in attendance, and the commission officers. (The officers listed were in office at the time of meeting. Terms of new officers begin after confirmation of their election during the international congress, and continue for a 3-year period corresponding to the interval between congresses. Honorary chairmen are not listed.)

Date and place of meeting	Members present	Officers
1925, London	(?)	E. A. Owen (United Kingdom), secretary.
1928, Stockholm	40	Do.
1931, Paris	39	Do.
1934, Zurich	33	Do.
1937, Chicago	41	L. S. Taylor (United States), secretary.
1950, London	13	W. V. Mayneord (United Kingdom), chairman; L. S. Taylor (United States), secretary.
1953, Copenhagen	12	Do.
1956, Geneva	[1] 11	L. S. Taylor (United States), chairman; W. J. Oosterkamp (Netherlands), secretary.

[1] Main commission only. 11 subcommittee members and 5 subcommittee consultants were also present.

The Eighth International Congress of Radiology is scheduled to met in Munich in 1959. The next formal meeting of the ICRU has not been scheduled and may be held prior to the congress. It is expected that committees of the ICRU will meet more frequently than the main commission to expedite the completion of their special studies. For the period 1956–59, the membership of the main commission numbers 13, and that of the 4 committees (including consultants) will be approximately 40 to 50. Officers of the main commission for this period are L. S. Taylor (United States), chairman; L. H. Gray (United Kingdom), vice chairman; H. O. Wyckoff (United States), secretary.

(Mr. Taylor's statement—continued.)

Beginning in 1928, the International Commission on Radiological Protection has set standards and protection procedures on an international basis.

Exhibit 13. ICRP History

October 1956

1. Formation

The ICRP was formed in 1928 under the auspices of the Second International Congress of Radiology then meeting in Stockholm. The formation of an international protection committee had been discussed by the first congress in 1925 but no active steps had been taken at that time toward this end.

During the second congress, plans for organizing an international protection committee were developed by G. W. C. Kaye and Stanley Melville (Great Britain). and L. S. Taylor (United States). Dr. Gustav Grossman (Germany) and Dr. Rolf Sievert (Sweden) were brought into the discussions and plans were completed for the organization of a committee. Their proposal and organization plan were presented to the second international congress and were approved. The committee was named the International X-Ray and Radium Protection Committee, and its membership consisted of the five individuals who planned the organization.

2. Mode of Operation and History

Because of the experience of the ICRU with its large and unwieldy membership, the committee decided at the outset to keep its membership as small as possible. It also decided to include in its membership only people who were working actively in the general field of radiation protection. For this reason, the initial committee consisted of only five persons. This was enlarged for the

next congress by adding Dr. Iser Solomon of France and Dr. Pugno-Vanoni of Italy.

The ICRP, because of its small size, was operated much more informally than the ICRU. The chairman from 1928 through 1937 was Dr. G. W. Kaye, then of the National Physical Laboratory, and the honorary chairman Dr. Stanley Melville. It might be remarked (with some interest in view of later events) that at the time of its formation the problem of radiation protection was not considered to be as important or as serious as that of radiation units. Members of the ICRP felt that the problem would grow and sought to be prepared.

1928

The committee held its initial meeting in 1928 for the purpose of adopting some interim protection regulations. Until that time, the only clearly formulated recommendations were those prepared a few years earlier by the British committee. These were used as a basis for discussion. The first recommendations of the committee were very similar to the early British proposals, and until 1950 its recommendations were patterned around these early British recommendations.

1931

The second meeting of the committee took place in Paris in 1931 and was attended by all seven members. At this meeting, there was some discussion of the possibility of introducing so-called tolerance levels of radiation for radiation workers. This was recognized as desirable, but with very little evidence to go on, no specific recommendations were made.

1934

The third meeting took place in Zurich in 1934 and was again attended by all seven members. This meeting was notable because there was established for the first time a permissible level of radiation exposure. This was related to the newly established unit of X-ray dose—the roentgen. The level then set was 0.2 roentgens per day. (It might be noted that the year following this, the National Committee on Radiation Protection in the United States recommended a lower level of 0.1 roentgen per day.)

1937

The fourth meeting was held in Chicago in 1937 with the same seven members attending.

Throughout the period 1928 through 1937, the main work of the committee consisted of gradually enlarging and extending the recommendations originally developed at its first meeting. Until then, the main item of the protection recommendations was the specification of the thicknesses of barriers of various materials that should be interposed between a source and an individual in order to assure adequate protection. Prior to the establishment of the permissible dose in 1934, these barrier thicknesses had been somewhat arbitrary. In 1934 and again in 1937, the relationship of the barrier thickness to the actual dose in roentgens was clarified and was put on a sound quantitative basis.

Following the deaths of both the chairman and the honorary chairman, L. S. Taylor acted as secretary for the committee until its next meeting which was not to occur until 1950.

Under circumstances similar to those of the ICRU, the ICRP was reorganized in 1950 on a different basis from that which prevailed before the war. By that time, the questions of radiation protection had assumed far greater importance than it had earlier. This, of course, was due to the advent of atomic energy. The Committee was enlarged in size to 12 members in order to better deal with the problems facing it. The new membership was made up of individuals having recognized standing in the field without regard to nationality, and its operating rules were made the same as those of the ICRU. At this time, its name was changed to International Commission on Radiological Protection.

1950

The ICRP held its fifth meeting in London in 1950, attended by 9 members and a chairman. Between 1946 and 1950, a large body of protection information had been prepared by the NCRP in the United States, having to do with

exposure of the whole body to radiation from both external and internal sources. The NCRP had also developed considerably more details in the general field of X-ray protection. This large background of information was used as a basis for discussions of the ICRP which resulted in the development of its 1950 report. It should be mentioned that an extensive interchange of this information had previously been accomplished through the medium of the tripartite conferences between the United States, Canada, and England. The results of these conferences were in turn based to a considerable extent on data developed during the Manhattan District days.

With the availability of this new information, the ICRP lowered the maximum permissible dose for radiation workers from 0.2 r/day (in use since 1934) to 0.3 r/week. The report also included for the first time maximum permissible concentrations in the body of approximately ten radioactive isotopes.

Because of the very large task confronting it, the ICRP established subcommittees to deal with special phases of the radiation protection problem, their general structure following that developed a few years earlier by the NCRP. This arrangement has greatly facilitated the development and presentation of information.

1952

An interim meeting of the ICRP was held in Stockholm in 1952, with the ICRU and the UNESCO Joint Committee on Radiobiology. At this time, the major subject under consideration was the possible genetic effects of radiation. The meeting was attended by a number of geneticists from several countries. At this time, it was recognized that the genetic effects of radiation were becoming better understood and should be taken into consideration. Figures were discussed for an average per capita dose for a large population. However, because there was very little agreement between the geneticists themselves, and because of the meager information available on this question, it was decided not to make any specific recommendations. (It is interesting to note, however, that the recent recommendations made on this question in 1956 by the ICRP and by the National Academy of Sciences and other groups give approximately the same range of figures as those discussed at the Stockholm meeting in 1952.)

1953

The sixth meeting of the ICRP was held in Copenhagen in 1953. This was a joint meeting with the ICRU and was held during the week preceding the opening of the International Congress. Most of the representatives on the subcommittees also met.

The report of the commission as developed during these meetings marked a radical departure from all others. Its total size was 92 pages and it included a great amount of detail never before agreed upon on an international basis.

The main body of the report consisted essentially of the reports of its five subcommittees:

1. Permissible Dose for External Radiation
2. Permissible Dose for Internal Radiation
3. Protection Against X-rays Generated at Potentials Up to Three Million Volts
4. Protection Against X-Rays Above Three Million Volts, Beta Rays, Gamma Rays, and Heavy Particles Including Neutrons and Protons
5. Handling and Disposal of Radioactive Isotopes

The recommendations on permissible dose from external radiation were somewhat broadened over those previously made, but the basic permissible exposure level of 0.3 r/week was reaffirmed.

Subcommittee 2 gave the maximum permissible concentrations of radioactive materials in the body, air, and water for nearly 100 radioactive isotopes, and included a large body of reference material.

Subcommittee 3 produced a very detailed report applying to both medical and industrial radiology, and included recommendations regarding radiation levels from special devices such as television receivers.

Subcommittees 4 and 5 issued brief reports; much of the material in their areas was still in an unsatisfactory state at that time.

With the development of this detailed report, a new problem arose. Previously the reports had been relatively short and could be published as a few pages in any of the technical journals. However, this report was too large to be published in this manner, and for some time there was uncertainty as to whether

or not it could be published in its entirety. Eventually it was published as a supplement to the British Journal of Radiology. This had to be underwritten, and for this purpose, some funds were obtained from the British electrical industry.

Further difficulty arose because of the expense of carrying out the duties of the secretariat of the ICRP. This expense was borne first by the National Physical Laboratory and then by the British Medical Research Council. All other expenses of the operation were handled on a voluntary basis with the parent organization of the individual members bearing their expenses.

1956

The seventh meeting of the ICRP took place in Geneva in March 1956, and was held jointly with the ICRU over a period of 12 days.

The report developed in 1953 was not changed in principle. However, greater emphasis was placed on the earlier cautions to keep radiation exposure levels as low as possible regardless of the weekly limits permitted. It was specifically recommended that the accumulated exposure of occupational workers be kept below 50 rem up to age 30. The compilation of permissible concentrations of radioactive isotopes in the body and in the air and water was considerably enlarged to include nearly 200 radioactive isotopes. A new subcommittee was established, in place of the former Subcommittee V, to deal with the problems of radioactive waste disposal.

During these meetings, the Commissions' affiliation with the World Health Organization as a nongovernmental participating organization in the WHO program was ratified. This affiliation came about as a result of contacts made in the fall of 1955 between the World Health Organization and the ICRP with a view to determining if the latter would participate in the WHO activities. The matter was taken up with the members by correspondence, and it was agreed that the ICRP should give its assistance to WHO. Under the arrangement made, the WHO will not influence the operation of the ICRP but will look to it for guidance in matters of radiation protection.

During the meetings, the relationships between the ICRP and the ICRU were more firmly established. They have always operated as separate commissions but since their inception they have always met at the same time and have maintained close collaboration. There has also been a certain degree of overlap in membership. There is now a clear statement of policy that the two Commissions will work together and consult one another on matters of common interest.

There were also extensive discussions regarding the possibility of forming an international protection organization, to be privately financed so as to avoid any nationalistic or political influence. If such an organization were formed, the ICRU and ICRP would be blended into the one organization. No attempt was made to work out the details of such an arrangement, but no difficulties in doing this are foreseen.

It was agreed that if such an organization were established, it should have some semiofficial ties with WHO and U. N. without becoming a part of their organization or subject to their direct control. It is felt that one of the strongest points of both Commissions is their freedom to select membership without regard to matters of national policy or other such influences.

Following the meetings of the ICRP in Geneva, a 3½ day joint session was held by some of its members, some members of the ICRU, and representatives from the WHO. The main purpose of this meeting was to outline to WHO the nature of the ICRP work and to point out ways in which WHO might assist in the worldwide implementation of the recommendations of the Commission. A number of specific recommendations were made to WHO and it is believed that they are taking steps to follow these through.

As of the time of writing, the 1956 report of the Commission has not been officially released. There is again the problem of financing the publication costs. In this connection, WHO has offered to indirectly subsidize the publication by guaranteeing the purchase of 500 copies of the report; this will offset the costs to some extent.

Listed below are the formal meetings of the ICRP held since its inception, the number of members in attendance, and the Commission officers. (Officers listed were in office at the time of meeting. Terms of new officers begin after confirmation of their election during the International Congress, and continue for a 3-year period coresponding to the interval between Congresses. Honorary chairmen are not listed.)

Date and place of meeting	Members present	Officers
1928, Stockholm	5	G. W. C. Kaye (United Kingdom), Chairman.
1931, Paris	7	Do.
1934, Zurich	7	Do.
1937, Chicago	7	Do.
1950, London	10	E. Rock-Carling (United Kingdom), Chairman, L. S. Taylor (United States), Secretary.
1953, Copenhagen	11	E. Rock-Carling (United Kingdom), Chairman, W. Binks (United Kingdom), Secretary.
1956, Geneva	[1] 12	Do.

[1] Main Commission only. Subcommittee membership totaled approximately 50: a substantial number of these were in attendance.

The Eighth International Congress of Radiology is scheduled to meet in Munich in 1959. The next formal meeting of the ICRP has not been scheduled and may be held prior to the Congress. For the period 1956–59, the officers of the main Commission are R. Sievert (Sweden), Chairman; G. Failla (United States), Vice Chairman; W. Binks (United Kingdom), Secretary.

(Mr. Taylor's statement—continued.)

The National Committee on Radiation Protection and Measurements beginning in 1929 has done the same for the United States on a continuing basis.

Exhibit 14. NCRP History

1929–46

1. Formation

The roots of the National Committee on Radiation Protection and Measurements go back to 1928 and are intimately related to the formation of the International Commission on Radiological Protection in July of that year. With the possibility in mind of forming an international organization on radiological protection, the Second International Congress of Radiology, before meeting in Stockholm in July 1928, invited several countries to send representatives to the Congress for the purpose of discussing protection problems and possibly preparing some initial X-ray protection recommendations. From the United States, L. S. Taylor was designated as representative of the National Bureau of Standards, and one representative each attended for the American Roentgen Ray Society and the Radiological Society of North America.

When attempts were made to reach agreement between the United States and other countries, serious difficulties arose. Each of our two radiological societies offered different recommendations and each claimed to be the authoritative body. The NBS had no recommendations to offer and was there more by way of an observer. As a result, the recommendations that were in fairly acceptable form, prepared by the British protection committee, were adopted as the first international recommendations. In the process, the United States delegates showed up rather poorly in that agreement could not be reached on who authoritatively represented the views of the United States.

Germany presented a somewhat similar though not quite so serious a situation as had the United States, in that its representatives at the preliminary discussions also could not agree on who carried the necessary authority.

Concurrent with the meetings of the Congress, G. W. C. Kaye and Stanley Melville (Great Britain) and L. S. Taylor (United States) set about to organize a permanent structure for an international organization. After preliminary discussions, during which some general rules of organization were developed, the International Commission on Radiological Protection[1] was organized, the membership consisting of the above-mentioned persons, Dr. Rolf Sievert of Sweden and Dr. Gustav Grossman of Germany. It was agreed by this group that the Commission should be kept small, and that wherever possible, representatives to the Commission should be chosen from national laboratories where such laboratories existed in member countries. This arrangement and the general philosophy of operation of the Commission was approved by the Second International Congress of Radiology before the close of its sessions.

[1] Until 1946, the Commission was called the International X-ray and Radium Protection Committee.

Because of the confusion regarding accredited representation, introduced primarily by the United States but also by Germany and to a lesser extent by France, the chairman of the ICRP, Dr. G. W. C. Kaye, recommended that a single central committee be established within those countries having more than one radiological organization for the purpose of consolidating national recommendations for presentation at the next meeting of the Commission. It was suggested that the members of the ICRP take this up individually with the various groups in their countries.

As the United States representative to the international protection group, it became the responsibility of L. S. Taylor, to convey its recommendations to the various groups involved in this country, to convince them of its soundness, to obtain their approval and suggestions, and to organize a national committee which could deal most effectively with the protection problems faced at that time.

In September 1928, this question was discussed informally with the president of the American Roentgen Ray Society, at its annual meeting in West Baden, Ind. Similar discussions were held with the president of the Radiological Society of North America in December of that year. As a result of these discussions, these organizations agreed to consolidate their protection activities into a single committee. They further recommended that the committee's activities be centralized at the National Bureau of Standards for the following reasons:

(1) It had by that time established a definite long-range program in the general field of radiation protection;

(2) It had the only laboratory in the country as its primary interest the development of radiation-protection data and information;

(3) It had no intersociety or political ties and therefore could be expected to retain an independent position and viewpoint; and

(4) It provided the official United States representative to the International Commission on Radiological Protection.

The two radiological societies each recommended a physicist and a radiologist for membership in the proposed national committee, and the American Medical Association appointed a member to represent its viewpoints. It was also felt that representation of the X-ray equipment manufacturers would be desirable and each of the manufacturers was asked to nominate candidates for this representation. Of the nominations received, the manufacturers then chose two to serve as their representatives.

Thus, early in 1929, the initial organization of the Advisory Committee on X-ray and Radium Protection was established with L. S. Taylor acting as chairman and with the following participating organizations and representatives:

American Roentgen Ray Society: H. K. Pancoast and J. L. Weatherwax
Radiological Society of North America: R. R. Newell and G. Failla
American Medical Association: Francis Carter Wood
X-ray equipment manufacturers: W. D. Coolidge and W. S. Werner
National Bureau of Standards and ICRP: Lauriston S. Taylor

2. HISTORY

The first meeting of the Committee was held in September 1929, during the annual meeting of the American Roentgen Ray Society. As its first objective, the Committee undertook the preparation of recommendations on X-ray protection. These were published on May 16, 1931, as National Bureau of Standards handbook 15.

The next effort was directed toward the preparation of recommendations on radium protection. For this purpose, Dr. L. F. Curtiss was named to the Committee as the NBS representative for radium protection recommendations, and Dr. C. F. Burnam as the representative of the American Radium Society. The first handbook on radium protection, NBS handbook 18, was prepared by Drs. Curtiss, Burnam, Failla, Newell, Weatherwax, and Wood, and was published March 17, 1934.

Soon after the publication of handbook 15 on X-ray protection, very rapid developments were made in the X-ray field; by 1934 or 1935 it was recognized that this handbook would have to be revised. This task was undertaken by the original Committee, except for the replacement of Dr. Pancoast by Dr. Eugene P. Pendergrass as representative of the American Roentgen Ray Society. The revised recommendations were issued in July 1936, as NBS handbook 20.

It might be worth noting that in this handbook, there appeared for the first time the recommendation of a specific permissible exposure level (then called tolerance dose) of radiation that could be allowed for occupational exposure. The figure recommended was 0.1 roentgen per week. This permissible exposure level remained in force for 12 years and was used by the Manhattan District in its operations. It was subsequently changed as a result of NCRP action in about 1948.

The revision of handbook 18 on radium protection was next undertaken and the new handbook (H 23) was issued August 25, 1938.

These two handbooks, H 20 and H 23, were accepted in this country as the primary guides for protection against X-rays and the radiations from radium. As noted above, they were also the primary guides in this field to the Manhattan project.

Through the war years, there was no formal activity by the Advisory Committee. During that time, however, most of the members of the Advisory Committee were drawn into the Manhattan District program and it was largely through their efforts that uniform safety regulations prevailed during that period.

During its early activities, it was customary for the full committee to work together on the development of protection recommendations. When completed, the recommendations were submitted through their respective representatives to the participating organizations for noting and approval. Formal approval was usually given at one of the regular business meetings of the societies. With the NBS as sponsor of the committee, the recommendations were published by the Government Printing Office as National Bureau of Standards handbooks thus receiving the usual NBS editorial processing.

In September 1946, an informal meeting of the Advisory Committee was held to discuss the extensive revision needed in the X-ray protection recommendations, particularly in the upper voltage regions. At this meeting, it was pointed out that protection problems had become too complex to permit their study and solution by the Committee as then constituted. It was recommended that steps be taken to secure the participation in this work of additional groups such as the Manhattan District and United States Public Health Service, military department, etc. This recommendation was presented to Dr. Condon, then Director of NBS, who communicated with the Manhattan District and the Public Health Service on October 8, 1946, inviting their participation through appointment of 2 representatives each (1 psysicist and 1 radiologist). In response to this invitation, in October 1946, Dr. Stafford L. Warren and Dr. K. Z. Morgan were appointed as representatives of the Manhattan District, and the Public Health Service named Dr. Howard L. Andrews and Dr. E. G. Williams.

The first formal postwar meeting of the committee was held on December 4, 1946. In the agenda for this meeting, it was pointed out that new data had become available since the issuance of the recommendations on X-ray protection, and that many new protection problems had arisen with the rapid expansion in the radiation yield (protection against neutrons, multi-million-volt X-rays, radioactive isotopes, etc.). It was suggested that the scope of the work be defined; and that consideration be given to organizing small working groups to deal with each of the problems, their completed reports to be submitted to the committee for approval.

3. ORGANIZATION

Discussions along these lines were held at the December 4, 1946, meeting, and as a result, it was agreed that the committee should be substantially enlarged and reorganized. At the same time, it was felt that the name of the committee should be made more inclusive and it was therefore renamed National Committee on Radiation Protection. The National Bureau of Standards was reaffirmed as the central coordinating agency for the work of the committee; sponsorship by an impartial agency was felt to be particularly advantageous in view of the various types of participating organizations (radiological societies, industry, government, and the possible inclusion of industrial and labor groups).

The general organization and operational procedures outlined below were agreed upon at this meeting, and have been the basis for the continuing operation of the committee:

1. The committee would consist of an executive committee, main committee, and as many subcommittees as necessary to consider the problems that come within the committee's scope.

2. The executive committee would be composed of five members appointed by the chairman and subject to the approval of the main committee. The committee chairman would act as chairman of the executive committee.

3. The main committee would be composed of (1) technically qualified representatives appointed by organizations interested in the scientific and technical aspects of radiation protection, (2) representatives at large whose services are felt to be of special value, appointed by the executive committee and (3) chairmen of subcommittees.

4. The choice, of chairmen and members of subcommittees would not be restricted to members of the main committee but would be based on the particular qualifications needed for the work. (In organizing subcommittee memberships, the following practice has been and still is followed: The subcommittee chairman is selected by the committee chairman with the approval of the executive committee; the subcommittee chairman chooses his working group, makes informal contacts, and submits to the committee chairman his membership selections; the committee chairman issues formal membership invitations to serve on the subcommittees. Additions may be made to the subcommittee membership if particular specialized information is found to be needed, or individuals may be invited to serve as consultants to the group with due acknowledgment of their assistance included in the published recommendations.)

5. The final reports of the subcommittees would be submitted to the executive and main committees for approval. Because of the high degree of success of the NBS handbook series, it was recommended that this mode of publication and distribution to the public be continued.

Because of the reorganization and enlargement of the committee, the chairmanship was thrown open for reconsideration . L. S. Taylor was nominated and approved by vote to continue indefinitely in this capacity.

Discussions were held regarding the organizations that might appropriately be invited to participate and the suggestions made were used as a basis for the subsequent enlargement of the representation on the main committees.

It was agreed to establish the following subcommittees:

1. Permissible external dose
2. Permissible internal dose
3. X-rays up to 2 million volts
4. Heavy ionizing particles (neutrons, protons, and heavier)
5. Electrons, radium, and X-rays above 2 Mev.
6. Radioactive isotopes, fission products, including the handling and disposal
7. Monitoring methods and instruments

With the formulation of these basic philosophies, the committee began its active program. Its accomplishments and growth since its reorganization in 1946 can be seen by the appended list of handbooks published to date, and the appended membership list showing the present representation, subcommittee structure, and complete membership.

Recommendations of NCRP, 1931–55

NBS Handbook No.	Title	Date
15	X-ray protection: Superseded by H-20	May 16, 1931.
18	Radium protection for amounts up to 300 mg.: Superseded by H-23	Mar. 17, 1934.
20	X-ray protection: Superseded by H-41	July 24, 1936.
23	Radium protection: Superseded by H-54	Aug. 25, 1938.
41	X-ray protection up to 2,000,000 volts: Superseded by H-60	Mar. 30, 1949.
42	Safe handling of radioactive isotopes	September 1949.
48	Control and removal of radioactive contamination in laboratories	Dec. 15, 1951.
49	Recommendations for waste disposal of phosphorus 32 and iodine 131 for medical users.	Nov. 2, 1951.
51	Radiological monitoring methods and instruments	Apr. 7, 1952.
52	Maximum permissible amounts of radioisotopes in the human body and maximum permissible concentrations in air and water.	Mar. 20, 1953.
53	Recommendations for the disposal of carbon 14 wastes	Oct. 26, 1953.
54	Protection against radiations from radium, cobalt 60, and cesium 137	Sept. 1, 1954.
55	Protection against betatron-synchrotron radiations up to 100,000,000 electron volts.	Feb. 26, 1954.
56	Safe handling of cadavers containing radioactive isotopes	Oct. 26, 1953.
58	Radioactive-waste disposal in the ocean	Aug. 25, 1954.
59	Permissible dose from external sources of ionizing radiation	Sept. 24, 1954.
60	X-ray protection	Dec. 1, 1955.
61	Regulation of radiation exposure by legislative means	Dec. 9, 1955.

EXHIBIT 15. LIST OF REPORTS OF NCRP

REFERENCES

NBS Circular No. 374, X-ray and radium protection. (Recommendations of the International Congress of Radiology, January 1929)

Handbooks

18	Radium Protection for Amounts up to 300 Milligrams, 1934
20	X-Ray Protection, 1936
23	Radium Protection, 1938
41	Medical X-Ray Protection up to Two Million Volts, 1949
42	Safe Handling of Radioactive Isotopes, 1949
47	Recommendations of the International Commission on Radiological Protection and of the International Commission on Radiological Units, 1951
48	Control and Removal of Radioactive Contamination in Laboratories, 1951
49	Recommendations for Waste Disposal of Phosphorus 32 and Iodine 131 for Medical Users, 1951
50	X-Ray Protection Design, 1952
51	Radiological Monitoring Methods and Instruments, 1952
52	Maximum Permissible Amounts of Radioisotopes in the Human Body and Maximum Permissible Concentrations in Air and Water, 1953
53	Recommendations for the Disposal of Carbon 14 Wastes, 1953
54	Protection Against Radiations From Radium, Cobalt 60, and Cesium 137, 1954
55	Protection Against Betatron-Synchroton Radiations up to 100 Million Electron Volts, 1954
56	Safe Handling of Cadavers Containing Radioactive Isotopes, 1953
57	Photographic Dosimetry of X- and Gamma-Rays, 1954
58	Radioactive-Waste Disposal in the Ocean, 1954
59	Permissible Dose From External Sources of Ionizing Radiation, 1954
60	X-Ray Protection, 1955
61	Regulation of Radiation Exposure by Legislative Means, 1955
62	Report of the International Commission on Radiological Units and Measurements (ICRUO 1956)

CURRENT HANDBOOKS IN PROCESS

Handbooks

59	Modification A [1]
52	Modification A—Also increasing coverage from 100 to 300 radioisotopes and revising some of the old MPC's
60	Modification A [1]
63	In Press. "Protection Against Neutrons"
--	"Safe Handling of Radioactive Isotopes"—complete revision of H-42
54	Modification A [1]
61	Modification A [1]
--	"Incineration of Radioactive Waste"—completion in about 6 months
--	"Protection Against High Intensity, High Energy Electrons"—mainly for food sterilization programs
56	Modification A [1]
50	Modification A [1]
—	"Radiation Exposure Under Emergency Conditions"—treatment of problem of large and small radiation doses mainly under civil defense or civil disaster conditions
	RBE Values for All Radiations

[1] Modification mainly to reflect the new permissible dose levels of January 8, 1957.

(Mr. Taylor's statement—continued.)

Since the war the recommendations of the ICRP have been guided, in a large measure, from those developed by the NCRP and the international recommendations and standards reflect pretty generally the United States standards. This is not surprising when one considers that the shaping of the program and the

chairmanship of some of the committees on the ICRP are in the same hands as of corresponding subcommittees of the NCRP.

EXHIBIT 16. RELATIONSHIP BETWEEN THE WORK OF THE NCRP AND THE INTERNATIONAL COMMISSIONS

OCTOBER 1956.

RELATIONSHIP BETWEEN THE WORK OF THE NCRP AND THE INTERNATIONAL COMMISSIONS

As noted in the foregoing, the NCRP has been a very active continuing organization since its inception in 1929. Also because of the very much greater activity in the radiation field in the United States as compared with most other countries, the NCRP has been able to develop much more detailed information in matters of radiation protection. For this reason, it is not unnatural that there should have been a number of areas in which the general recommendations and philosophies of the NCRP have influenced the international recommendations.

A major example was the adoption by the ICRP in 1950 of the lower maximum permissible exposure levels recommended by the NCRP 2 or 3 years earlier and already in use in this country. The adoption of these levels was also facilitated by the tripartite conferences between England, Canada, and the United States, the results of which were based on the information supplied by the NCRP.

When the ICRP subcommittee structure was first established in 1950, it was patterned fairly closely after that already in use for several years by the NCRP. In fact the chairman selected for subcommittees I and II of the ICRP were the chairman of the corresponding subcommittees of the NCRP. In addition, many of the individuals selected for membership on the other subcommittees of the ICRP were members of the corresponding NCRP subcommittees.

The maximum permissible concentrations of radioactive isotopes in the body, and in air and water, which were included in the 1950 ICRP report, were those developed initially by the NCRP for its Handbook 52, then in the course of preparation.

In the report of the ICRP developed in 1953, the reports by three of the subcommittees (on permissible dose from external and from internal sources of radiation and on X-ray protection) were taken very largely from the corresponding reports developed by the NCRP. In fact, these NCRP reports were used as the basis for consideration of these subjects by the ICRP.

Similar close relationships have existed between the NCRP and the ICRU. It could not be said with fairness that the most recent recommendations of the ICRU reflect dominantly the opinion of any one group, as all of the representatives participated actively in its preparation . On the other hand, the new committee structure of the ICRU is patterned after that which had been adopted by the NCRP a short time previously. With this extension of activities, the ICRU organizational structure now includes the following:

Committee I, Standards and Measurements of Radioactivity for Radiological Use
Committee II, Standards and Measurements of Radiological Exposure Dose
Committee III, Measurement of Absorbed Dose and Clinical Dosimetry
Committee IV, Standard Methods of Measurement of Characteristic Data of Radiological Equipment and Material

Throughout the membership composition of these four committees, there is substantial overlap in both officers and members with the corresponding subcommittees of the NCRP.

(Mr. Taylor's statement—continued.)

The National Committee on Radiation Protection (NCRP) is currently sponsored by the National Bureau of Standards and is made up of representatives of various scientific and technical organizations and governmental departments. In this way, there is close coordination between the committee's activities and those of other interested groups. This applies particularly to the relationships with the AEC with whom there is the closest collaboration. In fact, the Atomic Energy Commission supplies a small allotment of funds to the NCRP for its work. At the same time, the recommendations of the NCRP are made without undue influence on the part of the AEC.

EXHIBIT 17. LIST OF NCRP PARTICIPATING ORGANIZATIONS, SUBCOMMITTEES AND MEMBERS, MEMBERSHIP OF EXECUTIVE COMMITTEE, JULY 1956

Lauriston S. Taylor, chairman
Sarah W. Raskin, secretary

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(Mr. Taylor's statement—continued.)

TRENDS OF PROTECTION STANDARDS

The trends of protection standards has been steadily downward. This is partly because of improvement in our knowledge and partly because a constant improvement in the techniques in the use of radiation make it possible to revise the standards downward. The two factors are in constant interplay. The most recently recommended permissible dose levels must be regarded now as practically at rockbottom if we are to continue to use radiation at all. Exposures for persons in the environment of plants are only 3 or 4 times the level of natural background radiation. Since in some circumstances, background radiation may be several times higher than the average it makes little sense to attempt to work to much lower levels.

EXHIBIT 18. MAJOR STEPS IN THE DEVELOPMENT OF PROTECTION STANDARDS

(Item V F-4 in outline)

1925: ICRU established by First International Congress of Radiology:
MPD=0.1 erythema dose/r=50–100 r/yr (only visible signs of radiation reaction)
1926: ICRP established by Second International Congress of Radiology:
(a) First set of international protection recommendations adopted.
(b) First unit of dose adopted: the roentgen (r)
1929: NCRP formed in the United States.
1931: First set of radiation protection rules produced by NCRP (H-15).
1934: ICRP:
(a) Adopted first permissible dose of 0.2 r/day (=72 r/yr).
(b) Based on erythema dose.
1935: NCRP:
(a) Adopted value of 0.1 r/day for MPD (=36 r/yr).
(b) More conservative MPD adopted on basis of their suspected other effects.
(c) First discussions on genetic effects.
1947: NCRP:
(a) Lowered MPD to 0.3 r/wk (=15 r/yr).
(b) Lowered because of animal experience in Manhattan District studies.
(c) Genetic effects considered. (See H-59.)
(d) Longevity effects considered.
(e) Large population effects considered.
(f) Standards still based mainly on absence of detectable injury to individuals.
1950: ICRP:
(a) Adopted MPD=0.3 r/wk.
(b) Used values of NCRP for standards for external irradiation.
(c) Used unpublished values of NCRP for internal emitters. (See H-47.)
1952: ICRP/ICRU:
(a) Held first conference on genetic effect.
(b) Reached conclusions essentially as recommended by ICRP in April 1956 and NAS in June 1956.
(c) Felt there was no need to rush to lower levels—better to wait for better data.
(d) Still feel somewhat same about lowered MPD but were stampeded by public clamor.
1953: NCRP listed MPC for 100 radioisotopes (H-52).

1953: ICRP:
(*a*) Adopted NCRP listings of MPC for 100 radioisotopes.
(*b*) Recommended 1/10 levels for large population groups.
1953: ICRU adopted unit of absorbed dose (rad).
April 1956: ICRP introduced accumulated dose concept for both occupational and whole population exposure.
June 1956: NAS/BMRC made same general recommendations as ICRP. (Natural in view of substantial overlap of membership.)
January 1957: NCRP:
(*a*) Formulated the age-prorated occupational dose to insure proper distribution with age.
(*b*) Population dose concept of ICRP adopted.
April 1957: ICRP/ICRU:
(*a*) In work for UNSC made recommendations population exposure evaluations by sampling techniques.
(*b*) Tentatively decided that somatic damage is more immediately important than genetic damage.

(Mr. Taylor's statement—continued.)

The impact of the new standards on our budding nuclear industry will not be as serious as might be thought at first glance. The major atomic energy installations have been working within the new levels for years.

EXHIBIT 19—THE IMPACT OF LOWERED RADIATION EXPOSURE LEVELS ON RADIATION CONTROL PROBLEMS

Lauriston S. Taylor, Atomic and Radiation Physics Division, National Bureau of Standards

You have already heard, in the preceding paper, about the proposed lowering of the levels of maximum permissible dose for radiation workers, and the general population, as proposed by the International Commission on Radiological Protection at its meeting in April of last year. In essence these new recommendations call for a reduction in the MPD by a factor of about 3, as compared with the levels that have been allowed for the past 10 years. The phraseology of these recommendations, in keeping with the normal pattern of the ICRP, was left in somewhat general terms, thus allowing to each country the opportunity of incorporating them in the normal pattern of their individual protection codes or recommendations.

The impact of these proposed changes will be quite different depending upon whether the source of radiation is external to the body or is from radioactive emitters that may enter the body. The impact will be substantially different as between the present type of large-scale atomic energy operations, scattered industrial operations, or the medical uses of X-rays and radioactive isotopes. Probably the most severe difficulties will arise in connection with the further reduction of the permissible exposures to external radiation that will be allowed to persons outside of controlled areas.

With regard to population exposure, the ICRP made the statement that, "It is prudent to limit the dose of radiation received by the gains from all sources additional to the natural background to an amount of the order of natural background in presently inhabited regions of the earth."

At the November meeting of the ICRP, there was considerable debate centered about this point, and it was then agreed that "by the order of natural background" was meant, not equal to natural background but approximately twice the background. Thus in clarification of the earlier statement, the ICRP allowance for general population exposure, will be an average of 10 rem to the gonads up to age 30, and 10 rem per decade thereafter.

The statement was also made that the average exposure of radiation workers should be about 5 rem per year. Considerable fear was expressed that by stating this latter figure in such simple terms, undue hardship on all radiation applications might result, if it were to be interpreted so literally that no individual was allowed to receive an excess of this amount in any 1 year. Various attempts were made to introduce some latitude in this interpretation but at the moment I am not sure as to what the final decision was.

The difficulty up to now is dealing with a single number giving the individual's total permissible lifetime exposure, is introduced because of the fact that the safety recommendations of a few years ago have now entered our

legal machinery and are now legal requirements backed up by varying degrees of enforcement. For the 25 years beginning with the appearance of the first NCRP Handbook in 1931, radiation safety control in this country was on the basis of recommendations backed up by voluntary compliance and good sense. It must be admitted that there were times when neither compliance nor good sense prevailed, but in the great majority of situations it did.

The basic scientific aims and information regarding permissible radiation exposures have been well understood for a number of years. On the other hand, there have been situations where the information was capable of misinterpretation by people who were inexperienced in the field. Partly as a result of this, there has been increased tendency on the part of industry and by State and Federal Government to call for strict codification and the development of radiation protection rules wherein a situation is either black or white. Here begins our trouble.

Let us consider as acceptable a total accumulated exposure of 250 rem over a period of 50 years, up to age 70. Now it is obviously impossible, with any means that we now know of, to measure and record this exposure for each individual. For the purposes of design and operation, one might want to integrate the exposure over a shorter period of time, say 1 year, in which case the average allowable amount would be 5 rem. This is also difficult to control but it is suspected that before too long adequate instrumentation will be obtainable such that this can be done within adequate accuracy limits. At least as far as plant operations are concerned the level of 5 rem per year is a much better figure to work with.

It is important to limit the exposure to individuals during their childbearing age. For this purpose the NCRP is recommending a permissible exposure of approximately 60 rem up to age 30, and an additional 50 rem up to age 40.

Here again it is important to assure adequate distribution of this dose over time. If one considers that 50 percent of the children are born to parents by the time they reach age 30, and 90 percent by the time they reach age 40, it is clearly more important to curtail exposure at the lower end of this age range than at the upper age range. There would be a large difference in the total genetic damage due to an exposure of 50 rem at age 20, as compared to the same exposure at age 30. At the age of 40 this exposure would only affect 10 percent of the genes that might be expected to be transmitted. Consequently there is some wisdom in stating that for operational purposes the average occupational exposure of individuals should not exceed 5 rem per year.

But even here there are difficulties. Suppose that the NCRP were to recommend 5 rem per year for the basic permissible level as compared with the present level of 0.3 rem per week. As soon as this figure is even mentioned it will find its way into various laws and regulations. Everything automatically becomes black or white and any individual who may be so unfortunate as to receive 5.001 rem in any 1 year might think he had legal grounds for seeking redress from his employer. This is, of course, sheer nonsense and yet it is difficult to develop a law in this field in which there is adequate latitude for such reasonableness. The NCRP has worried a great deal about this problem and various suggestions have been put forward.

Consider the cost to a plant operation of a legal limitation of 5 rem per year to the individual worker. Plant management would be foolish to plan their operations in such a way that part or all of its workers would be allowed to take a dose closely approaching 5 rem per year. Experience has demonstrated that surely 1 or 2 people are going to accidentally exceed this and even though we know that a slightly higher exposure is without harm the employee might seek redress. Because of this fear, the plant must plan its operation so that in general they will not exceed, say, 40 or 50 percent of the permissible exposure. This can be very costly and time-consuming and could seriously retard the atomic energy industry.

It has also been suggested that the yearly limit be specified as 5 rem plus or minus 20 percent. This would certainly simplify plant planning and operations but I am very doubtful as to how and whether this could be dealt with to the satisfaction of our legal authorities. Another suggestion has been to specify a total exposure not exceeding 50 rem in 10 years delivered at an average rate not exceeding 5 rem per year. Here again this situation is wide open to abuse or difficulty in legal interpretation. For example, an employer could allow an individual to receive 25 r in each of 2 years and then

lay him off under circumstance which would make him unemployable in the radiation industry. Also this would be unacceptable because one should not allow an individual at the beginning of his reproductive age to receive this large dose. If he is to have a large dose it is much better that he receive this after the conception of his last child and this, of course, changes with age.

Another alternative is to specify the 5 rem per year average but with the further stipulation that no one be allowed to receive in excess of 10 rem in any consecutive 2 years. Even this would allow some abuse but such a provision would vastly simplify plant operations. For example, if an occupational individual received, say, 6 rem in 1 year it would be up to the plant to insure that he could receive no more than 4 rem in the following year. If one decides on an average figure of 5 rem per year the plant will design accordingly. The plant will possibly go further than this since control over a year is not always easy. It may well, for purposes of simplicity, make its designs so as not to allow the exposures to exceed 0.1 rem per week, which is one-third of the present maximum permissible exposure.

The National Committee on Radiation Protection has been increasingly concerned, over the past year, by the trend to incorporate its recommendations into documents having legal status. Almost invariably when this is done, the degree of suppression of radiation exposure goes beyond the philosophies of the committee. As a rule these are in the direction that tend to hamper the applications of radiation, without at the same time, providing any real and significant improvement in radiation protection. In an endeavor to curtail unduly restrictive interpretations of the relatively simple basic rules for radiation protection, there has been a tendency in the past for the National Committee to include in its recommendations various modifications and interpretations of its basic rules. Because of this, the administrative problems involved in radiation control have become more complicated.

I mention this situation because unless great care is taken in the future with our lowered permissible exposure levels, their administrative control may become so restrictive as to make certain types of radiation usage impractical.

It is for this reason that in its latest recommendations the National Committee has tied its basic permissible exposure to the individual's age, rather than to a limitation determined on the basis of a week or even a year of radiation exposure. The basic protection requirement now specified by the National Committee is designed to assure that a person will not receive an average of more than 5 rem per year occupational exposure during his working lifetime from age 18 to 70. In order, however, to get away from the legal statement of 5 rem per year, it is now recommended that individuals be allowed to receive a dose in rems equal to five times the number of years over age 18 of that individual's age. This will tend to hold down exposure at the younger ages when it is genetically more important. It will allow the building up of a reserve of exposure in situations where an individual's age-prorated exposure is less than that allowed.

Any wise management must employ operating safety factors in his allowable radiation exposures and if these are tied rigidly to periods as short as a month or a year, the cost to the plant of introducing the radiation safety factors can be very great. With the new system there is ample latitude for taking care of the very rare instance where an individual may for some reason or other have exceeded an average weekly, monthly, or yearly radiation exposure.

The new recommendations, even though three times lower than the earlier ones, will place a premium on good management. For example, in a large operation it will certainly pay to have good monitoring and recording procedures. Where it can be demonstrated beyond reasonable doubt that an individual's exposure through the use of these is below the permissible amounts, management can permit its employees to build up a reserve exposure. The larger the reserve the greater the flexibility in the use of that employee in subsequent operations. For small operations the cost of instrumentation and recording may be unduly costly, in which case it would probably be more economical to simply set the operating exposure levels low enough so that relatively infrequent monitoring will suffice to assure adequate protection of the individual.

The new recommendations, as well as the old, have specified that the levels of radiation exposure for individuals outside of controlled areas can only be one-tenth of those permitted within controlled areas. This means that on the average a person outside of the controlled areas can receive only 0.5 rem per

year. This is a level which under most circumstances is only a few times greater than background and will in some cases necessitate substantial increases in existing shielding, or means for further reducing the admission of certain contaminants into the air or water. In the case of radiation from external sources, radiation levels outside of controlled areas will have to be reduced to about one-thirtieth of the present MPD allowed workers within a plant at the present time. This may make for difficulties in situations where persons in uncontrolled areas are in very close proximity to sources within a controlled area. On the other hand, the range of such hazard is relatively limited since the dose rate falls in accordance with the inverse square law. From the point of economics it is, therefore, desirable to separate as far as possible controlled and occupied uncontrolled areas. This situation is not likely to seriously affect atomic energy operations even at the new low levels. It will create new problems in the use of byproduct materials.

For radioactive contaminants in the air, the problem is simpler on one hand and is more complicated on the other. The new permissible levels for internal emitters will not be substantially different from the present permissible concentrations except in a very few cases. These cases will include the situations where the critical organ is the gonads or the whole-body and as far as our knowledge goes today, these are the critical organs only for tritium, sodium, and phosphorous. In other words, the new levels for permissible concentrations of radio-nuclides in air and water will not materially affect present-day practices.

Airborne activity carries with it the usual difficulty in no way changed by the lowered levels. This difficulty is introduced because of the fact that such contaminants may stray very great distances from the original sources so that one needs to be concerned about population protection outside of controlled areas covering a much greater area than would be involved in the case of the radiation from external sources.

Let us examine rather briefly some of the areas that may or may not be affected by the lowered permissible levels.

The present impact on nuclear industry will be relatively small insofar as their own operations are concerned. Within the plants of the Atomic Energy Commission and its contractors, there is already a history of amazingly low radiation exposure levels to individuals. The vast majority of the individuals in controlled areas receive today less than 1 rem per year. Only a very small number receive up to the full allowance of about 15 rem per year and I believe that only 2 or 3 out of some 65,000 persons during the past year exceeded the yearly allowance. In other words, these operations are already working well within the new limits.

As already noted, contamination levels of air and water are not going to be substantially changed except for 2 or 3 radio-nuclides. Consequently if the industry has been providing adequate protection in the past it can continue to do so in the future without serious additional strain.

The impact of the lowered levels on other applications of radiation may be more severe particularly with regard to the use of X-rays, sealed sources of radioactive material and accelerators. In the nonnuclear industry where isolated sources of radiation may be scattered around a plant or in various parts of a city there has frequently been the tendency to allow exposure levels of both occupational and nonoccupational persons to go up fairly close to the ceiling. In those instances where this has been the practice additional shielding is going to be required. This can be costly when it comes to making additions and alterations to existing installations. The matter will not be too serious in new installations. In general it simply means adding approximately two half-value layers of shielding material over what has been required up to the present.

The impact on the medical uses of radiation will also be relatively severe. Here because of the nature of the work, it is necessary for the physicians and to a lesser extent the technicians, to expose themselves to some degree of radiation. To accomplish their medical task it is often necessary to take unto themselves some degree of risk beyond that expected in other types of operations. To achieve the newly imposed levels will necessitate some changes in working procedures and some improvements in equipment design and shielding. Also some improvement will, in all probability, be needed in the shielding normally surrounding the rooms where radiation is used. Greater care will also have to be taken to assure that radiation that escapes from the controlled area does not penetrate to uncontrolled areas that may be occupied by persons not subject to radiation monitoring. The radiological profession is always

fully conscious of this problem and over the years have established a very creditable record of constant improvement in their protection practices.

In closing, a few remarks should be made about the effects of medical exposures to radiation superposed on occupational exposures. This is a question that arises frequently and for which there is no simple solution. For practical reasons the various national and international bodies concerned with radiation protection problems have been forced to exclude from consideration the added effects of radiation received by an individual for medical reasons. At the moment this is not too serious. At present the occupational exposure per million people amounts to only about 3 percent of that received for medical reasons. From a genetic point of view this amount is insignificant and one can neglect occupational exposure in relation to medical exposure, when dealing with an average over a large population group. The situation may be more serious for the occasional individual who may receive both the maximum occupational exposure and a large medical exposure. While it is not feasible to regulate or control these in relation to each other, it would be prudent for the employer to keep aware of such situations and make allowance for them in the assignment of an individual to further radiation work. It would also be prudent on the part of the employee to inform his physician that he is a radiation worker and that any additional medical radiation should be kept to a minimum. The decision on this is then up to the physician. It will only be in the rare instance when the two types of exposure can be compounded to dangerous proportions. In such instances it will usually be the case that the individual's well-being or even life is at stake for reasons unassociated with any radiation damage to himself.

(Mr. Taylor's statement—continued.)

The basic assumptions on which permissible exposure is based have changed emphasis several times. Originally, the permissible dose level was based on the erythema or visible skin-burn. Later, emphasis was shifted to any detectable injury that might occur to the individual. More recently, there has been great emphasis placed on the genetic damage to reproductive cells. It is now beginning to appear that the somatic effects on the individual may be the more critical item. In spite of these changes of emphasis the trend has been toward becoming more critical rather than less critical.

THRESHOLD LIMITS

There is presently a great deal of uncertainty as to whether or not there exists any threshold limit for radiation exposure below which damage will not occur. A probable exception to this is genetic damage. In arriving at permissible dose levels it has never been assumed that there were any thresholds for radiation injury. Damage to an individual from any exposure is a matter of probability and the limits are believed to have been set low enough to make the risk small and acceptable. There is always some risk involved in radiation exposure but also there is risk in virtually everything else that we do. With radiation the problem is to balance the risk by exposure against the tangible and intangible gains to be gained by this exposure and against the risks that occur from a variety of other sources.

Exhibit 20. Permissible Exposure to Ionization Radiation, International Conference on Peaceful Uses of Atomic Energy, June 1955, Geneva, Switzerland

By Lauriston S. Taylor, National Bureau of Standards

This short note, reflecting the general opinions of most members of the National Committee on Radiation Protection, directs attention to a major problem that must be faced when we consider large-scale peaceful developments of nuclear energy. The problem directly influences our whole philosophy with regard to radiation protection and centers about the fact that the genetic effects of low-level exposures of a large fraction of the population may ultimately decide the permissible dose for all persons. Up until relatively recently the prime consideration in deciding upon the maximum permissible dose of ionizing radiation has been the exposure of the single individual. Future considerations of the maximum permissible dose may conceivably involve an entirely new feature; whereas past decisions have been based on scientific principles alone, future decisions may have to include political and economic factors.

The current maximum level of permissible exposure for the single individual rests on the philosophy that exposure at this level throughout his adult lifetime is believed unlikely to cause him bodily injury at any time during his lifetime. Based on this major premise, the present permissible exposure levels are acceptable both from the plant and the individual's viewpoint, and do not appear to involve an unreasonable working risk. Such levels are, however, also based on the additional premise that only a small portion of the world population will be so exposed up to the close of their reproductive lifetime.

On the other hand, where large population groups may be exposed, the preservation of the genetic balance of the population may require that the exposure per individual be limited to only a very small fraction of the individual occupational exposure.[1] Up to the present, most of the pertinent data is from animal rather than human experiments, yet we are forced to accept tentatively the animal data as applying to man.

For purposes of discussion, let us take Muller's[2] statement that an exposure of 80 r to the gonads would double the natural mutation rate and that such exposure repeated generation after generation might seriously upset the genetic equilibrium. Presumably this would be genetically unacceptable in view of the doubly heavy genetic load thrust upon the unexposed population, and in view of the present trends in reproductive practices. Muller suggests a maximum average exposure per individual per reproductive lifetime of 20 r, which would result in an increase in the mutation rate of only 25 percent. Other authorities have arrived at figures as low as 3 r for a permissible average "lifetime" exposure. Either figure is much lower than the presently accepted individual occupational exposure limit which may be as high as about 400 r per lifetime.

If we are to adhere to the 20 r average for the whole population, not more than 5 percent of the people could be permitted to receive a full occupational exposure of 400 r to the gonads. For the United States this would be some eight million persons—a figure that we are unlikely to attain for many years to come. One should also consider that most radiation workers do not receive exposures over periods as long as 25 years, and that in fact only a very small number receive more than about one-third of the permissible exposure. On the other hand, there must be added to this, medical and diagnostic exposures wherein radiation may reach the gonads.

Before facing the problem of determining how much additional radiation may be received by nonoccupational population groups, it is essential to make a careful evaluation of existing exposure patterns. Since the prime consideration will be genetic effects, such exposure evaluation should be limited to the gonads of persons before the close of their reproductive lifetime.

Concurrent with, or a part of, such a study should be certain sociological investigations. Reproduction habits will play an important role, and these will vary markedly depending upon such factors as race, education, inbreeding within certain geographical limits, etc. In averaging the exposure of population groups, erroneous results would be obtained by equal weighting of say the population of New York City and an Indian reservation in the West.

Since the size, distribution, and nature of a population group may influence the pattern of crossbreeding, it may be worthwhile even within a single country such as the United States, to consider very different average exposures for different parts of the country. Such differentiation might present almost impossible administrative problems because of population movements, yet there are relatively large and different population groups between which crossbreeding is negligible. This same consideration will, however, be likely to necessitate lowering of average exposures in some areas where inbreeding is high within a population group that remains somewhat static in location. (This may be somewhat less of a problem in cities of the United States as compared with those of Europe and Asia.) There will undoubtedly be special problems of this sort in certain areas and it might be worthwhile to treat them specially, rather than inflict unnecessarily low permissible exposures over the country as a whole. Analysis of individual situations, while costly to perform, may nevertheless be sound economy in the end.

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[1] Recommendations of the International Commission on Radiological Protection (1953).
[2] Muller, H. J., The manner of dependence of the "permissible dose" of radiation on the amount of genetic damage, Acta Radiol. 41 : 6–20 (1954).

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From the point of view of power reactors, the limits set for radioactive wastes will vary considerably, dependent upon location. This, of course, has been one of the prime considerations in locating reactors thus far, and each case has been treated on its own merits.

We have to face the high probability of an enormous growth in the uses of nuclear energy. It is doubted whether anyone has ventured to predict this for the next 20 or 100 years. Looking backward at the enormous changes in our civilization brought about by technilogical advances in the past 4 or 5 generations, indicates the futility of planning ahead in detail a similar range of time. This does not imply, however, that we should stand passively by and let nature take its course. There is much that can be done in preparation for the future.

As the invention of the internal combustion engine revolutionized the world, so also may we expect controlled nuclear energy to do so again. The engine has wrought vast improvements into our material ways of life. One wonders whether, if in 1910 it had been anticipated that the engine would be responsible 40 years later for 30,000 deaths a year on United States highways alone, there would have been a hue and cry to curtail its further use. In spite of the fact that we somehow accept this carnage, we might have been able to have held it down, had the problem been attacked while automobiles were in their infancy.

With nuclear energy the situation is importantly different—even if more complicated. We know a great deal about its potential hazards and to some extent how to cope with them. In addition, we have some time within which to solve the special problems. However rapid the growth in the use of atomic energy, there is still some leeway that will allow us to proceed with technological developments before we outrun our practical limits in the methods of protection. The greatest and most serious limitation is that imposed upon us by genetic considerations. Our most substantial advances in knowledge of genteics have been within the past two or three decades—a very short time. The results of this knowledge have been a major consideration in the discussion leading to our present concepts of permissible dose, yet we have avoided any attempts to rigorously define the genetic limitations. (It is presumed that the geneticists, to be safe, assume the most pessimistic conditions.

At some time, probably not within the immediate future, man will be faced with making an inescapable decision. At what point may the advantages of atomic energy be offset by the disadvantages to the future man? And who will have the abundant wisdom to recognize that point and do something about it? Will it be known, in time for such a decision, just what radiation may do to man's future? I believe that at some point a decision involving an educated gamble with man's future will have to be made, and history of the past indicates that such a decision may be made on the less rather than the more conservative side. The decision does not need to be made tomorrow or probably for some years. In the meantime, we can continue our present pace with relatively little risk. But in that same meantime, it is felt that we should start to condition our thinking for a change in philosophy with regard to radiation exposure. On the basis of today's knowledge of ourselves, we may be expected to show a willingness to accept more rather than less radiation exposure insofar as its effects 10 or 20 generations hence may be felt.

In this same time, we should also devote our every energy to keeping radiation exposure of persons to the minimum compatible with reasonable progress and good sense. Through education and the dissemination of wisely chosen information, we can do much better than at present in matters of radiation protection, without at the same time fettering a source of great benefit to mankind. The better we do the ordinary job of protection today, the longer we postpone the fateful decision on man's future.

There has been a tendency to believe that maximum permissible dose levels can be established on a firm and irrefutable physical basis. This is certainly not true. The establishment of proper dose levels is importantly a matter for physics and biology. It is, however, equally important in reaching a final conclusion that we exercise the highest standards of morality and ethics, and that we bring to bear our utmost in wisdom.

For many years the risk due to radiation has obviously been very small. Even though the standards of the past have been much more liberal than those of the present, we are today forced to the conclusion that the effects of radiation are so slight for such small exposure that it is difficult to establish a causative relation-

ship between exposure and damage even with the more critical of statistical sampling.

RECORDING OF DOSE FOR THE POPULATION

For many years the NCRP and ICRP have considered the problem of recording the dose of all individuals as an aid in the statistical studies of the effect of small or moderate radiation exposures on man. Such a plan has always been ruled out because even at enormous cost their results would have so many uncertainties introduced by poor input data from medical usage as to render useless the application of the information for statistical analysis.

The International Commission on Radiological Protection and the International Commission on Radiological Units and Measurements have completed a study of this question as a result of a specific request from the U. N. Scientific Committee on the Effects of Atomic Radiation. This study substantiates the conclusions mentioned above, namely that at the present time and for the immediate future any system for recording individual medical dose given during diagnostic medical procedures is without value. This applies to recording either by a card-carrying system or by a central recording agency. On the other hand, the commissions do recommend the establishment of sampling procedures, which will provide useful information if carried out on a systematic basis. The cost of such surveys will be only a small fraction of that for a universal registration system and yet will give results with an accuracy commensurate with that of the input data. At the same time it is expected that the input data can be improved by the sampling technique as it goes along.

(Exhibit 21 to be filed with the committee at a later date.) (Material consists of the preliminary report of the ICRP/ICRU to the U. N. and is submitted for use by the Committee. Since the report is not in final form it is requested that it not be published in the record. If the Committee wishes to include a final report of the international commissions one can be made available by about the middle of July.)

EXHIBIT 22. REPORTS OF UNITED STATES TASK GROUP

UNITED NATIONS,
New York, November 5, 1956.

File: PO 131/242.
Prof. ROLF SIEVERT,
Chairman, International Commission on Radiological Protection,
New York, N. Y.

DEAR PROFESSOR SIEVERT: It is my pleasant duty to transmit to you, in your capacity as chairman of the International Commission on Radiological Protection, the following resolution passed unanimously by the Scientific Committee on the Effects of Atomic Radiation at its second session:

The Radiation Committee

Recognizing that in certain countries[1] one of the most important contributions to the present irradiation of human populations caused by manmade sources of ionizing radiations has been shown to originate from the use of radiation in the diagnosis and treatment of diseases;

Believing that a careful study of the doses, particularly to the gonads, received by patients undergoing radiological treatment and examination is desirable;

Believing that the International Commission on Radiological Protection and International Commission on Radiological Units and Measurements by virtue of their connection with the radiological profession are the most suitable bodies to study the problem of recording, measurement and evaluation of doses involved and having reason to believe that they would be interested in doing so;

Invites the International Commission on Radiological Protection together with the International Commission on Radiological Units and Measurements—

(*a*) to consider and discuss the question of how to arrive at reliable data indicating the doses to different parts of the body (particularly the

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[1] See reports submitted to the Committee by Sweden, the United Kingdom, and the United States.

gonads) received by individuals and, in the aggregate, by large population groups due to the medical use of ionizing radiations;

(*b*) to examine what recording system, if any, is at present feasible for the determination of the relevant dose values; and

(*c*) as a result of these studies, to submit to the Scientific Committee on the Effects of Atomic Radiation as soon as possible, and in any event before September 1, 1957, a report upon their deliberations and conclusions on the subjects (*a*) and (*b*), and to make any appropriate recommendations;

Requests the Secretary General to make the necessary arrangements with the two Commissions to implement the above invitation, including provision for reimbursement of expenses incurred by them to an amount not exceeding $10,000.

May I ask you to place the resolution before your members, and to inform me whether the Commission accepts the invitation contained in it. Upon receiving from you an affirmative response, which I appreciate may be conditional upon the financial support mentioned, I will seek the necessary authority for expenditure under the last clause of the resolution.

May I, in conclusion, express my personal hope that this marks the initiation of a prolonged, close and fruitful cooperation between all of the international bodies concerned.

Yours sincerely,

(Signature) RAYMOND K. APPLEYARD,
Acting Secretary, Scientific Committee on the Effects of Atomic Radiation.

NOTES.—1. A similar letter has been received by the Chairman of the ICRU. 2. It was decided at the New York joint meetings that the $10,000 would be used to assist in paying the travel expenses of the task group.

[ICRP/56/43, ICRU/56/26. References: ICRP/56/41, ICRU/56/23; ICRP/56/44, ICRU/56/27]

OUTLINE OF STUDY FOR U. N. SCIENTIFIC COMMITTEE ON EFFECTS OF ATOMIC RADIATION

I. INTRODUCTION

The ICRP and ICRU have recently been invited by the U. N. Scientific Committee on the Effects of Atomic Radiation to study the radiation dose received by various organs of the body when persons are undergoing radiation examination and treatment (see ICRP/56/41 or ICRU/56/23). It should be noted that the Committee has asked for evaluation and recommendations of *methods* for determining the dose and not for detailed data. However, it may be necessary to obtain some exploratory data in order to set limits for accuracy. It should also be noted that the present request could be interpreted as being limited to examination and treatment, but it is considered desirable to include occupational exposures. While it is thought that at the present time the latter adds only small amounts to the radiation burden of the world population, this should be examined at some future date.

At a recent informal meeting of the two Commissions, methods of facilitating the study were discussed. Several methods were suggested and are incorporated in ICRP/56/44, ICRU/56/27, but others may be thought of in the process of obtaining information by those methods suggested. If these are suggested in time, they can also be included in the study.

II. ORGANIZATION

As this problem is principally one of protection, the work will be coordinated by the Secretary of the ICRP, Mr. Walter Binks. He, together with the other officers of the ICRP, the officers of the ICRU, and members of the Commission assigned to coordinate the various tasks constitute the Task Group.

The study has been divided into the following tasks (discussed in more detail in ICRP/56/44, ICRU/56/27):

Task 1. General: Gonad dose versus skin dose and average dose per examination.

Task 2. Complete recording for entire population.

Task 3. Sampling procedure.

Task 4. Approximate methods:
(a) Health and social security agency records
(b) Film or X-ray tube records
(c) Questionnaire to professional people.

Task 5. Dose from unsealed radionuclides.

Given below are the names and addresses of the task group and the Commission task assigned to each:

Chairman, ICRP: R. Sievert, Institute of Radiophysics, Stockholm, Sweden
Vice Chairman, ICRP: G. Failla, department of radiology, Columbia University, 630 West 168th St., New York, N. Y.
Secretary, ICRP: W. Binks, National Radiological Protection Service, Clifton Ave., Belmont, Sutton, Surrey, England
Chairman, ICRU: L. S. Taylor, National Bureau of Standards, Washington, D. C.
Vice Chairman, ICRU: L. H. Gray, radiobiological research department, Mount Vernon Hospital, Northwood, Middlesex, England
Secretary, ICRU: H. O. Wyckoff, National Bureau of Standards, Washington, D. C.
Task 1: E. E. Smith, National Radiological Protection Service, Clifton Avenue, Belmont, Sutton, Surrey, England
Task 2: E. A. Watkinson, environmental health and special projects, National Health and Welfare, Jackson Building, Ottawa, Ontario, Canada
Task 3: K. Z. Morgan, Health Physics Division, Oak Ridge National Laboratory, P. O. Box P, Oak Ridge, Tenn.
Task 4: A. Allisy, Laboratoire de Recherches, Ecole Normale Superieure, 24 rue L'homond, Paris, France.
Task 5: W. G. Marley, Health Physics Division, Atomic Energy Research Establishment, Harwell, Didcot, Berkshire, England.

A national leader has been named to provide a focal point in each of the countries conducting this study. He is responsible for naming national task leaders to consider each one of the tasks. During the meetings of the two Commissions, nationals of seven countries were present. Accordingly, it was possible to appoint the following national leaders at that time:

Canada: E. A. Watkinson, environmental health and specialist projects, National Health and Welfare, Jackson Building, Ottawa, Ontario, Canada.
France: L. Bugnard, Institut National d'Hygiene, Ministry of Health, 3 rue Leon Bonnat, Paris 16E, France.
Germany: H. Holthusen, Direcktor de Strahleninstitut Krankenhauses St. Georg, Hamburg, Germany.
Sweden: R. Sievert, Institute of Radiophysics, Stockholm, Sweden.
United Kingdom: E. E. Pochin, Department of clinical research, University College Hospital Medical School, University Street, London W. C., England.
United States: L. S. Taylor, National Bureau oif Standards, Washington, D. C.
Urugay: F. E. Leborgne, Institute of Radiology, Hospital Pereira Rossell, 1210 Ibicuy, Montevideo, Uruguay.

Other countries having nationals on either of the commissions or their committees are invited to cooperate, as are the radiological societies of all the other countries officially represented at the 8th International Congress of Radiology at Mexico City.

No information is yet available on the names and addresses of national task leaders.

III. OUTLINE OF PLAN FOR STUDY

1. National Studies

(a) Names and addresses of national task leaders to be sent by national leader to all members of the task group by December 15, 1956.

(b) Copies of correspondence between commission task leaders and national task leaders should be sent to national leader.

(c) Reports (in English) to be sent by national leader to each member of task group by March 1, 1957. (Individual reports on tasks should be sent to commission task leaders as completed to allow more time for their study.)

2. Commission study

(a) Commission task leaders to prepare and distribute consolidated report on their tasks to all participants in the study (task group, national leaders, and national task leders) and to ICRP and ICRU main commissions by April 1, 1957.

(b) Fifteen copies of each person's comments on consolidated reports to be sent to ICRP Secretary in Geneva before April 19, 1957.

(c) Meeting in Geneva of task group April 23–27, 1957, to prepare draft of final report.

(d) Draft of final report to be distributed to all participants and main commissions by June 1, 1957.

(e) All comments on draft of final report to be sent to each member of the task group by June 20, 1957.

(f) Meeting, if necessary, of task group July 1, 1957, to prepare final report.

(g) Final report to U. N. Scientific Committee (with copies to participants in the study and members of main commissions) before September 1, 1957.

[ICRP/56/44, ICRU/56/27. Reference: ICRP/56/43, ICRU/56/26]

Memorandum Regarding Resolution of U. N. Scientific Committee on the Effects of Atomic Radiation

1. INTRODUCTION

The U. N. resolution invites the two Commissions—

"(a) To consider and discuss the question of how to arrive at reliable data indicating the doses to different parts of the body (particularly the gonads) received by individuals and, in the aggregate, by large population groups due to the medical use of ionizing radiations, and

"(b) To examine what recording system, if any, is at present feasible for the determination of the relevant dose values, and

"(c) As a result of these studies, to submit to the Scientific Committee on the Effects of Atomic Radiation as soon as possible, and in any event before September 1, 1957, a report upon their deliberations and conclusions on the subjects (a) and (b), and to make any appropriate recommendations."

2. SCOPE OF PROBLEM

The problem resolves itself into three main aspects:

(a) The doses to the gonads of patients;

(b) The doses to other organs of patients;

(c) The occupational exposure of all those using, or in the vicinity of, sources of ionizing radiation in medical and allied fields.

It is considered that by far the most important of these aspects at the present time is (a) and this receives most attention in what follows. It is assumed that both diagnosis and therapy are involved and that by medical use is meant not only the patients of radiologists but also those other medical practitioners, dentists, osteopaths, chiropractors, etc., and also patients undergoing diagnosis and therapy with radioactive nuclides.

It will be possible to extend the scope of the inquiry to include other organs and also to include occupational exposure once the main problem has been dealt with.

3. THE DOSE TO THE GONADS OF PATIENTS

This dose is specifically of interest because of its genetic significance, and in consequence, only the irradiation of persons who are likely to become parents is of importance..

(a) Diagnostic radiology

The following information is required for a proper assessment of the significance of doses to the gonads:

(I) The vital statistics of the population as regards mortality, the ages at which both males and females become parents, and also the average number of children born to each parent.

(II) The number and types of radiological examinations, both radiographic and fluoroscopic, and with radionuclides, performed on both sexes as a function of age up to the age of 50, which would include the number and types of examinations performed on pregnant women (in which both the parents and the foetus are involved).

(III) The doses delivered to the gonads of each sex (and to the foetus where involved) during each type of examination, both radiographic and fluoroscopic and with radionuclides.. These doses are liable to vary in different hospitals and disciplines and will also depend on the size of the patient, e. g., whether infant or adult.

The combination of the information referred to in 3 (a) (I)–(III) above, weighted where necessary to allow for the fact that the individuals undergoing certain types of examination, e. g., hysterosalpingography, may be abnormal as regards expectant parenthood, permits an assessment of the genetically significant dose to be made.

(*b*) *Therapeutic radiology*

This requires essentially the same treatment as that of diagnostic radiology. Therapy with radionuclides includes teletherapy, interstitial and intercavitary insertions and also the injection or ingestion of radioactive solutions and the like. These various types of therapy involve the gonads in different ways and would need to be assessed separately.

It should be noted that in the case of therapy, the character of the disease may affect the probability of the patient subsequently becoming a parent.

(*c*) *Methods of obtaining the data*

The genetically significant dose may be obtained for a particular country in a variety of ways:

(I) By keeping records of the radiological exposures of all individual members of the community up to the age of 50;

(II) By obtaining statistically adequate samples of the items referred to in 3 (a) above as applied to both diagnosis and therapy;

As far as 3 (a) (I) is concerned, the necessary data are available in many countries as population statistics. 3 (a) (II) may be obtained either by taking a sample of institutions and installations of all types and examining the techniques and patient records, or by taking a random sample of the population and studying their X-ray diagnostic and therapeutic experiences during a specified period. The data referred to in 3 (a) (III) requires evaluation for the variety of conditions that arise. Existing information on this subject is extremely meager and further studies are urgently required. If a limited sample of the population is studied, it may be possible to evaluate the doses from direct measurements on each patient.

(III) In some countries, information as to the statistics of diagnostic and therapeutic radiology can be obtained from the records of health insurance companies and similar organizations. It is considered that the records of these organizations will include details a sto age, sex, type of examination, etc.

(IV) An indirect approach, which would provide an assessment of the amount of diagnostic radiography carried out, could be made by obtaining from the suppliers and/or manufacturers of X-ray films the total quantity of films supplied for medical purposes in a country during any particular period. Such information, combined with more detailed information obtained possibly in another country by methods 3 (c) (I) or (II) would enable a rough estimate to be made of the relative quantities of radiography in the two countries concerned. No information would be obtained by this means regarding the relative amounts of fluoroscopy or therapy, and we have reason to believe that these vary considerably from country to country. In consequence, the approach suggested in this section would lead only to a very approximate assessment of the total significant gonad dose.

(V) Another possible indirect method of obtaining relative data from which assessments could be made is to obtain information from the manufacturers and/or suppliers of X-ray tubes regarding the turnover of tubes of all types in the country concerned. This would provide a rough estimate of the total X-ray tube loading, which would then have to be combined with statistical data regarding the average gonad dose per unit of tube loading. Although very approximate, the figure derived in this way would include both radiography and fluoroscopy and possibly X-ray therapy as well.

(VI) A further possibility of obtaining a rough estimate of the extent of the gonad dose would be to keep records of the use to which X-ray diagnostic and therapeutic equipment is put. At the present time, this is not regarded as a practical approach, but if all diagnostic X-ray apparatus (in particular) were fitted with a method for registering the total tube load in milliampere-minutes, both for radiographic and fluorscopic work, the information thus obtained, combined with data of a statistical nature as to operating voltage, focal distance, etc., would again permit some relative data to be obtained. An alternative procedure would be to fit each tube with an integrating chemical dosimeter and thus determine directly the dose delivered to an arbitrary point, from which calculations as to the gonad dose could proceed. An approach to the

manufacturers of X-ray apparatus is recommended in connection with these suggestions.

(VII) As far as the internal use of radionuclides is concerned, it is thought that their contribution to the gonad dose is small at present. However, an assessment could be made, starting from the data which are commonly available, on the quantities of such nuclides which are supplied in each country for medical purposes.

(VIII) Details of the distribution of examinations and treatments in terms of age and sex may be most readily obtained by enlisting the assistance of national medical and similar societies. For this purpose, such societies could circulate their members with a questionnaire of an approved type and furnish the data to the commissions or to a national representative of the commissions.

4. THE DOSES TO OTHER ORGANS OF PATIENTS [1]

Detailed consideration was not given to this aspect of the problem during the informal meeting of the two commissions in New York. It is obvious that any particular organ or organs can be dealt with in a similar manner to that outlined for the gonads, once the investigation into average organ-dose versus skin-dose has been completed. However, the skin itself is one of the organs of importance, and in this case it appears desirable to estimate the aggregate dose to each part of the skin having an area in excess of the "significant area" of 1 cm^2.

It is clear that an attempt to obtain records or data on this aspect of the problem would assume enormous proportions unless the number of organs selected for investigation were to be strictly limited; and it is suggested, at the outset, that consideration be given to the "critical organs" specified by ICRP for protection purposes. These are (apart from the gonads) the skin, the blood-forming organs, and the iris (lens).

It is important to note that where those other organs are concerned, the maximum dose likely to be received by an individual is the primary consideration, although doses averaged over the community might be of interest as regards effects that are only detectable statistically. In these circumstances, if it is decided to keep records, it would not suffice to confine them merely to age and sex. Sampling procedures would also have a rather restricted value.

5. OCCUPATIONAL EXPOSURE OF ALL THOSE USING, OR IN THE VICINITY OF, SOURCES OF IONIZING RADIATION IN MEDICAL AND ALLIED FIELDS.

It is not clear whether or not the U. N. Committee intended to include this subject in the study. However, it is of sufficient importance to merit some consideration, although the contribution to the aggregate gonad dose of the population is at the present time relatively small. Provided the recommendations of ICRP are followed, the dose to the gonads received from occupational exposure up to the age of 30 (the approximate mean age of reproduction) will not exceed 50 rem. The injected dose from those occupationally exposed will therefore not exceed 50 rem multiplied by the number so exposed. To this must be added the dose received by those persons in uncontrolled areas around each installation, where the recommended dose in 30 years would not exceed 15 rem (5 rem per decade). It is highly probable that, although this latter group might be larger than the occupationally exposed group, the injected dose would be less, and thus the total injected dose will be less than twice the dose received in controlled areas.

From the point of view of the population, the study should not be compared to the medical and allied fields, but should include all sources of occupational exposure; and it is suggested that the evaluation of the total injected dose due to the occupational use of ionizing radiation be given consideration in all the countries concerned in the main study.

In this connection, the keeping of formal records is a much less formidable problem than for the general population, and is beginning to be considered a worthwhile undertaking in order to insure that the limits set for exposure up to the age of 30 and for each succeeding decade are not exceeded.

[1] Secs. 1–3 of this memorandum were approved by an informal joint meeting of ICRP and ICRU in New York, November 1956. Secs. 4–6 were prepared by the acting secretary of the ICRP at the request of the meeting, and have not been reviewed by the Commissions.

6. IMPLEMENTATION OF THE STUDY

Document ICRP/56/43, ICRU/56/26 outlines the scheme prepared during the joint meetings for implementing the study. Briefly, the study is divided into 5 tasks, each the responsibility of a person selected by the joint Commissions. These tasks are as follows:

Task 1. The coordination of information regarding the gonad dose for males and females in terms of skin dose for each type of X-ray or γ-ray beam therapy. Also, the coordination of information relating to the average skin dose and doses to other organs of the body during such examinations and treatment.

Task 2. The coordination of information regarding the possibility and merits of the complete recording of the radiological history of the whole population—

(*a*) for each individual;
(*b*) for each age group and sex.

This study is to include the cost and the effort required for this complete recording.

Task 3. The coordination of information regarding various methods of sampling, e. g., of individuals, of installations, of professional people.

Task 4. The coordination of information regarding the possibility of using approximate methods, e. g. data supplied from health or social security agency records; film supplies; X-ray tube turnover; recording total exposure of X-ray tubes; integrated dose measurements; questionnaire of professional people.

Task 5. Coordination of information regarding the doses delivered by the use of radionuclides directly injected or ingested in the body, both for the diagnostic and therapeutic purposes.

National leaders for some countries have been selected and others are being sought from among the members of the two Commissions. In addition, the radiological societies of those countries affiliated to I. C. R. and not covered by the above are being approached. In this way it is hoped to initiate studies in a large number of countries, which will be coordinated by the Commission task leaders. Arrangements have been made for a full exchange of information between the various persons concerned and it was considered that the work would be considerably facilitated if each of the Commission task leaders prepared a brief memorandum outlining his own particular task for the benefit of the national leaders.

As the time available for the study is very short, a time table has been worked out which it is essential to follow. The whole study will be coordinated by the Secretary of ICRP,[3] to whom any requests for further information should be addressed.

APRIL 19, 1957.

UNITED STATES TASK REPORT ON ICRP/ICRU/UN STUDY

THE PROBLEM

During the New York meetings of the ICRP/U in October-November 1956, the Commissions were invited by the U. N. Scientific Committee on the Effects of Atomic Radiation—

(*a*) To consider and discuss the question of how to arrive at reliable data indicating the doses to different parts of the body (particularly the gonads) received by individuals and, in the aggregate, by large population groups due to the medical use of ionizing radiations;

(*b*) To examine what recording system, if any, is at present feasible for the determination of the relevant dose values; and

(*c*) As a result of these studies, to submit to the Scientific Committee on the Effects of Atomic Radiation as soon as possible, and in any event before September 1, 1957, a report upon their deliberations and conclusions on the subjects (*a*) and (*b*), and to make any appropriate recommendations.

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[3] Mr. W. Binks, director, Radiological Protection Service, Clifton Avenue, Belmont, Sutton, Surrey, England.

In the following, the discussion applies primarily to the gonadal dose, but it is understood that the same arguments apply to the dose to other parts of the body. The problem, of course, becomes more complex for multiple organs, whole body or integral dose, bone marrow, etc., in ascending order.

In order to complete this project and to spread the work out as much as possible, the two Commissions at that time decided to divide the work into five tasks.

Task 1. General: Gonad dose versus skin dose and average dose per examination.
Task 2. Complete recording for entire population.
Task 3. Sampling procedure.
Task 4. Approximate methods:

(*a*) Health and social security agency records.
(*b*) Film or X-ray tube records.
(*c*) Questionnaire to professional people.

Task 5. Dose from unsealed radionuclides.

The first deals with the determination of the dosimetry factors (dose to the gonads per unit skin dose or per examination of the different types); the next three deal with methods of arriving at statistics either on the number of examinations on, or the skin dose to, the population from different diagnostic and therapeutic applications of external radiation; the fifth deals with the gonadal dose from diagnostic and therapeutic applications of internal irradiation.

During a meeting of the United States task leaders in New York on March 19, 1957, the problem was further discussed and the following position stand taken:

RECOMMENDATIONS

1. In view of the attainable accuracy and the significance of the final results by any method, it is considered that the most useful immediate method for obtaining an estimate of the total gonadal dose to the population would be through the use of *indirect statistical sampling.* Parameters in this sampling include film consumption, number and types of X-ray machines, number of users,[1] distribution of types of examinations, published data on gonadal dose, etc. The results in the report by Laughlin and Pullman on the estimated gonadal dose to the United States population from the medical use of X-rays was arrived at by the methods considered independently at the October-November 1956 meeting of the two Commissions. The outstanding factors which make the final results uncertain include (1) the variation in the gonadal dose per examination for different users and in certain types of examinations such as fluoroscopy plus weighting errors in age and procreative potentiality, and (2) the actual number of examinations performed by each group of users. The former, (1), probably produces the greater uncertainty in the final results; the latter, (2), may be somewhat indirectly checked by, for example, the number of films sold. Presently published results of data on gonadal dose probably represent the best that can be done without a major effort to improve the overall accuracy of the input data. The elimination of some data of dubious value and the addition of new data from types of users that have not been adequately sampled could possibly result in some improvement of the final result by this technique. However, such results would have to be tested for statistical adequacy.

Until there is much better knowledge of the gonad dose in relation to specific type of examination, there seems to be little point in the employment of more elaborate and more costly evaluation procedures. For purposes of determining the future trend (increase or decrease) of the population gonadal dose by this sampling method, it would probably be necessary to introduce some relatively simple record procedures to improve the significance and accuracy of the results.

2. It was agreed that a preferable method of arriving at the gonadal dose is by *population sampling.* This could best be done by determining the gonadal dose administered by a representative sample of the users of radiation. Such a survey should probably also include a sampling of the recipients of the radiation to determine if, in the future, information obtained from them would be of value. Sampling would include not only the distribution of examination types but also the actual dose delivered to the gonads from each examination. No

[1] By user is meant the person applying radiation to a patient for medical purposes.

attempt would be made to do this on a name basis but rather on an age basis. It was also recognized that the investigation itself would encourage improved procedures resulting in a reduction of the gonadal dose, and this would make uncertain the dose that had been delivered prior to the investigation. However, this reduction was considered more important than the determination of the dose given in the past.

A full-fledged sampling program, of course, should be preceded by several trial sampling programs. Population sampling does not have to be continuous but should be periodically repeated. The change in the dose during intervening periods might be adequately estimated from data on supplies such as films, X-ray tubes, etc. This may be worth doing even if the larger sampling program does not prove to be economically or technically feasible as learned on basis of trials.

3. From the point of view of bringing about a desirable decrease in population dose, it is believed that direct efforts toward this end would be most profitable. This can be accomplished best by educating the user rather than by alarming the patient. Educational procedures would include such items as:

(*a*) Preparing and circulating through professional channels suitable information on good radiologic practices.

(*b*) Including in medical school curriculums adequate instruction on radiation effects and on improved radiologic practices and procedures.

(*c*) Providing information and guidance that emphasizes the need for judgment in respect to the indications for radiologic diagnosis and therapy in relation to possible radiation hazards; in other words, develop an awareness of the need to balance carefully the health benefit against the possible risk to either the individual or the race.

Concurrently, there should be additional study and research on:

(*a*) Methods and techniques for performing radiation diagnostic procedures with less exposure of the patient generally and the gonads in particular.

(*b*) Development of new apparatus and techniques that will provide the requisite diagnostic results with minimum patient exposure.

(*c*) Establishment of more reliable data on the relationship between gonad (and other organ) dose and given types of diagnostic procedures.

(*d*) Development of improved methods and apparatus for measurement of radiation dose, particularly during fluoroscopic procedures.

REMARKS ON OTHER MEANS FOR EVALUATING POPULATION DOSE

Task I

4. Before any precise evaluation of gonadal dose to the population can be made, certain new basic studies will be necessary. Analysis of existing data has yielded about all the information possible; at this point most past information might as well be disregarded. Acceptable procedures and instrumentation must be established and evaluated for practical application in determining gonad dose under the wide variety of conditions encountered in medical practice. Statistical analysis of the results must be proven by actually working with suitably sized patient samples. Field studies will be necessary.

For many radiographic procedures the problem is relatively simple, but for some the problem can be quite difficult. For fluoroscopic procedures it will be extremely difficult to determine the gonadal dose under "average" conditions, as this involves so many personal factors that will vary widely between physicians, between different patients when something abnormal is found, etc. Until these studies can be completed (probably many years, if ever), there is little point in setting up elaborate and costly recording systems. It is possible that as a normal consequence of these studies such improvements will develop as to eliminate the supposed need for recording.

Task II

5. An attempt to determine the dose to each person of the population, either by name or by age group, is not considered feasible for the United States for such a system would have to depend upon the users of the radiation reporting accurately the dose received. This cannot be done accurately because (1) the gonadal dose per examination is not accurately known, and (2) only a very small percentage of significant returns would be anticipated. This anticipation is

based upon results of surveys covering items that are much easier to report. Because of the small percentage of good returns expected, the results of such a study may be badly biased in the direction of the procedures used by the more conscientious reporters.

6. No system or plan for universal recording of the gonadal dose of the whole population is considered as desirable or feasible, certainly for the foreseeable future.

(*a*) Complete recording by individual and age groups:

Basic gonad dose data is lacking;
Cost is excessive;
Purpose is questionable;
Genetic analysis in future is uncertain (10 to 20 generations of data needed);
Method is unnecessarily complicated, if only to determine population gonad dose;
Accuracy is no greater than from simpler methods.

(*b*) Recording by age groups only:

Basic gonad dose data is lacking;
No way to adequately collect and utilize records so kept;
Loss of records would destroy statistical value;
Self-carried cards may have adverse psychological effect on individuals.

Task III

7. Certain types of sampling procedures may be of value and will assist in providing some of the necessary data on gonad dose. (See par. 2.) These procedures must not be entered upon hastily. A full-fledged population sampling program must be preceded by several trial sampling programs. It is quite possible that, in time, a suitable sampling may be developed such that if repeated, say, every 10 years, the trends of population dose may be determined. A check every 10 or 20 years should be adequate for control purposes. Development of a reasonably adequate sampling procedure for the United States might cost of the order of $300,000 per year for 5 years. Once developed, a population sampling procedure might replace the indirect statistical sampling procedure outlined in paragraph 1.

Task IV

8. Approximate sampling methods have been recommended for use in estimating the total gonadal dose of the whole population.

(*a*) Health and social security records have a limited usefulness; by themselves they will not provide the information needed.

(*b*) Film sales provide one useful source of information on the radiographic load in this country.

(*c*) X-ray tube records are of little value in arriving at the dose administered.

(*d*) X-ray apparatus records will provide some useful information.

9. Some estimates of the gonadal dose to the population may be made by means of questionnaires sent to radiation users through their professional organizations. Experience with questionnaires in other connections indicates that their effectiveness may be doubtful for various reasons:

(*a*) Basic data on gonadal dose is lacking;
(*b*) Reporting will be too careless;
(*c*) Returns will be small and are likely to be biased by data submitted by the more conscientious reporters.

Task V

10. The gonadal dose from unsealed radionuclides is inconsequential at present.

[Task 1, United States]

RECOMMENDATIONS FOR PLAN OF STUDY OF TASK 1 WITHIN THE UNITED STATES, UNDER UNITED NATIONS' SCIENTIFIC COMMITTEE ON THE EFFECTS OF ATOMIC RADIATION

Task 1 is concerned with the procurement of data on the gonadal dose received during medical diagnostic and therapeutic X-ray procedures. In the outline of study by the ICRP, task 1 is 1 of 5 tasks slated for investigation

by the U. N. Scientific Committee on the Effects of Atomic Radiation. Tasks 2, 3, and 4 are also closely related to the problem of radiation dosage during medical X-ray procedures. Task 1, however, is primarily concerned with dosimetry, whereas tasks 2, 3, and 4 are primarily concerned with statistics. For example, E. E. Smith, international commission leader for task 1, cites in his instructions to national task leaders that task 1 is concerned with the development of "a set of factors which gives directly the average gonad dose for each type of examination." Smith then goes on to state that "when the table of factors has been obtained, it will be possible, by combining them with details of numbers of examinations of each type in terms of age and sex, to assess the aggregate genetically significant dose."

Although task 1 is primarily concerned with dosimetry, there is a major statistical aspect involved in it. For example, the dosimetric values obtained in task 1 will be greatly reduced in value unless they have been obtained on subjects who are representative of the general population receiving diagnostic or therapeutic exposure. It is therefore strongly recommended that, at the outset, there be established for the United States task force, at least, an advisory committee consisting of a biostatistician, an epidemiologist, a geneticist, a physicist, and a radiologist to coordinate the efforts undertaken within the various tasks.

To illustrate the need for such an advisory committee still further, attention is called to a study to be undertaken soon under the Radiation Study Section of the National Institutes of Health in which a statistically wide range of individuals of varying physical types will be studied dosimetrically for gonadal dose. More will be said about this study a little later. However, it will be quite clear that unless tasks 2, 3, and 4 provide data of the range of body types which are expected in the various age and sex groups, the dosimetric study will be greatly reduced in value. As far as the United States is concerned, I do not know how an advisory committee of the type suggested can be financed. However, unless an advisory committee with representation of the several disciplines which have been recommended is established, this study is likely to progress little further than previous studies.

It may be argued that the various task leaders may seek, at their discression, consultation in statistics, epidemiology, genetics, etc. However, such consultation does not provide the unifying force which is needed for the various task leaders to proceed most effectively.

As far as the procurement of gonadal dosage data is concerned, one may proceed in several ways. Before any of these is discussed, it may be well to outline the major variables which influence the gonadal dose following a diagnostic or therapeutic procedure. These factors may be divided into four principal groups:

(*a*) Patient factors:
(*b*) Procedural factors;
(*c*) Equipment factors; and
(*d*) In the case of fluoroscopy, professional factors.

Under patient factors, sternal-public distance and anatomical thickness perhaps are two of the principal variables. Under procedural factors, field location, field size, anatomical projection and beam angulation constitute some of the principal variables. In regard to equipment, kilovoltage, filtration, and generator wave forms are three important variables. In regard to the professional variable of fluoroscopy, marked variation occurs, of course, between one physician and another.

From the large number of variables set forth in the preceding paragraph, it is quite clear that the procurement of data for the fulfillment of task 1 necessarily is quite complex. A number of investigators have suggested that these data may be obtained by dosimetric measurements on a large population group involving many hospitals and physicians' offices, sufficient to obtain statistically significant values through the range of radiographic, fluoroscopic and therapeutic procedures normally encountered in medical radiology. Such a study, however, seems enormously costly, and, furthermore, much of the work involved in this type of investigation would be concerned with work carried out by the remaining tasks 2, 3 and 4. Furthermore, when all of the data under such a plan would be at hand, they would tell little more than what the dosage values have been under past circumstances. They would not be particularly valuable in the estimation of the gonadal dose under future conditions. Perhaps, most important of all, they would not be too valuable in the development of a set

of standards of radiologic procedure which the medical profession in the future might follow to insure minimum dose to the gonads.

The establishment of a set of standards of radiologic procedure seems very important from a number of aspects. First, from the data supplied by the British and United States Committees, presented late in 1956, it appears that the gonadal dose of the general population during the significant reproductive period of life, is of the same order as the dosage received from background radiation. A perusal of the radiologic literature during the last several years indicates that radiologists have not been greatly concerned with the maintenance of minimum dosage to the gonads and hence there is some reason to believe that, by a well-regulated set of standards of radiologic procedure, the gonadal dose of the general population might be reduced to a relatively small fraction of that received from background radiation. Therefore, it appears quite clear that, if at all possible, the data obtained under Task 1 should be of a type which is not only useful in telling what the gonadal dosage has been in the past, but what procedures and regulations might be developed in the future to reduce substantially the gonadal dose in medical radiologic procedures and yet retain all of the benefits that these procedures yield.

The studies, referred to above, to be undertaken under the auspices of the Radiation Study Section of the National Institutes of Health, appear to be of a type which will accomplish just such objectives. In this study, a statistical representative group of subjects will be irradiated through a wide range of kilovoltage, filtration, and wave form, and with the data recorded in such a manner that gonadal dose may be readily determined for essentially a continuous range of field sizes and field locations. In this study, considerable reliance will be placed upon computing techniques. The data will be collected also in such a manner that, as new techniques develop in the future, they will provide ready assessment of the dosage conditions under these new techniques.

It is not implied from the foregoing that the techniques to be followed under the Radiation Study Section investigation are the only ones which should be investigated in the United States. Indeed, the Advisory Committee, referred to above, may well determine that a number of other studies should be pursued. However, it appears quite desirable that whatever studies are followed, their ultimate objective should not only be the procurement of dosage data under present-day conditions, but also data in such a form that ultimately, within a short time, there may be established a set of standards of operating procedure in radiology which may be adopted and employed by physicians everywhere to insure that the gonadal dose in medical radiology to the population at large is substantially below that received from background radiation.

RUSSELL H. MORGAN, M. D.

FEBRUARY 25, 1957.

[Task 2, United States]

COSTS OF RECORDING PROCEDURES (TO THE PHYSICIAN)

To obtain some rough idea of the cost to the medical profession (and hence to the patient) of a central recording system, the attached forms were submitted to two radiological groups for study. One group was a moderately large private office, and the other a hospital clinic. Each was known to have good cost-accounting practices.

Estimates from both were essentially the same. Costs were based on a daily load of 50 patients, thus making it possible to use quantity recording techniques.

Use of either form A or form B would be about the same. While the former calls for more individual record forms, the latter requires the same information and, in addition, necessitates a sorting process to group the irradiations in the proper category. It was thought that if a cost difference were to develop, the form B would be the more costly.

For a daily workload of about 50 patients, the *minimum* cost of recording and reporting the examinations or treatments would be about $0.30 per patient. For patient loads of less than 10 per day, the probable minimum cost would be about double, or $0.60 per patient.

If it were required to estimate the gonad dose for each patient the costs would be substantially larger if it was necessary to estimate any closer than within a factor of 2 or 3. This is based on current practices, equipment, and techniques.

Costs might be lowered somewhat, if in the future, suitable instrumentation techniques were made available—provided the instrumentation is not too costly.

L. S. TAYLOR.

Various proposals relative to the recording of the medical exposure of the whole population are being studied. We are now concerned with the cost of this to the physician, from the point of record keeping and reporting to some central agency.

Attached are two sample forms.

Form A would be filled out in triplicate for each patient, and copies of all forms sent in weekly to the central agency.

Form B would be filled out weekly and sent to the agency. This would group all exposures together by age, type of irradiation, and in the case of therapy, the dose. In essence this gives the total dose delivered per week by the physician, but is not associated with any individual patient.

For conditions in your office will you please estimate the number of man-hours per week and the cost that would be required for filling in and submitting form A? Form B? What would it cost per year to maintain your own permanent records and copies of the same?

For purposes of comparing costs where large and small numbers of patients may be involved, it is necessary to relate these to the approximate weekly patient load.

Do not be concerned about the particular sample forms attached. They are admittedly inadequate, and are designed mainly to show the number of entries.

FORM A

Information needed (for individual report):
Name:
Address:
Age:
Date:
Sex:
Social security number:[1]

1. Radiological examination:
   (a) Radiographic:
       Region
       Film size
       Number of films
       K. V. P.
   (b) Fluoroscopic
       Region
       Ma-sec
       K. V. P.
2. Therapy: State dose and region.
3. Instructions: Send 1 copy to agency, 1 copy to patient, and retain 1 copy.

FORM B

Information needed (weekly group report):
1. Radiographic (age: 9 months to 30 years).
   Number of exams by type (probably about 20 types):
       Head
       Neck
       Chest
       Heart
       G. I.
       Extremities, etc.
2. Radiographic (age, over 30 years) (same types).
3. Fluoroscopic (age, 9 months to 30 years):
       Number of milliampere minutes by type of examination (probably about 8 types).
4. Fluoroscopic (age, over 30 years) (same types).
5. Therapy: Give total dose (skin) by regions (probably about 10 regions).

[1] If person has no social security number, provided necessary application forms and submit same within report.

[Office memorandum]

UNITED STATES GOVERNMENT,
BUREAU OF THE CENSUS,
April 1, 1957.

To: Dr. Lauriston S. Taylor, Chief, Atomic and Radiation Physics Division, National Bureau of Standards.
From: Morris H. Hansen, Assistant Director for Statistical Standards.
Subject: National radiation registry.

This relates to your memorandum of March 18 and our telephone discussions as well as your conversation with Mr. McPherson of our Bureau.

We have estimated only very rough order-of-magnitude costs. Our estimates can be viewed, we believe, as minimal since we have assumed that the most modern (in terms of present art) equipment can be used. Particularly important is our assumption that manual transcription of information on report forms will not be necessary. Instead, we assume that Fosdic report forms could be used. As you know, Fosdic scans microfilm copies of reports and transcribes the intelligence recorded on the form to magnetic tape.

Our approach contemplates continuous processing of the reports through the microfilming operation and periodic processing of the rolls of microfilm through Fosdic and an electronic computer. We arbitrarily decided that the Fosdic and computer work might occur 4 times every 10 years. Unless the microfilming is kept current the accumulation of reports would constitute a tremendous storage problem.

We estimate that between 35 and 50 million dollars would be required to defray the cost for the first 10 years of the method we visualize. Most of this would be spent at a rate of 2½ to 3½ million dollars per year to defray the cost of microfilming and the work precedent to microfilming.

About $6 million would be required to buy microfilm cameras, Fosdics, and magnetic tape. We have not included the capital cost of electronic computing equipment. Instead we have assumed that equipment owned by the Government would be available when needed. The only computer costs we have included are the operating and maintenance costs for 3 computer years to be used 4 times per decade. These are estimated to be from $300,000 to $500,000 each of the 4 times we have assumed they would be required during a decade.

Broadly described, the plan we used to make these estimates would be operated as follows:

Steps 1 to 3 below would be continuing work:

1. The reports, both individual and weekly group reports, would be designed using a Fosdic format. We believe all the information you describe with the exception of name and address could be recorded so that a Fosdic could read it.

2. A. The individual reports would be manually processed through a name-coding operation in which 3 or 4 significant characters of surname would be recorded in a manner amenable to reading by Fosdic.

B. The weekly reports would also be manually coded and edited prior to microfilming. The details of this process would have to be worked out. Our estimate makes allowance for some premicrofilming work.

3. Microfilm the reports.

Steps 4 and following would be prosecuted when the necessary electronic computer time was available.

4. Transcribe from microfilm to magnetic tape through Fosdic.

5. For individual reports match each report against a master list showing social security number and name. Note here that the 3 or 4 letters of surname coded in accordance with 2A above would be used to isolate problems for special handling.

6. Update a master file on which—

(*a*) For individuals—the exposure history for each different person would be summarized.

(*b*) For groups—a similar summary would be made.

Obviously, we have only a sketchy picture of the job to be done. To make firm estimates a much more detailed job of planning would be necessary. A few of the considerations neglected completely in our estimate are:

1. How critical a review of the original reports is necessary? Obviously, if high standards are imposed there would be a lot of communication with original reporting sources. On the other hand, an attitude that says we'll do the best we can with what we get, can practically eliminate the need for

checking back with the original reporter. Of course, costs are affected by the policy established here.

2. How much will it cost to obtain a file on magnetic tape giving social security number and name? Our estimates assume that such a file could be made available at essentially no cost.

3. From the point of view of long-range use is name of person adequate? Would names of parents, spouse, and issue be necessary to facilitate analyses of a genealogical nature? We have completely disregarded these questions.

Our efforts to prepare estimates for you lead us to the following question which you may want to consider.

> In view of the long-range objectives of a national registry of radiation exposure should our generation attempt to do anything more than storage of information in a form we hope would be useful to future students of this problem?

Perhaps the most that should be attempted now would be a plan to store microfilm copies of forms containing information such as you describe. We might very well leave to the future the task of deciding how to process such information. As indicated above something of the order of 2 to 4 millions of dollars per year would in our opinion, defray the cost of the preparation of such microfilm.

[Task 2—ICRP/ICRU]

Complete Radiation Exposure Recording for the Entire Population

Feasibility Study for the United Nations Scientific Committee on the Effects of Atomic Radiation

The feasibility of recording the radiological history of the entire population of the United States was examined and will be discussed below in terms of the following three systems:

A. Centralized recordkeeping of the total radiation exposure identified by individual.

B. Centralized recordkeeping of the total radiation exposure by groups without identification of the individuals.

C. Radiation exposure record keeping by means of records carried by the exposed individual.

The general nature of the first system is probably self-explanatory. Although final details were not developed, it was visualized that a system resembling, at least in broad outline, the current old-age and survivors insurance (social security) recording system in use for over 100 million individuals in the United States today, could be developed.

The second proposal would be similar except that no attempt would be made to keep a record of radiation exposure by individuals, but rather the total exposures would be recorded by groups selected in such a manner as to provide the desired data regarding the probability of reproduction.

The third system implies that each individual would carry a card or other type of record on which could be recorded his radiation exposure each time it occurs.

A problem common to any system is related to the lack of knowledge, by many practitioners of the healing arts, of the actual dose delivered in any particular examination or treatment. A solution to this problem is assumed to be a responsibility of task 1, but is mentioned here in order to emphasize that any of the above three systems, even if feasible in all other regards, could not be put into effect pending the completion of at least a major portion of the activities involved in task 1.

A. Centralized recordkeeping of the total radiation exposure identified by individual

Assuming that task 1 were sufficiently well completed to make the data reasonably available, this system of recordkeeping theoretically should provide information regarding the radiation exposure of any individual in such a form that it could be utilized for followup studies of a research nature, and be susceptible to relatively simple tabulation of information concerning the average radiation ex-

posure of groups of individuals with any given probability of reproduction. The problem was explored primarily with the assumption that the magnitude of the task would be such as to preclude the possibility of recording exposure data other than that to the reproductive organs. It was further assumed that the advantages of this system, as compared to system B, would be lost unless actual data concerning each individual exposure (rather than average figures for typical examinations under typical circumstances) were available. It appeared that the procedure most likely to be feasible would involve the design and distribution, under the supervision of a central repository, of exposure record cards which would be coded in such a manner that for each individual radiation exposure it would be necessary to indicate only the following information:

1. Name of individual
2. A serial number
3. The amount of radiation exposure to the gonads

The card might be prepared in such a manner that the information regarding dose could be indicated by filling in a printed circle on the card corresponding to the proper code. If an individual card were prepared for each exposure, all cards could be mailed to the central repository where the necessary tabulation would be accomplished automatically by tabulation equipment, or more likely by "magnetic memory." Based on the assumption (which checks reasonably well with the data in the report entitled "Gonadal Doses Received in the Medical Use of X-Rays" prepared for the genetics panel of the National Academy of Sciences by Drs. J. S. Laughlin and I. Pullman dated November 19, 1956) that there would be approximately 100 million individual exposure items reported annually, and the further assumptions that reports would be received from approximately 100,000 reporting entities (practitioners, hospitals, etc.) and that there would be 5 million requests annually for information, these requests requiring only a statement of total accumulated exposure of an individual to date, the Bureau of Old-Age and Survivors Insurance has estimated that the cost to the central recording office would be approximately between $8.5 million and $15 million per year. To be possible in operation, the system could not keep exposure data current on a day-to-day or even week-to-week basis. It would therefore not be usable in determining the total exposure including that from recent series of examinations, as a basis for deciding the desirability of any particular proposed exposure. This estimate does not include the cost to the reporting entities of preparing and submitting the data. The cost of this latter item is not susceptible to accurate estimate because of lack of valid data.

Following informal discussions with practicing radiologists, it seems fair to assume, however, that the additional expense involved would not be sufficiently great to significantly affect the cost limits set forth above for the central operation alone. Although the cost factor to the reporting entities may be relatively small, the impact on these entities of requiring such reports would be by no means insignificant. Thus, although the workload added to the reporting entities is such that it would probably not require the addition of personnel for the sole purpose of handling this problem, except in relatively large installations, the burden of preparing these reports, no matter how simple it might be, when added to the other recording problems inherent in the clinical practice of radiology, whether by radiologists or nonradiologists, might well be great enough to seriously affect the completeness and accuracy of reporting and thus the completeness and accuracy of the final data.

The cost estimate indicated above does not include an item to cover the assignment of identification numbers. Almost all working males and the majority of those females who are gainfully employed presumably already have assigned to them a social security number which could be used also for the purpose of radiation record keeping. Since a large portion of the population has or will have such a number assigned irrespective of whether or not a central radiation exposure recordkeeping system were established, it was felt justifiable not to charge the cost of this item, for purposes of this discussion, to radiation recordkeeping. At the same time it must be recognized that there are millions of people who would be required to obtain an identification number for radiation exposure records and who would otherwise not find such a number necessary. It is obvious, too, that in most cases the need for a radiation exposure number would occur in an individual's lifetime prior to his need for a social security number. In fact the feasibility should be explored of assigning to each individual at birth a number which could be used as needed in the future.

In considering its feasibility, it is essential to take into account the probable errors in this system. In addition to those errors which will inevitably be

introduced by lack of complete or accurate data in all cases, the degree of error depending upon the completeness and accuracy of Task 1, the most obvious source of error is related to incompleteness of reporting. Although it appears possible that with proper controls the completeness of reporting might be such that the data would be usable for determination of average exposures, it also appears likely that in many cases the data would not be sufficiently complete to be satisfactory as a record of exposure to individuals. Thus, it is likely that one of the principal theoretical advantages of this system would not exist in practice. Data in situations which might be considered reasonably comparable are not available. However, it has been found, for example, that appreciable difficulty is encountered in obtaining complete reporting on such relatively simple matters as births and deaths. The importance of recording of this type is universally recognized. In spite of this fact, strong legal and administrative controls are necessary to maintain this reporting at a high rate. Because of the existence of these controls, which would be difficult or impossible to duplicate in the field of radiation recordkeeping, the experience with birth and death records is not considered applicable to this problem. A situation which is perhaps more comparable is that of the reporting of communicable diseases. It has ben estimated that the completeness of reporting of communicable diseases averages less than 50 percent. Even here, the situation is not considered directly comparable to radiation exposure records because of the relative simplicity of communicable disease reporting, as well as the indeterminate factor of the psychological approach of reporting entities.

It is interesting to note that the only identifying data necessary would be the name and number. The Bureau of Old-Age and Survivors Insurance has made extensive studies which indicate that the possibility that a minor error in recording the number would lead to the crediting of the data to another individual with the same name and a similar number is so remote as to be negligible.

B. Centralized recordkeeping of the total radiation exposure by groups without identification of the individuals

The system under consideration here would be somewhat similar to system A except that no attempt would be made to record the data with identification of the individual involved. Instead, the data would be recorded by groups established on the basis of reproductive probability. The task would be made somewhat complicated as there are many factors in this country which affect the probability of reproduction. The most obvious one and the one mentioned most commonly is that of age. A most important factor, and one which is often overlooked, is that of health status. To the best of my knowledge there are no data currently available concerning the degree to which radiation exposure is a function of diseases which affect reproduction, but it is logical to assume that a large proportion of both diagnostic and therapeutic radiation exposure is to individuals suffering from a disease which has an adverse effect on the probability of reproduction. There are several additional factors, such as socioeconomic status and religion, to mention but two. The number of groups necessary to maintain validity of the data would, of course, depend on the number of the variables. These would probably not have a major impact on the cost of the system in terms of the central operation, but would add to the number of factors to be coded for each exposure and thus would add significantly to the load on the reporting entities. The system, as suggested here, could probably be accomplished with the least expense and difficulty to both the central agency and the reporting entities if it were set up technically on a system similar to system A—that is with individual record cards for each exposure, but without name or identification number, and with the addition of appropriate coding for the other variables involved.

It has already been pointed out that the task of the reporting entities under the system would be approximately the same as that under system A. However, it would not be necessary to include names, but would be necessary to include additional data regarding the variables which affect the probability of reproduction.

System B could be used if the reporting entities indicated the type of examination, rather than individual dose, provided that information is available from task 1 concerning average exposure data and if it is found that the factors related to reproductive probability do not affect the average dose for any given type of examination.

The cost of maintaining a central recording agency to record statistics from group reports has been estimated to be between $250,000 and $500,000 per year. This figure includes the cost of maintaining a file of reporting entities to provide followup on the completeness of reporting.

C. Radiation exposure recordkeeping by means of records carried by the exposed individual

The success of this system would depend to a great extent on the cooperation of exposed individuals in carrying and maintaining individual exposure records. If such cooperation were complete, this system could, in theory, provide the same information obtainable under system A, except that the information would be available only to persons having access to the individual personal exposure records.

This system is dependent upon the successful completion of task 1 to the same degree and with the same accuracy as for system A. It is quite possible that the element of bias on the part of the reporting entities would be more serious under this system than under system A.

There are no data available on comparable systems that might have been used for other than radiation-exposure recording. It has been established, however, that even in the instance of such relatively simple information as immunization records, where the importance is readily recognizable, individuals do not maintain and retain records which are sufficiently complete and accurate to be worthwhile except under specialized circumstances (for example, where immunization cards are required by immigration authorities). Although health records are maintained on the military population on a fairly successful basis, this is assumed to be true only because of the strict administrative control which can be exerted in the maintenance of such records. Other experience in this same general area leads inevitably to the conclusion that the maintenance of exposure records by the individual concerned would not be of suufficient completeness and accuracy to serve any useful purpose in determination of total population radiation exposures. Realistically, it appears that such a system would be of benefit only to that relatively small number of individuals whose personal interest and concern is sufficiently high to assure that his record would be reasonably accurate.

Of the three systems proposed, this system would presumably have the least impact on the reporting entities, once task 1 were accomplished. It would require only the simplest of recording techniques. Although the exposure data to be entered on the exposure record is not currently available to the majority of the users of radiation in the healing arts, such information should be made available to these practitioners regardless of whether this or any other recording system is instituted, and therefore the cost should not be charged against the reporting system.

In addition to the obvious weakness of this system in that it would not be sufficiently complete and accurate to provide data regarding total radiation exposure of the population, there are other serious and overriding considerations arguing against its use. The first and one of the more important is that of the psychological impact on the patient if he is provided with all data regarding his total radiation exposure. There has already been some difficulty arise as a result of the recent awareness of lay persons of the potential hazard attending medical X-ray exposure. This has, in turn, led some patients to attempt to make decisions regarding the need or hazard of specific examinations. The need for any individual examination or treatment involving a radiation exposure can properly be made only by an individual in a position to balance the possible benefit against the potential harm. Obviously, the individual ordinarily in the best position to make this decision is the practitioner, and the patient is often in the poorest position to make this decision. The possibility that patients might refuse examination once an arbitrary limit of exposure (for example, 10 roentgens) has been exceeded, regardless of the beneficial or possibly lifesaving result expected, is real. It must be remembered that in many cases where repeated or extensive radiation exposure is indicated, it would be the decision of the practitioner to withhold from the patient, for his own benefit, information regarding the seriousness of his condition.

Another major difficulty in having the patient carry his own exposure card, particularly in this country, is the medicolegal one. The medicolegal hazards in the practice of radiology are already sufficiently serious that a substantial extra premium is charged for professional liability insurance covering individuals

using radiation in their practices. To provide a record to the patient which established the fact that in his case certain arbitrary limits of exposure had been exceeded would seem to inevitably lead to a large number of malpractice suits. Such suits could have a serious impact on the use of radiation in the healing arts whether or not they could be successfully defended. Perhaps an even more important implication of the medicolegal problem is the bias which it might create in the recording of the data. This point seems so obvious that further discussion is not needed.

The State radiological society which recently had the suggestion made in its name that such a system be instituted, has suspended this recommendation on the basis of a preliminary feasibility study.

SUMMARY

The feasibility of radiation exposure recordkeeping for the entire population has been discussed with agencies most knowledgeable concerning the technical problems involved and with practitioners of the healing arts upon whom the impact would be most severe. The more important considerations stemming from these discussions are pointed out above. Following is a summary of the conclusions reached regarding the feasibility of the three alternate systems which appear most reasonable:

1. System A would be possible of accomplishment when task 1 is completed in all respects, and information regarding radiation exposure under all the variable circumstances if any given examination by any given practitioner on any given patient are known. (Pending the report from task 1, it is assumed that this Utopia will not take place in the immediate future). The technical problem of centralized recording and compiling of the data is one which experience with social security records indicates could be mastered. The high probability of incomplete reporting implies that the primary aim of such a system (i. e., accurate data regarding the exposure of each individual) would not be accomplished. The expense would appear high. Equal funds expended in activities designed to reduce the radiation exposure of the population might well be of greater benefit than funds expended for recording the present status.
2. System B would appear to be possible once adequate data are available from task 1. Such data would not need to be as complete as those for either system A or C. The cost would be substantially less than for system A. The impact on the reporting entities (practitioners, etc.) would be great enough to significantly affect the accuracy of the final data. Pending reports regarding task 3, it is assumed that essentially the same information as would be obtained by this system could be obtained with better accuracy and less expense by a sampling procedure.
3. System C could not be expected to provide reasonably accurate data regarding *total* radiation exposure of the population. Although it is the least expensive of the systems discussed, consideration of the psychological and medicolegal impact make it appear not to be feasible, even if it could provide the desired data for some individuals.

It is concluded therefore, that after task 1 has accomplished at least a substantial part of its duties, complete recordkeeping would be possible, but would not be desirable, because information of essentially equal value could be obtained with substantially less expense and with probably greater accuracy on a sampling basis.

[Task 3—ICRP/ICRU]

DIRECT SAMPLING METHODS

FEASIBILITY STUDY FOR THE UNITED NATIONS SCIENTIFIC COMMITTEE ON THE TOTAL POPULATION SAMPLING

The feasibility of determining the total radiation exposure of the population by means of (1) *direct sampling of the population* as well as by means of (2) *sampling medical institutions* was studied with the cooperation and assistance of individuals and groups most intimately concerned and most knowledgeable regarding sampling methodology, as well as those on whom the impact of such a system would be most serious. The feasibility of arriving at exposure data

for the total population by (3) *a sampling of practitioners of the healing arts* was also examined and will be reported separately as task rc.

The different methods which might be applied to direct sampling of the population or sampling of medical institutions which were reviewed in preparing this report are:

1. Mail questionnaire sampling;
2. Personal interview sampling;
3. Dosimetric instrument survey sampling.

Basic to a consideration of the problem is the assumption, which can be adequately substantiated, that analysis of data derived from an adequately designed sample, making appropriate allowances for all of the variables involved, can provide information applicable to the entire group under consideration. A further consideration is that by the use of sampling techniques it is possible to limit the number of individuals studied and the amount of data collected to a degree where a greater effort can be expended on each individual or group, and thus the accuracy of the resulting data and its analysis may be higher. It can thus be assumed that if a sampling procedure is used, the accuracy of the final data may exceed that obtained from a survey of the total group.

Various methods are available for collecting the data from the sample population but there are certain elements common to them all. Although the object of the survey would be to obtain data regarding the total radiation exposure of the sample population, this population would not be in a position to provide quantitative information regarding its exposure. Therefore, the most optimistic estimate as to the information that could be obtained directly from the sample indicates that no information beyond the presence or absence of exposure during a recent relatively short time interval would be forthcoming, together with information of indeterminate accuracy regarding the source of exposure. Thus, a study of a sample population would not, by itself be expected to provide the desired information. The extent to which even limited information might be obtained cannot be determined without the use of a pilot study or presurvey. Experience accumulated to date on the basis of other sampling does not provide firm figures on such important elements as cost and accuracy, because of the peculiar circumstances attending the problem of radiation exposures.

Regardless of the approach to the problem it can be said with definiteness that the information obtained by any sampling technique, would not be of appreciable value pending a report from task 1 indicating substantial accomplishment of its objectives. The possible use of an instrument sampling procedure as a substitute for information from task 1 will be discussed later in this report.

EFFECTS OF ATOMIC RADIATION

The three possible methods by which information might be obtained in a sample population and which have been considered are: (1) mail survey; (2) a survey by personal contact; and (3) survey by instrumentation.

Mail questionnaire

A survey by this technique would involve the preparation of a questionnaire, properly designed to elicit the desired information from all individuals in the sample, which by definition would require that it be understandable to persons at all socioecenomic levels. This is, of course, an impossible task, and the question is actually one of the degree of inaccuracy which would result. The experience of radiologists whose practice includes a broad spectrum of patient types indicates that a very significant segment of the population does not have accurate knowledge concerning the presence or absence of X-ray exposure as differentiated from other therapeutic or diagnostic procedures such as diathermy, infrared, etc.

Additionally, there is a large element of error concerning many individuals' knowledge concerning the correct name and address of the source of the exposures. The response of a general population sample to mail questionnaires is notoriously poor, and is subject to an extremely large element of bias. These considerations have been sufficiently serious and have led to such serious error in the data obtained that it can be assumed that a mail survey would not yield data adequate to accomplish the objectives.

Sampling by personal contact

The overriding difficulties and complications of a mail sampling technique for the population have been so serious that, in spite of the relative economy, this

technique has been largely supplanted by surveys by trained interviewers, making personal contacts. This latter technique has been developed to a relatively high state of perfection and is widely utilized in this country for many purposes. The accuracy to which data in general can be provided by this technique has also been established.

In examining the various existing systems which might be put to use to meet the problem at hand, the two which appeared most appropriate were the national health survey of the United States Public Healh Service and the current population survey of the Bureau of Census. These two systems were therefore examined in more detail.

The national health survey of the Public Health Service is currently in a state of development. It is proposed as a continuing survey of a sample population for the purpose of providing a wide variety of information regarding health status. It represents one method by which the information currently available to the population concerning its radiation exposure might be assembled. At present, however, the national health survey is still in relatively early stages of its operation, and there would likely be an appreciable time delay before it would be desirable to introduce questions regarding radiation exposure to supplement the current questionnaire.

The current population survey of the Bureau of the Census is a continuing survey which has been in existence for some time. It surveys a population sample consisting of approximat ly 35,000 households. The nature of its operations is such that it would appear feasible to introduce a small number of questions regarding radiation exposure and obtain a compilation of the resulting data in the period of a few months. The cost of collecting such data from a sample population has been estimated at $10,000 to $50,000 depending upon the number and nature of the questions to be asked. The sample survey is a fair representation of the entire United States population except that it excludes the resident instiuional populaion. The exact exent to which his exclusion would affect the validity of the resulting data is not immediately apparent. In terms of the total United States population, however, this group is relatively small. Although the total radiation exposure of this group may not be insignificant, yet it is likely that the reduced reproductive probability of this group, together with its small size, would not introduce a major source of error in determination of the total dose to the genetically significant population.

It is important to recognize, however, the limited amount of information which could be obtained by this means. As pointed out above, it must be assumed that the population does not have accurate data concerning radiation exposure in terms of roentgens. The most that could be hoped for would be some indication as to the number of individuals exposed to radiation in any given period of time, with some data regarding the source of exposure. In order to convert this into meaningful terms, it would be necessary to contact the source of exposure with the hope of obtaining dose data. It is not reasonable to attempt to assess the probable cost of converting the original information received from the survey into usable form, nor the accuracy of the final data, at this time. The cost and effort of obtaining the information which the current population survey might provide appears so reasonable, however, that serious consideration should be given to utilization of this approach with the expectation that, even if the information were not adequate to accomplish the desired results, it would be worthwhile from the standpoint of future planning.

Survey by instrumentation

The possibility was also examined of arriving at a figure concerning total population radiation exposure by means of a survey of a sample of the population to whom appropriate individual radiation dosimetric instruments are provided. This method would require the individual to use the instrument each time he received a radiation exposure. The obvious advantage to a survey of this nature is that it would not require successful completion of task 1 before it might yield data. It would, however, presumably require a personal visitation to all members of the sample population, with adequate instruction on how to use a properly designed meter for recording the exposuure at any particular time. If the current population survey of the Bureau of the Census can be taken as a proper indication, a fair population sample might involve something in the order of at least 35,000 househol ds on a preliminary basis. It is entirely possible that, in view of the fact that the current incidence of radiation exposures for the population is somewhat indeterminate at least in its distribution,

the results in this population group will not provide a sufficient number of positive replies to be statistically adequate.

For this reason it cannot be concluded that 35,000 households would necessarily represent an adequate sample for this purpose. It does give some appreciation of the minimum sample size, however. On this basis it is assumed that the number of instruments to be distributed, used, processed, and recorded, would be at least 100,000. The total cost of such a survey would, then, approximate at least several hundred thousand dollars. Again, there is no satisfactory experience on which to base an estimate as to the accuracy of the data which would be obtained. An obvious source of error would be in failure of individuals in the sample to use their instrumentation correctly, if at all, during a radiation exposure. It seems fair to assume that this would introduce a very large source of error and that there would be an appreciable bias in this error, as the failure to utilize the instruments correctly would be a function of socioeconomic status and other factors which might be related to total radiation exposure. Many other factors would have an important bearing on the results of such a study, not the least of which would be the psychological impact upon the persons surveyed, as well as the impact on the practitioners of the healing arts. In many cases, valid objection on the part of the practitioners could be expected with reference to confidentiality of medical information and related problems. This sampling problem is so unique that even approximate answers to the problems involved and probable accuracy of data could not be given until it has been tried on a pilot basis. The limited nature of this feasibility study does not permit a conclusion as to whether or not they are insurmountable.

INSTITUTION SAMPLING

Approaching the problem of total population radiation exposure by a sampling of medical institutions would appear to be appreciably less difficult than would sampling the population. At the same time, however, the information obtained would be incomplete in several important respects:

1. Although medical institutions presumably represent a major source of radiation exposure of the population, the extent to which noninstitutional practice contributes is one of the major unanswered questions and would not be resolved by this type of study.
2. The information already available concerning institutional practice is appreciably more adequate and accurate than similar information regarding noninstitutional practice, and it might therefore be argued that this area is in least need of emphasis.
3. Since institutional practice can by no means be considered average for the practitioners of the healing arts as a whole, there is a strong element of bias if these figures are extrapolated to the general population.

Among the advantages of such a study would be the relative ease and relatively low cost involved. A vast majority of the medical institutions in this country which would be likely engaged in a large scale use of radiation are affiliated with the American Hospital Association. Thus, there is an existing facility to assist in implementation of such a study. The impact on the medical institutions of compiling and reporting on the data requested would be appreciable, however. Only a few highly selective institutions would have the required data readily available. Thus the task to the large majority of institutions would be quite formidable. This would obviously affect the cost to the reporting entities and would introduce a major element of bias.

The American Hospital Association is currently conducting, at the request of its Committee on the Use of Radioisotopes in Hospitals, a survey on the use of radionuclides. The experience of this survey should provide some data concerning the cost and probable accuracy of a study in the radiation field. It is estimated that this survey will cost between $1,000 and $10,000. Based on previous experience with surveys by the AHA, the yield may well be 50 to 75 percent. There are several reasons why these figures should not be translated directly for use in a general radiation exposure survey, however. For example, the isotope survey is being conducted on a total study, rather than on a sampling basis. A very large percentage of replies is expected to show no use of radioisotopes and therefore will not require a major effort on the part of the reporting institution nor will they contribute appreciably to the problem in the compilation of data.

If it were decided to approach the problem of exposure in medical institutions by personal contact rather than by mail questionnaire, one of the major

factors which would increase the cost would be the necessity for adequate geographic distribution. It is assumed that there is a highly significant element of "clustering" which tends to result in significant variation among institutions when all factors except geographic location are held constant.

A survey of medical institutions using the instrument sampling technique would appear to have similar characteristics to a survey by mail questionnaire or personal contact. The important difference with regard to feasibility is that sampling by the instrument method would not require the successful completion of a major portion of task 1 as a prerequisite. In fact, such a survey might be expected to yield important information helpful in the completion of task 1. A byproduct of an instrument sampling project which might increase its value would be the inevitable educational impact which would result.

SUMMARY

The possible utilization of sampling techniques applied to the entire population and to medical institutions as a means of deriving data regarding the total population radiation exposure has been explored. Definitive answers regarding the feasibility of such projects cannot be provided until pilot studies have been made. In general, a sampling procedure should provide information with equal or greater accuracy at appreciably less cost than would a total population study. Sampling of the population by mail questionnaire would probably not yield sufficient data to be justified. Sampling of the population by use of an existing system, such as the Bureau of Census current population survey might be expected to provide some data regarding the presence or absence of radiation exposure history over a relatively short period of time and would probably provide much information regarding the feasibility of more extensive population studies. Sampling of the population by instrumentation would probably not yield information of sufficient accuracy to be warranted.

Sampling of medical institutions would probably yield additional data regarding radiation exposures at such institutions. This information would not be susceptible to extrapolation to noninstitutional radiation exposure unless it could be proven to be typical of the latter, a situation which is highly improbable. Sampling of medical institutions by personal contact or instrument sampling would obviate the need for prior completion of task 1 and might provide significant information of value in completing that task. If task 1 were successfully completed, however, the information to be derived by personal contact or instrument sampling of medical institutions might not be significantly greater or more accurate than could be obtained by mail questionnaire and would be appreciably more expensive and difficult.

[Task 4a and 4b, United States]

FEBRUARY 28, 1957.

APPROXIMATE METHODS FOR MEDICAL X-RAY EXPOSURE DETERMINATION

The determination of the average gonadal dose received by the public due to medical procedures by approximate methods requires information on certain specific points. It is assumed that some of the information mentioned in task 1 is available; i. e. that for every type of examination or treatment a gonadal dose can be assigned. Survey of the scattered measurements reported in the literature determines these only approximately.

The objectives of approximate methods consist primarily of the establishment of the following additional items:

(1) Number of different specialists in the various categories which use X-rays medically. These categories include: (*a*) hospitals; (*b*) clinics; (*c*) radiologists in private practice; (*d*) general practitioners; (*e*) physician specialists;[1] (*f*) osteopaths; (*g*) chiropractors; (*h*) chiropodists; and (*i*) dentists.

(2) The number of patients examined or treated annually in each of the above categories classified according to age and sex.

(3) Distribution of the type of examination or treatment by each of these patients in (2) in each of the categories in (1).

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[1] Physician specialists include: Pediatrics orthopedic surgery, general surgery, internal medicine, industrial medicine, urology, gastroenterology and tuberculosis, obstetrics and gynecology, eye, ear, nose, and throat, cardiology, dermatology, allergy.

The establishment of these three points together with information on the gonad dose per examination, and such statistics as total population, age distribution of population at birth of each child, etc. then permits a determination of the genetically effective gonad dose per examination.

The data on (1) are available partially from Public Health Service records and reports by Modern Medicine Survey, the 1951 report by Dr. Donaldson, American Medical Association, and American Dental Association. The number of practitioners in the categories (1*a*, 1*b*, 1*c*, 1*d*, 1*e*, and 1*i*) and the number of X-ray units which they employ are believed known within about ±10 percent. The accuracy of the numbers of osteopaths, chiropodists, and chiropractors who own X-ray equipment is much less certain.

(2) With regard to the number of patients examined in each of the categories, information is available for some hospitals (1*a*) and clinics (1*b*). Much of this information is not available in a convenient form because of the way in which the records in many hospitals are kept. In some instances analysis is greatly facilitated by the records of group health plans such as the plan used in Little Rock, Ark. Another plan worthy of note is the Commission on Professional and Hospital Activities in Ann Arbor, Mich. Their record form provides complete information on all of their patients and specifically includes total information with regard to X-ray studies. There are group health plans in various parts of the country which also have sufficiently good records to supply data on the number and type of X-ray treatments taken by their patient members. A similar study, of a more specialized nature is the compilation of obstetrical data including number and type of X-ray examinations from a number of hospitals which is under the supervision of Prof. Schuyler Kohl at the College of Medicine of the State University of New York in New York City (Kings County Hospital). However, the vast majority of the hospitals in this country do not have their records in such a conveniently available form. Wtih regard to the general practitioners (1*d*), information is available from surveys made by the National Electrical Manufacturers Association (NEMA). Based on questionnaires, they have estimated the number of general practitioners who own X-ray equipment, what kind of equipment, and the number of radiographs and fluoroscopies performed weekly by the practitioners. Unfortunately such information does not exist for the other categories (1*e*, 1*f*, 1*g*, and 1*h*) listed above.

With regard to the third point above, distribution of type of examination or treatment in each of the categories is available from individual hospitals and clinics and some individual radiologists and general practitioners. With much effort this information could probably be obtained from most of the hospitals and radiologists. However, this information does not exist for the other categories listed above (1*d*, 1*e*, 1*f*, 1*g*, and 1*h*).

It can be concluded on the basis of the reports and sources referred to above, that for certain small collections of patients accurate information on points (1), (2), and (3), is obtainable. This represents a small sample of the total patient volume so that there is an unknown, and possibly large uncertainty in the reality of estimates in these bases.

DATA BASED ON FILM SALES

Information with regard to the total number of radiographic examinations made annually can be obtained from data supplied by film companies. Allowing for uncertainties in the knowledge of the company of the final destination of their film, it is estimated that the total number of radiographs can be determined to within 20 percent from the use of film data from manufacturers. The distribution of this film with regard to size, but not sensitivity, is also partially available. Further contacts with film companies may well produce more information. The records of the Department of Commerce provide information on the importation of film, and also on the amount of film exported annually with some idea of its destination.

For the first three categories above (1*a*, 1*b*, 1*c*) it is possible to give an estimated classification of radiographic examinations and relative frequency based on only limited supporting data. Presently available data do not give directly the distribution of examinations by types. The relative number of examinations in the indicated age groups is estimated as: 0–15 years, 15 percent; 16–30 years, 20 percent; 31–50 years, 35 percent; and above 50 years, 30 percent. As an overall average, three films are used per radiographic examination.

DATA OBTAINED FROM X-RAY TUBE RECORDS

Through the National Electrical Manufacturing Association some data are available with respect to tubes produced and sold in the United States annually. These data apply to about 80 percent of the tubes sold since the North American Phillips Co., Eureka Co., and Dunlee, Co. do not participate in the NEMA. These data on X-ray tubes are not sufficiently complete to provide information on those indtended for a specific radiographic installation or whether or not they are to be used primarily for fluoroscopic or radiographic purposes. NEMA has no records of dental tubes. The census of manufactures (Department of Commerce) has data on annual tube production but these do not distinguish between medical and industrial use.

The records of the Department of Commerce include the importation and exportation of X-ray tubes but do not indicate whether they are therapeutic, diagnostic, or other.

There is no automatic way of determining the mean value of the quantity of electricity which passes through the diagnostic tube during its life, nor are the available lifetime records of individual companies very good on this point. Rough estimates can be made by individuals experienced in the X-ray field.

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[Task 4c, ICRP/ICRU]

APPROXIMATE METHODS: QUESTIONNAIRE TO PROFESSIONAL PEOPLE

FEASIBILITY STUDY FOR THE UNITED NATIONS SCIENTIFIC COMMITTEE ON THE EFFECTS OF ATOMIC RADIATION

The feasibility of obtaining data on the total population radiation exposure by means of a mail questionnaire to professional people was explored. Before presentation of the information obtained, the following two points should be made clear: (1) the medical profession in most instances lacks the exposure information needed to answer the questionnaire. Information would be needed regarding the radiation exposure under all of the varying conditions present in each individual examination. In other words, such a questionnaire would be essentially unsuccessful in terms of actual dosage until completion of task 1, (2) experience with all of the variables involved in questionnaires to professional people indicates that definitive answers as to the feasibility, sample size, cost, and related questions cannot be given until the particular survey in mind has been tried on a sample or presurvey basis.

It is apparent that a correctly designed sample could provide information regarding radiation exposure with an accuracy which would equal or exceed that of a survey of all professional people.

Among the major factors which might affect the feasibility of a survey of this nature are the cost, sample size, percentage return, and probable accuracy. Unfortunately, none of the surveys done to date among professional groups are completely comparable to the ones suggested here. Certain principles and technical factors can be extrapolated, however. Thus, in considering such a survey, it would appear desirable to select the sample to be queried by stratifying professional people into groups depending upon the probability of their making a significant contribution to the total radiation exposure of the population. The proportion of the total number within each stratum which it would be necessary to sample depends upon the contribution of that stratum to the total radiation exposure as well as other factors.

Thus, it might be desirable and feasible to query all of those practitioners in the group likely to produce heavy radiation exposure, such as radiologists. Among certain medical specialties where the use of radiation might be quite heavy (but presumably not as great as among radiologists) one would sample only a percentage, for example, one-quarter of those involved. Among those individuals with an expected moderate and light use of radiation, an even smaller percentage could be sampled, for example, 1 in 20. Additionally, there would be a group which could be segregated on the basis of type of practice (such as administrators, public-health workers, or psychiatrists), where it could

be assumed that there is no use of radiation or that the use is so slight as to not affect the total radiation dose of the population.

Basing estimates on this approach to the problem it is possible that a reasonably accurate estimate might be obtained by a survey of something in the order of 10 percent of the total professional group. Based on an estimate of approximately 250,000 physicians in the country, would lead to a sample size in the order of 25,000. Whether or not this sample size would be adequate could not be finally determined until after returns were received, although it could be estimated on the basis of a pilot study. One of the major factors in determining the adequacy of the sample would be the percentage of satisfactory returns. The difficulty of estimating the percentage returns is indicated by the fact that although many surveys have considered a return of 10 to 25 percent to be quite satisfactory, one survey currently being conducted has shown a return of over 90 percent from a mail questionnaire with 2 or 3 mail followups. Much of this variability in the percentage return can be accounted for by the interest of the profession and the stimulation and support given by prominent governmental and nongovernmental agencies, as well as the difficulty of answering the questionnaire.

Experience indicates that if mailing lists were available and did not have to be compiled separately, a mail questionnaire with 2 or 3 mail follow-ups costs in the order of $2.00 to $5.00 per questionnaire, not including the cost of the analysis of the returns. Thus, a preliminary estimate can be made that the cost of the questionnaire procedure, not including compilation of the data and analysis of the returns would be within one order of magnitude of $100,000. The cost of analysis of the data is even more difficult to estimate. It would appear almost certain to exceed the cost of the questionnaire itself, but the factor by which it exceeded the cost of the questionnaire would be determined by the nature of the replies and the extent to which it was desired to extract detailed information.

The impact on the persons queried could range from minimal to essentially impossibly serious depending upon the amount of information requested. One of the more important problems in this connection would be the time period over which exposure data were requested. It seems reasonable to assume that providing a relatively small amount of information covering operations for one day or possibly for one week would not represent an unjustified imposition on the reporting entities. On the other hand, data would, in most cases, not be readily available for any extended period of time and it would therefore be necessary for the data to be compiled especially for the purpose of answering the questionnaire. Therefore, if data were requested for a more extended period of time, for example, a month or a year, the problem to the reporting entities of compiling these data would become so great that, if replies were received they would probably be grossly inaccurate.

The question as to whether or not sampling for a short time interval would be adequate is difficult to assess. There certainly would be some errors brought about by seasonal or weekly variations but the errors thus introduced might not be sufficiently great to adversely affect the final data. This is a point which could be resolved only by repeated sampling for short periods of time on several occasions, thus spreading the task of completion of questionnaire through different groups, or by sampling within the sample for possible time variations. If the latter method were used it might be necessary to provide financial or other support to this sample of the sample in completing its task.

In summary therefore it can be stated that:

1. A sampling among physicians would not provide the desired data regarding the total population exposure until substantial progress is made toward the completion of task 1.

2. The firm estimates of even such fundamental items as sample size and overall costs cannot be made until a pilot study has been conducted.

3. It appears likely that, if sufficient information were available from task 1, a sampling of the medical professions by mail questionnaire, with the use of proper controls could provide valuable information regarding total exposure of the population to radiation.

4. Pending the conduct of pilot studies it seems reasonable to assume that a mail survey of a sample of the professions would be a large, but feasible task, following completion of task 1.

[Task 5, United States]

GONADAL RADIATION IN THE GENETICALLY SIGNIFICANT PORTION OF THE POPULATION DERIVED FROM RADIOACTIVE ISOTOPE PROCEDURES IN MEDICINE

GENERAL CONSIDERATIONS

In analyzing the contribution of gonadal radiation derived from radioactive isotopes in medical practice, the first consideration is to deal only with patients in the genetically significant portion of the population. For this purpose we will use only the age group from conception to the age of thirty. The second feature is that within this patient group, the only significant radiation is that which is given to the gonads before children are conceived by these patients, and this immediately discards a considerable number of very ill patients who have no chance of conceiving children after therapeutic procedures, those in whom sterility is a necessary antecedent or result of therapeutic procedures (as in carcinoma of the cervix and carcinoma of the breast), as well as some more subtle features which have to do with conception being less likely in people who have had serious medical problems. In setting up a valid statistical study, all of these features would have to be included as well as a proper study of the numbers of procedures within the specific separate ages involved. We do not have anything which approaches satisfactory data on these aspects, but in the discussions below, some attempt is made to bring them into focus. At any rate, it is unrealistic to take the total shipments of curies of any particular isotope from Oak Ridge and to assume a high proportion of utilization within the genetically significant population, because by far the larger doses are used in the treatment problems which automatically limit the probability of conception in the large dose treatment cases.

The problems of dosage delivered to the gonads in any particular case of the use of a specific isotope for a specific purpose are not easy. The beta and gamma components must be individually considered and with due regard to the geometry of the position of the ovaries or testis, not only in relationship to the whole body general distribution of a specific isotope, but also for such special features as radioiodine concentration in the thyroid, urinary bladder, stomach, etc. in certain phases of the metabolic management of the isotope, with special respect to the gamma component. The beta component must take due regard for minor degrees of specific localization in the gonads which would not be accurately available without biopsy and assay, but which can be accurately available without biopsy and assay, but which can be estimated from "blood dose" or from actual assays at necropsy or in surgical specimens (see reference by George S. Kurland and A. Stone Freedberg).

MOST USUAL TYPES OF ISOTYPES EMPLOYED IN MEDICAL PRACTICE. (INTERNAL USE ONLY).

The following is a listing of the most common current uses of radioactive isotypes with notations as to the type of patients for each use and some comments as to the age grouping, life expectancy and probability of conception of

children within these groups. The relationship of the procedures to gonadal radiation when significant are analyzed separately below:

Isotope	Type of use	Type of patients (with respect to age and life expectancy)	Relationship of dose of gonadal radiation in significant population
I^{131}	A. Diagnostic: Thyroid tracer scans, etc.	Used in all ages, but cautious choice in children.	See analysis below (I).
	B. Treatment: Hyperthyroidism	Most users conservative about use below age 30; require special indications.	See analysis (II).
	Cardian and pulmonary disease.	None below age 30	No significant contribution.
	Thyroid cancer	Few cases below age 30. Only used in cases with metastasis. Life expectancy short. Children after treatment relatively unlikely.	Do.
	C. Combined diagnostic; RISA, tagged dyes, etc.	Some studies done below age 30; small tracer doses, short, effective half-life.	See analysis (III).
P^{32}	Treatment: Polycythemia vera	Very rare below age 30	No significant contribution.
	Breast cancer (advanced)	Very few below age 30 and all sterilized before P^{32} given.	Do.
	Leukemia	Few cases below 30 and chances of having children almost nil after P^{32}.	Do.
	Colloid in advanced cancer or leukemia.	do	Do.
Au^{198}	Serious effusions (malignant)	No cases likely below age 30. Very limited life expectancy.	Do.
	Leukemia		Do.
	Carcinoma of cervix	Very few cases below age 30. Children nil after treatment.	Do.
	Carcinoma of prostate	Very few cases below 30. Chances of having children very remote after treatment.	Do.
	Diagnostic: Liver scans for malignancy, etc.	Almost no cases below age 30; very limited life expectancy.	Do.
Cr^{51}, Fe^{59}	Red cell volume, life, etc.	Some studies below age 30. Small doses. Good proportion of patients quite ill with limited life expectancy.	See analysis (III).
Na^{24}, K^{42}	Metabolic states	Few studies below 30. Most patients quite ill with limited life expectancy.	Do.
Co^{60}	Labeled Vit. B_{12}	Few cases below age 30 for differentiation of pernicious anemia and nontropical sprue.	Do.

ANALYIS OF SITUATIONS IN WHICH GONADAL RADIATION DOSE SEEMS POSSIBLY SIGNIFICANT

I. Diagnostic studies of the thyroid by tracers, scientillation scanning, etc., with radioiodine I^{131}

In the papers by Trunnell et al., and Kurland and Freedberg, the beta components of dosage per millicurie of I–131 are listed as follows: ovary 0.6 rads per millicurie; testis 0.3 rads per millicurie. In the paper by Seidlin, et al., the beta and gamma components together are approximately 0.6 rads plus or minus 0.3 per millicurie. To this might be added approximately 0.1 to 0.3 rads per millicurie from the transient dosage in the bladder in the case of both ovary and testis.

Using a 25 microcurie tracer, the average dose to either ovary or testis might be estimated at approximately 0.015 rads per tracer procedure. A rough analysis of the number of tracer studies performed in the United States per year would suggest that approximately 150,000 to 200,000 would seem like a reasonable figure. Of these, probably not more than 25,000 are performed per year in patients below the age of 30 and in the genetically significant group. On the basis of these figures it turns out that only approximately 375 rads would be delivered by all of the tracers in the genetically significant portion of the population from this tracer used. Even if the frequency of such studies is considerably greater than listed, it still would not get into a figure which is of any great concern.

II. Treatment of hyperthyroidism with radioactive iodine

Using the same source material as listed above, a 10 millicurie (rather large) full treatment dose of radioactive iodine would give approximately 6 rads to the gonads. On the basis of approximately 2 million persons, it seems unlikely that over 1,000 or at the most, 2,000 hyperthyroids below the age of 30 and in the genetically significant group are treated per year in the total population (the best estimate we can get for Philadelphia is about 15 to 30 such cases per year). Of course, Philadelphia might not be representative, but if it is more conservative with respect to the choice of patients, it also is probably somewhat better covered with respect to isotope users than one would expect for the average of the country. At any rate, the figures on this basis would be 6,000 to 12,000 rads per year to the total population.

III. Other tracer procedures such as blood volumes, red cell volumes, red cell life, tag dyes, etc.

The practice in different parts of the country in the use of these procedures, particularly in our selected genetically significant population is quite variable. At any rate, one would not think it reasonable that this would approach the more widespread use of radioiodine in thyroid studies, volumewise, and the dosage to the gonads derived in these studies is not usually as great as one would expect with the usual methods of radioiodine studies for the thyroid. If this reasoning is correct, one could assume that the maximum would not exceed the figure of approximately 375 rads as listed in example roman numeral one above.

SUMMARY AND CONCLUSIONS

The exact data for deriving the gonadal radiation contributed by internal radioisotope procedures in the United States within the genetically significant portion of the population is not available. In searching for a reasonable figure, it is not permissible to use the total distribution of radioisotopes from Oak Ridge because of the very great variations in the types of procedures, dosages, etc., that are used for different conditions and because of the very different practice within age groups and life expectancy groups within the population. A great majority of treatment procedures are used in patients who cannot have any reasonable likelihood of adding to future populations. In the whole spectrum of internal radioisotope use, only three major procedures seem to contribute significantly to the gonadal radiation that we are interested in. Thyroid tracer studies probably do not account for more than 375 rads per year to the entire population (this probably should be expressed as 375 man-rads per year). All other tracer procedures within this population group would not seem to add more than an equivalent additional 375 man-rads. The only probable contribution treatmentwise is in the treatment of hyperthyroidism with I–131. This most likely does not contribute over approximately 6,000 to 12,000 man-rads per year.

ADDITIONAL COMMENT

In trying to compare the figures so derived with those of Stanley Clark, one should divide the totals, something less than 15,000 rads, by 60 million persons as representing the population below age 30 and this gives a figure of 0.00025 man-rads per potentially genetically significant person per year in the United States. Clark's figures for radioiodine represented a very small portion of his total "theorizing", but were some 32 times larger (0.008 roentgen per year per person). He ignored the selection of the age and procreative potentiality, except as an afterthought in his summary and discussion and thought that it was compensated for by underestimates in other directions. He furthermore thought that his estimates would not likely be off by any such large factor. It is my considered opinion that Clark's approach to this problem is entirely unwarranted and has led him to ridiculous conclusions. It is Clark's paper which is the only one that Doctor Laughlin quotes from in his comprehensive summary of the literature on the subject.

R. H. CHAMBERLAIN.

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(Mr. Taylor's statement—continued.)

OTHER ITEMS

The question has been raised as to the distinction between exposures of individuals and of population groups. This has been considered for many years. Where exposures run to fairly high levels it is desirable that exposure records be kept for individuals. Where exposure levels are low and where the persons exposed may be random segments of the population, it is not important to retain this information by individuals. For low doses, of the order of perhaps 50 roentgens average, per individual, per lifetime, the effects are probably so marginal that averages of population suffice for all analytical and record purposes. For genetic damage it is the total dose to the whole inbreeding part of the population that counts most. There may be some exceptions to this for large individual doses. In summing up the genetic damage it is important to weed out that part of the population in which there is expectation of having children.

For somatic damage the individual is the controlling unit but on the assumption that there is no threshold exposure and for low exposure levels the dose averaged over the whole population is the primary factor influencing the statistical risk to an individual.

A question has been raised as to the possible effect of the use of different protection standards by states or nations. This is somewhat of an academic question because such a situation does not exist. In the United States the basic protection standards, as set by the NCRP, have been adopted without exception by the individual States and by the Federal departments concerned. Over the world all nations, including the Iron Curtain countries, presently use the standards set by the ICRP. As noted above these are the same as those established by the NCRP. (For further information on international recommendations see Recommendations of the International Commission on Radiological Protection published as supplement No. 6 of the British Journal of Radiology, London, 1955.) This report is presently under some revision. Because of the national and international acceptance of existing standards it is not deemed likely that confusion will result from the use of different standards.

A problem does exist, however, on both a national and international basis, namely, that there seems to be developing a plethora of organizations that want to set up various kinds of radiation protection bodies or committees.

(Exhibit 23 is as follows:)

JANUARY 19, 1957.

EXHIBIT 23: RELATIONSHIP BETWEEN VARIOUS TECHNICAL BODIES AND THE INTERNATIONAL ATOMIC ENERGY AGENCY

In paragraph 6 of article III–A, describing the functions of the new International Atomic Energy Agency, the following statement is included.

The Agency is authorized "to establish or adopt, in consultation and, where appropriate, in collaboration with the competent organs of the United Nations and with the specialized agencies concerned, standards of safety for protection of health and minimization of danger to life and property (including such standards for labor conditions), and to provide for the application of these standards to its own operations as well as to the operations making use of materials * * * made available by the Agency * * *."

This is a rather all-inclusive statement but one of the given parts undoubtedly refers to matters of protection against the harmful effect of radiation. It is particularly in this area that clarification as to area of responsibility is needed.

It is essential that as far as possible the IAEA make use of existing organizations and facilities in this field, rather than to establish a new radiation protection organization of its own. One reason for this is the fact that there is a critical shortage of individuals in the world who have broad experience in the radiation protection field. This small number, which probably does not exceed some 2 dozen internationally recognized experts, is already called upon repeatedly by different organizations to perform the same or very similar functions, but usually under different organizational units. This is not only wasteful of manpower but it is already overstraining this small group of experts. At the same time such a multiplicity of committee activities may lead to areas of conflict, and certainly leads to uncertainty as to which recommendations carry weight. To illustrate the problem, there are listed below some of the organizations and activities already well established in the field.

The International Commission on Radiological Protection (ICRP) was established in 1928 and since that time has provided the international philosophy and guidance in the broad field of radiation protection. Their recommendations have been followed throughout the world. The International Commission on Radiological Units and Measurements (ICRU) was established in 1925 and its recommendations are also used almost universally. The ICRP and the ICRU are sister commissions, originally sponsored and still operating under the aegis of the International Congresses of Radiology. The commissions usually held their meetings jointly.

The ICRP and the ICRU entered into official relationship with the World Health Organization (WHO) in February 1956, as a nongovernmental participating organization. In this capacity the two commissions are regarded as the scientific bodies for developing the standards of radiation protection and units for WHO. WHO, in turn, will undertake the publication and dissimination of this information throughout the world.

As a result of an offer by UNESCO, an informal relationship exists between the ICRU and UNESCO, in that the latter has provided some financial support to assist in the establishment of radiation standards in various countries. The mechanics of this will be handled through WHO.

In 1956 the United Nations established its Scientific Committee on the Effects of Atomic Radiation. Included in the membership is a substantial number of individuals who are also members of the ICRP and the ICRU.

In September 1956, the U. N. Scientific Committee requested the ICRP and the ICRU jointly to undertake certain studies related to the evaluation of radiation exposure to man, and the U. N. has provided some financial assistance for this study. The program is now underway in about 15 countries and there is a substantial overlap of membership between the various participants in the study, the two international commissions and the U. N. Scientific Committee. At the moment it is not clear to what extent the U. N. Scientific Committee will use the joint commissions as the technical bodies that should be responsible for specific technical tasks, but there is some implication that some official relationship may develop.

The International Labor Organization (ILO) has a Consulting Committee on Radiation Protection. This includes individuals who are also members of the U. N. Scientific Committee, the ICRP and the ICRU. They are developing standards of protection in the industrial field including atomic energy installations. A meeting of this committee is planned for the fall of 1957 for the purpose of reaching agreement on the ILO radiation protection recommendations.

The International Standards Organization (ISO) is a recognized nongovernmental body having official relationships in many countries as, for example, the American Standards Association in the United States. The ISO has recently indicated that they plan to expand their scope of interest to include all phases of atomic industry including radiation protection and have applied to UNESCO for recognition as the standards body primarily responsible in this area.

In a recent communication from the Secretary General of the WHO, it is understood that he is establishing some kind of advisory committee on radiation matters. The relationship between this and the various other bodies mentioned above is unclear.

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The brief outline above typifies the complicated world situation that is developing in the field of radiation-protection standards. Of all of the organizations mentioned above, the only ones that have been in continuous and active existence for any length of time are the ICRP and the ICRU. Every time a new organization is set up, many members are drawn from amongst the members of these two commissions. Fear is expressed that this same situation may develop in the event that the IAEA decides to organize a radiation protection group. It can only draw upon the same limited number of qualified people; there are no others. It would appear to be desirable for the IAEA to take advantage of existing organizations, rather than to establish new ones. The same ends would be accomplished, but they would be accomplished more rapidly without the time and effort involved in establishing a new organization and membership, and with a higher degree of enthusiasm because of the good working relationship already existing between the members of the existing groups. In this way also the IAEA will be dealing with an organization that has been preeminent in the field of radiation protection and units for many years.

POSITION PLAN

In view of the existence of well-established internationally recognized organizations having high competence in the broad field of standards of radiation protection and units, it is recommended that such organizations be utilized by the IAEA as the official technical body for the development of the necessary standards for use by the IAEA. Specific reference is made to the International Commission on Radiological Protection and the International Commission on Radiological Units and Measurement. Since matters of radiation protection and units are largely in the same area of health, it is recommended that problems in this area be referred through the existing channels to the ICRP and the ICRU. At the present time the channel of communication would be via the United Nations; through the World Health Organization, an official specialized agency of the U. N.; to the ICRP and ICRU, a nongovernmental organization having official recognition of the WHO. Once a relationship and program has been established between the IAEA on the one hand, and the ICRP on the other hand, direct technical contact should be permissible with information and reports concurrently being put through the normal channels of communication.

By utilizing the above procedure, the IAEA can take full advantage of established competence in the field of radiation protection and units without the necessity of establishing a new and separate body of its own.

(Mr. Taylor's statement—continued.)

Manpower knowledgeable in the field of radiation protection is very limited and the same people the world over are called upon repeatedly for similar and overlapping activities. This is extremely wasteful of manpower effort and it was a similar situation on a smaller scale in this country that was instrumental in the formation of the NCRP in 1929. To avoid this situation on an international basis the ICRP and ICRU, jointly, are establishing formal working relationships with various interested international organizations while having certain specialized interests; nevertheless by common consent look to the ICRP and ICRU for their basic guides on radiation protection standards. With the establishment of the new International Atomic Energy Agency, it is possible that they will also require assistance in this direction. It is hoped that they will, along with the other agencies, look to the international commissions for this guidance rather than setting up a new and undoubtedly overlapping body of their own.

Representative HOLIFIELD. Our next witness is Dr. H. L. Friedell, School of Medicine, Western Reserve University.

Dr. Friedell, we are happy to have you here this morning. You may proceed.

STATEMENT OF DR. H. L. FRIEDELL, SCHOOL OF MEDICINE, WESTERN RESERVE UNIVERSITY[1]

Dr. FRIEDELL. Thank you, sir.

Mr. Chairman, and members of the joint committee, and ladies and gentlemen, I have the unenviable task of trying to introduce an exceedingly difficult and complex subject. We have gathered information on the whole problem of radiation, and the biological effects of radiation, essentially within the past 20 years, and I think it takes a little while before this matures so we can undertstand it fully. Nevertheless, I think there is a place for orientation here for examining some of the basic concepts of what we do know about radiation, about trying to separate various kinds of effects one from the other, when radiation is administered to a biological system. I would like to briefly introduce this.

Time is limited, but I think the others will very readily fill in any hiatuses that exist. There are many none of us can fill in, I think. They will augment wherever necessary the things I talk about.

Representative HOLIFIELD. While we are trying to keep to the schedule, we are not going to cut any witness short. We may ask for documentation, and if you have something that you feel the committee should know, you may proceed to give it.

Dr. FRIEDELL. I think we will want to take a look at how radiation introduces the biological effect, and then we will try to separate some of the things that occur.

It is interesting that radiation we cannot see, hear, feel, or smell, will initiate very profound effects, the way it appears to do this is by this radiation interacting primarily with the atoms that comprise the biological molecules.

The way they interact with the atoms is, in essence, interaction with their electron shells. Most of the physical changes involve these electron orbits, but there are some others which do occur which are essentially insignificant in the broad overall picture.

Specifically, an ionizing particle or powerful photon, a piece of electromagnetic radiation, will come in and pull away an electron out of the atom. Once it has done this, it has now disturbed the atom, made it into an ion, and this ionized atom and the electron will form an ion pair—or it may move the electron into a different energy level. Then it is excited, and may then concern itself with various chemical and biochemical reactions.

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[1] Professional background: University of Minnesota, M. D., 1936; University of Minnesota, Ph. D., 1939; National Cancer Institute Fellow; Chicago Tumor Institute, 1939–40; Memorial Hospital, New York, 1940–41; University of California, 1941–42; instructor in radiology, University of California, 1941–42; United States Army, lieutenant colonel, 1942–46; executive officer and Deputy Chief, Medical Division, Manhattan District. Present work: Professor of radiology, Western Reserve University; director, department of radiology, University Hospitals of Cleveland; director, Atomic Energy Medical Research project, Western Reserve University. Committee appointments: Veterans' Administration, Central Advisory Committee, Radioisotope Section, Reserve and Education Service; National Research Council, Subcommittee on Radiobiology; Atomic Energy Commission, Advisory Committee on Reactor Safeguards; National Bureau of Standards, Subcommittee on Permissible External Dose; State of Ohio, advisory committee on atomic energy. Member; American College of Radiology, American Medical Association, American Radium Society, Association for Advancement of Science, Association of University Radiologists, American Roentgen Ray Society, Radiological Society of North America, Society of Experimental Biology and Medicine, Radiation Research Society, Sigma Xi, Alpha Omega Alpha. (Submitted by witness.)

When this occurs it is obvious immediately that the molecule that is then so vital and important to the cell has been disrupted or disturbed and many things can happen to this molecule.

It is of interest to observe that a cell has roughly 10 to the 14th molecules, and a thousand roentgens, a dose which generally is lethal, will affect only about 10 to the seventh molecules. In other words, one ten-millionth of these are affected, and yet this single injury to an atom or molecule among these many will introduce violent and very serious biological effects. The physical effects are over generally in a very short period of time. Immediately thereafter the disrupted molecules become involved in various kinds of chemical and biochemical changes. And again these are over in a few microseconds. So that the process of the physical effects and the biochemical effects are finished within a very, very short period of time, and yet we observe the biological effect in hours, days, months, and possibly even years later. This is an important concept to retain and keep in mind.

Representative HOLIFIELD. This statement is based on experiments with animals?

Dr. FRIEDELL. These are based on experiments primarily in vitro; in other words, studying tissues or systems outside of complex animal, because it would be very difficult to observe it in an animal itself. From the point of view of the occurrence of biological effects, these are observed in animals—correct.

Representative HOLIFIELD. And is applicable to man?

Dr. FRIEDELL. And is applicable to man.

Representative VAN ZANDT. Dr. Friedell, at this point, do you have information concerning the animals that were exposed to radiation in the Mariannas in the 1954 tests?

Dr. FRIEDELL. I am aware of it. I am not entirely familiar with it.

Representative VAN ZANDT. In other words, the Mariannas tests are not involved in your presentation?

Dr. FRIEDELL. I would say what I am going to present would be involved in all biological effects of radiation. These are the basic things that occur at the beginning. They really are the initial things, and I want to proceed much further in developing this.

Once we get the injury at the chemical and biochemical level, obviously the first unit that may be injured is the cell, and all organisms are comprised of cells, as we know, and are complex organizations of cells. We, therefore, can perhaps begin, once we take a look at this matter, to look at the cells themselves and see what kind of biological effects occur here.

Before I go on to the cell, I would like to make this point: Undoubtedly many of you are familiar with the effects of protecting cells with various chemical and biochemical agents. The way this has been done, in effect, is to take a look at some of the biochemical changes that might occur in a system, and see if it would be possible to prevent them or counteract them. Specifically, we might think briefly of a system that can be affected easily and studied readily, and that is the disruption of the water molecules.

Water is an abundant material in biological systems—ordinary biological systems. This water molecule will undergo exactly the same changes that any vital complex molecule might undergo in the cell itself, because the ionization makes no distinction between these. As-

suming roughly the same conditions, it will ionize water just as well as it will ionize anything else. And if you ionize the water, tear it apart, you now produce radicals, so to speak, which will either reunite or will be modified in some other way.

If oxygen is present, which is another very important element, and present in the biological system, they may combine with oxygen to make very powerful oxidizing agents.

It is presumed at levels we talk about, up to several thousand roentgens, that this effect, which is considered an indirect effect (in other words, producing ionization and modifications of the atoms that may not be directly involved in the biological systems, such as water), in turn produces serious effects, because they become noxious radicals, so to speak. They become oxidants, highly powerful oxidizing agents, and may, in the presence of very vital atoms or molecules, alter them and, in turn, produce these serious biological effects.

Therefore, if you were going to attempt biochemical repair of this, or chemical repair of this, you would either prevent the oxidation from producing radicals, or you might introduce something that is an oxygen acceptor, a reducing agent so to speak, and therefore either spare the effect on the molecules or in some way interfere with this occurring.

One of the common compounds we know fairly well is cysteine, which has sulfhydral groups. We do not need to go into the chemistry and exact nature of these things, but they will accept oxygen, and if you introduce enough of these into the cell, these will, in effect, either combine with the noxious radicals to start with, or by the statistical process of dilution prevent some of the vital cell molecules from being affected.

So that this is one attack that has been made in altering or in preventing this biochemical change from occurring and, therefore, being seriously damaging to the cell.

I said earlier that oxygen needed to be present in order for a large number of these oxidizing radicals to be produced, and this is another way in which we can protect the cell. You can reduce the amount of oxygen. You can either limit the amount of oxygen physically by putting the organisms in oxygen-free atmosphere, or by making some physiological change so that the oxygen is low in vital areas of the cell. When you do this you also protect the organism.

So that our beginning knowledge about the biochemical effects are extremely important in giving us an understanding how biological effects will occur, and how we might modify them in the biological system.

Representative HOLIFIELD. Does that have any practical effect on radiation sickness?

Dr. FRIEDELL. Unfortunately, its practical effect is rather small, for this reason: These things must be done immediately before the radiation is delivered, or at the time the radiation is delivered. Unfortunately, if it is done after the radiation is delivered, this, of course, is no longer effective because all of these things we are talking about would have occurred already.

Representative HOLIFIELD. So this is an interesting scientific fact, but from the standpoint of protecting the people from radiation it is inapplicable?

Dr. FRIEDELL. Essentially and practically inapplicable, but it is important in understanding the mechanisms that occur.

I think it would be well to then begin to take a look at what happens in the cell itself, and the people after me are going to talk about this, and extend some of the basic concepts further. But I believe it would be useful to look at the cells and see what we know about them from a radiological point of view.

We have for a long time studied the various responses of cells to radiation, and have made up a little chart which tells us something about how sensitive these are to radiation, and how easily affected they are by radiation. It is important to understand this because, if you are going to understand what happens to the whole organism, you must obviously know how dependent the whole organism is on the economy of any single cell and how easily this is affected by radiation.

I would like to read this to you from the statement that will be introduced in the record. I will read a list of cells I have made up and listed as extremely sensitive, highly sensitive to moderately sensitive, and insensitive.

The basic cells of the hematopoietic system—lymphocytes, erythroblasts, myeloblasts—closely associated, are extremely sensitive to radiation, and small doses will injure these cells severely.

In the same category, I would include the germinal cells of ovary and the germinal cells of testis. As far as our purposes, I would consider these as highly sensitive, and very readily and quickly affected by radiation.

Mr. RAMEY. When you use the word "lymphocyte" what would be the common name for that?

Dr. FRIEDELLL. I would guess that you call these the germinal cells in lymphatic tissues, such as lymph nodes and other tissues that are related to lymph nodes. These also possibly have their origin in the hemotopoietic tissue as well. In other words, the blood-forming organs as well. Perhaps that is what you were referring to.

The next group, which is a little less sensitive—and I would consider these as moderately sensitive to possibly highly sensitive—would be the epithelium of intestinal crypts lining the insides of the intestines, and certain basal layers that originate in the epidermis.

These basal layers of the epidermis and the epithelium of the intestinal crypts, I would say, would be less sensitive, but nevertheless easily affected by radiation.

Now, there are a group of cells which seem to be unaffected except by extremely large doses. I would like to say that all cells can be affected by radiation; if you introduce enough energy, transfer enough energy to the vital systems of the cell, you can destroy them all. But some of the cells require very large doses. Generally the way we look at this is that cells that are highly active and rapidly dividing seem to be affected by radiation more easily than those that are slower growing and more highly differentiated in the sense they are more highly specialized.

These latter seem to be affected by radiation less. I would include in these things like muscles, bone cells proper, liver cells, brain cells, nerve cells, kidney cells. And ordinarily, when lethal doses of radiation are given to the organisms, we will find that these cells are essentially unaffected. You can find no important change in the cells proper.

Now, if you begin to accept this—and it is somewhat difficult to digest without studying it a little bit—you can then begin to understand what happens to organisms as a whole when the organism receives large doses of radiation.

First of all, we can see that certain tissues are going to be promptly injured, and these tissues are going to be the blood-forming cells, such as the leukocytes, and the gastrointestinal cells. Most of the others will be unaffected.

If the organism is vitally dependent on the cells, it will be fatally injured. If it is not vitally dependent upon these cells, there may be modifications, but the organisms proper may not be injured. Therefore, we can begin to understand how we can injure certain cells and yet not affect the organisms seriously.

For example, you can give a fair dose of radiation, which might kill the organisms, to the liver cells alone, and yet the organisms will not die. You can give this kind of radiation to the muscle cells, for example, and the organisms will not die. On the other hand, if you deliver this radiation to the hemotopoietic system, the blood-forming tissue, the organism will die because these blood-forming organs are very vital to the cell.

One of the important things involved is defense against infection. That is, the white cells of the blood-forming organs are very important against infection, and, therefore, reducing the cells would seriously affect the organism and various kinds of infections would rapidly take over.

Representative HOLIFIELD. There is an old saying that a chain is only as strong as its weakest link.

Dr. FRIEDELL. Correct.

Representative HOLIFIELD. When we are talking about the effects of radiation on the human body, and the life span, we must of necessity address our remarks principally to the weakest link in evaluation of the radiation.

Dr. FRIEDELL. Right.

Representative HOLIFIELD. It is of small comfort to know that one section of the body is not so badly affected by radiation, if in the meantime another section of the body which is vital to existence has been destroyed.

I am not saying we should not know this, but I am saying the important thing is to evaluate its effect upon that weakest link in the life cell, the reproductive chain.

Dr. FRIEDELL. This is very true, and when we speak of total body radiation, in other words, when we irradiate the whole organism, then obviously we have to examine the weakest link, and the weakest link would be the hemotopoietic system, and the gastrointestinal tract.

However, when you deal with radio elements, they have certain preferential deposition, so to speak, and therefore, in order to orient yourself, you must understand that certain radio elements that may be administered to an individual will deposit themselves preferentially in one area and, therefore, will essentially have no effect on the gross economy of the individual.

One of the examples I can cite to you is the use of modest doses of radioiodine.

In the adult, the thyroid is a relative insensitive organ, and you can deliver doses to the thyroid in the order of 500 roentgens which

will to all intents and purposes produce no demonstrable effect. On the other hand if you gave 500 roentgens to the total body, or to a very vital structure, you would injury the animal perhaps fatally. This is the reason I introduce this.

Representative HOLIFIELD. By the same token, radio isotopes such as strontium 90, which have been deposited directly into the bone structure and goes right on shooting the powerful rays into the cells around it, would be more damaging than the comparable amount of radiation that was external to the body, would it not?

Dr. FRIEDELL. That is essentially correct. Of course, now we come to one point which is included on our outline—How do we make a decision as to whether certain radio elements are likely to be injurious, and how do we separate radiation coming from radioactive elements or radiation coming from cosmic rays or X-ray machines?

I would like to say this: That all particles or photons (electromagnetic radiation) which are energetic enough to produce ionization will produce the same kind of biological effects, roughly. There are modest differences, but in essence they would produce the same kind of biological effects.

How do we compare radio strontium, for example, with X-rays, or one radio element to another. Let's look at that first.

First of all, the half life of the element is very important. Will it last? Will it radiate a long period of time?—because this is going to determine what the dose is.

Another very important item is how energetic is this particle, and what is the range of this particle. This is tied in with its energy. So we have to know whether it is long lived, what kind of particle it produces, how energetic it is, what is its deposition in the body, will it deposit in vital areas or will it not deposit in vital areas.

These are the kinds of things we have to look at and examine in making any decision about whether a radio element will be serious or not.

Now, strontium 90 happens to fit some of these categories because it is a very long-lived material, and it deposits itself in areas which are vital to the economy of the organism.

Representative HOLIFIELD. Would it be inclined to deposit itself in concentrated areas in the bone, or diffuse through the bone structure?

Dr. FRIEDELL. It appears that strontium 90 is chemically very much like calcium. Therefore, as a good first approximation, we would assume, and I think reasonably conclude, that it distributes itself as calcium does in the bone, which is widely throughout the bone.

Representative HOLIFIELD. But in the case of a broken bone, for instance, that was being repaired, the tendency would be for it to concentrate during the repairing——

Dr. FRIEDELL. During the process of healing, we know there is more calcium deposited at the site of fracture, and, therefore, more strontium 90 would be deposited at the site of fracture.

Representative HOLIFIELD. We hear of bone cancer. Does that take place as a result of bombardment of strontium 90? Does that take place throughout the bone, or is it localized in certain areas of the bone, in the marrow, for instance?

Dr. FRIEDELL. Strontium 90, after you once introduce strontium 90 or, for that matter, almost any element that will seek the bone—and

we have gotten to use the term "bone seeker"—this will distribute itself more or less throughout the bones. Some have special depositions, but it is also the long continued radiation which does the damage. Therefore, it is a question of dose. There is evidence that no matter what radio element you use, if it is a bone seeker, and if it will radiate long enough to give a high enough dose, you will produce bone cancers—at high enough levels. That is what I would like to emphasize.

Senator HICKENLOOPER. Mr. Chairman?

Representative HOLIFIELD. Senator Hickenlooper.

Senator HICKENLOOPER. Doctor, is bone cancer a new thing?

Dr. FRIEDELL. Is it a good thing?

Senator HICKENLOOPER. A new thing.

Dr. FRIEDELL. No, sir.

Senator HICKENLOOPER. Is it something recently discovered?

Dr. FRIEDELL. No, sir.

Senator HICKENLOOPER. Have we not had bone cancer——

Dr. FRIEDELL. Bone cancer has been known almost since time immemorial.

Senator HICKENLOOPER. As long as we have had real medical knowledge?

Dr. FRIEDELL. I think so.

Senator HICKENLOOPER. Bone cancer occurred before we ever had any atomic tests or explosions, did it not?

Dr. FRIEDELL. Yes, it did.

Senator HICKENLOOPER. What would have caused bone cancer many years ago? Is that the absorption of certain nuclear particles, or does it come from some unknown activity of the cells as a starter?

Dr. FRIEDELL. I personally would hesitate to attribute this to the absorption of previous radiation or previous nuclear particles before we began the fallout tests. I think that on the whole this is related to some special biological factor that is yet unknown, and I hope we will hear a little later from one of the other witnesses about some of these special things that might contribute to the production of cancer in general.

Senator HICKENLOOPER. Yes. I mean we have heard a great deal about bone cancer since there has been some radiation released through bomb explosions, but I just wanted to at least assure myself that my belief was right that we had had bone cancer from time immemorial.

Dr. FRIEDELL. Yes, that is true. I will be glad to insert in the record the assertion that bone cancer has been present long before the tests began.

Representative HOLIFIELD. Our concern with strontium 90, though, is that it is an artificial element that is created by thermonuclear explosions and atomic explosions, and it is now a new factor, an additive factor, and experiments have proven that this new element which has been introduced is a cause of bone cancer. That is our concern, is it not?

Dr. FRIEDELL. This is true. But I think there is one very important point we have to look at very hard. That is, what are the levels of radiation? And what evidence do we have that these levels of radiation have produced bone cancer? And what are the bases for

assertions by some that bone cancers will be produced at very low levels in a small percentage of people?

Perhaps later, if I do not forget—I would be glad to be reminded of this—I would offer my humble opinion of this, because I have been looking at this as a radiologist for a number of years, and I am interested in this whole problem.

Representative HOLIFIELD. Why do you not discuss it now? We are on the question now.

Senator BRICKER. May I ask one question before he goes into that?

Representative HOLIFIELD. Yes.

Senator BRICKER. We know that radiation has a tendency to prevent the development of cancer in certain organs?

Dr. FRIEDELL. Yes, sir.

Senator BRICKER. And it is used for that purpose. Would there be any beneficial radiation that might come from strontium 90?

Dr. FRIEDELL. I would say that no radiation is for preventive purposes. I think radiation is used for curative purposes.

Senator BRICKER. For curative, palliative purposes.

Dr. FRIEDELL. Yes, sir.

Senator BRICKER. Would there be any of that effect come from ingested strontium 90?

Dr. FRIEDELL. I could see no benefit that might arise from deposition of radioactive elements.

Senator BRICKER. I have never heard it intimated, but I do know that radiation has been used in the cure of cancer, to help palliate the pain and prevent the growth.

Dr. FRIEDELL. Yes, sir. But in normal tissues I would be opposed, as a matter of fact, to the introduction of radioactive elements as a possible preventive measure.

Senator BRICKER. Of course, we all would. I would not want to take a chance. I wondered if there was any thinking along this line.

Dr. FRIEDELL. No, sir; I do not know of any.

Senator BRICKER. You have not heard it suggested.

Senator HICKENLOOPER. Mr. Chairman, along that line, I would ask one other question, if I may. That is along the line Senator Bricker is discussing.

Could there be any beneficial effect possibly flowing from the introduction of some of these radioactive elements, so far as a cancer that was in the process of formation, or growth within the system which came from other than causes which might have resulted from radiation?

Mr. FRIEDELL. I would say "No."

First of all, the levels of radiation—and again I want to emphasize this: We are talking about entirely different levels. To give you some idea of what the levels are to be curative in the case of cancer (incidentally bone cancer is an extremely resistant form of cancer, and radiation even in large doses is essentially ineffective), the doses that are necessary to cure cancer are in the order of five to ten thousand roentgens. The doses we are talking about, especially from the fallout levels, are in the thousandths of roentgens. So that we are not talking about the same order of magnitude at all.

Senator ANDERSON. I did not follow you on that.

Senator HICKENLOOPER. I think we have done quite a little experimental work in the radioactive iodine in thyroid, and cancer.

Dr. FRIEDELL. Yes, this is true.

Senator HICKENLOOPER. And at least some other attempted specifics along that line.

Dr. FRIEDELL. Yes, sir. In the case of radioiodine, certain cancers of the thyroid are very beneficially affected.

Representative HOLIFIELD. Mr. Van Zandt.

Senator ANDERSON. I want to clear up one thing first.

When you said from five to ten thousand roentgens, then you said the levels we are using here are "thousands" of roentgens?

Dr. FRIEDELL. "Thousandths." Decimal point zero zero one (0.001).

Senator ANDERSON. That is what I wanted. It was not very clear.

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. Dr. Friedell, in the event of a fracture with the presence of strontium 90, would the strontium 90 in any way slow down the mending of the bone?

Dr. FRIEDELL. I hesitate to answer that, because I have no specific information. But if I may conjecture, I would say it would be slowed down at very high levels of radiation, far above anything we have considered here. And I do not believe you could establish any difference in the growth rate at the kind of levels that are being talked about from fallout.

Representative VAN ZANDT. Then you cannot state whether a low dose of radiation would have any effect on the mending of the bones?

Dr. FRIEDELL. I would hesitate to propose that. I doubt it.

Representative HOLIFIELD. We recognize, Doctor, you are providing us the background statement, and others will go into these different facets.

Dr. FRIEDELL. Very well.

Representative HOLIFIELD. Will you proceed?

Dr. FRIEDELL. With regard to understanding what happens to the whole organism concerning the radiation syndrome, I think we have to look at what happens to the individual from the point of view of the systems that were injured. We try to point out a very cursory relationship between tissue sensitivity, the kind of effects that would produce, cellular effects where the economy of the organs was dependent upon these; and then we can look at some of the systems and pathological findings that might occur.

Since we know the gastrointestinal tract, and the hemotopoietic system are very sensitive to radiation, we can observe, with fairly large doses of radiation, symptoms and pathological effects that are directly related to these. The hematopoietic effect, of course, will appear as a severe drop in the white cells.

I will not go into the kinds of white cells. There are many more competent in this field than I, but this is generally true.

Some of the basic cells in the hematopoietic system are affected, which in turn affects the production of the platelets, which are tissue components in the blood required for the proper function of the clotting mechanism. These are seriously depleted, and under such circumstances you will get all kinds of bleeding tendencies. An individual who is heavily irradiated will show symptoms associated with the gastrointestinal tract, and with the hematopoietic system more or less simultaneously. In the doses that are high, the patient will become nauseated and vomit because of the immediate effects on the gastrointestinal tract, possibly also because some of the vital large

molecules are disrupted, so to speak, by ionization, which we point out can split some of these things up, and it may be these are circulating about and produce some of these effects.

So that an animal that is immediately irradiated in a very few hours may show nausea, vomiting, anorexia, severe diarrhea. This is directly related to what we can observe in the cells themselves, and in the tissue systems. There will be severe hematopoietic changes (the blood changes).

The organism has now lost its defense against infection, and infections will take over very promptly, and we can begin to observe in obvious areas the oropharynx, respiratory, and gastrointestinal tract, ulcerations and infection as a result of this injury to the tissue.

There will be little bleeding points throughout, as a result of interference with platelet formation. If they go on, the animal will be severely injured, and will die, partly as a result of these intercurrent effects, but also because we are unable to replenish some of the vital cells, or the body itself cannot replenish any of the vital cells.

This brings me to a point which we discuss not infrequently——

Representative HOLIFIELD. You are talking of large doses now?

Dr. FRIEDELL. I am talking of large doses in the order of 500 to 1,000 roentgens delivered to the individual.

This brings us to a point of how we might possibly protect the organism against radiation effects.

If we look at some of the very vital cells, it is reasonable to conclude that if it were possible to get these cells to be repopulated, possibly from an outside source, then the animal might be able to recover if the doses have not been really too large.

The recent efforts in this direction have been to get bone marrow cells introduced into the organism that has been heavily irradiated to see whether these cannot repopulate the hematopoietic system, at least until the cells themselves may have had an opportunity to recover.

Representative HOLIFIELD. This would indicate, from a practical standpoint, that you would have to have a bank of bone marrow cells for introduction into the system.

Dr. FRIEDELL. That is correct.

Representative HOLIFIELD. The same as you have to have a blood bank for transfusions?

Dr. FRIEDELL. This introduces many practical problems, and I am not sure it will have any place at all in attacking this problem.

Representative HOLIFIELD. I think it is important to bring this to the point of practical application, because a great many lay readers might think this could be a remedial measure which could be taken in a practical way. Of course, even transfusions would not be of any permanent lasting good if the spleen was affected.

Dr. FRIEDELL. Yes.

Representative HOLIFIELD. Or other blood producing organs.

Dr. FRIEDELL. Right. Essentially, if blood producing organs are seriously affected, it is doubtful if the transfusions have anything other than a transient effect. Generally, in doses of about 500 roentgens, which is presumed to kill, roughly, about half of the humans that may be affected by such a dose, supportive measures might be helpful, such as transfusions, replacing the fluid that is lost as a result of gastrointestinal injury. The use of antibiotics would be very effective be-

cause they would help to combat the infections occurring while the defenses were down.

Representative HOLIFIELD. From a remedial standpoint, this would be more valuable to those who had not received a lethal dose. To people, say, who received 100 or 200 roentgens, these measures would be of some value?

Dr. FRIEDELL. These measures may be valuable even at high doses, because it is possible—if 50 percent survive, say at 500 roentgens, it might be possible to push that up a little further, 60 or 70 percent. This is conjecture. We do not know. This is very important. When we get too high doses, over 1,500 roentgens, it seems that no measures are effective and we are unable to use any of these in any useful way.

Representative HOLIFIELD. Of course, from a practical standpoint, in an exposure of our people, it would be completely beyond the resources of the medical world to give this remedial treatment, would it not?

Dr. FRIEDELL. I think this could be true. But, if we are examining the whole problem, I think we would be overwhelmed by other things that would occur at the same time, and this would be essentially a small problem. There would be many, many more severe and difficult problems.

I have devoted my remarks primarily to the acute effects up to the present time, and we have talked about how we can assess these changes in the whole organism more or less immediately, and in fairly large doses.

It is well also to consider what would happen if the doses are lower, and if the animal survives. Is the animal completely unscathed if radiation has been delivered in smaller doses when comparatively few, or perhaps none have been killed?

Here I think we get into the problems that are very difficult to answer, and very difficult to prove effectively at the present time. This is an area where a great deal of study and research is required.

I would like to divide these, roughly, into three areas:

1. What is the effect on the vitality of the organism?
2. What is the effect on the production of malignant tumors?
3. What is the possible effect on future generations, the genetic effect?

The last I will speak very briefly upon, because many better speakers than I am will discuss it further. But I would like to say something about these points.

First of all, there is evidence indicated in animals with high doses—and by "high doses" I mean accumulation of many hundreds and even thousands of roentgens—that you can produce leukemia in susceptible strains.

I would like to point out that to produce leukemia a susceptible strain of mice must be used—that is, these mice must be such that they are genetically able to produce leukemia spontaneously. If the mice are not a susceptible strain—that is are not leukemia bearing—then the production of leukemia in such a strain is extremely difficult if not impossible. Thus one element that is essential is that the animal must have had inherent tendency to produce leukemia in the first place.

Secondly, tumors have been amply produced in animals with large doses of radiation, and tumors of all kinds. Whether it is strontium

90, phosphorus 32, or total body radiation, or radium, wherever you produce large doses and selective deposition in sensitive areas, you can produce tumors of all sorts. This is unquestioned.

Senator HICKENLOOPER. Benign, or other kinds of tumors?

Dr. FRIEDELL. Let us for the moment consider only the malignant tumors, tumors that will destroy the animal and fit all the criteria that people insist upon being characteristic of malignant, that is, they will spread to other tissues, and generally have the appearance of cancer. I think here is where we get into a problem.

If it is clear that there is evidence that tumors can be produced, and leukemia can be produced in various kinds of organisms under various conditions, it would be well to see if we could quantitate this. In other words, are there twice as many tumors produced when the dose is twice as high?

In general, this appears to be not well controlled, but there appear to be more tumors produced when the doses are higher. Under these circumstances, you can set yourselves up a little model or framework in which you show that the dose is related to the production of tumors, and the number of tumors.

Senator ANDERSON. Can I ask you there what you mean by "when the dose is high"? Can you give us the level again?

Dr. FRIEDELL. Yes. Generally, when we think of high doses, we think of doses in the lethal range, and perhaps I have been a little bit loose in this regard.

If you take animals that have been exposed to a lethal dose, 50 percent dose, that is, a dose in which 50 percent of the animals will succumb, and keep the survivors, the amount of radiation will be very high.

Senator ANDERSON. What I am trying to get to is this: We were talking previously about 5,000 to 10,000 roentgens.

Dr. FRIEDELL. Yes, sir.

Senator ANDERSON. Whereas, from fallout we are talking in thousandths, tiny fractions.

Dr. FRIEDELL. Yes, sir.

Senator ANDERSON. Now the things you are discussing, are they connected with fallout from nuclear weapons in any way, or an accumulation?

Dr. FRIEDELL. It is what we may be discussing, sir, and I would like to amplify this a little bit to show how this concept is approached. I will talk about those very low levels in just a moment.

Senator ANDERSON. All right.

Dr. FRIEDELL. In effect, what I am saying is large doses produce tumors and leukemia, and by "large doses," I am talking about thousands of roentgens, many hundreds of roentgens.

If you set yourself up a model in which you show that these doses will produce tumors and leukemia, and then extrapolate down to low levels, especially on the basis of how the data looked at high levels, you can begin to conjecture that perhaps these lower levels could in a very small percentage of patients or individuals produce these kinds of tumors.

Now, I think what we need to look at, and what this group is going to look at in the next couple of days, is how good are these extrapolations—Is this conjecture? Is this soundly conceived?

I wish I could offer an authoritative statement right now to end all of this discussion, but unfortunately I cannot. However, I would like to say this: That I am concerned about the fact that there are no data at the very low levels. It is just nonexistent. Much below a hundred roentgens, or 25 roentgens in the case of mutations, we have no data.

Representative HOLIFIELD. You are speaking of man?

Dr. FRIEDELL. In animals as well. I am speaking of all complex biological systems.

Representative HOLIFIELD. Have not you been able through following mice, for instance, through several generations, to establish any data of this type?

Dr. FRIEDELL. Yes, but these have been in large doses. These have not been in hundredths, or tenths of roentgens, they have been in doses far larger.

One of the reasons we are using large doses is that you have to have some kind of statistical security in looking at the information. To discover an effect which would occur once in 10,000 times, you would require an inordinate number of biological specimens, and so on.

But I would like to point out that this difficulty exists, and for this reason we do not have really secure data.

Now the people who propose that the doses at very low levels can produce effects have pointed out the data at higher doses are such that permit them to make these extrapolations, and there are many ways of looking at this. You can do it mathematically, you can do it by examing the mechanism by which these effects are produced, and in this way kind of develop some hypotheses which will permit you to make some conclusions.

I feel that the data at the very low levels are based on this kind of hypothesizing, and therefore, correctly are not available at the present time, and perhaps will not be available for a long, long time because of the difficulty.

We should, therefore, be slow in accepting these if we need to use it for a vital decision.

I think at the present time these data are not good enough to make very extreme or vital decisions in this regard. I think all of us should look at this to see what is the truth of the matter and what scientific evidence we can find which will permit us to make these conclusions.

Senator ANDERSON. May I try to translate that to myself and see if I got it correctly?

Dr. FRIEDELL. Yes, sir.

Senator ANDERSON. Do you tell us the data are not now good enough for the Congress, for example, to reach a decision on whether continuation of tests at the present level is wise or unwise?

Dr. FRIEDELL. I would say that, sir. I do not believe the data at the present time are good enough to make conclusive decisions.

Senator ANDERSON. If it is not good enough for the Congress, it is not good enough for the Atomic Energy Commission, either, then, is it?

Dr. FRIEDELL. Let me revise that statement.

Senator ANDERSON. That is the trouble. If it is not good enough for the Congress to reach a decision, it does seem to be good enough for the Atomic Energy Commission to reach a decision. They can sit in their ivory tower and say, "This is all right," but to get back to

the Congress which is having to deal with human beings, the data are not good enough.

Dr. FRIEDELL. I would say the data are not such as to suggest any vital or important decisions which would alter the course being pursued at this time.

First of all, they are not good enough to be conclusive, and there are other reasons I will go into further, which would make me have reservations on what they mean in general.

One of these is, when talking about these doses we are talking about the levels which fit into the dose levels we are receiving right now. If you are interested in numbers, each one of us are receiving or having about 3,000 to 5,000 ionizing events per cubic centimeter per second. Now it is 10,000, now it is 15,000, something of that order. So there are a lot of ionizing events going on now. We are living in a sea of radiation rising from various things, and this will be discussed, I am sure, or has already been discussed.

Senator ANDERSON. I think that is a very useful statement, and I appreciate it. I am only trying to say, if it is difficult for the Congress to get any satisfactory or conclusive answer from the existing data, that it must be equally disturbing, I would think, to the Atomic Energy Commission if they want to take a fair look at it. That is my only point.

Dr. FRIEDELL. I would think—if I were going to conjecture again, on how they are looking at it. I think they are disturbed by this, and I think their examination of the data would suggest to them there is no reason to stop these tests because of the levels of radiation. The levels are apparently at levels which are far below levels which we have established as being the acceptable doses, and are quite within the range of radiation occurring at the present time all around.

Senator BRICKER. Mr. Chairman?

Representative HOLIFIELD. Senator Bricker.

Senator BRICKER. Is there any thinking along the line that, if there were no background ionizing radiation at all, the human body would be devoid of cancer?

Dr. FRIEDELL. I do not have any opinion about this, sir. But again I will conjecture that I think the cause for malignant disease lies in some biological derangement that is really not related——

Senator BRICKER. To radiation?

Dr. FRIEDELL. Alone.

Senator BRICKER. But ionization of the cells?

Dr. FRIEDELL. Right.

Representative HOLIFIELD. You used the word "alone"; it is not related alone to that point. You think there may be other causes? I was afraid that word was missed by the audience. I think it is important.

Dr. FRIEDELL. I think at the proper levels, high enough levels, these effects can be produced. At the very low levels where the levels begin to approach the natural levels we are facing, I think there is grave uncertainty. This, of course, is concerned with the whole concept of whether the effects will be occurring at low levels in the same rate that they are occurring at high levels, and whether there is such a thing as threshhold. In other words, is there some level below which nothing will happen?

Again, this is very difficult to establish. The evidence, as I see it, is inconclusive in this direction, and if I had to choose, if I had to make a decision now, if I were compelled to make a decision, I would hesitate to accept this concept that a threshhold does not exist.

Senator BRICKER. That is the reason I asked the question, frankly. It is your thinking, then, that there is a biological cause of these abnormal growths in the human body?

Dr. FRIEDELL. I do, sir.

Senator BRICKER. Above and beyond and separate from the radiation?

Dr. FRIEDELL. Yes; I do.

Representative HOLIFIELD. Will you state your observation in an affirmative way rather than a negative way? And then tell me if you apply that equally to somatic as well as reproductive cells.

Dr. FRIEDELL. I sort of left out the reproductive aspect of this.

Representative HOLIFIELD. That is just what I thought maybe you left out. That is why I wanted you to restate it.

Dr. FRIEDELL. I would say, from the point of view of production of tumors, and leukemias, I would hesitate to accept the concept that a threshold does not exist. From a point of view of genetics—now I am in a field where I am even less familiar—I think the data are not unassailable, but I think they are stronger than they are in the concept of cancers or leukemias.

Again I would like to point out the data on mutations and genetic effects do not exist below 25 roentgens.

The basis for making these decisions is careful study of the data, by protracting the radiation, by fractionating it, by observing the effect of dose, and this gives them a line which can be extrapolated down below. I have no objection to these extrapolations, and ever since Descartes introduced the coordinate system, this is a privilege of all. I do not really understand whether these things necessarily follow this rule. I would think I would want a much better and much more carefully controlled examination of the effect at very low levels.

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. Dr. Friedell, to be conclusive, would you go into a little more detail as to what must be required?

Dr. FRIEDELL. What must be required?

Representative VAN ZANDT. Yes.

Dr. FRIEDELL. As far as our studies go?

Representative VAN ZANDT. Yes.

Dr. FRIEDELL. I think probably the most important thing is to look at the basic aspects of what occurs in biological systems, so that we can understand the mechanism, so that we can see whether once we understand this mechanism it fits in with the data which we already have. And here I feel is where the greatest possibility for really learning something about it exists. I would like to see this emphasized over and above the efforts to perhaps use 10 million mice at very low levels. I would think that basic studies of biochemical effects, the possible way in which these things occur, would contribute more than doing such statistical studies——

Representative VAN ZANDT. Would you apply a time factor?

Dr. FRIEDELL. I would hesitate to apply a time factor, but since I am making all sorts of conjectures, I will add one here.

I will say that perhaps in 5 to 10 years we would have a much better understanding of this.

Representative HOLIFIELD. Of course, if your understanding at that time had to be revised downward as the chart this morning has been revised downward, we would be dealing then with an accumulation of substance which would be ineradicable, and we would have it; would we not?

Dr. FRIEDELL. Yes.

On the last page of my little statement, I tried to put these things together. I think two problems exist.

First of all, I think there is a problem of examining the data scientifically to know where the truth lies.

Assuming the correct consequences of this, assuming no threshold, and all radiation is injurious and produces some effect, I think we have to fairly assess this kind of hazard compared with the hazard which now exists. I do not feel we have yet really looked at this in an unbiased and nonemotional manner. I think it can be done, especially if we look at it over a long period of time so we do not rush into any important decisions at this time.

Senator BRICKER. You have discussed the control of abnormal growths, the cause of them, the somatic effects in a limited way. What have you to say about the length of life?

Dr. FRIEDELL. Here again I do not have any good, well-founded opinion. The data that are available indicate that for large doses in animals, there is a decreasing survival due to all the causes that would occur ordinarily in these animals. In other words, they die of various things, only these various causes of death appear a little earlier in heavily irradiated animals.

Again the same problem exists. Can you extrapolate down below?

This figure we heard earlier that somebody will have suffered a loss of 20 days in survival. It seems to me there can be no data at this level, because this would require an inordinate amount of animals at very low levels to establish this, and I just do not have that kind of sureness about studies in which you observe one event in hundreds of thousands of others.

From the point of view of the span of life, I feel for projections to low levels this falls in exactly the same kind of category. We cannot determine what is happening at very low levels.

I think I can understand the reasons and conjectures and hypotheses of people who propose that this occurs, but they make me uneasy, and I am loath and not ready to fully accept them. I think they are not incontrovertible.

From the point of data on humans, there is some published evidence to show a radiologist may, by the nature of his activities, have received more radiation than others. I am a radiologist myself. I turned some data recently published over to the statistician, and he wrote me a letter saying that these data were suggestive, but by no means conclusive. And the way in which you sample the various groups makes a tremendous amount of difference, and even though averages of the compared group, for example, might be the same, the distribu-

tion could make a tremendous difference. I know this has been touched upon by others who feel the same way.

Representative HOLIFIELD. We found that averages are a little bit unreliable to rely on in some instances.

Dr. FRIEDELL. Yes.

Representative HOLIFIELD. Thank you very much. Are you planning to stay the rest of the day? We might have you on in the discussion late this afternoon.

Dr. FRIEDELL. Yes, sir.

Representative HOLIFIELD. Thank you, sir. Your prepared statement will be placed in the record at this point.

(The prepared statement referred to follows:)

MATERIAL PRESENTED BEFORE THE JOINT COMMITTEE ON ATOMIC ENERGY BY H. L. FRIEDELL, M. D.

The biological effects that are observed when tissues are irradiated must begin as a result of the physical interaction of ionizing radiation and the atoms that comprise the biological specimen.

This interaction appears primarily as ionization—that is, ejection of an electron from the orbit by excitation, in which the energy level of the electron without ejection probably also plays a part.

The excited and ionized atoms and molecules then appear to interact in various ways, eventually producing profound chemical and biochemical change. The immediate physical and chemical changes are probably over in fractions of a microsecond, or at most a few microseconds. The biological effects may not appear for hours, days, or months.

One interesting aspect of this energy absorption is that only a small absorption of energy produces such widespread biological effects. One thousand roentgens, a lethal dose, involves only a very small fraction of a calory per gram (2×10^{-3} calories per gram). Another way to look at this is that the energy which is absorbed appears to affect directly only about 10^{7} molecules in a cell which generally contains 10^{14} molecules.

In outline form, we need to think of the chain of events as (1) physical interaction, (2) chemical and biochemical changes, (3) cellular changes, (4) going on to tissue and organ system alteration, and, finally (5) injury to the whole organism.

The chemical and biochemical effects which occur are at the present time somewhat obscure and receiving much study. One of these effects that has been of interest and which appears to be tied up with some of the observable biological changes are the indirect effects resulting from the disruption of the water molecule abundantly present in living tissue. In the presence of oxygen, this results in producing highly active water radicals which in turn attack vital molecules in the cell since they are very active oxidants.

It has been found that, by depriving the cell of oxygen during the radiation period, these effects can be markedly minimized. By introducing chemicals which are in themselves oxygen acceptors, the oxidation effect on sensitive tissue systems may be spared and the radiation injury is markedly minimized.

At the present time, the best working concept is that the indirect effects are very important at the levels of radiation with which we are concerned (500 to 1,000 r.), that efforts to correct or prevent the chemical and biochemical disturbances as a result of disruption of the water molecules protects biological systems in an effective manner. It should be pointed out that this must be done during the radiation and is completely ineffective after the radiation has been delivered.

The cellular effects have been quite thoroughly studied. On the whole, the nucleus is known to be more sensitive than the cytoplasm. Cells appear to be affected primarily with respect to their function of division and recent studies have, therefore, been directed at this aspect. From the biochemical point of view, the nucleic acid metabolism, and particularly DNA in the nucleus, has received considerable attention.

From a general point of view, it is best to look at the cellular changes and try to understand the difference between cells and their place in the economy

of the whole organism. At one end we have extremely sensitive cells. These may be listed as follows:

(a) Extremely sensitive: Lymphocytes, erythroblasts, germinal epithelium of testis, myeloblasts, germinal cells of ovary.

(b) Highly sensitive to moderately sensitive: Epithelium of intestinal crypts, basal layers of the skin.

(c) Insensitive: Connective tissue, bone, liver, pancreas, kidney, nerve, brain, muscle.

An estimate of the variation in sensitivity permits us to understand better the effects on tissue and on the whole organism. The effect on the whole organism is obviously determined by how dependent the organism is upon extremely radiosensitive tissues. Since the hematopoietic system is one of the extremely important tissues upon which the organism vitally depends, it can be explained that irradiated animals can be readily injured by comparatively modest doses. The animals suffer infections and will die a hematopoietic death if some measure for correction is not instituted. The epithelium of the gastrointestinal tract is less sensitive but nevertheless readily affected by large doses of radiation. At the lower dose levels there is rapid recovery. At the higher dose levels recovery is markedly impaired and the animal may succumb to what is known as a gastrointestinal death, sometimes even before the hematopoietic changes can manifest themselves.

Many tissues are quite unaffected by radiation at levels which would cause death of the whole organism. Therefore, under certain circumstances, particularly when certain radio elements are used, considerable radiation may be delivered without seriously affecting the organism as a whole since the radiation is confined to a comparatively insensitive structure. Also, radiation delivered to sensitive tissues which may not be vital to the organism proper will have comparatively little effect on the individual. As an example, radiation delivered to the thyroid, which in older individuals is comparatively insensitive to radiation, will not produce any appreciable effect on the whole organism. Also, radiation delivered in modest doses to the gonads may produce sterility but will otherwise appear to have no demonstrable effect on the individual proper.

It would be well to point out that the manner in which radiation is delivered is highly important in considering the possible biological effects (excepting genetic changes which will be discussed briefly later). Protraction and fractionation of the radiation markedly reduces the total somatic biological effect. Radiation delivered to specific parts of the body markedly alters the response so that shielding of part of the body increases the dose necessary for lethal effects.

Generally, radiation delivered over a long period of time gives some of the tissues an opportunity to recover (a process which is poorly understood) and, therefore, increases survival.

Specifically, it is well to point out that species sensitivity varies among mammals. Following is a list which gives some concept of the range that may exist:

LD_{50} dose:	*Roentgens*	LD_{50} dose:	*Roentgens*
Guinea pigs	200	Rats	700
Pigs	300	Hamsters	750
Dogs	350	Rabbits	800
Mice	450	Bacteria	100,000
Monkeys	500	Viruses	1,000,000

Man is estimated to fall somewhere halfway through this range of mammals and the LD_{50} dose (that is, the dose necessary to kill 50 percent of the individuals) is presumed to be about 500 roentgens.

As a result of whole-body radiation, certain specific tissues effects are produced. These in turn determine the clinical syndrome. Briefly, the effects which first appear are nausea and vomiting, which can be explained on the injury to the gastrointestinal tract. Prostration, diarrhea, and anorexia may promptly occur with larger doses—again the result of interference with gastroitestinal function and dehydration. The blood forming tissues are simultaneously affected, but evidence of their severe depression is slightly delayed. There is marked depletion of the white cells—later the red cells. The elements involved in clotting are seriously affected and hemorrhages as a result of this derangement soon appear. The individual is susceptible to infection for two reasons—one, depletion of the white cells, and secondly, by impairment of the ability to form antibodies. As a result of this susceptibility to infection, the

oropharynx, respiratory and gastrointestinal tract are prone to ulceration and infection. The central nervous system is essentially not affected.

The neuromuscular system and the specific function of the liver and kidney appear not affected at lethal doses, fitting in with our general concept of radiation sensitivity of tissues. Epilation occurs as the dose approaches the LD_{50} range, since the basal cells of the skin and their derivatives are quite sensitive.

Of concern also are effects which do not appear immediately as the result of radiation but are either postponed until late in the life cycle of the organism or may be observed only by special methods of testing. One of these is the question of general impairment of viability of the organism which may be susceptible of determination by observation on longevity.

In animals at fairly large doses there is good evidence that animals do not survive as long as nonirradiated controls. Whether this may be extrapolated to low dose levels is uncertain and is by no means conclusively established. There are no good data at levels of less than 100 roentgens and those that are available do not indicate any change in longevity. Recently, there has been presented evidence that radiologists who, having received more radiation than others by the nature of their activities, have suffered a reduction in their life span. Although the data are suggestive, statisticians have seriously questioned the significance of these data because of the method of sampling and of the uncertain relationship of the age groups.

Another late consequence of radiation in which the animal survives is the production of malignant new growths (tumors of various kinds) and leukemia. In animals, large doses unquestionably produce an increase in the incidence of cancers and leukemias. It should be pointed out that it is necessary to use a susceptible strain and that in certain insensitive strains it is not possible to produce these changes. The question as to whether this occurs in man, I think, has been amply demonstrated.

I believe there is evidence to show that when humans are heavily irradiated, tumors and lukemia will appear. The question is whether this occurrence may be satisfactorily quantitated and attributed to low levels of radiation. We have no data in this respect. Theoretically, considerations suggest that this may occur, but at present are entirely in the realm of hypothesis and must be considered inconclusive.

A third important late effect is concerned with the injury to the genetic tissue of the organism, and here I believe we should now make a distinction between sterility and genetic alteration.

The cells of the gonads which develop into sperm and ova and concerned with reproduction are extremely sensitive—comparable to that of hematopoietic tissue, and are injured with modest doses of radiation. From the point of view of sterility, it requires about 300 to 400 *r* to induce sterility in the female and perhaps 500 *r* to induce sterility in the male—that is, there is essentially complete loss of viability of the reproductive cells so that no progeny is possible.

This must also be distinguished from injury to the cells in the reproductive organs having to do with sexual characteristics—that is, male and female characteristics and other hormonal influences. These cells are not readily injured by radiation and are comparatively insensitive. Although it is easy to produce sterility, it is very difficult to eliminate the normal sexual characteristics—that is, male and female characteristics and other related functions.

The important change which has significance for all of society concerns itself with the alteration of the genes proper. Without going into the concepts of physical characteristics of the gene and its position in the reproductive apparatus, it is sufficient to say that these alterations are known as mutations which are essentially uninvolved in the reproductive capacity of the individual but produce its effects in subsequent generations.

Briefly, these mutations as a result of radiation appear to be similar to mutations produced by other causes. (Radiation is not the only cause for mutation.) The number of mutants appears to be directly related to the amount of radiation; that is, doubling the dose doubles the number of mutants. It is presumed that the radiation would have exactly the same importance and effect no matter how low the radiation level. It should be pointed out that we have no data below 25 roentgens and that extrapolations to very low levels are made on theoretical grounds.

It has also been generally accepted that the radiation effects on the extent of mutations are cumulative. That is, whether the dose is given at one time or distributed over long periods of time, the effects are exactly the same. Although

these data appear sound, they may still be considered incomplete and there are minor discrepancies which have appeared and which may require some elaboration. There is also reason to discuss the place of the production of mutations compared with the general mutations that are being retained in the genetic pool.

The radiation dose necessary to double the mutation rate appears to be about 50 roentgens. It should be clearly understood that this is an estimate, and competent geneticists have submitted proposals from 5 to 150 roentgens.

It is known that there are many diseases of heredity (that is, genetic origin) which are almost certainly the result of mutants and may therefore be examined in the same light as mutants due to radiation. Since these may be retained in the pool because of the amelioration of the rigors of selection, it would be possible to assess all of these mutants in terms of roentgens. Therefore, a better estimate of the total hazard as a result of low doses of radiation would be possible.

It appears that most mutations appear to be of the recessive variety which would therefore, in effect, not permit their immediate recognition or elimination until after many, many generations. This means that the mutant will become widely disseminated in the genetic pool. It also means that the radiation received by a small segment of society may be of little consequence since the radiation to the total population would be roughly the ratio of the total population to this small segment. The genetic effects are best surveyed from the point of view of its effect on the whole population and, generally speaking, the genetic effects become significant when delivered to either the whole population or large segments of it.

I am inclined to make these observations from the point of view of long-term effects of radiation—that is, the production of tumors, leukemia, and the decrease in longevity.

All data presented at the present time are either presumptive or speculative for very low doses. They rest in hypotheses derived from the theoretical aspect of dose effects at high levels. I believe there is sufficient uncertainty so that it would be unwise, and in fact nonscientific, to make conclusive decisions on the basis of these extrapolations.

With respect to the genetic effects, which have been extensively studied by biologists, there are sufficient uncertainties even in these data so that it is not possible to accept them as entirely unassailable. These include the fact that data at low levels do not exist, that data are confined at present to Drosophila and to a few small mammals such as mice, that the mutation rate due to ultraviolet radiation appears to be nonlinear, and there is reason to believe that some of the energy transfer with ionizing radiation is in part of the same character as that with ultraviolet radiation. Man has existed since time immemorial in a sea of radiation where fairly large differences because of altitude and special geographic places also are present. It is difficult to reconcile some of the conjectures to be made at very low levels with the natural radiation doses to which man has already been subjected.

To my mind, the problems of biologic effects at low doses are in essence these:

1. The data on the biological effects at low levels of radiation are by no means conclusive. At best they must be considered highly presumptive This suggests that extensive, carefully considered research is necessary.

2. Even if one assumes that the low-level effects of radiation are established, the problem of establishing the hazard and the risk rate at these levels has not yet been fully and properly evaluated. With specific regard to the fallout problem, it is my opinion that at the low levels which now appear to exist, no immediate decision on any vital problems is now necessary.

With respect to the general overall consideration regarding all-out nuclear warfare, a different order of magnitude is introduced and I must join with others in pointing out that this is fraught with the direst consequences, and that every effort must be expended to the elimination of nuclear warfare.

With specific respect to the fallout problem, it is my opinion that with the low levels which now exist, no precipitate alteration in our course is required. There are a number of organizations on radiation protection that are continually looking at this problem with representatives of all disciplines, and they are gradually modifying the acceptable levels wherever it is found desirable.

Representative HOLIFIELD. Before we hear our next witness, I would like to insert in the record a report from the Armed Forces Institute of Pathology.

(The report referred to follows:)

ARMED FORCES INSTITUTE OF PATHOLOGY,
WALTER REED ARMY MEDICAL CENTER,
Washington, D. C., May 16, 1957.

Subject: Statements for congressional hearings.
To: Chief of Research and Development, Department of the Army, Washington, D. C.
(Attn. Chief, Atomic Division.)

The following report is submitted in accordance with a verbal request to the Director of the Armed Forces Institute of Pathology from Lieutenant Colonel Ransom of the Research and Development Office of the Department of the Army, May 14, 1957. The time limit of 24 hours for the preparation of such an extensive report, and the absence on TDY of the Chief and Assistant Chief of the Section on Radiobiology, Armed Forces Institute of Pathology at the Nevada test site on Operation Plumbob 4.1 necessarily resulted in some limitation on presentation of material requested which under more favorable circumstances could possibly be more fully covered. The discussions and answers as presented represent a combined effort of the professional staff of the Armed Forces Institute of Pathology with some assistance obtained from Naval Medical Research Institute and Walter Reed Army Institute of Research.

W. M. SILLIPHANT,
Captain, MC, USN, The Director.

CONCERNING TOPIC IX

A detailed discussion of the occurrence of strontium 90 and cesium 137 in the atmosphere and its uptake and behavior in man is contained in the remarks prepared by Dr. Willard F. Libby, Commissioner, United States Atomic Energy Commission, for delivery before the spring meeting of the American Physical Society, Washington, D. C., April 26, 1957. A copy is attached (see p. 1519). These findings have also been discussed and confirmed by Drs. J. L. Kulp, W. R. Eckelmann, A. R. Schulert (Strontium 90 in Man. Science, 125, p. 219, February 8, 1957). However, Dr. Lapp (Science, vol. 125, p. 933, May 10, 1957) criticizes some of these conclusions, and points out some pertinent factors for consideration. His critique is attached (see pp. 694, 704).

CONCERNING TOPIC X

SOMATIC EFFECTS—PATHOLOGY

A. Distinction must be made between the somatic and genetic effects of radiation

The genetic cells carry on from generation to generation the damage which has been received. The somatic cells receive the injury but this is not transmitted from one generation to another. The effects of high level radiation may be manifested not only immediately but also after a delayed period. There are also effects from a low level of radiation and some organs are more readily injured than others.

B. Early effects of exposure of animals and man to external radiation

1. *Gama and X-radiation.*—Syndrome of radiation sickness. Individuals receiving doses of total body radiation can probably be best divided from a standpoint of prognosis according to the clinical signs and symptoms they present. This is particularly true because of individual variation in the response of different people to the same dose of irradiation. Roughly, casualties may be grouped into those in which survival is improbable, possible, and probable.. There is, however, no very sharp line of demarcation among the groups. The signs and symptoms have been described for the Japanese casualties at Hiroshima and Nagasaki in a report by Liebow, Warren, and DeCoursey in the American Journal of Pathology and in a report entitled "Some Effects of Iionizing Radiation on Human Beings" involving particularly the Marshallese casualties. In doses of more than 3,000 roentgens one may encounter a hyperacute reaction within an hour whereas in the range of about 3,000 to 2,000 roentgens nauseau, vomiting, and some diarrhea and fatigue may be the initial reaction in 2 to 4 hours after exposure. In individuals receiving doses between the range of 2,000 down to 800 roentgens there may be a period of relative well-being following the initial reaction for a few days and then a gradual return of

anorexia, malaise, severe diarrhea, thirst, fever, delirium, and leucopenia. In individuals between 800 and 300 roentgens this reaction may come in about 2 to 3 weeks with acute bone marrow failure, ulceration of the gastrointestinal tract, epilation, and bacterial infection. A subacute reaction consisting of subacute marrow failure, subacute infection in the lungs, brain, and bowel and general malnutrition may manifest itself in about 6 weeks after exposure in patients receiving 350 to 250 roentgens. In those receiving less than 250 roentgens and in some survivors from doses in the lethal range, there may be a chronic reaction of varying degrees extending for a period of months or longer of malnutrition, chronic anemia, premature aging, leukemia, and possibly neoplasia. The above acute syndrome varies with the geometry of the source of radiation in relation to the exposed person.

(*a*) Marshallese: See reference.

(*b*) The Los Alamos incidents referred to under X, B, 1, b are covered in a single entire issue of the Annals of Internal Medicine February 2, 1952.

2. *Beta radiation—Beta burns.*—As long as only very penetrating radiations are involved in exposure of the entire body, skin injury would rarely be a problem, because a dose sufficient to permanently affect it would kill the patient before dermatologic lesions were of any concern. Epilation is an exception to this statement since it was present, though only temporarily, in some of the Japanese atom-bomb victims. During fallout from bomb clouds, however, radioactive particles may settle on the exposed skin of anyone outdoors, and the hazards of beta particle radiation burns are added to the effect produced by penetrating gamma rays. Beta particle burns resulting from fallout first came into public prominence with the announcement that some of the inhabitants of the Marshall Islands were exposed to such a hazard during the 1954 weapons-testing program. However, the problem of fallout was not a new thing to those charged with the responsibility of conducting tests of nuclear weapons. At the time of the first nuclear detonation at Alamagordo, N. Mex., a number of cattle about 10 miles from the blast received fallout on their backs. The fine particles were retained by the hair, and in a few weeks epilation and blisterlike lesions occurred. The lesions healed much like ordinary thermal burns, and the hair grew again, but the original red color was replaced by grey or white. Late effects of this exposure have recently been reported in studies conducted at the AFIP.

(*a*) Marshallese: In the Marshallese group individuals were exposed to gamma and beta radiation. The injuries due to beta burns were local and confined to the areas of contact. The reaction manifested itself by initial tingling and itching at the time of exposure, followed by erythema and edema in a few hours, lasting for 2 to 3 days. There was then a latent asymptomatic 3- to 5-day period with a return of secondary erythema with vesicle formation. Drying and desquamation takes place in about 3 weeks and the individual then may enter a chronic phase with some atrophy of the involved parts taking place. Where both types of radiation occur concomitantly, the gamma radiation generally overrides the beta in clinical significance.

The effects of ionizing radiation amongst the Marshallese has been extensively covered in the report Some Effects of Ionizing Radiation on Human Beings from the Naval Medical Research Institute, Bethesda, Md.; United States Naval Radiological Defense Laboratory, California; and Medical Department, Brookhaven National Laboratory, Upton, N. Y.; United States Atomic Energy Commission, July 1955. Values for gamma and beta radiation could only be approximated but there was a high enough dose on the skin to produce lesions. The estimated "point source" doses were:

Rongelap, group I, 260 r.
Uterik, group IV, 20 r.

Some of the patients showed acute symptoms of diarrhea and vomiting and itching and burning of the skin in group I (Rongelap) but none in group IV (Uterik) showed these symptoms. Biopsies were taken of the skin at various stages. These showed changes typical of radiation reaction. Ultimately there was complete restoration of the skin.

(*b*) Other examples: Skin lesions, acute, chronic and neoplastic were one of the earliest hazards to be recognized in human beings exposed to low energy radiation. Human casualties from ionizing radiation have been of increasing concern since the turn of the century. These include in addition to skin lesions, a higher incidence of leukemia among radiologists than among the general population. The occurrence of cataracts among early workers with cyclotrons, the

high incidence of cancer of the lungs as an occupational hazard among certain miners in Czechoslovakia, and the bone cancers that occurred in watch dial painters in this country.

(c) *The early effects of internal radiation are dependent upon the amount, type, and area where material is deposited*

If the material is insoluble and taken into the gastrointestinal tract, it might produce only local irritation of the intestinal tract but not be absorbed within the body economy. Another example would be in giving I–131, the early manifestations of which would be some soreness of the thyroid and hematopoietic changes (approximately 2 to 3 weeks). However, this would require a large therapeutic dose.

(d) *Criteria include*

Half life (the physical and biological half lives), body utilization, solubility and excretion.

(e) *The degree to which late effects, readily produced in animals by single "massive" doses of total body ionizing radiation, may turn up in survivors in Japan is still under investigation*

Such effects include the occurrence of tumors in various organs after long latent periods following a single exposure to total body radiations in the lethal dose range; genetic mutations that affect subsequent generations; and aging. Such injuries are obviously far more difficult to follow in man than in controlled laboratory animal populations. It is only very recently that quantitative data on genetic mutations have been extended from fruitflies to a mammal, namely, the laboratory mouse, and this may still be a long way from the problem in man. An increased incidence of myelogenous leukemia and radiation cataracts has been found in the followup studies of the Japanese to date.

In the course of radiotherapy, it seems that serious late effects can result from a single exposure or a series of exposures to X or isotopic radiations. Thyroid cancer has resulted in children being given X-radiation for thymic disease. Leukemia has also been reported in individuals receiving X-radiation for spondylitis or those receiving repeated I–131 for cancer. The increased incidence in leukemia in the Japanese exposed to nuclear explosions at Hiroshima and Nagasaki is the only example of this disease occurring in man after a single acute exposure of the entire body to ionizing radiation.

(f) *General*

Exposure of the entire body, or a major portion thereof, to significant amounts of penetrating ionizing radiation interferes with the proliferation of normally self-replenishing tissues essential to life, namely the bone marrow, and under certain circumstances, the small bowel epitheliu. Within the lethal dose range, most of the stem cells responsible for the continued replacement of these tissues are still capable of recovery, with survival being dependent upon the time and extent of regeneration. The acute radiation syndrome, therefore, is a clinical entity resulting from an action of ionizing radiation from which recovery is potentially possible. It is a diagnosis that includes the signs and symptoms that evolve following exposure of the whole body or a major portion thereof to penetrating ionizing radiation.

It has been estimated that the human bone marrow pours into the blood stream each day 1 trillion red blood cells, 10 billion granulocytes and 500 billion platelets. The epithelial lining of the small bowel of a rat is replaced every day and a half. In the human, the rate of replacement is not accurately known, but it is also quite rapid. The rate of cell division in these tissues, throughout life, is as high as that encountered in a great many malignant tumors. Interference with the continuous proliferation or replacement of these tissues results in a secondary aplastic anemia and damage to the integrity of the alimentary tract.

The sequelae of panhematocytopenia from any cause have been known for a number of years. They include (1) thrombocytopenic purpura, (2) anemia, and (3) agranolocytic infections.

Anemia is due to a variety of factors including (1) inadequate hematopoiesis, (2) widespread purpuric hemorrhage, and (3) increased destruction of red blood cells. Hemorrhage is most prone to occur at sites of injury due to radiation damage, accidental trauma, and physiologic activity. Huge numbers of extravasated erythrocytes return to the bloow stream via the lymphatic system and thoracic ducts. Many are phagocytized by macrophages. Increased destruc-

tion of red blood cells occurs, and leads to increased deposits of hemosiderin in the spleen.

Vincent's Angina is a common complication of agranulocytosis from any cause. Mechanical trauma and poor oral hygience invite septic ulcerations, particularly in the presence of agranulocytosis. The tonsils, as is well known, may serve as portals of entry for bacteria with the subsequent development of a bacteremia or septicemia.

Focal hemorrhages from radiation-induced thrombocytopenic purpura may be followed by septic ulcerations of the large bowel and the onset of diarrhea several weeks after exposure, even though the dose of radiation to the abdomen has not been sufficient to permanently interfere with recovery of the more radiosensitive small bowel. Such things as focal hemorrhages, delayed vascular reactions to irradiation, and to injured tissue, damage to the solitary lymphoid follicles and smoldering superficial infections contribute to the development of such ulcers.

Recovery of the small bowel epithelium generally occurs following exposure to total body ionizing radiation up to 100 percent lethal dose. Failure of recovery, however, may be an important factor in early deaths resulting from exposure to supralethal doses, or where the small intenstine is the principal site of injury.

1. In the various mechanisms of response of man to radiation the injury is caused by the energy imparted by the various ionizing radiations. This energy is dissipated in matter through excitation or ionization, depending upon the energy level of the radiation. The total ionizing action is related to the number of ion pairs formed per unit limit. This may be expressed as the density of ionization. Alpha particles have a high ionization density but a short range; beta particles a less dense ionization pattern but a range of a few millimeters in tissue and a few centimeters in air. Gamma radiation has a long range with the lightest ionization density. Neutrons have a somewhat shorter range than gamma rays. This is significant in that gamma and neutrons can penetrate with ease into the body from external sources. In contradistinction, alpha and beta particles are limited in such penetration from practically 0 for the alphas to a few millimeters through the skin for the betas. However, from an internal source, alpha emitters take on particular importance because of their unrestricted local activity over very long periods of time.

Certain effects of ionizing radiation on living cells in both plant and animal tissues have been clearly established for many years. These include (1) acute cell destruction, associated with nuclear vacuolization, rupture, and fragmentation; (2) a variety of chromosomal alterations and; (3) delay in division. Less well understood actions include (1) differentiations, aging and death of so-called vegetative intermitotic or stem cells; (2) effects which interfere with the action of humoral factors involved in the regeneration of certain tissues, including derivatives of the reticuloendothelial system; and (3) effects involving the cellular and noncellular immune responses of the organism.

2. Significance of different types of ionizing radiation in process: There are several important differences between lesions to be expected from penetrating radiation and from beta radiation from fallout particles. Once the beta particles have reached the surface of the earth, they contribute to the general activity of the area, but do not endanger the skin surfaces to any extent, because they penetrate only a few millimeters of tissue and almost any covering affords some protection. Overexposure to gamma rays may be followed by the acute radiation syndrome, and death or recovery in a matter of weeks, while exposure to high levels of beta radiation may result in third-degree burns requiring long hospitalization and extensive skin grafting.

In early casualties due to fallout in the general vicinity of the nuclear weapon used, one is concerned chiefly with the "recoverable component" of radiation injury. With such fallout pattern, depending on meteorological conditions downwind from the site of detonation, the terrain, weapon, point of detonation, etc., time, intensity and quality factors of irradiation become as important for prognosis, as they are in formulating a radiation prescription for the treatment of malignant disease. From a research standpoint also, the recoverable component of irradiation injury appears to be the key to survival following total body irradiation.

3. Ionization is thought to result in the breakdown products of water in the presence of oxygen into OH, H, O_2H, and H_2O_2; with the exception of hydrogen, these are powerful oxidizing agents. As to the locus of the radiation effects, in cells, two theories are advanced. One, the target theory localizes the action with some vital component of the cells. The other, the indirect theory, relates more

to the general action of the breakdown products of water. Both types of action probably account for radiation injury. One of the most important cellular effects is enzyme alteration. This generally occurs by oxidization of the SH groups or by protein denaturation. There is also a reduction of nucleic acid synthesis and arrest of mitosis. The use of the terms direct and indirect effects of irradiation should distinguish whether one is speaking of a single cell or the whole organism. Thus, ionizing radiation effects on the small bowel epithelium are direct in the sense that they are not appreciably inflamed by shielding various portions of the body other than the area of small intestine irradiated. Such effects within a single proliferating mucosal crypt cell may be both direct and indirect, although the latter, presumably mediated by the production of certain highly reactive radicals, appear to be the most important.

Various tissues of the body respond quite differently, in terms of ultimate effect, to the same cumulative dose of irradiation—total body and otherwise—fractionated in different ways. (See also data by Nachmansohn and Cotzias and Serlin under X, Bl.)

4. As a general rule, the sensitivity of a call to radiation varies as the mitotic activity and inversely as the degree of differentiation. Ranging from the most sensitive to the least sensitive, are the lymphocytes, erythroblasts germinal epithelium of tastes, myloblasts, intestinal crypt epithelium, ovarian germinal cells, basal layer of skin, connective tissue, liver, pancreas, kidney, bone, brain, nerve, and muscle. It is important to distinguish between radiosensitivity and radiocurability as well as the biological effect under consideration.

5. Effects of the whole organism.

(*a*) There is a wide difference in susceptibility of various animals to total body irradiation. The approximate LD 50/30 doses of total body radiation are as follows:

	Roentgens		*Roentgens*
Guinea pig	250	Rat	590
Dog	300–430	Mouse	500–650
Swine	420	Burro	580–780
Man	450 (estimated)	Rabbit	790–875
Monkey	500 [1]	Chicken	1,000
Sheep	520	Turtle	15,000

[1] For servival period of 67 monkeys at various gamma radiation doses see Effects of Barium^{140}–Lanthanum^{140} etc., under B.1. For recent review of the Effects of Radiation in Mammals, E. P. Cronkite and V. P. Bond, American Review of Physiology, vol. 18, 1956.

The difference in the lethal dose of total body irradiation upon various mammalian species: guinea pig, 200 roentgens, rabbits, 800 roentgens, has been directly correlated with degree of the recovery of delay in bone marrow produced in the particular species involved by such dose.

(*b*) Micro-organisms vary tremendously in their susceptibility to radiation. To destroy all bacteria in milk, for example, requires at least 750,000 roentgens. Tobacco mosaic virus requires 1,800,000 roentgens.

(1) Position of man: There are no exact data. The LD50 figure of 350 roentgens proposed from the Marshallese contrasts with a commonly quoted value of 400 roentgens or 450 roentgens. (Handbook of Atomic Weapons for Medical Officers prepared by the Armed Forces Medical Policy Council for the Army, Navy, and Air Force, June 1951), and a recent evaluation of the Japanese World War II casualty data something in figures well bove 400 to 450 roentgens for the immediate radiation from the bomb. (See Marshallese report).

6. The clinical syndrome in man of radiation injury in the sublethal and lethal range presents a fairly uniform hematopoietic pattern. In the sublethal group, there is an early and profound drop in lymphocytes with the neutrophil count showing an initial rise in 12 to 48 hours and then falling to pre-exposure level with a maximum drop from 5 to 6 weeks. Platelets start to decrease in a few weeks with a maximum low in about one month. During the first few weeks the hematocrit falls off only slightly if there is no bleeding. In the lethal ranges the same course of events occur but are markedly acceleraed and of greater intensity. The platelets drop off by the 4th day and completely disappear by the 10th. This general hematopoietic depression ties in with the subsequent bleeding and infection susceptibility. In the delayed effects the shortening of life span may result from such general factors as lowered immunity, damage to connective tissue, and premature aging. The question of specific tissue damage is indicated by the increased tendency to leukemia and skin cancer in certain exposed individuals. However, the carcinogenic factor is not too well established in humans.

For syndrome of nervous symptoms, see joint report of Hiroshima and Nagasaki casualties, etc., by Shiraki et al., under X, B,1. Also in National Academy Sciences report, 452, pages v-5–v-62.

G. Relationships of damage mechanisms to dosages

1. Production of leukemia and neoplasms (under mechanisms and response of man to radiation and radioactivity) exposure to ionizing radiation has been generally accepted as a leukemogenic factor in man (Kaplan, H. S., Cancer Research, 14, 535, 1954).

The high incidence of leukemia in radiologists, 8 to 10 times the incidence in nonradiologists has been widely accepted as evidence of this factor (Ulrich, H., New England Journal of Medicine, 234:45, 1946). Further evidence has been the cases of leukemia and malignant epithelial lesions (Hepatomas) many years after the diagnostic use of Thorium dioxide (Thorotrast).

More recent evidence is the preliminary report from England in 1956 on the apparent increased incidence of leukemias in children following exposure to weak irradiation received through prenatal diagnostic pelvimetry (Stewart, A., Webb, J., Giles, D., and Hewitt, D., Lancet 2: 447, 1956).

Aplastic anemia: It is well known that the atomic bomb victims that survived the blast and were exposed to extensive radiation died with aplastic or hypoplastic bone marrows. The sequence of the morphologic changes in the bone marrow have clearly been described by Liebow, Warren, and DeCoursey (American Journal of Pathology 25: 853, 1949). In experimental animals evaluation of bone marrow radiosensitivity indicates a variation in degree of sensitivity of the hematopoietic elements with the granulocytic and erythroid elements being most sensitive and fat cells and reticulum cells the least sensitive and even quite radioresistant (Bloom, M. A., and Bloom, W., Journal of Laboratory and Clinical Medicine, 32: 654, 1947). However, more recent studies have indicated that erythropoietic elements are definitely less sensitive than granulocytic (Valentine, W. N., and Pearce, M. L., Blood, 7: 1, 1952).

The use of repeated large whole-body irradiation exposures has been studied by Valentine, Pearce, and Lawrence in the cat using 4 exposures of 200 r over a period of 1½ years. Although the exact L. D. 50/30 days is not known, their preliminary work indicated that probably was in the 300 to 350 r range.

Nevertheless, a single dose of 200 r represented a severe hematologic insult. Recovery occurred within 30 days following each exposure with very little detectible marrow damage after four exposures. (Valentine, W. N., Pearce, M. L., and Lawrence, J. S., Blood 7: 14, 1952.)

For a population of 100 million with a lifespan like that of the United States, each absorbed roentgen of whole-body radiation would result in about 6,000 cases of leukemia during their life time, while one-tenth the "maximum permissible dose" of Sr^{90} would result in 35,000 cases. (E. B. Lewis, Leukemia and Ionizing Radiation. Science, 1957, 125 in press.)

GENETIC EFFECTS

H. The nature of genetic effects: Studies, beginning with Mendel, demonstrated that the characteristics of living things were inherited following certain specific laws. Animal-husbandry men and farmers knew most of this but could not interpret the genetics laws properly because of ignorance and lack of information concerning genes and the requirements for expression of inherited characteristics. The germ cells containing only a single set of chromosones which in turn carry only a single set of genes transmit the characteristic of one parent to the child. The child has a double set of chromosones and genes consisting of one set from each parent. Since the characteristic for one parent may be dominant over that of the other, the child will show a mixture of characteristics; some from one parent, some from the other, and some which were common to both parents. Studies with plants, insects, and animals have demonstrated the accuracy of these concepts.

Because there are so many genes and so many variations among the genes for the same characteristic, there is considerable opportunity for variation which in turn permits opportunity to meet changes in the environment. There is still another mechanism which acts as a safeguard to allow the various species to change and thus adapt themselves to severe and marked alterations in the environment. This mechanism is called mutation. It consists of an abrupt, spontaneous change in a gene, producing a change in a recognizable characteristic. Most mutations are detrimental to the species and would be of value

only if there was a considerable change in the environment. It has been estimated that approximately 1 in 10,000 germ cells will undergo such mutation.

Frequency of tangible genetic effects as given by NAS report, i. e., mental defects, epilepsy, congenital malformations, neuromuscular defects, defects in vision or hearing, cutaneous and skeletal defects, or defects in the gastrointestinal or genito-urinary tracts, make up about 4 to 5 percent of all the live births of the United States. Of these about 2 percent are genetically caused. But this is not the natural mutation rate, which also includes lethals, changes in fertility, life span, etc., which are hard to detect and other nonharmful changes (eye color, etc.). Therefore it may be as Muller suggests, more like 1 in every 5, or 20 percent.

Recognized causes for natural mutation are temperature, chemical substances (particularly azone), and radiation. Again based on experiments with insects and animals it has been estimated that radiation equivalent to 30 to 80 r, whole-body dose, will double the normal spontaneous mutation rate. Further it has been demonstrated that the time over which the radiation is received does not affect the mutation rate.

Russell's studies on mutation of seven genes in mice show that about 30 r delivered to immatured germ cells constituted the doubling dose. There is probably not much higher in man, it may even be lower.

Since man exhibits a longer life span than mice and Drosophilia, it is likely that more of the spontaneous mutations are due to background radiation. If it were equal to it (3 r) then the doubling rate would also be 3 r. It is more likely that it is about 3 times as large (10 r) as recommended by the NAS reports.

The frequency of point mutations increases linearly with radiation dosage. In Drosophilia this has been demonstrated for a range from 25 r to 6,000 r. In certain plants this is extended down to 5 r. In mice this has only been tested from 300 to 800 r. but there is no indication that it does not hold outside this range. There is no sign of a threshold below which mutations are not produced, but rather even the lowest are proportionately mutagenic, and all doses are additive or cumulate in effect.

Because gene changes are inherited and because it is very rare for genes to mutate back, the occurrence of a mutation is thereafter inherited until the end of that cell line. Consequently, the effects of mutation accumulate within the population. With random matings these genetic changes become dispersed among the population. If the mutations are detrimental they are likely to cause decreased viability and ultimately death when accumulated in the population to such an extent that both parents transmit the detrimental character to the child. In effect this eliminates the mutant from the population. Ultimately a level is reached whereby for each new mutation arising an old mutant accumulated in the population will be eliminated.

Because of these reasons it has been believed by one group of investigators led by Muller that any increase in radiation can only be harmful and ultimately will lead to degradation and degeneration of the race. However, this will require many generations before such effects could become apparent. A smaller group believes that there are certain inherent safeguards which would protect the species by decreasing mutation rate in response to radiation.

Sturtevant of the California Institute of Technology has calculated that, if the irradiation from fallout increases at its present rate, it will produce some 70 children a year carrying a mutation. This estimate he adds may be too low and, in fact 7,000 may be a better estimate. This has no noticeable impact statistically, that is, about 2 percent (150) will actually show changes from the normal. If compared to the 4 million born yearly and 40,000 defective ones at birth we need not be concerned about the effect of fallout on the future of the people at large or on mankind. Yet if the statistical approach is not used 150 individual newborn children each year will be affected.

Some of the current problems in this field are discussed in the following articles:

Crow, James F., The Estimation of Spontaneous and Radiation-Induced Mutation Rates in Man, from Eugenics Quarterly, vol. 3, page 201, 1956.

Crow, James F., Possible Consequences of an Increased Mutation Rate, from Eugenics Quarterly, in press.

Glass, H. B., The Induction of Mutations with Radiation, talk delivered at International Agency for Peaceful Application of Atomic Energy, Brookhaven National Laboratory, May 15, 1957.

Stern, Curt, Genetics in the Atomic Age, from Eugenics Quarterly, vol. 3, page 131, 1956.
Muller, H. J., Potential Hazards of Radiation, from Excerpta Medica (Amsterdam) in press.
Muller, H. J., Damage from Point Mutations in Relation to Radiation Dose and Biological Conditions, in press.

L. Concepts and definitions for standards pertaining to external radiation effects are covered in Relative Biological Efficiency of Different Ionizing Radiations, John W. Borg, National Bureau of Standards Report 2946, December 30, 1953.

M. Standards for internal radiation effects:

1. Reference is made to the report of the Subcommittee on Toxicity of Internal Emitters as given in Pathologic Effects of Atomic Radiation, National Academy of Sciences—National Research Council publication 452.

Also reference is made to the report Tentative Recommendation of the NCRP for the Maximum Permissible Levels of Radiation to Man, a copy of which is attached.

2. For methods of determining total accumulated doses and dose rates from external radiation, see Doses and Dose Rate Cures, AFSWP Manual No. 99.

N. ———.

SPECIFIC QUESTIONS FOR DISCUSSION

A. All low level effects are not extrapolations from high level effects, (for example see studies by E. Lorenz). Such extrapolations would be hazardous. However, further studies on low-level effects are particularly important since the explosion on March 1, 1954, of an experimental thermonuclear device at the United States Atomic Energy Commission Eniwetok Proving Grounds in the Marshall Islands.

B. There are quite definite distinctions between temporary and permanent (long-term) damages, and between repairable and irrepairable damage. The problem of certain long-term damages may be complicated by sequelae from effects upon tissues other than the one(s) in which the most serious lesion(s) may ultimately appear, as in the development of certain neoplasms. This has been demonstrated in the case of malignant tumors arising in the thymus following irradiation by Kaplan, and may be true also for certain other types of neoplasms arising many years after exposure, as an example, in the skin. While repairable effects are well known, the differential sensitivity of anatomical units of an apparently, morphologically, homogenous tissue may result in incomplete recovery of a sufficient number of components after high doses to result in death of the organism. Recovery of self-replenishing tissues essential to life, such as the bone marrow and small intestine (when the abdomen is the principal site of injury and after supralethal doses of total body radiation) may be sufficiently delayed until sequelae, such as those associated with panhematocytopaenia result in death even though in the case of the bone marrow recovery may still occur if such complications can be controlled.

C. * * *

D. The effects on behavior in Hiroshima and Nagasaki casualities who died during the period of 16 to 69 days is mentioned under Joint Report—Effects of Atomic Radiation on the Brain of Man, Etc., by Shiraki, et al., under X, B–1. There was little evidence of changes in mental posture, personality, and intelligence in those who died during the first 3 months after exposure. Under such conditions the dose level was great enough to cause death from anemia and other factors, but was insufficient to affect directly the brain. Japanese physicians have stated that many patients who survived the bombings have shown no neurological disabilities but have complained of generalized weakness, easy fatigability, and nervousness for years after the bombings.

E. To date we are probably limited for practical purposes in the event of mass casualities due to exposure to ionizing radiation to procedures which will (1) reduce the dose received by such things as shelter, evacuation, clothing, bathing, washing down ships of the fleet, etc.; (2) reduce and combat complications such as burns, indirect injuries from blast effects, and infection; and control the sequelae of panhematocytopaenia, and disturbances in water and electrolyte balance, by procedures in general use for such syndrones from any cause. The possibility of adding to this armamentarium by more specific therapeutic measures, including both humoral and cellular factors appears probable from research to date, but has not been consummated.

F. Unless all radiological factors are reported, and radiation procedures such as fluoroscopy standardized as far as practical, a record of the number of roentgens received by each person during his lifetime would probably not be very meaningful. For example, to record the fact that on a film badge a patient received 10 roentgens, per se, is no more informative than a statement that he was given 10 milliliters of a substance intravenously without indicating the concentration of the solution.

G. The total estimated dose rate to gonads from natural sources of radiation both internal and external is 0.095 roentgens per year. In addition it is estimated that diagnostic radiology contributes 22 percent of the above natural radiation dose. Occupational exposure in radiology and industry adds at least another 1.6 percent of the natural radiation dose. (The Hazard to Man of Nuclear and Allied Radiation, presented to the Lord President of the Council to Parliament by Command of Her Majesty, June 1956.) H. (The numbering of the questions skips from H to J).

J. * * *

K. Radioiodine acts principally on the thyroid, but a possible relationship to leukopenia and anemia has been suggested. The doses and expected effects are as follows:

(*a*) 1 or 2 milliocuries I^{131}: This is the lowest amount that will cause transient alteration of physiological activity of the thyroid. No recognizable histologic changes would be expected.

(*b*) 10 to 15 millicuries I^{131}: This amount will cause a mild transient decrease of thyroid activity, probably detectable only by laboratory tests. The depression may last a few months. Histologic alterations, if any, would be in the form of mild fibrosis and slight los of follicular epithelium.

(*c*) 35 to 75 millicuries I^{131}: Usually given in fractional doses, this total amount can be expected to produce definite clinical hypothyroid state for between 6 and 12 months. Histologically, there would be varying degrees of fibrosis and follicle destruction.

(*d*) Two courses of 35 to 75 millicuries I^{131} can be expected to produce almost complete cessation of thyroid function with severe myxedema. The duration of the myxedema cannot be predicted, as the patients tend to develop thyroid activity over the course of a few years. Histologically, one would expect virtually complete fibrosis of thyroid with a few surviving distorted epithelial cells and possibly a few distorted follicles. Eventually, some regeneration of follicles might occur. Even though there may be widespread destruction of thyroid, the parathyroids are unaffected.

(*e*) 1,200 to 1,500 millicuries: This total amount has been given over a period of several years to a few patients. Leukopenia and/or anemia has sometimes developed and been attributed to the radiation effect or circulating I^{131}, but there is no proof that the hematologic changes were due to I^{131}. Amenorrhea has been reported, but there is no proof it was the result of I^{131}.

Cs^{137}: There is no evidence so far that Cs^{137} has any unusual biological properties. It does not seem to localize in bone.

C^{14}: This is eliminated fairly rapidly (about 97 percent in 3 or 4 days) from the body, largely as CO_2. It does not localize in bone.

L. * * *

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Representative HOLIFIELD. Our next witness is Dr. Austin M. Brues, of the Argonne National Laboratory, director of the Biological and Medical Research Division since 1946, and delegate to the U. N. Radiation Committee.

All right, Dr. Brues.

STATEMENT OF DR. AUSTIN BRUES, DIRECTOR, BIOLOGICAL AND MEDICAL RESEARCH DIVISION, ARGONNE NATIONAL LABORATORY [1]

Dr. BRUES. Thank you, Mr. Chairman.

I do have a short prepared statement which I intend to read in its entirety.

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[1] April 25, 1906. A B. Harvard University 1926; M. D. Harvard Medical School 1930, Med. Resident, Huntington Mem., Hosp., Boston, Mass., 1930–31; House Officer, Mass. Gen. Hosp., Boston, Mass., 1931–32; Asst. in Med., Harvard Med. Schl., Boston, Mass., 1932–34; Grad. Asst., Mass. Gen. Hosp., ibid., 1932–34; Asst. Phys., Huntington Mem. Hosp., ibid., 1932–37; Rsch. Fellow in Med., Harvard Med. Schl., ibid., 1934–35; Instr. and Tutor in Biochem. Sci., Harvard Coll., ibid., 1934–42: Moseley Traveling Fellow, Harvard Univ. and Royal Cancer Hosp., London, England, 1935–36; Grad. Asst., Peter Bent Brigham Hosp., Boston, Mass., 1936–37; Instr. in Med., Harvard Med. School, ibid., 1936–37; Assoc. Phys., Huntington Mem. Hosp., ibid., 1937–41; Assoc. in Med., Harvard Med. Coll., ibid., 1937–42; Ward Visit, Boston City Hosp., ibid., 1938–39; Freshman Advisor, Harvard Coll., ibid., 1939–42; Jr. Assoc., Peter Bent Brigham Hosp., ibid., 1940–41; Intern, Army Induction Center, ibid., 1940–44; Assoc. Phys., Peter Bent Brigham Hosp., ibid., 1941–42; Responsible Investigator, Office of Sci. Rsch. and Develop., ibid., 1941–44; Asst. Prof. of Med., Harvard Med. Schl., ibid., 1942–45; Asst. Phys., Mass. Gen. Hosp., ibid., 1942–45; Sr. Biol., Metallurgical Lab., Univ. of Chicago, Chicago, Ill., 1944–46: Assoc. Prof. of Med., Dept. of Med. and Inst. of Radiobiol. and Biophysics, ibid., 1945–52; Prof., 1952—; Sr. Biol. and Dir., Biol. Div., Argonne Nat'l. Lab., Lemont, Ill., 1946–50: Sr. Biol. and Dir., Div. of Biol. and Med. Rsch., ibid., 1950—. American Assoc. of Anatomists: A. A. A. S.: Amer. Assn. for Cancer Rsch. (Pres. 1954–55): Amer. Physiol. Soc.; Amer. Soc. for Clin. Investigation; Assn. for Study of Internal Secretions; Assn. of Amer. Phys.; Cent. Soc. for Clin. Rsch.; Inst. of Med. of Chicago; Soc. for Cell Biol.; Soc. of Exptl. Biol. and Med.; Tissue Culture Assn.; Radiation Rsch. Soc. (Pres., 1955–56). Member of United Nations Scientific Committee on the Effects of Atomic Radiation. (Submitted by witness.)

Representative HOLIFIELD. All right.

Dr. BRUES. I want to speak chiefly concerning the philosophy of setting and determining the permissible levels which Dr. Taylor spoke of earlier this morning, and what the true basis of our understanding of these is.

The presently accepted safe levels of radiation and of radioisotope incorporation are based on a long history of clinical observation and experimental research. These levels have been determined on the basis of making the most pessimistic assumptions where knowledge is lacking and then introducing a factor of safety.

Where direct observations on human beings or suitable animals have been at hand, the practice has been to divide those levels which produce any detectable effects by 10 to arrive at a permissible level. It was on this basis that the permissible daily X-ray exposure of 0.2 roentgen to the whole body was reduced many years ago to 0.1 roentgen, and then, as a result of some further work which was done during the Manhattan District days, particularly having to do with the production of sperm in dogs given one-half roentgen a day, which showed dogs had some changes in the rate at which they produced a sperm, it was considered one-twentieth of a roentgen a day would be a safer dose.

The question then came up as to whether these levels had to be adhered to each day or whether one might not receive a week's dosage on Monday morning without any further effect than that incurred by spreading it through the week. While an acutely dangerous dose of radiation—say 500 roentgens—is less toxic if spread out over a week owing to the rapid recovery of the blood-forming tissues, there seemed to be good experimental evidence that the late consequences of low doses were rather independent of time, and so three-tenths of a roentgen per week was accepted. It seems quite likely that the whole yearly quota of 15 roentgens—which in itself produce no obvious effects—might as well be incurred on a single day, but three-tenths per week appears to be more practical, except for special cases such as might arise under civilian defense conditions, where a calculated risk might be acceptable.

That sort of thing has also been allowed for.

Ingestion or inhalation of radioactive substances presents a different problem. These sources of radiation may become concentrated in certain tissues and organs, and the greater part of the radiation energy, depending on type, may be given locally. The most striking example of this is the concentration of radioactive iodine in the thyroid gland, where half of the dose may be deposited in one one-thousandth of the body. We do not, unfortunately, for purposes of cancer treatment, know of any other such striking case of an extreme localization.

There have been two ways of solving this question. One has been to calculate the radiation dose in the "critical organ," that is, the organ with the highest radioactive concentration, and then to set levels of exposure such that the equivalent of three-tenths roentgen a week will not be exceeded. The other has been—and this is used where the bony tissues receive the highest dose—to compare the possible damage with that produced by radium in the human skeleton, since we have knowledge derived directly from the histories of persons who have been poisoned by radium through industrial exposure in the watch dial painting business or through administration of

radium as a drug in the days, 25 to 30 years ago, when it was thought that radium might be beneficial in certain conditions for which there was no known effective treatment.

Since no damage had been observed in patients who retained less than 1 microcurie of radium in the skeleton, about the amount in a radioactive watch, this was again divided by 10 and one-tenth microcurie was established as a permissible amount of radium. To this day, no detrimental effects have been seen in persons containing this amount of radium.

Since the preparations used to paint luminous watch dials and for some medical uses contained considerable amounts of other radioactive elements—specifically mesothorium—it has been suggested that pure radium may be less toxic than indicated here. This point is not settled, and a search is being made for other persons who may contain abnormal amounts of radium in order to improve our knowledge. In particular, it is becoming clear that a good number of persons may harbor more than 1 microcurie without detectable harm of any sort, and that the proportion who do suffer for a given amount is lower than was believed.

Since the first cases seen, and the majority, were found out because they had complaints which directed the attention of physicians to them, we see a selected group of people who met with the worst result; the well ones are much less likely to come to our attention.

This, again, I think, introduces somewhat a factor of safety in the question of how much radium is likely to produce serious effects on the human being.

These radium levels have been transferred to other radioactive materials as a result of comparing effects of radium on animals against those of plutonium or radioactive strontium. Plutonium turns out to be somewhat more toxic than would be expected from physical calculations of the radiation to bone, and this is apparently because plutonium is deposited near those cells which are active in bone growth.

Radioactive strontium 90 has been determined to be one-tenth or less as likely to produce bone tumors as radium for a given number of microcuries. On this basis, 1 microcurie of strontium 90 is considered as the equivalent of one-tenth microcurie of radium and, therefore, is designated as the maximum permissible level.

These levels were employed very successfully in the atomic-bomb project during wartime. Most of the workers remained very far below the permissible levels.

If you set up a level which is not to be exceeded, it happens, administratively, that things work out so that people get very much less.

There is the story of one individual on the project who attempted to receive his 10th roentgen per day because that is what he thought he was supposed to do. But, in general, nothing like this happened.

For practical purposes it is necessary to determine many more things than just the safe level of body content. We must also translate this into the amount which can safely exist in the air breathed and in the food and water ingested, in order to regulate these concentrations at a level which will not permit an excessive load to exist in the body. These are, then, the MPC's, or maximum permissible concentrations. This means we must use our best information as to how much is retained in the body from inhalation and from the digestive tract, and how fast it is lost from the body by excretory processes.

Many of these things have to be decided upon before the enormous amount of experimental work required for an exact answer can be carried out. There is no time here to discuss all this, but I can say that those committees shouldered with responsibility for such decisions always use the strictest possible assumptions, and, since several separate assumptions must be made—for example, how much in the air gets into the lung, how much in the lung is kept there until it gets into the circulation, how much of that is deposited, and how fast it is lost from the organ—as well as the relative effects of types of radiations from different elements, each of these considered in the worst light, we end up by multiplying a number of different factors of safety and are almost certain to come out with a level much lower than the correct one.

To give a few examples:

When tritium was first under consideration, it was noted that it has a remarkably short-range beta radiation, and nothing like it had been studied experimentally. So a factor of safety of 10 was introduced until it was shown that it acts about the same as the more familiar radiations, when this factor could be thrown out. Similarly, the strontium and radium levels were based on an early assumpion that they are lost from the bone according to a very slow process, which was measured on patients and animals a long time after its acquisition. This led to very low levels being recommended in water. It has since become known that loss occurs very rapidly at first, so that it requires about 10 times as much taken in to maintain a given level. The MPC's in this case have not yet been changed until complete study of the problem can be made, although the evidence is now fairly clear.

In another instance, a stringent level of radium in water was suggested unofficially, and we found that it was actually less than that in the drinking water of our laboratory. Had this been adopted, we would have been required to distill our own domestic supply before we could be permitted to let it flow off the grounds.

Of course, radioactive materials, as you have probably heard in the last few days, because of their special nature and the degree of development of our instrumentation, can be detected in relatively much smaller amounts than almost any other toxic material. This may be a large part of the reason for the disproportionate public concern about radioactivity relative to other noxious things.

As you are aware, there has been a general lowering of levels recently, since artifically produced radioactivity has become wider in its scope.

Here, we have to keep two things quite distinct. First is the problem of genetic effects, which will be discussed by others. The special features of these is that they seem to be produced without threshold: that is, any small amount of radiation will produce its proportion of changed genes; and that almost all of these are hidden and are perpetuated through generations till they come together accidentally through interbreeding. Thus, very stringent levels are recommended, but they do not refer to any individual but to the whole population; thus an average figure for the whole population is all that is to be looked for.

The other is concerned with the matter that we must not only consider, as was the basis of the original levels, a selected group of in-

dustrially exposed persons, but also many persons outside this group who might be close to installations where exposures could occur.

One asks, of course, why if a level is safe for one group, it is not for another. There are several reasons for this, none complete in itself. One is that the occupationally exposed group are selected, do their work voluntarily, are under medical control and are monitored. Another is that persons not in the atomic energy business may be in other fields of work which have their own peculiar hazards. Still another may be that there are more chances for an overexposure to occur. So that we might set the levels so that a considerable "overexposure," on that basis, would still not be an overexposure in the sense that it get to a level which would be within the potential danger zone. For these and other reasons, in one sense or another philosophical, we have adopted another safety factor of 10.

Mr. RAMEY. On our last point there, about your safety factor of 10 with respect to strontium 90, would the fact it applies mostly to the takeup of strontium 90 as it affects persons that it would be more as it applies to children, and therefore a lower factor when you go from an occupational group to a population group to take into account the bone-forming period?

Dr. BRUES. Well, this question has been raised. Actually I do not know of evidence that the skeleton of the child is more sensitive, except with respect to the fact that if one starts as a child and continues to an adult, he puts more of the material away. This, I think, has already been taken into consideration. We could still have more evidence on this, but data I have seen does not suggest the child at these low levels, where not stunting his growth, is going to show any more results than certain total amounts for others.

Representative HOLIFIELD. It is a fact that the bones of a child are growing and accumulating more cells at a faster rate of cell growth than the adult, is he not?

Dr. BRUES. That is true, yes; and on a given intake level, a larger total will be evident.

Another consideration which has led to extra safety is that of the fluctuating level of exposure. Where we have set conditions for exposure to external radiation we have allowed for such fluctuations. It seems equally reasonable for the level to say, radioactive strontium in water to exceed the MPC by 7 times 1 day a week; or for the point of disposal in a highly polluted river to exceed the MPC so long as it is diluted out before it reaches a point where it would conceivably be ingested—remembering also, that the MPC is based on the assumption of continuous intake for a lifetime. The same situation applies to shifting winds around a stack. For exadministrative reasons, it is therefore highly likely that conditions will be set which are much more stringent than those leading to a maximum possible concentration in personnel. It is most important to remember that that is what we are really concerned with, and that no legal culpability should be involved in an occasional fluctuation in the environment above that which would be one-tenth or one-hundredth of a dangerous level but only if it were kept up indefinitely.

The whole basis of the concept of a permissible amount, or level, by the way, rests on the assumption that there is a threshold; that is, that no harm will be done by smaller amounts. In genetics, we have reason to doubt that there is a threshold at all, so that the total popula-

tion average of exposure is set so low that it falls close to the natural variations in the natural radiation background.

Where the question is applied to other effects of radiation, such as longevity or cancer, we do not know whether they have thresholds or not. It has been suggested that they do not, but on the basis of very scanty evidence so far, and in no case is there information much below 100 r; and there are also good reasons from what we know about the nature of cancer to suspect that the hazard goes down faster than the insulating agent. An animal experiment to guarantee the existence of a human threshold below suggested off-site MPC's would be a prodigious undertaking and would drain off much of our talent from work which is really more basic to the problem. It would, moreover, detract both talent and public attention from problems of the same sort that seem, to me at least, as urgent.

For instance, millions of Americans now living will die of cancer of the lung due to something in the environment that we did not have a few decades ago. I once made a calculation by exactly the same means as are used in the calculations of MPC's, comparing lung cancer with radium cancer, and derived an MPC—occupational criteria—of 2.4 cigarettes a day. An off-site MPC would be 1 every 4 days. The only assumption made here was that cigarettes are the causative agent. If it is city smoke, this would have to be reduced in a similar proportion before the criteria used in determining permissible levels of radioactive substances would find it allowable.

Senator ANDERSON. What year would become a basis for this new item which has come into the picture? You say something about decades. How far back?

Dr. BRUES. These figures, of course, vary from place to place, and they have been coming up more slowly in the female than in the male. But in general there has been at least a tenfold increase in lung cancer since 1900, in the rates for age since 1910 up to the decade 1940–50. This is apparently still rising at a considerable rate, and, as I say, people are very much concerned about this problem.

Senator ANDERSON. Particularly with all the millions of dollars we are spending on cancer research. The more we study it, the worse it appears to get.

Dr. BRUES. This may be repeating my colleague slightly, but I will mention it again.

If we are to settle the question of threshold satisfactorily, I would say that we should carry out expanded studies on large populations of animals, but not rely on this to the extent of reducing the amount of basic work which will probably lead us sooner to a clear answer. I refer to many things, but chiefly studies on the nature and origin of cancer, the effects of radiation on cells, the nature of the aging process—for example, why a mouse lives little more than four score weeks and ten—and broad studies of medical and population statistics in relation to natural radiation.

Along with this are the whole unexplored fields within the medical and biological sciences, any one of which might turn out to be crucial to the radiation problem; recruiting and training good talent; and communication of scientific research findings.

As one who sits on various committees to discuss and, we hope, solve these problems, I am also impressed with the danger that more and

more of the best talent and time for the imaginative approach to these questions may be drawn away from the work and thought that they ought to be producing, into more and more debate over the same scanty knowledge.

May I just conclude with what might be a scientific parable, by pointing up the potential difficulties of the whole problem from an experiment done by the late Dr. Egon Lorenz.

Dr. Lorenz carried out the lowest-level experiment in chronic irradiation that has been done, giving mice a little over 0.1 roentgen daily throughout their life. He found, and thus confirmed an earlier experiment, that the irradiated mice developed more leukemias than those that were not irradiated, but that their average life span was almost 10 percent longer.

What a mouse would do in this case if he had a free choice, I am not sure.

Representative HOLIFIELD. Thank you very much, Dr. Brues, for your illuminating discussion.

Are there any questions?

Senator BRICKER. You would not want to conclude from that, if a human being was given 1 roentgen a day for his life, he would live 10 percent longer, would you, Dr. Brues?

Dr. BRUES. No, sir. Part of the parable was to say that I do not like to extrapolate animal experiments to man until they have reached a fairly good degree of ramification.

Senator BRICKER. I would not want to give him his free choice in that case.

Representative HOLIFIELD. The chart that Dr. Friedell gave in his statement showed that a lethal dose of 50 percent would apply to dogs, 350 roentgens; mice, 450 roentgens; monkeys, 500 roentgens. So that seems to be the nearest reaction as between your permissible dose of 400 roentgens to animals you are experimenting on. Is that right?

Dr. BRUES. That is right.

We do not know that the acute results run into the same proportions as late chronic effects.

Representative HOLIFIELD. There must have been some reason why they were in the same dose range, rather than rabbits, at 800, bacteria at 100,000 roentgens.

Dr. BRUES. The mammals do run, as far as acute kill goes, between perhaps 300 and 900. There is that degree of variation between the species, and this is just the amount that will kill them in a couple of weeks.

Representative HOLIFIELD. But you draw no parallel between setting the lethal dose of roentgens for man in that category?

Dr. BRUES. I would be afraid to.

Representative HOLIFIELD. Are there any further questions?

Thank you very much.

We will adjourn now until 2 o'clock, when we will have Dr. E. P. Cronkite, Dr. Edward Lewis, and Dr. Shields Warren as our witnesses this afternoon.

(Whereupon, at 12:45 p. m., the committee was recessed, to reconvene at 2 p. m., of the same day.)

AFTERNOON SESSION

Representative HOLIFIELD. The committee will be in order.

Our first witness this afternoon is Dr. Eugene Cronkite of the Brookhaven National Laboratory. He is a senior physician there.

At this time we will be glad to have your testimony, Doctor Cronkite.

I understand that your testimony will take up the subject of the effect on the Marshallese Islanders this afternoon.

STATEMENT OF DR. EUGENE P. CRONKITE, BROOKHAVEN NATIONAL LABORATORY[1]

Dr. CRONKITE. That is right, Mr. Holifield. With your permission I would like to state that due to duties I had in Nevada I was unable to properly proofread the prepared testimony and would like permission to do so later in the week.

In addition, since the subject material is rather extensive, I would like to submit for the record the official report on the Marshallese incident and also the 6-month, the 1-year, and the 2-year reports.

Representative HOLIFIELD. Without objection, they will be received and filed with the committee.

Dr. CRONKITE. In the prepared statement, I will just mention that the first nine pages go over the general problems of whole body radiation of man, and I am essentially in agreement with all that Dr. Friedell has said this morning.

I would like to make one comment in respect to the treatment of radiation injury that came up this morning. That is, that much has been learned from the experimental therapy of radiation injury in animals. It has been conclusively shown that protection can be afforded by the transplantation of bone marrow from one strain of animal to another. The protection afforded by transplantation of genetically specific material; that is, from one member of the same strain to an irradiated member of the same strain, is very good and long last-

[1] Born December 11, 1914, Los Angeles, Calif. Undergraduate studies, University of California at Los Angeles and Stanford University. A. B. Stanford University, California, 1936. M. D. Stanford University, School of Medicine, San Francisco, Calif., 1941. 1940–41, interne in medicine, Stanford University Hospitals, San Francisco, Calif. 1941–42, assistant resident in medicine, Stanford University Hospitals, San Francisco, Calif. May 1942, commissioned lieutenant (junior grade), Medical Corps, United States Navy. 1943, under instruction in general surgery, U. S. Naval Hospital, Naval Operating Base, Norfolk, Va., under J. M. Deaver. 1943–44, medical officer, Third Marine Aircraft Wing, FMF, U. S. Marine Corps Air Station, Cherry Point, N. C., and U. S. Marine Corps Air Facility, Walnut Ridge, Ark. 1945, medical officer, U. S. S. *Sylvania* AKA 44. 1946–54, head, Hematology Division, Naval Medical Research Institute, Bethesda, Md. 1946, participated as hematologist, Operation Crossroads, atomic bomb field tests, Bikini, Marshall Islands. 1950–51, project officer, Operation Crossroads, biological effectiveness of atomic bomb gamma radiation and hematological studies. 1952, assistant project officer, biological effectiveness of neutrons, Operation Tumbler-Snapper, Nevada Proving Ground. 1953, director biomedical program, Operation Upshort/knothole, civil effects test group. 1954, Operation Castle, project officer for the study of accidental human radiation casualties. Recipient Sir Henry Wellcome Prize in Military Medicine, 1949. Secretary of the Army Commendation for work, Operation Castel, 1954. Member of Phi Beta Kappa 1937; Olpha Omega Olpha 1940; Sigma Xi 1948; Fellow of the American Medical Association: American Association for the Advancement of Science Society for Experimental Biology and Medicine; American Federation of Clinical Research; International Society of Hematology; American Physiological Society; New York Academy of Science; American Society for Clinical Investigation. Resigned from the United States Navy September 30, 1954, as commander, Medical Corps, United States Navy. Presently senior physician, head of Division of Experimental Pathology, Medical Department, Brookhaven National Laboratory. Member National Research Council civil defense committee. Member of National Academy of Sciences panel to evaluate the acute and chronic effects of atomic radiation on man. Chairman of National Academy of Sciences subpanel of acute and chronic hematologic effects of atomic radiation on man. Member of Advisory Committee Atomic Bomb Casualty Commission. (Submitted by witness.)

ing. If the material for transplantation has its source in another strain of mouse, the protection is less marked or not as long lasting. If the protective material comes from another species of animal, the protection is very short lived and not nearly as effective. In principle, the transplantation of bone marrow would significantly increase the survival rate of exposed human beings to doses of radiation that would be uniformly fatal. The amounts of bone marrow needed are large, and the mongrel nature of man makes it unlikely that very much could be expected in the way of long-term protective effect. In my opinion it would be the worst type of wishful thinking to expect that one could have an effective bone-marrow bank in the case of an atomic catastrophe. Much work is yet to be done under carefully controlled clinical conditions before one could be optimistic about the use of this procedure in man under highly controlled conditions in an individual patient, let alone under conditions of a nuclear catastrophe.

With this general statement I would also like to state that certainly in human beings exposed in the midlethal dose of radiation, the clinical picture of which is very similar to that produced by depression of bone marrow due to various drugs and so on, one would expect that antibiotics and judicious use of blood transfusions would be most helpful in increasing the survival rate in the midlethal range but not in the range in which spontaneous survival is not likely at the present time.

With these general comments I would like to go to the prepared statement.

I have been asked to summarize the early effects of exposure of animals and man to external radiation with particular reference to the effects of fallout radiation on the Marshallese, the Los Alamos accident, and radium. In addition, I have been asked to comment on the beta burns in the Marshallese, and other examples of beta burns. Since my personal experience is limited to the Marshallese and animal experimentation, I shall limit myself to these and supply reference material for the others.

It is quite impossible to cover all of this material in a reasonable period of time, so I shall concentrate upon the effects of exposure to external radiation on animals and man with a clinical description of the syndrome of radiation sickness as a function of dose of radiation and highlight the discussion with illustrative material collected in the study of the Marshallese (reference 1).

My prepared statement includes numerous references and further material that time will not permit discussion of at length here.

Radiation syndromes vary as a function of the type of exposure, the dose, and the time after exposure to radiation. In general radiation injuries can be divided into three general classes:

(*a*) The syndromes of whole body radiation injury produced by penetrating ionizing radiation which are dose and time dependent.

(*b*) Superficial radiation burns produced by soft radiations—beta and low energy x or gamma radiations.

(*c*) Radiation injury produced by the deposition of radionuclides within the body.

In the latter case the clinical picture varies with the site and amount of deposition.

Each of the above is associated with an early phase in which acute symptoms and signs may be observed, and a late phase in which chronic changes or manifestations such as cancer may be observed.

I wish to emphasize also that the degree of injury and the clinical manifestations are proportionate to the dose. This is particularly true of the syndromes of whole body radiation. The latter is, and I repeat, highly dependent on dose and time after exposure. There is no simple description. The problem is subtle and complex and one must always bear dose and time in mind.

THE SYNDROMES FROM TOTAL BODY PENETRATING RADIATIONS

The dose dependent syndromes resulting from total body exposure in the mammal have been described in detail, and I shall only summarize them here. For further details one is referred to references 1 to 12.

After large doses—approximately 6,000 r or more—the central nervous system syndrome, which can be abbreviated CNS, is produced. Death may occur under the beam while being irradiated or after some hours. The clinical picture is characterized by hyperexcitability, disorders of equilibrium, incoordination, respiratory distress and intermittent stupor. Convulsions may preceed death. Doses capable of producing this syndrome are always uniformly fatal If an occasional animal, and presumably man, survives this CNS he has yet to experience the gastrointestinal syndrome (GIS), which when produced by doses in excess of 1500 r is always fatal within 3 to 9 days for mammals. Presumably man also will respond in a comparable manner as laboratory animals.

The GIS is so named because of the marked nausea, vomiting, diarrhea, and denudation of the lining of the small bowel. The GIS is a uniformly fatal syndrome in most laboratory animals. If the short duration GIS of a few hours resulting from lower doses, does not produce the 3- to 9-day death, the survivors of this syndrome have yet to experience the sequelae of bone marrow depression which has been termed the hemopoietic syndrome (HS). The HS is not necessarily fatal. It is the clinical picture that is seen in the lethal range for all mammals and in general the 50 percent lethal dose values reported, represent the LD^{50} for the sequela of hemopoietic depression—namely granulocytopenia, depressed defenses against infection, thrombopenia, and anemia with the possible resulting infections, diffuse purpurpa, and hypoxia due to anemia, any one of which may be fatal. Many detailed descriptions of the pathogenesis of these phenomena have been published (references 1–4, and 12–16).

The above picture of radiation syndromes is based on animal experimentation; however, human experience, particularly as observed at Hiroshima and Nagasaki and in the Marshallese natives has indicated that man probably corresponds quite closely to the general mammalian response outlined above with the exception of some differences in time of occurrence. The CNS apparently was not observed by the Japanese at Hiroshima and Nagasaki. One would not expect it to have been observed since doses to produce this syndrome were well within the area of almost total destruction.

The GIS with deaths in the first week are well documented clinically and pathologically as are deaths from the HS. However, in the

case of man, deaths from infection were most prevalent in the second to fourth weeks—maximum incidence during the third week—and from hemorrhagic phenomena in the third to the sixth weeks—maximum incidence in the fourth week.

In the Japanese, after the bombing of Hiroshima and Nagasaki, deaths from radiation injuries were occurring as late as the seventh week. This is in contrast to other animals where deaths from the acute phase are uncommon after the 30th day.

PROBABILITY OF SURVIVAL AS RELATED TO SYMPTOMS

Predictions of the effects of various doses of gamma radiation are essential to military and civil defense planning. If the absolute sensitivity of man to radiation were known, and if it were practical to determine the dose to groups under catastrophic conditions, a realistic statistical prognosis could be made. However, these predictions cannot be made accurately at the present time. Problems involved with estimation of dose received by the individual, present real practical difficulties. It is probable that dose estimates will be available from dosimetry devices or from dose contour lines, and the position of individuals during exposure.

The difficulties of relying heavily on the dose estimates for a single individual are apparent. The exact position of the individual and the degree of shielding will not be known precisely. The dosimetry device records the dose or dose rate, which may not reflect accurately, because of shielding and energy dependence of the device, the deposition of radiant energy at the site of interest, namely the bone marrow and the lining of the intestinal tract.

The problems associated with predicting medical effects from physical estimation of dose can be further illustrated as follows:

It is apparent that dose estimates available will be air doses. The dose received by the air is of academic interest only, since the degree of effect in living things is due to the deposition of energy and its distribution in the critical tissues. Accordingly one must know the depth dose pattern or more precisely the pattern of absorbed dose in sensitive critical tissues.

This problem has been explored experimentally at great length and is described in detail in reference 17.

More need not be said here other than the fact that the uniform field of radiation coming at an individual from all directions is more effective per *r* in air than dose of radiation of directional quality. In urban areas there may be shielding from the fallout field by buildings or inhomogeneities in the field due to drift in dry windy areas.

With the prompt radiation shielding may be significant and the proximity of large buildings may seriously influence scatter and perturb the uniformity of the radiation.

Lastly a fallout field may be made undirectional by heavy shielding from buildings on one side. Thus estimates of dose calculated from dose rates or derived from an integrating type of dosimeter that adds up to the total dose received, or from position of an individual during exposure, cannot be accepted as the best index of the probable fate of this individual, or as the final guide to treatment. The physician is interested in the individual from the standpoint of sorting casualties with some chance of survival and those for whom nothing can be done,

and of equal importance the troop commander is interested in the overall prognosis of large groups of individuals in order to make plans for disposition and replacement. These two different desires cannot be completely met by physical estimates of dose. However, nature has been helpful.

The manifestations of radiation injury vary with the amount of radiation received. In other words, the symptoms of the individual or groups of individuals becomes in a sense a personal indicator of one's own fate.

Experience with human radiation injury in Hiroshima and Nagasaki (references 4, 5, 10, and 11) with reactor accidents (references 3, 6, 9) and the fallout accident (references 1 and 18) to be described later, strongly suggest that the best index of the seriousness of exposure of the individual is the symptomatology, in addition to the physical estimate of dose.

Radiation injury is complex and subtle, and the manifestations of the injury vary considerably. In a general sense, individuals exposed in the lethal range—here some, but not all, will die in the first several weeks following exposure—can be divided according to symptoms and signs, into groups having a different prognosis. Thus they may be divided into three groups in which survival is, respectively, improbable, possible, and probable. It will be apparent that there is no sharp line of demarcation among the groups. The distinguishing features are severity and persistence of vomiting and diarrhea.

GROUP I SURVIVAL IMPROBABLE

If vomiting occurs promptly or within a few hours, and continues and is followed in rapid succession by prostration, diarrhea, anorexia and fever, the prognosis is grave: Death will almost definitely occur in 100 percent of the individuals within the first week. There is no known therapy for these people; accordingly in a catastrophe attention should be devoted principally to others for whom there is some hope or in whom therapy is clearly indicated.

GROUP II SURVIVAL POSSIBLE

Vomiting may occur early but will be of relatively short duration followed by a period of well-being. In this period of well-being marked changes are taking place in the hemopoietic tissues. Lymphocytes are profoundly depressed within hours and remain so for months. The neutrophil count is depressed to low levels, the degree and time of maximum depression depending upon the dose. Signs of infection may be seen when the total neutrophil count has reached virtually zero—7 to 9 days. The platelet count may reach very low levels after 2 weeks. External evidence of bleeding may occur within 2 to 4 weeks.

This group represents the lethal-dose range in the classical pharmacologic sense. In this group the symptom-free period—the latent period—lasts from 1 to 3 weeks, with little clinical evidence of injuries other than slight fatigue.

At the termination of the latent period, the patient may develop purpura, which is bleeding into skin, epilation oral and cutaneous lesions, infections of wounds or burns, diarrhea, or melena—black stools

from digested blood. The mortality will be significant. With therapy the survival time can be expected to be prolonged, and if sufficient time is provided for bone-marrow regeneration, the survival rate will be increased.

In group I, survival improbable, and group II, survival possible, the blood picture is not as well documented as in group III, survival probable. There are good clinical reasons to believe that in the lethal range the granulocyte depressions will be marked and below 1,000 per cubic millimeter during the second week. Observations made in Japan confirm this contention.

However, in the sublethal range it takes much longer for the granulocyte and platelet count of man to reach minimal values, as compared to other mammals. Despite the chaotic conditions that existed in Hiroshima, the data of Kikuchi and Wakisaka (reference 11) shows that there was a more rapid and marked decrease in group I, survival improbable, and group II, survival possible, than in group III, survival probable. Before going on to group III, survival probable, I cannot refrain from a comment on therapy.

Much has been learned from the experimental therapy of radiation injury in animals. It has been conclusively shown that protection can be afforded by the transplantation of bone marrow from one strain of animal to another. The protection afforded by transplantation of genetically specific material, that is from one member of the same strain to an irradiated member of the same strain, is very good and long lasting. If the material for transplantation has its source in another strain of mouse, the protection is less marked and not as long lasting. If the protective material comes from another species of animal the protection is very short lived and not nearly as effective. In principle the transplantation of bone marrow would significantly increase the survival rate of exposed human beings in the group II, survival possible, and possible to a lesser extent in the group I, survival improbable, casualties.

The amounts of bone marrow needed are large, and the mongrel nature of man makes it unlikely that very much could be expected in the way of long term protective effect. In my opinion it would be the worst type of wishful thinking to expect that one could have an effective bone-marrow bank in the case of an atomic catastrophe.

Much work is yet to be done under carefully controlled clinical conditions before one could be optimistic about the use of this procedure in man under highly controlled conditions, for an individual patient, let alone under conditions of a nuclear catastrophe.

However, the treatment of group II casualties is not at all hopeless. There is ample clinical experience in conditions where the bone marrow is severely depressed and in which there are inadequate numbers of circulating cells.

In these relatively common clinical conditions produced by sensitivity to drugs, or occurring naturally as disease processes, the combined use of the wide-spectrum antibiotics that are now available, and the judicious use of blood transfusions significantly increases the survival time of the individuals, thus giving nature a longer period of time to repair spontaneously the damage that has been suffered. Accordingly, one could feel optimistic in respect to increasing significantly the survival rate of the group II, survival possible, casualties by widespread controlled use of antibiotics. Blood transfusions would

be helpful to a limited extent for anemia. The probability of availability of enough blood for burns and other injuries is low. Hence, when blood may be needed for radiation injury, supply may be exhausted. Preparation and stockpiling for such an emergency is obviously required.

GROUP III SURVIVAL PROBABLE

This group consists of individuals who may or may not have had fleeting nausea and vomiting on the day of exposure. In this group there is no further evidence of effects of the exposure except the hematologic—blood—changes that can be detected by serial studies of the blood with particular reference to lymphocytes and platelets. The lymphocytes reach low levels early, within 48 hours, and may show little evidence of recovery for many months after exposure. The granulocytes may show some depression during the second and third week. However, considerable variation is encountered. The late fall in the granulocytes, during the sixth or seventh week, may occur and should be watched for. Platelet counts reach lowest levels on approximately the 30th day at the time when maximum bleeding was observed in Japanese who were exposed at Hiroshima and Nagasaki. This time trend in the platelet count and the development of hemorrhage is in marked contrast to that seen in laboratory animals where platelets reach their lowest levels between the 10th and 15th days and hemorrhage occurs shortly thereafter.

In this group individuals with neutrophil counts below 1,000 per cubic millimeter may be completely asymptomatic. Likewise, patients with platelet counts of 75,000 per cubic millimeters or less may show no external signs of bleeding.

It is well known that all defenses against infection are lowered, even by sublethal doses of radiation, and thus, patients with severe hematologic depression should be kept under close observation and administered appropriate therapy as indicated. There is reasonably good animal experimentation to indicate that sublethally exposed colonies of animals are more susceptible to endemic and epidemic infection.

The numbers of individuals in group III—survival probable—will be greater than in group II—survival possible—and the number in group II will be greater than in group I—survival improbable. Group I casualties will be helplessly injured. Group II casualties will be able to help in their own care to a limited extent. Group III casualties will be useful and a moderate amount of work will not be harmful. No therapy other than observation is needed for this group.

The rest of my comments will be focused on the fallout accident that occurred on March 1, 1954.

Following detonation, unexpected changes in the wind structure deposited radioactive materials on inhabited atolls and on ships of Joint Task Force 7, which was conducting the tests.

Radiation surveys of the areas revealed injurious radiation levels; therefore evacuation was ordered, and was carried out as quickly as possible with the facilities available. Although the estimated accumulated doses to human beings were believed to be below dangerous levels that would produce lasting injury or mortality, the commander of the task force requested assistance of the Department of Defense and the United States Atomic Energy Commission. A medical team was requested which would be organized to provide the best possible

care of the exposed persons and to make a medical study of the exposures. The responsibility for organization of the medical team was shared between the Armed Forces special weapons project of the Department of Defense, and the Division of Biology and Medicine of the Atomic Energy Commission.

Since speed was essential, and since the United States Navy Medical Department had experienced personnel available at the Naval Medical Research Institute and the United States Naval Radiological Defense Laboratory, the Surgeon General of the Department of the Navy was requested to provide assistance.

He promptly complied, and directed the organization of a team from the two above-mentioned laboratories. I had the privilege to be the director of this team.

Within a period of 3 days, equipment was assembled and packed, and the team was airlifted to the Marshall Islands, arriving on the eighth day after the explosion.

The interim care and study of the exposed individuals had been ably taken care of by the limited medical facilities of the United States Naval Station, Kwajalein. I am pleased to call attention to the fact of the very high degree of cooperation between all Government agencies concerned and to the numerous individuals who selflessly gave of their time and efforts. The number is large, and due credit and acknowledgments are given in the official report of the incidence published by the United States Government Printing Office, and listed in reference 1.

NATURE OF THE EVENT AND DESCRIPTION OF THE EXPOSED GROUPS

The radioactive material fell on the inhabited atolls of Rongelap, the heaviest dose; on Ailinginae; on Rongerik where American servicemen were stationed, and Utirik where the smallest dose was received, but by the largest number of people. The Marshallese were living under relatively primitive conditions in lightly constructed palm houses.

The American military personnel had the second highest exposure. They were more aware of the significance of the fallout than where the Marshallese, and promptly put on additional clothing to protect their skin. As far as duties would permit, they remained inside of aluminum buildings. In contrast to this the Marshallese in general remained outside, and accordingly were more heavily contaminated by the material falling upon the atoll and upon them.

All of the exposed human beings were evacuated by air and surface transportation to the United States naval station, Kwajalein, as promptly as facilities would permit. Since a survey of the individuals showed that there was significant contamination of the skin, clothes, and hair, the clothes were removed and laundered and repeated washings of the skin and hair were carried out with fresh water and soap. The hair of the Marshallese was decontaminated with difficulty because of the heavy coconut-oil hair dressing they used.

On Rongelap there were 64 individuals that received an estimated dose of 175 r. On Airlinginae there were 18 individuals receiving approximately 69 r. On Rongerik there were 28 American servicemen receiving approximately 78 r. on Utirik there were 157 individuals receiving approximately 14 r.

Senator ANDERSON. Where do you get those figures, Doctor?

Dr. CRONKITE. I will come to that in the next section. I will discuss how the doses were arrived at.

Senator ANDERSON. We heard it suggested this morning that lots of figures are not too reliable. I am wondering if you had a way of measuring this so you could be fairly sure of these figures.

Dr. CRONKITE. I will come to this in the next section and discuss the reliability of the dose estimates and the various variables that go into it.

Senator ANDERSON. Thank you.

WHOLE BODY GAMMA DOSES

Dr. CRONKITE. The determination of the whole body gamma doses are dependent upon the surveys that were made with calibrated instruments approximately 3 feet above the ground several days after the inhabitants were evacuated. In addition certain assumptions had to be made about the arrival time of the cloud and the rate of fallout of the material. Only on Rongerik where there was a recording dosimeter is arrival time known precisely. The dose rate of the continuing fallout of material was in part neutralized by the progressive radioactive decay. In addition the transit dose from the cloud passing over the atolls could not be estimated. All of these variables were taken into account and the doses calculated. These doses were consistent with the doses that were actually measured on Rongerik by film that was stored in refrigerators and by film exposed outside on this atoll.

In view of this internal consistency it is believed that the dose of calculated radiation on the atolls is reasonably accurate. Details of the calculation of the dose are in the official report which discusses in detail the probable range in values (reference 1, ch. 1).

CHARACTERISTICS OF THE GAMMA RADIATION

The fallout material when deposited on the ground formed a large planar source of radiation. The energy distribution of the radiation reaching an exposed individual is influenced by its passage through the intervening air. A knowledge of the inherent gamma spectrum as it eminates from the material itself is essential in order to determine the spectrum that impinges upon exposed individuals.

When one takes into account the spectometric data on the mixed fission products and the degredation by Compton scattering along the path in air, a dose energy histogram can be constructed, showing that there are roughly 3 regions with maxima at 100, 700, and 1500 Kev. The total exposure is thus the resultant effect of partial doses from each energy region, making the exposure energy condition significantly different from those of radiation therapy, experimental biology, or from the prompt gamma radiation of the bomb.

Details of the characteristics of the exposure are discussed in reference (reference 1, ch. 1).

Actually the overall effect of the geometry and spectrum is to produce a very uniform deposition of energy throughout the body so that per roentgen in air fallout radiation is relatively more effective than the prompt radiation from the bomb or the radiation from an X-ray tube.

THE CHARACTERISTICS OF THE FALLOUT MATERIAL

The fallout material consisted predominantly of flakes of calcium oxide resulting from the incineration of the coral. Upon the flakes of calcium oxide fission products were deposited. At Rongelap Atoll the material was visible and described as snowlike. It stuck to the skin, adhered to the air and clothes, the vegetation, and the habitations.

Senator ANDERSON. That is what they talked about with respect to the Japanese who were in the fishing boat.

Dr. CRONKITE. Yes, sir.

Senator ANDERSON. They had this white fallout that they thought was some sort of manifestation from heaven and would not wash off for a while, and suffered as a consequence. You are describing the same sort of thing that happened down there.

Dr. CRONKITE. They were in approximately the same or a comparable position as the Rongelap natives and experienced very closely the same thing, except in their case working with their fishlines, and so on, grinding the material into their hands, they got worse skin burns than the Marshallese.

Senator ANDERSON. Thank you.

GEOMETRY OF THE EXPOSURE

Dr. CRONKITE. Time does not permit a discussion of the effect of this, but it has been alluded to earlier and details of the influence of geometry of the exposure to biologic effect are in references 1 and 17.

SUPERFICIAL DOSES OF RADIATION FROM BETA AND SOFT GAMMA RADIATION

There is no doubt that the dose of radiation to the first few millimeters of the skin is substantially higher than that at the midline of the body from the more penetrating gamma component. Problems concerned with the estimation of the dose of radiation to the skin are discussed in detail in reference 1, chapter 1.

To arrive at some physical estimate of the skin dose, an attempt must be made to add up the contributions of the penetrating gamma, the less penetrating gamma, the beta bath to which the individuals were exposed from the relatively uniform deposition of fission products in the environment and the point contact source of material deposited on the skin. By all means the largest component of skin irradiation resulted from the spotty local deposits of fallout material on exposed surfaces of the body.

To put it in reverse, the individuals who remained inside had no skin burn. It was only on those on whom the material was directly deposited on the skin that received burns.

It is completely impossible to estimate the dose from material that was deposited on the skin. The relative hazard of the beta path is discussed in detail in the previously mentioned reference 1.

CLINICAL OBSERVATIONS AND TREATMENT: SYMPTOMS AND SIGNS RELATED TO RADIATION INJURY

Itching and burning of the skin occurred in 28 percent of the people on Rongelap, 20 percent of the group on Ailinginae, and 5 percent of

the Americans. There were no symptoms referable to the skin in the individuals on Utirik. In addition to the itching of the skin there was burning of the eyes and lacrimation in people on Rongelap and Ailinginae. It is probable that these initial skin symptoms were due to irradiation since all individuals who experienced the initial symptoms later developed unquestioned radiation-induced skin lesions that will be described in detail later. It is is possible however, that the intensely alkaline nature of the calcium oxide when dissolved in perspiration might have contributed to the initial symptoms.

About two-thirds of the Rongelap group were nauseated during the first 2 days, and one-tenth vomited and had diarrhea. One person in the Ailinginae group was nauseated. No one in the Rongerik or Utirik group, or Americans, had gastrointestinal symptoms.

CLINICAL OBSERVATIONS AND LEUKOCYTE COUNTS

Between the 33d and 43d post exposure day, 10 percent of the individuals from Roneglap had an absolute granulocyte level of 1,000 per cubic millimeter or less. The lowest count during this period was 700 per cubic millimeter.

Representative HOLIFIELD. How does that compare with the normal?

Dr. CRONKITE. The normal count would be approximately 5,000 to 6,000 in American population. They were very seriously depressed at this time.

Representative HOLIFIELD. This was with an average of around what?

Dr. CRONKITE. 175 roentgens. I am sorry I did not mention it earlier. I am limiting my comments to the highest dose group. The time sequence of events in the other groups was similar but just to a less extent more or less proportionate to the decrease in dose received.

During this interval the advisability of prophylactic administration of antibiotics was seriously considered. However, prophylactic administration of antibiotics was not instituted for the following reasons:

(1) All individuals were under continuous medical observation so that infection, if it developed, would have been discovered in its earlier stages.

(2) Premature administration of antibiotics might have obscured medical indications for treatment, and might also have led to the development of drum resistant organisms in individuals with lowered resistance to bacterial infection.

(3) There was no accurate knowledge of the number of granulocytes requires by man to prevent infection with this type of granuloctyopenia as occurred in the Marshallese.

The observed situation was not strictly comparable to agranulocytosis with an aplastic marrow as seen following known lethal doses of radiation. In the latter instant, granulocytes fall rapidly with practically none in the circulation and no evidence of granulocyte regeneration when infection occurs. In the present group of individuals exposed to radiation, most counts reached approximately one-fourth the normal value, but the fall to that level was gradual and the presence of immature granulocytes in the peripheral blood during the period of granulocytopenia was indicative of some new granulocyte production. In other words, the bone marrow had not been completely eradicated by the dose of radiation received.

The few individuals that received antibiotics had conditions that would have been treated with antibiotics in the absence of any previous exposure to irradiation. During the fourth and fifth exposure weeks an epidemic of upper respiratory infection occurred. The respiratory infection consisted of moderate malaise, pharyngitis with prominent lymphoid follicles, fever during the first day, and a purulent nasal and tracheal discharge for about 10 days.

It was of interest to determine whether this respiratory infection could be correlated with the dose of radiation received or changes in the leukocyte count. There was no correlation. The respiratory infection in the medical personnel involved in the care and study of the irradiated individuals was similar in incidence and severity.

Earlier today Doctor Friedell commented upon platelets, and these were followed very carefully in the Marshallese.

CLINICAL OBSERVATIONS AND PLATELET COUNTS

Eleven individuals had platelet counts that fell as low as 35,000 to 65,000/mm.^{3}. All individuals with platelet counts less than 100,000 per mm.^{3} were examined daily for evidence of hemorrhage into the skin, mucous membranes and retina. Urine was examined daily for red cells and albumin. Women were questioned concerning excessive menstruation. The only evidence for any undue bleeding were two women who menstruated profusely at the time of their maximum platelet depression. It was not sufficient to cause them undue concern and subsided without any specific treatment.

THE EFFECTS ON PREGNANCY

Four women in the Rongelap group were pregnant when brought to Kwajalein. Two were in the first trimester, one in the second trimester and one in the third trimester. There were no abnormal symptoms referable to pregnancy. As far as could be determined the pregnancy continued in the normal fashion.

In the Ailinginae group of 69 r, one woman was in the second trimester. Fetal movements were unaffected in the individual in the third trimester. The pregnant women had a marked depression of platelet counts but at no time was there any vaginal bleeding. At the 12-month reexamination of the above women, all had delivered. One baby was born dead; the others were normal.

In the case of the one stillborn, irradiation occurred to the mother either before conception or early in the first trimester. It is possible that the irradiation may have contributed but there is no way to prove this.

SPECIAL EXAMINATION OF EYES

At all followup examinations an ophthalomogist has examined the eyes of all individuals. To date no lesions ascribable to ionizing radiation have been found. Similar studies have been made on the eyes of nonexposed Marshallese and the incidence of eye lesions is identical in the two groups.

SKIN LESIONS AND EPILATION

As mentioned earlier there was burning of the skin. On first examination by the medical team on the ninth post exposure day the ex-

posed people appeared to be in good health and the skin was definitely normal in external appearance. Evidence for the development of skin lesions commenced approximately 2 weeks after exposure.

During the early stages of development of the lesions, itching, burning and slight pain were experienced with the more superficial lesions. With deeper lesions the pain was more severe. The deeper foot lesions were the most painful and caused some of the people to walk on their heels for several days during the acute stages. Some of the more severe lesions of the neck and axillae were painful. There were no constitutional symptoms associated with the skin lesions.

The characteristic sequence of events in the development of the lesions was the occurrence of symptoms, then of black pigmented areas, small in size, which grew larger in size and coalesced. Later the skin began to shed from the inside of the pigmented plaques to the outside, and in some cases resulted in the production of large depigmented areas. In most of the lesions the shedding was limited to the superficial layers of the skin. In some the process continued with the development of superficial ulcers. A few became infected.

The appearance of these skin burns can best be illustrated by referred to chapter III of reference 1 where Kodachrome pictures illustrate the sequence of events.

In addition to the skin burns, loss of hair, spotty in nature, occurred in some of the individuals. The hair grew in again with normal color and texture and the regrowth was complete in all except possibly one middle-aged man in whom it came in somewhat sparsely. Small pieces of skin were removed surgically from some of the burned areas for microscopic study. These pieces of skin demonstrate the typical findings of radiation injury. Some of the skin burns became infected, particularly those on the feet, and were treated locally by cleansing and applications of antibiotic ointments. The skin burns healed in most cases with return of normal color and texture of the skin, and in some cases scars were left with depigmented areas.

The worst burn occurred on the back of the ear of a middle aged man. It produced a permanent scar with absence of pigment and abnormal blood vessels and a slight horny growth of the overlying skin has developed. The skin has been carefully observed at 6 months, 12 months, 2 years, and 3 years after exposure, and there is no evidence at the present time of any breakdown in the early burns of the skin. There is no evidence of the development of cancer at this time. In some the depigmented scars are still evident. The individuals have been seen on two occasions by a plastic surgeon, Dr. Bradford Cannon, of the Harvard Medical School, who feels that no plastic repair is necessary and that the prognosis in general is good.

FACTORS INFLUENCING SEVERITY OF THE LESIONS

Certain lessons were learned from the Marshallese experience.

Burns were caused by direct contact of the radioactive material with the skin. The perspiration as common in the tropics, the delay in decontamination and the difficulties in decontamination certainly favored the development of the skin burns. Those individuals who remained indoors or under trees during the fallout developed less severe skin burns. The children who went wading in the ocean devel-

oped fewer lesions of the feet and most of the Americans who were more aware of the dangers of the fallout, took shelter in aluminum buildings and bathed and changed clothes. Consequently they developed only very mild beta burns.

Lastly, a single layer of cotton material offered almost complete protection, as was demonstrated by the fact that skin burns developed almost entirely on the exposed parts of the body.

The prognosis of beta skin burns and radiation burns of the skin is excellently described in chapter III of reference 1.

HEMATOLOGIC OBSERVATIONS

It is generally considered that changes in the blood are the most sensitive biologic indexes of exposure of living human beings to radiation. Accordingly extensive simple hematologic studies were performed on the Marshallese. Since there were no previous hematologic studies on the exposed Marshallese, it was necessary to set up control groups of nonexposed Marshallese of the same age and sex distribution for comparative purposes.

I shall restrict my comments to the findings in the group from Rongelap since the temporal sequence of events are identical in all of the exposed groups. Of course the depression was less marked in the less severely exposed groups.

NEUTROPHILE COUNT

The absolute neutrophile count of both the younger and older age groups fell during the second week to a value approximating 70 to 80 percent of that of the controls. Following the depression there was an oscillation roughly around the control value until about the 30th postexposure day at which time there was a progressive decrease in the blood count with minimum values being attained around the 45th day after exposure. It is of interest that the depression in the children less than 5 years of age was greater than in the individuals who were greater than 5 years of age.

Following this maximal depression there was a slow return of the neutrophile counts toward normal. However, at 6 months they were still depressed. At 1 year and 2 years the neutrophile counts were back to the control level. However, at 3 years there was a drop in the absolute mean neutrophile count but this also occurred in the control population. It is not known whether lower counts represent a population trend as has been noted in the Japanese for both irradiated and nonirradiated populations, or whether it is merely a statistical fluctuation that is to be expected in this type of study. More work is necessary on this point.

LYMPHOCYTE COUNT

By 3 days the lymphocytes dropped to 50 percent of the controls. The percent drop in the children less than 5 years of age was greater than that of the people older than 5 years. The lymphocyte count remained at approximately the same level through the exposure period. At 6 months, 12 months, 2 years, and 3 years, the level, though increasing, had not quite reached that of the control population.

PLATELETS

The maximum depression in platelets was obtained approximately 28 to 30 days after exposure in contrast to laboratory animals that attain their minimum values between the 10th and 15th days after exposure. In this case the children under 10 years of age had a greater percentage drop than those who were older. The platelets began to recover after the 30th day, attain a maximum about the 45th day.

There was then a secondary drop with a leveling off for the remainder of the postexposure period, and at 6 months, 12 months, 2 years, and 3 years, slow recovery was still underway. The levels of the population were approaching the controls but have not yet reached it.

In all of the hematologic studies mentioned above, it is stated that the present levels are not equal to that of the control population. However, I wish to emphasize that the current levels of the blood cells of all types is more than adequate to take care of the infections and the various troubles of everyday existence. This statistical expression of an inadequate recovery probably represents the residual radiation injury that is of considerable interest to study but does not appear to be overtly harmful to the individuals. One can be reasonably confident in this because they are not faring less well in resistance to disease than are the Marshallese who were nonexposed and living in the same area.

INTERNAL ABSORPTION OF RADIONUCLIDES

During the 2 days before evacuation, the Rongelap people lived under conditions of extreme contamination without any concerted efforts to protect themselves against the dangers of internal contamination. These individuals drank contaminated water, and ate their natural foodstuffs which were contaminated externally. Their hands were contaminated; they inhaled and obviously ingested certain indeterminate amounts of material.

The body burdens of isotopes in these individuals was evaluated by radiochemical analysis of the urine of the exposed people and assisted by studies on swine. These swine were removed from the island at a later date. The urinary and fecal excretion was studied and ultimately the animals were killed. Extensive radiochemical analyses were made of their entire bodies. By comparison, approximations of body burdens of radionuclides was made. From a combination of urinary excretion and animal studies estimates were made of the probable body burden.

Rare and alkaline earths accounted for about 70 percent of the urine activity. Strontium 89 was about at the maximum permissible level at 1 day. Iodine 131 and other members of the iodine family which had to be present early, resulted in a dose of radiation to the thyroid glands, estimated between 100 and 150 rep. To this of course, must be added the penetrating external gamma component. By 6 months radiation was barely detectable in the urine. At 2 years from analysis of pooled urine samples and individual samples, very tiny amounts of strontium 90, calcium 45, praseodymium and cesium were present. Studies were performed both at United States Naval Radiological Defense Laboratory, and Walter Reed Army Medical Center.

The results of the 3-year radiochemical analysis of the urines that were recently collected are not completed as yet.

It was believed that the body burdens of these people was very low and probably biologically insignificant. However it was decided to bring some of the individuals to the United States for study with the total body gamma counter at the Argonne National Laboratory. This decision was made not because of any fear but because the analysis of the urine and the animal analysis were an indirect means to obtain probable body burdens.

It was obviously desirable to obtain a firm direct measurement of the body burden from the scientific standpoint and to determine the precise body burdens. Four individuals from the Rongelap group, 2 from the Utirik group, and 1 control Marshallese—a total of 7—were brought to the United States and taken to the Argonne National Laboratory. There, under the direction of Doctors Marinelli, Rose, and Miller, the total body gamma activity was measured. The results are yet incomplete and have to be analyzed further. It was found that the exposed Marshallese had counts that were higher than nonexposed peoples in the United States. However, the values were far below the current permissible levels.

Since there has been some misunderstanding in the press about children being brought to the United States for study, I would like to state that all the individuals brought to the United States were adults, with the exception of one 16-year-old boy. They have subsequently been returned to the Marshall Islands.

THE CONTINUING STUDY OF THE MARSHALLESE

My associate in the Medical Department of Brookhaven National Laboratory, Dr. Robert A. Conard, a member of the original team that took care of and studied the Marshallese, and director of the 2- and 3-year surveys, has retained an abiding interest in the Marshallese. On behalf of the Atomic Energy Commission and Brookhaven National Laboratory, he has undertaken the continuing responsibility of yearly surveys of these people. These surveys are being made possible by the cooperation of the Medical Department of the United States Navy and its activities, the Medical Research Institute at Bethesda, Md., and the United States Naval Radiological Defense Laboratory in San Francisco. The continuing project is a joint effort directed by Dr. Conard and participated in by the Medical Department of Brookhaven National Laboratory, the two Navy institutions mentioned earlier, and interested physicians and scientists of various American universities and medical schools. The probabilities of getting a good scientific followup are excellent.

One cannot leave this tremendously important subject of fallout and the unfortunate accident that occurred in the Marshall Islands in 1954 without the frank recognition that late effects of ionizing radiation are possible. Many late effects have been observed in man and in animals. These are condensed in detail in the National Academy of Sciences report (reference 8). Accordingly, a search for late effects is an essential part of the continuing survey.

A summary of the 3-year status of these people, which will be reported in detail in reference 22, now being prepared, follows:

FERTILITY

Effect of radiation exposure on fertility is difficult to assess in the Marshallese. If there has been any effect on fertility, it must have been very short lived, since pregnancies are occurring normally and at rates similar to other groups of Marshallese.

PREGNANCY

There has been no apparent effect of radiation on the course of pregnancy in the Marshallese. Since the delivery of the 4 women who were pregnant at the time of the event, there have been 12 pregnancies which have terminated. Ten of these terminated normally, one terminated in a stillbirth, and one baby died several hours after birth, apparently of an infection of the cord. The lack of vital statistics makes this data difficult to interpret. However, it does not appear that this incidence of stillbirths is greater than that of other comparable native groups in the mid-Pacific area.

EFFECTS ON THE FETUS

The three babies irradiated in utero have not shown any abnormalities such as was observed in some of the Japanese babies irradiated in utero. For example, microcephaly.

GROWTH AND DEVELOPMENT

On each resurvey the expcsed and control children have been matched for age and sex. Measurements on growth and development have been carried out. Anthropometric measurements have been incompletely analyzed as yet. Since the numbers of children are small, the data is not easily subjected to statistical analysis. There were 17 children less than 7 years of age and 24 less than 16 years of age at the time of exposure. However, there does appear to be a statistical evidence suggestive of a slight impairment of growth and development as measured by comparison of height and weight in the control and exposed children. You cannot look at these children and pick out any abnormalities.

I would like to comment on this rather emphatically, because of the headlines that I saw a few minutes ago. There is no gross stunting of the growth. It can only be detected by a careful statistical analysis of the data, by taking measurements of weight and height.

SHORTENING OF LIFE SPAN

In animals, the evidence for shortening of life span is quite good. It is evidence that the life shortening is some function of the dose of radiation. However, the extrapolation from mice to man is extremely difficult. It is unlikely that any good statistical analysis can be made on the Marshallese because of the small numbers of individuals and the uncertainty of the precise birth date in the older groups, prior to the American occupation in 1944.

There has been one death in the Rongelap group who, at autopsy, showed evidence of heart disease. In the larger group from Utirik there have been five deaths. The number of deaths is comparable in both groups, one having received 175 r. and the latter only 14 r. To date, one must conclude that there has been no significant evidence for premature aging or shortening of the life span of the Marshallese.

LEUKEMIA AND CANCER

Leukemia is one of the things that is known to have occurred in the Japanese and is prevalent in irradiated laboratory animals. To date, no leukemia has occurred and there is no evidence of leukemic tendencies. This is being studied intensively by the use of alkaline phosphatase studies on the granulocytes and basophile counts on the blood. It has been shown by the studies of Moloney et al. in Japan that a basophilia and decrease in alkaline phosphatase precedes the development of leukemia. They picked up a precursor tendency that is detectable prior to the frank morphological picture of general leukemia.

Genetic effects, I think, I will defer comment on, since I am not a geneticist. The number is small, and I see little chance of detecting anything of note in the Marshallese.

Representative Holifield. Is it not true that, if the male and female are married to a group that have been irradiated, there is a much greater chance of the genes being effected than if one irradiated person and a nonirradiated person were married?

Dr. Cronkite. Yes; I am sure the probability of detecting is greater by consanguineous marriage than by nonconsanguineous. I am sure this is a subject that Dr. Russell will go into in considerable detail, and I will have to confess relative ignorance on the subject.

LONG-TERM EFFECTS OF INTERNALLY DEPOSITED RADIONUCLIDES

The very small amounts of radioactive materials that are deposited internally are, by themselves, inadequate to produce serious, long-term effects. However, the subject is complicated by the fact that the individuals had a substantial initial insult from whole-body radiation. In addition to the whole-body radiation, the thyroid gland received approximately 100 to 150 r. e. p. from the short-lived iodine family. It has been reported that irradiation of the thyroid area in early life increases the incidence of cancer of the thyroid. Accordingly, thyroid function and the possibility of thyroid cancer is being studied in the Marshallese children. To date, there is no evidence of abnormality.

Before concluding, I cannot refrain from expressing my personal opinion and conviction on two aspects of the fallout problem.

First, the acute and long-term hazards of fallout, such as would occur following the use of thermonuclear devices in warfare, are simply unthinkable. The widespread contamination over continental areas from multiple detonations of thermonuclear devices over populated areas would produce radiation hazards for all living things and for generations to come. These hazards are rather well understood. These hazards cannot be considered on the usual calculated risk basis of warfare in the past. One can only make a plea that an enlightened world will demand that their representatives in government also appreciate these hazards and with this recognition bring every conceivable

effort of diplomacy to solve the problems posed by differences in political and economic ideologies and thus prevent a type of warfare that cannot be considered in terms of calculated risk.

Second, the worldwide, low-level radiation of today from diverse sources has been analyzed thoughtfully by competent people, individually and in assembly. Note the sober and realistic reports of the National Academy of Sciences, the British Medical Research Council, and the United Nations. These reports point out the multiple sources of radiation in our lives today and the necessity for continuous scrutiny. Let us not confuse unavoidable radiation exposure with radiation hazard. Let us not lose sight of the multiple sources by undue preoccupation with worldwide fallout. Let us not be so preoccupied with radiation in general that we forget about industrial pollution of our environment in general by nonradioactive but toxic substances.

Lastly, the incidence of leukemia was apparently increasing prior to the development of atomic energy. Why?

Representative HOLIFIELD. Thank you very much, Dr. Cronkite, for that fine presentation.

(The references referred to in the statement follow:)

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Representative HOLIFIELD. I think the committee is conscious of your deep feeling on the fact that regardless of how this testimony comes out in relation to the dangers of bomb testing at the rate they have been made, that there is a tremendous concern as to war with nuclear weapons. If the consensus of scientific opinion is that there has been no appreciable damage in testing at the rate we have had it to date, it is certainly not to be taken that there is any less danger from a full-scale nuclear type of attack in war.

Are there any questions of Dr. Cronkite?

Representative COLE. Yes, Mr. Chairman, I would like to ask 1 or 2.

Doctor, for my edification, would you indicate the difference, if any, and the biological consequences in the exposure to cosmic radiation as against fission radiation?

Dr. CRONKITE. I do not think there would be any qualitative difference. I think it is purely a matter of quantity. The cosmic radiation is low, if it were possible to increase the cosmic radiation the effects to be expected would be the same as with any source of radiation—external penetrating radiation.

Representative COLE. Then it is your understanding that the result and effect on the anatomy would be the same, whether from cosmic radiation or induced or artificial or fission radiation?

Dr. CRONKITE. I believe so, Mr. Cole.

Representative COLE. From what you know of the observations that have been made as a result of the studies of the Japanese population, who are exposed to radiation from the weapons fission, did those lessons vary in any degree with the lessons and observations that have resulted from the Marshallese people who were exposed?

Dr. CRONKITE. The only significant difference in the response is the Japanese were not exposed to fission products deposited in their environment but to the initial radiation from the bomb and accordingly did not have any skin burns resulting from radiation. Their skin burns were thermal in origin.

I would say that there is remarkable correspondence.

Representative COLE. I am referring to the Hiroshima and Nagasaki people; not the fishermen.

Dr. CRONKITE. I was referring to the Hiroshima and Nagasaki group. At Hiroshima and Nagasaki they were exposed to initial gamma radiation from the bomb. The fission products were not deposited on the ground. So that the Japanese there did not receive any skin burns due to contacting material. In this respect the Marshallese differ from the Japanese because they had a mixture of the radiation injury produced by the penetrating component of the gamma rays from the fission products and by the direct contact of the material on the skin with the resulting beta burns.

Representative COLE. You concluded your very fine statement with a rather imponderable question, and I am going to ask you to suggest possible answers to the question which you have raised.

Dr. CRONKITE. I think there are many possible answers. It has been abundantly proved for example that benzol and various other industrial poisons are also capable of inducing leukemia in experimental animals and presumably also man. If my memory does not fail me, and I could document this, in the lithographing industry at one time there were a few cases of leukemia presumably induced by overexposure to benzol. Certainly, many of the things that are used in medicine, where a calculated risk is deliberately taken, for the immediate welfare of a patient, occasionally one gets into trouble with drugs.

There are many things I could think of. The heavy metals. There are numerous things in the industrial life of today that can be toxic and can produce the same things that radiation does.

Representative COLE. I was impressed by an observation made by a witness this morning which, as I recall, was to the effect that since toxicity from radiation is a condition that is readily detectable by reason of our devices, that there might be an inclination to attribute the biological damage to that toxicity which is identifiable, rather than to another toxicity which might exist but which could not be so readily identified.

Do you subscribe to that general observation?

Dr. CRONKITE. I certainly do, Mr. Cole. I think you have expressed it better than I can.

Senator BRICKER. Mr. Chairman, I have just one question.

What is the ratio of body exposure to radiation from the cosmic rays and from the background material from the earth radiation?

Dr. CRONKITE. I am sorry, I did not follow you.

Senator BRICKER. The ratio that the human body is exposed to from the cosmic rays coming from the atmosphere and that radiation to which we are exposed from the materials of the earth.

Dr. CRONKITE. I am fairly confident that it is larger from cosmic radiation than from the earth, but I would have to defer to somebody who has personally investigated this field, which I have not done.

Representative HOLIFIELD. Thank you very much, Doctor Cronkite.

Our next witness is Dr. Edward Lewis. He is a professor of biology at the California Institute of Technology, and his present work is on the nature of the gene and mutational processes. He has a notable scientific background, and we will be glad to hear from Dr. Lewis at this time.

STATEMENT OF DR. EDWARD LEWIS, CALIFORNIA INSTITUTE OF TECHNOLOGY [1]

Dr. LEWIS. Mr. Chairman, with your permission I would like to use the podium.

Representative HOLIFIELD. You may proceed.

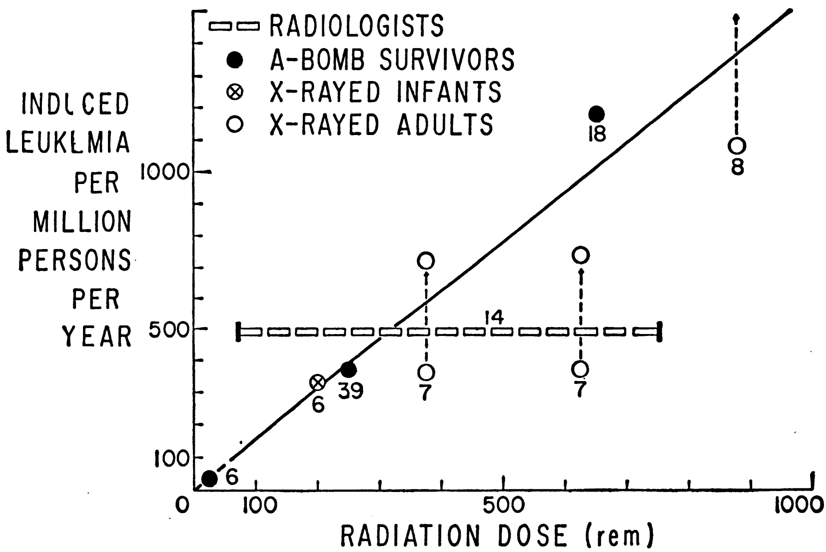
Dr. LEWIS. Mr. Chairman, I would like to thank you for this opportunity to testify. I will confine my remarks to the subject of

[1] Date and place of birth: May 20, 1918, Wilkes-Barre, Pa. Education: B. A., University of Minnesota, 1939; Ph. D., California Institute of Technology, 1942. Captain in USAF, 1942–46 (specialty—meteorology and oceanography). Fellow of Rockefeller Foundation, 1948–49; Cambridge, England. Present position; Professor of biology, California Institute of Technology, Pasadena, Calif. Present work: Nature of the gene and of the mutation process. Member of the Genetics Society of America and the American Society of Naturalists. (Submitted by witness.)

leukemia. I have been asked to do this, and I want to point out that in doing so I do not wish to imply that I think that leukemia is the most important effect of radiation on man. I think in fact possibly the genetic effects may be more important. There may also be other malignant diseases that are more important than leukemia with respect to ionizing radiation.

However, the reason that I am stressing leukemia today is that we have rather good data and rather good evidence on leukemia as compared to data on other effects on man from ionizing radiation and it is this evidence I would like to go into today.

I would like to present this evidence by means of this chart. A detailed account of this evidence has been recently published in Science (vol. 125, pp. 965–972).



Summary of incidences of induced leukemia among various groups of persons subjected to ionizing radiations. The numbers under the circles refer to the number of cases of leukemia that are estimated to have resulted from the radiation. "X-rayed infants" refers to children treated for thymic enlargement. "X-rayed adults" refers to patients with ankylosing spondylitis. (References to the original literature and methods of estimating incidence of radiation-induced leukemia and radiation dose are contained in Science, vol. 125, pp. 965–872. May 17, 1957.)

This chart shows radiation dose along the bottom in the rem unit which Dr. Taylor talked about this morning, essentially the same as the roentgen unit. We use it here because we are going to include some information on atomic bomb survivors whose total irradiation included a percentage due to neutrons, possibly 10 to 20 percent.

Along here we have induced leukemia per million persons per year. The first thing we might consider are the circles. These are people who have died of leukemia as a result of a sudden dose of radiation. We all know about the atom bomb survivors receiving varying amounts of radiation in Hiroshima and Nagasaki as a result of the August 1945 bursts. These points are shown in blue. The largest

number we have is shown here as 39 persons estimated to have died of leukemia as a result of radiation. There were actually in a certain zone in these cities 41 persons who died of leukemia, and who were estimated to have obtained approximately 250 rem units. Of the 41, 39 are those we would estimate died from the radiation. The remaining 2 would have been expected to die as a result of the natural leukemia incidence within the population. This result is based on some 14,000 people. The period of time is almost 8 years. (This is a study from 1948 to 1955—September of 1955—and that is a 7.8 year period).

Representative COLE. Mr. Chairman, in order that we might have a better understanding of the observations which the chart discloses, may I inquire if this chart reflects the studies on all of the people affected by both the atom weapons or 1 of them or a segment of 1 of them? Thirty-nine out of perhaps 5,000 persons might be significant. If it is 39 out of 500,000 it would be less significant. In order to really evaluate and appreciate the significance of the chart I think we should know the extent or the area included in the observation.

Dr. LEWIS. Thirty-nine of the 14,000 in this particular case, sir, is the number of cases of leukemia in a certain zone in both cities—a certain area, namely in this particular case, extending from 1,000 to 1,500 meters from the point on the ground under the bursts. The combined total number of people in this area in both cities was 14,000, approximately, from census estimates in 1950.

I might add that these data have been published by other investigators and collected by a great number of people. The only thing that we do here is to relate incidence of leukemia to dosage. This has not been done before for the reason that the Atomic Bomb Casualty Commission did not have the doses for so relating the incidence of leukemia.

There are 18 cases of leukemia which have arisen in the zone from zero to 1,000 meters from the point on the ground under the aerial bursts in Hiroshima and Nagasaki. There were only approximately 1,800 people who survived in that zone and among these the 18 cases of leukemia accumulated over the 7.8 years. This is about a 1 percent incidence of leukemia. The average absorbed dose in this case is estimated to be about 650 rem.

This point down here represents six people, a very small number of persons as far as our purposes here are concerned, which are to try to relate the dose to the incidence. There were in fact 10 persons who have died already in this region from leukemia, among approximately 23,000 people who were exposed in this dose range. That is, the combined total in the 2 cities of Hiroshima and Nagasaki was 23,000 persons who were beyond the 1,500 meter point and who were between 1,500 meters and 2,000 meters.

Of the 10 persons who died of leukemia, 4 would have been expected to have died on the basis of the spontaneous incidence. That leaves six, which is the expectation for the number who have died from A-bomb radiation. That is a low number and is subject to considerable error. The average absorbed dose in this region is estimated to be only 25 rem, and the maximum possible dose in this region was 100 rem.

I call your attention to the fact that we haven't much information in this part of the curve and yet that is the part of the curve that we are interested in here because we are considering small amounts of radiation.

I will only quickly point out that there have been children who have received a rather large dose of radiation, some averaging possibly anywhere between 100 and 300 of these units. They have an increased rate of leukemia which is statistically significant. There were 7 cases of leukemia in a group of some 1,400 children in this case. These 7 have developed leukemia after having been X-rayed as infants for a chest condition; 6 of these is the number that we would estimate were due to radiation, because at best only less than 1 is expected from the natural incidence of leukemia. I have been referring to a study by Dr. Simpson and associates.

A study is being made in Great Britain by Dr. Court Brown and colleagues of X-rayed adult males who had been treated for a serious spinal abnormality which is alleviated to some extent by this treatment.

In this case we have in the orange circles different treatments given to 11,287 male patients in Great Britain. These are the numbers of individuals, and this [indicating] would be the rate of leukemia. It is a significantly increased rate. They were irradiated only in partial body form to the spinal area, and seemed to develop only the kind of leukemia that stems from the bone marrow and hence it is possible that the total leukemia rate per unit of radiation dose would be higher by a factor of 2 (shown by the dashed vertical lines).

As I said, these are acute doses of radiation—sudden doses. We are interested in slow chronic radiation exposure. We have some evidence on this which has been accumulated again by a number of investigators. I refer to a group of occupationally exposed persons, namely radiologists. The period of time that we are going to talk about is from 1938 to 1952, inclusive. It is estimated that 14 radiologists died of radiation-induced leukemia out of 17 who died of leukemia. That is, three might have been expected according to the spontaneous incidence figures. This is corrected for the fact that we would expect radiologists to have a higher rate of leukemia than the United States white male population owing to differences in age composition.

The long range here means that we do not know what dose radiologists got because, as we know, they got it as a chronic exposure over many years.

I point out that we estimate that somewhere around 250 to 300 r. units or rem units would be the accumulated dose for their mean period of occupational exposure and that period is about 25 years, as it turns out.

Representative Cole. Mr. Chairman, may I inquire with respect to the radiologists over a period of 14 years, from 1938 to 1952? You say that 14 out of 17 radiologists who died from leukemia were traceable to their work. That is a total of 17 radiologists who died as a result of leukemia out of how many radiologists?

Dr. Lewis. 1,860, sir. That population developed from a figure of about 1,300 in 1938 and has increased by 1952 to about 2,500. But the average for that period was 1,860 radiologists. Also, only radiologists who were at ages 35 to 74, inclusive, during the 1938–52 period are considered here.

I want to point out now that one can draw various curves to express these data. I have drawn here a straight-line curve which would say that the incidence of leukemia is directly proportional to the dose. I

feel that the evidence supports this to some extent in the high-dose region. In the low-dose region here, there is a dashed line, and there are only six individuals on which to say anything. The point here, however, is that in the absence of any other information it seems to me—this is my personal opinion—that the only prudent course is to assume that a straight-line relationship holds here as well as elsewhere in the higher dose region.

It may be that there is a threshold—that is, a dose below which leukemia will not develop. However, we can say safely, I think, that if there is a threshold dose it must be below 100 r. The reason for saying that is that in the region below 100 r. you would not expect to have gotten the 6 cases of leukemia as a result of chance more than 1 in 50 times.

I would like to point out one figure that we are very sure of, and that is simply the spontaneous incidence of leukemia in this country. This is 10,000 deaths from leukemia per year in the United States in 1954. It is actually 10,500, but in round numbers 10,000 is the number who die of leukemia in this country at the present time.

If we use this straight line and assume it is a straight line in the low-dose regions—as far as I can see there is little reason to believe that this is not the correct assumption in this region—then we can make some simple calculations, making use of that line. These calculations come out as follows:

One thousand deaths from leukemia per year is what we would expect from natural background radiation. The natural background radiations include cosmic rays, and radiation from rocks, buildings, and the radioactive isotopes in the human body. We receive a dose rate of 100 millirem, or one-tenth of an r. unit, per year from such sources. That is approximate. It varies somewhat from place to place and from time to time and therefore it is not quite clear what the precise figure is.

We see from this that if the straight-line relation holds, there are a fair number of cases actually attributable to the natural radiation. There are some other figures here that are of interest. The figure that we were given of 14 million rem per million people per generation this morning by Dr. Taylor would work out to be approximately five-tenths of a rem per year—not quite—0.47 rem per year. That would be the new public's permissible dose that was mentioned, I believe, particularly with the gonadal dose in mind. But you can't avoid that dose for the whole body as well in most procedures, so that such a dose leads to a sizable number of leukemia cases compared to the spontaneous incidence.

Representative HOLIFIELD. What does that lead to? What would be the number that five would lead to? Would it be 5 times 1,000?

Dr. LEWIS. Do you mean this? [indicating], namely 5,000 cases per year.

Representative HOLIFIELD. Yes. Assuming that 0.5 r. per year is the burden of your argument that it would increase the 1,000 cases to 5,000 per year.

Dr. LEWIS. It would add the 5,000 to the 10,000 total. The new proposed permissible dose for the United States public, would lead to 5,000 cases per year on this calculation, of which 1,000 cases per year would be due to the natural background. So 4,000 cases per year would be the added rate.

As far as fallout is concerned, which is the relevant thing today, I think 0.001 r. is a conservative rate per year that one can estimate we have at the present time reached on the basis of an estimate that was given to the Genetics Committee of the National Academy of Sciences; namely, that the United States now gets one-tenth of an r. per generation—per 30 years—from fallout. One-tenth of an r. per 30 years corresponds to three one-thousandths of an r. per year. I have not used 0.003 because it is possible that the whole body absorption dose would be less than this. So we take 1 milliroentgen (0.001 r.) per year as a conservative rate. Then if we reach this level and maintain it continuously, that leads to 10 as the number of deaths from leukemia per year from fallout sources. We have not had this exposure long enough to make it 10 per year as yet. This exposure would have to go on for 60 years. That sounds like a long period of time. However, radiation from strontium 90 in the bones will help to contribute at least this high a dose rate for quite a while to come because of the retention of strontium 90 in the bones and because of its long half-life (28 years).

This particular estimate of 10 deaths from leukemia per year at the present fallout rate would not be this high at the present moment in the United States. I do not think it would be higher than 1 to 3 deaths per year at the present time from fallout that has accumulated so far. In terms of our population that is a very minute fraction of the population—an exceedingly minute fraction—but after all, it does correspond to somebody.

Thank you.

Representative HOLIFIELD. Thank you very much, sir.

Senator ANDERSON. Did you say if a threshold exists at all it must be below 100 r. for the Hiroshima-Nagaski victims?

Dr. LEWIS. Yes, sir.

Senator ANDERSON. Is that not about the lowest we have had yet on that?

Dr. LEWIS. I think it is for human beings. There are data indicating that in mice such a dose would be still lower.

Senator ANDERSON. I am talking about human beings.

Dr. LEWIS. Yes.

Representative HOLIFIELD. In the concluding part of your statement you say that if the present population of the United States were to be constantly exposed to 100 sunshine units of strontium 90, the prediction calculated in this way is that 500 to 1,000 cases of leukemia would occur annually from this source alone.

The direct proportionality law further predicts that constant exposure to 1 sunshine unit of strontium 90 would be capable of producing from 5 to 10 cases of leukemia annually in the United States population. You have illustrated that by the chart.

So the position that you take, then, is that any radiation would have an effect and that therefore there is a threshold.

Dr. LEWIS. Is not a threshold.

Representative HOLIFIELD. Is not a threshold, I should say.

Dr. LEWIS. That is right. The threshold concept would say that you would not get any leukemia at all until you reach, say 500 r., which in a sense was the assumption when it was thought you could safely accumulate radiation as an occupational worker at a rate of 15 r. per year. Now we have more information that says, no, the threshold

dose had better be put well below 500 r. I would say we must put it well below 100 r.; in fact, I doubt that a threshold exists.

As far as I can see from analogy with phenomena of genetics, which is my field, there is a possible theoretical basis for predicting that there would be no threshold, namely, if leukemia is due to a somatic mutation. That is the interest of geneticists in this disease. However, these calculations do not assume that leukemia is due to a mutation. It does not matter what leukemia is due to; if this line continues as a straight line to zero then these calculations are valid.

Representative HOLIFIELD. Are there any further questions?

If not, thank you, Dr. Lewis.

(A statement and an article entitled "Leukemia and Ionizing Radiation," by Dr. Edward Lewis, follows:)

STATEMENT BY E. B. LEWIS, JUNE 3, 1957

Exposure of human beings to radioactive fallout is expected to have two types of biological effects: (1) genetic effects on the descendants of the exposed individuals, and (2) direct effects on the exposed individuals themselves. The present testimony is restricted to considering one of the direct effects; namely, the induction of a specific malignant disease, leukemia.

In recent years, a number of investigators have been making careful followup studies of persons who are known to have been exposed to man-made sources of radiation. These studies have provided us with abundant evidence that radiation induces leukemia in man. The following four groups of people have been the principal ones investigated: (1) survivors of the atomic bomb bursts over Hiroshima and Nagasaki; (2) patients irradiated with X-rays for the purpose of alleviating a spinal abnormality (ankylosing syondylitis); (3) children irradiated with X-rays as infants for the purpose of alleviating an enlarged thymus gland; and (4) radiologists, who, of course, are occupationally exposed to ionizing radiations.

For each of the above four groups of persons, it is possible to make estimates of the doses of radiation which they received and then to relate such estimates to the number of cases of leukemia that developed subsequent to the irradiation. When this is done, it is found that the incidence of the disease tends to vary in direct proportion to the dose of radiation. That is, doubling the dose tends to double the number of people who will develop radiation-induced leukemia, other factors being equal.

The Japanese survivors and the X-rayed patients were subjected to sudden doses of radiation, whereas the radiation from current levels of radioactive fallout is expected to be delivered gradually over many years with only a very small amount occurring at any one instant. How effective, then, is chronic irradiation in producing leukemia? Radiologists have received their radiation as a chronic occupational exposure extending over many years and in relatively small amounts at any one time. Yet, radiologists die of leukemia at a rate which is about five times that expected if they had received no occupational exposure to radiation. Also, radiologists seem to have about the same chance of developing leukemia, after a given dose of radiation, as do the survivors of atom-bomb radiation or the patients treated with X-rays.

The above findings suggest that the small amounts of radiation which are expected to accumulate from radioactive fallout may also be effective in producing leukemia. It should be noted here that there are two routes by which fallout may exert its effects. Mixed fallout products outside the body emit long-range or gamma radiation which then can cause whole-body irradiation. Fallout products also emit short-range radiation which will be effective as far as leukemia is concerned when the fallout products are ingested into the body. Here, radiostrontium is especially important since it accumulates near the bone marrow where certain types of leukemia are thought to originate.

Recently there have been suggestions that the public would not suffer any appreciable effects if the body level of strontium 90 were to reach 100 sunshine units of this element—this is an amount which is one-tenth that permitted workers with radioactive materials. Now, if the direct proportionality law continues to hold at even the lowest dose levels—and there is little reason to believe it will not hold—then it becomes possible to calculate the number of cases of leukemia

that will arise if, for example, the present population of the United States were to be constantly exposed to 100 sunshine units of strontium 90. The prediction calculated in this way is that 500 to 1,000 cases of leukemia would occur annually from this source alone. The direct proportionality law further predicts that constant exposure to even one sunshine unit of strontium 90 would be capable of producing about 5 to 10 cases of leukemia annually in the United States population.

[Reprinted from Science, May 17, 1957]

Leukemia and Ionizing Radiation [1]

(By E. B. Lewis)

Quantitative estimates of the genetic effects of ionizing radiation on human beings have been carried out by a number of investigators (1–3). Estimates of this kind involve extrapolating from induced mutation rates in such organisms as *Drosophila* and mice. Quantitative estimates of the somatic, or direct, effects of radiation must also be attempted if the biological hazards of ionizing radiation are to be fully assessed. In the case of direct effects, it is particularly difficult to extrapolate from results with lower organisms, and it becomes important to have data on man himself.

It is the purpose of this article to examine the evidence for the induction of leukemia in man by ionizing radiation. Although ionizing radiation has been implicated in the production of other human malignancies, such as bone tumors (4) and thyroid carcinoma (5, 6), only the data on induction of leukemia seem sufficiently extensive to warrant a study at this time of the quantitative relationship between incidence of the disease and dose of radiation. Evidence bearing on this relationship is drawn from studies of leukemia among four groups of individuals: (i) Survivors of atomic bomb radiation in Japan; (ii) patients irradiated for ankylosing spondylitis; (iii) children irradiated as infants for thymic enlargement; and (iv) radiologists. An estimate of the probability of developing leukemia per unit dose of radiation (7) per time unit is derived for each of these groups. This probability of radiation-induced leukemia is discussed and its application to a specific example of a possible radiation hazard—namely radiostrontium—is outlined. Certain properties of the disease, relevant to the radiation studies, are presented first.

DESCRIPTION OF THE DISEASE

Leukemia is a malignant disease in which the leucocytes undergo a more or less unrestricted proliferation. The "acute" form of leukemia differs from the "chronic" form, not only in being usually of shorter duration, but also in being a more severe disease with a higher percentage of immature white blood cells in the circulating blood. Another classification of the leukemias is based on the type of white blood cell predominating in the marrow or in the circulating blood. The two most common of these types are known as granulocytic (or myelogenous) and lymphocytic (or lymphatic). The presumption is that the granulocytic type arises in the red bone marrow. The lymphocytic type is thought to arise in the lymphatic elements of the blood-forming system (thymus, spleen, and other lymph glands), although the marrow is not excluded as a source for this type.

SPONTANEOUS INCIDENCE OF LEUKEMIA

In 1947 Sacks and Seeman (8) reported that the recorded death rate from leukemia had increased steadily from 1900 to 1944 and at an accelerated rate after 1930. The death rate has continued to increase (9). By 1954 the crude mortality rate for leukemia among the United States white population had reached 68 per million individuals per year (10) compared with 42 per million in 1940 (11). The male and female crude death rates in that population were 79 and 58 per million per year, respectively, in 1954 (10). The observed increase in death rate from this disease may be partly due to improvements in diagnosis. Other factors may also be responsible, such as the increased exposure of the population to ionizing radiations employed in medicine and dentistry, as was recently discussed by Dameshek and Gunz (12).

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[1] Dr. Lewis is professor of biology at the California Institute of Technology, Pasadena.

MacMahon and Clark (18) have recently studied the spontaneous incidence of the common forms of leukemia. They have attempted to determine the total number of valid cases diagnosed among residents of the borough of Brooklyn from 1948 to 1952, inclusive. In this study the over-all ratio of acute to chronic forms among the white population was nearly 1/1 (726/732), but there were marked differences in the incidence of these two forms with respect to age at time of diagnosis, as is shown in table 1 (14). The ratio of granulocytic to lymphocytic types in the Brooklyn study was 1.6/1 (512/318).

LEUKEMIA IN HIROSHIMA AND NAGASAKI

Studies of the incidence of leukemia among survivors of the atomic bomb bursts over Hiroshima and Nagasaki have established that ionizing radiations induce leukemia in man (15–17). Table 2 summarizes the incidence of leukemia in terms of four concentric zones about the hypocenter (the point on the ground under the aerial burst). This table includes only those cases of leukemia which were (i) diagnosed during the period January 1948 to September 1955, inclusive; (ii) resident in the city at the time of diagnosis (Hiroshima) or at the time of death (Nagasaki); and (iii) considered by several criteria to be valid cases of the disease (18). For each zone, the estimate of the number of exposed survivors resident in Hiroshima as of October 1950 (17) has been combined with the corresponding number for Nagasaki (15) to obtain a combined population estimate for both cities.

Lange et al. (16) have studied the pattern of types of leukemia in the exposed and unexposed populations of Hiroshima and Nagasaki. They conclude that radiation induces the same pathological types that are found spontaneously and, as far as can be judged by the limited data, induces them in roughly the same relative proportions that occur spontaneously. This is especially evident in the case of chronic lymphocytic leukemia, which is rare in both the exposed and unexposed Japanese populations, whereas it is the most common form of leukemia after age 50 in the United States (13). Lange et al. found no marked influence of sex or age on the incidence of leukemia among the exposed populations. However, they point out that, for a number of reasons, the data are not very satisfactory for assessing the incidence in individuals under 5 years of age (19).

TABLE 1.—*The spontaneous incidence of leukemia for the white population of Brooklyn, N. Y., 1943–52, according to chronicity—Data of MacMahon and Clark (13)*

Age	Percentage in age interval	Incidence per million per year [1]		
		Acute	Chronic	Total
0 to 9	15.3	48	1	49
10 to 19	13.5	24	2	26
20 to 29	16.5	12	6	18
30 to 39	16.5	20	14	34
40 to 49	14.8	22	28	50
50 to 59	11.8	44	64	108
60 to 69	7.6	58	133	191
70 and over	3.9	59	182	241

[1] The incidence of subacute and unknown types of leukemia have been allocated to the observed incidences for the acute and chronic forms in the proportions in which the latter were diagnosed at each age interval (14).

The published accounts of leukemia in Hiroshima and Nagasaki have not contained estimates of the doses received by the bomb survivors. Recently, however, distance-dose curves for these cities have been published (20). These curves give, for each city, the relationship between the slant distance from the burst and the "air" (unshielded) dose of gamma rays and of neutrons. From this information, curves have been constructed (fig. 1) showing the relation between distance from the hypocenter and the combined "air" dose from gamma rays and neutrons (fig. 1) in rem (7). In computing the latter dose, it has been assumed that

the relative biological effectiveness (RBE) of neutrons for inducing human leukemia is 1.7. This value is chosen since Dunning has recently stated that "for generalized whole-body effectiveness it is thought that 1.7 is a reasonable representative value for neutrons from a nuclear detonation" (21). This is believed to be a conservative estimate for the RBE, since Upton et al. (22) found that the RBE for induction of leukemia in mice by fast neutrons is somewhat lower than this.

In the absence of precise knowledge of the distribution of survivors within the different zones about the hypocenter, it is conservative to take the mean "air" dose for a zone as the average "air" dose received by survivors in that zone. The zone from 0 to 999 meters, which is designated here as zone A, is a special case, however, since there was heavy mortality near its center. The mean dose for this zone has been computed for the portion of this zone extending from 850 to 999 meters. The majority of leukemia cases in zone A occurred in this latter region (15); moreover, 2 (among 5) cases at a distance closer than 850 meters had the type of shielding specified, and in each case it was listed as heavy (18). Since the doses for the two cities are slightly different at a given distance from the hypocenter, the average value of these two doses is used without correcting for differences in population size. In this way, a dose of about 1300 rem is arrived at for zone A. For zones B (100 to 1,499 meters) and 3 (1,500 to 1,999 meters), the mean doses are approximately 500 rem and 50 rem, respectively. At 2,000 meters the dose has fallen to 14 rem and by 2,500 meters to less than 5 rem. Since the majority of the population in zone D (from 200 meters on) was beyond 2,500 meters, the average dose is under 5 rem and is thus so low that zone D can be treated as if it were a "control" zone.

The relation of dose, estimated as described in the preceding paragraph, to the incidence of leukemia per year, based on the combined Hiroshima and Nagasaki data, is shown in table 3. The incidence per year in the "control" zone, D, is subtracted from the incidence in each of the other zones to obtain the "incidence of induced leukemia per year" in these zones. The values of the incidence of induced leukemia are likely to be minimum ones, since, as Lange et al. have noted, "some cases of leukemia have undoubtedly been missed and other cases have been omitted because of lack of adequate material to confirm the diagnosis" (16). The incidences of induced leukemia in zones A, B, and C have been divided by the respective means "air" doses in rem, derived in the preceding paragraph, to give estimates for the probability of induced leukemia for these zones. The values for this probability are seen to range from 0.7×10^{-6} to 0.9×10^{-6} per individual per rem per year. These are minimum estimates of the probability of induced leukemia, since the survivors were shielded in varying degrees from the "air" doses, calculated in the preceding paragraph. The shielding of survivors has two major components: (i) the body's own shielding of its blood-forming tissues by the surrounding bone and soft tissues; and (ii) external shielding by buildings or other shelters. A shielding factor of 2 is believed to be a conservative one for correcting for both of these components—the true factor might be at least 4 (23). The "best" estimate for the probability of induced leukemia from these data is, therefore, taken as approximately twice the aforementioned minimum estimates of 2×10^{-6} per individual per rem per year (of the 7.75-year period). A rought range for this probability is 0.7×10^{-6} to 4×10^{-6}.

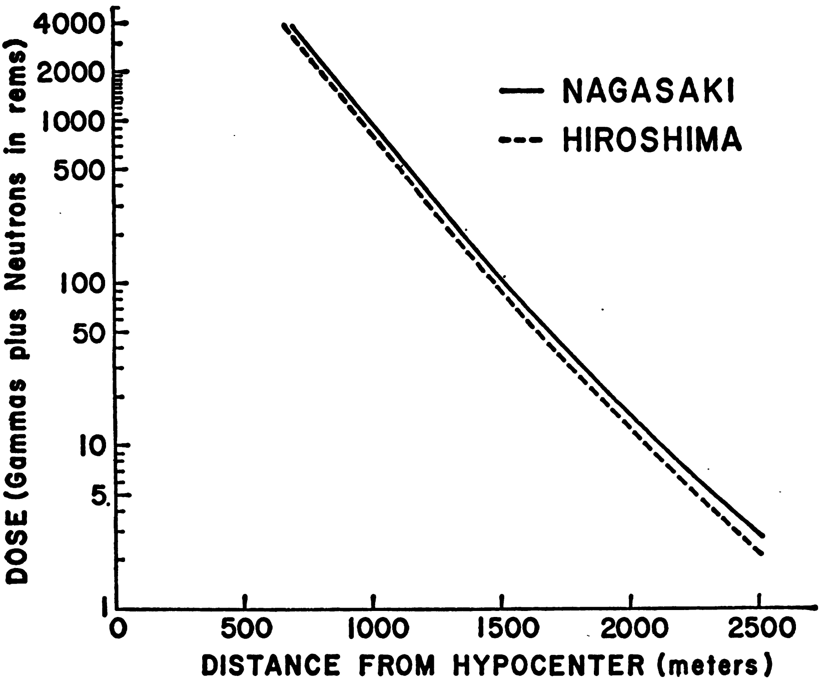

FIGURE 1. Distance-dose curves for atomic bomb blasts at Nagasaki and Hiroshima.

LEUKEMIA AND ANKYLOSING SPONDYLITIS

Court Brown and Doll (24) and others (25, 26) have investigated the incidence of leukemia among patients treated with X-rays for ankylosing spondylitis—a hereditary disease of the spine. Among 11,287 male patients irradiated during the period from 1935 to 1954, inclusive, 37 cases of leukemia were discovered. The average period of followup of these patients was "just under 5 years" (24). The distribution of cases by amount of treatment, measured as maximum dose in roentgens (7) to the spinal marrow, is shown in table 4. A highly significant increase in the incidence of leukemia is apparent among those receiving the heavier treatments.

Court Brown and Doll have estimated the expected incidence of leukemia in a comparable group of unirradiated normal males as 50 cases per million individuals per year. Subtraction of this expected incidence from the observed incidence of leukemia per year in the irradiated patients gives an estimate of the incidence of radiation-induced leukemia per year. This calculation has been carried out for each of the groupings of leukemia cases according to amount of treatment. For each such grouping between 500 and 2,750 roentgens, an average maximum dose to the spinal marrow is taken as the midpoint of the dose range (for example, for leukemia cases developing after treatments ranging from 500 to 999 roentgens, 750 roentgens is taken as the average dose). By dividing the calculated incidence of radiation-induced leukemia for each of the four groupings of this kind (col. 5, table 4) by the respective average maximum dose (col. 2, table 4), a set of four minimum estimates of the probability of leukemia per individual per roentgen (to the spinal marrow) per year is obtained. These latter estimates are seen to range from 0.3×10^{-6} to 0.6×10^{-6} per individual per roentgen per year (col. 6 of table 4). It seems likely that the absorbed dose to the entire red-marrow system would be lower than the stated doses to the spinal marrow by a factor of at least 2 or 3. Therefore, it is estimated that the probability of leukemia ranges from about 0.6×10^{-6} to 2×10^{-6} per individual per rad (to the red-marrow system) per year.

TABLE 2.—*Incidence of leukemia among the combined exposed populations of Hiroshima and Nagasaki by distance from the hypocenter (January 1948–September 1955)*

Zone	Distance from hypocenter (m)	Estimated population of exposed survivors (October 1950)	Number of confirmed cases of leukemia	Percentage of leukemia
A	0 to 999	1,870	18	0.96
B	1,000 to 1,499	13,730	41	.30
C	1,500 to 1,999	23,060	10	.043
D	2,000 and over	156,400	26	.017

TABLE 3.—*Incidence of leukemia per year among the combined exposed populations of Hiroshima and Nagasaki (January 1948 to September 1955) in relation to dose of radiation (gammas plus neutrons)*

Zone	Average maximum dose (rem)	Incidence of leukemia per million per year	Incidence of radiation-induced leukemia per million per year	Probability of leukemia per individual per rem per year
A	1,300	1,200	1,179	0.9×10^{-6}
B	500	390	369	$.7\times10^{-6}$
C	50	56	35	$.7\times10^{-6}$
D	5	21		

LEUKEMIA AND THYMIC ENLARGEMENT

Simpson et al. (6) have traced a series of 1,400 individuals who had irradiated as infants for an enlarged thymus condition. The average period of followup appears to have been about 15 years. As a "control" 1,795 unirradiated siblings were also traced. In the irradiated group there were 7 confirmed cases of leukemia (and 1 unconfirmed case), while there was none in the control group. The calculated number of cases of leukemia that would have been expected in a sample of comparable size and age from the general population was 0.6. The difference between this expectation and the observed number of cases (seven) is statistically significant (P less than 0.01).

In the majority of the 1,400 infants, the radiation (X-rays) had been more or less restricted to the chest region. It was estimated that the air dose to the thymus region was more than 200 roentgens ("the great majority being less than 600 r") in 57 percent of the treated individuals and under 200 roentgens in the remainder. The average absorbed dose to the entire lymphatic system is roughly estimated as 100 to 300 rad. On the basis of these dose estimates, the probability of leukemia ranges from 1×10^{-6} ($6.4/15\times1{,}400\times300$) to 3×10^{-6} per rad (to the lymphatic system) per individual per year. The number of cases (seven) on which this estimate is based is, of course, small. The 95-percent confidence interval for an observation of 7 when the frequency is as low as in the present case lies between 3.3 and 13.2 (27). Therefore, the probability of leukemia in the thymic enlargement group may well range from 0.4×10^{-6} to 6×10^{-6} per rad (to the lymphatic system) per individual per year.

LEUKEMIA AMONG RADIOLOGISTS

March (28, 29) and others (30, 31) have called attention to the fact that among physicians the percentage of deaths from leukemia is much higher for radiologists than for physicians who are not radiologists. The percentage of deaths that are due to leukemia can be a misleading statistic, however, since it is sensitive to differences in age distribution between the groups of individuals being compared. Such differences are marked in the case of radiologists, on the one hand, and all physicians, on the other, as is discussed later. To assess the radiation factor in the leukemia among radiologists, it becomes necessary to

estimate (i) the death rate from leukemia among radiologists (the number of deaths per total number of living radiologists), and (ii) the expected death rate from leukemia among radiologists if they had received no occupational exposure to radiation.

TABLE 4.—*Incidence of leukemia among ankylosing spondylitis patients receiving different doses of radiation (X-rays)—Data from Court Brown and Doll (24)*

Maximum dose to spinal marrow (r.)	Estimated average maximum dose (r.)	Number of males developing leukemia	Crude incidence per million males per year	Incidence of radiation-induced leukemia per million males per year	Probability of leukemia per individual per r. (to spinal marrow) per year
0			50		
Under 500		2	220	170	
500-999	750	8	410	360	0.5×10^{-6}
1,000-1,499	1,250	8	420	370	0.3×10^{-6}
1,500-1,999	1,750	8	1,130	1,080	0.6×10^{-6}
2,000-2,749	2,375	6	1,300	1,250	0.5×10^{-6}
2,750 or more		5	1,760		

The study of motality among medical specialists by Dublin and Spiegelman (32) has been used here as a guide in computing the afore-mentioned rates and as source of data for the years 1938 to 1942, inclusive. The latter data and additional data for the years 1943 to 1952, inclusive, are summarized in table 5. The term radiologist is restricted here, following Dublin and Spiegelman, to those physicians who were listed in editions of the American Medical Directory (33) as limiting their practice to radiology (and roentgenology). Since only deaths occurring at ages 35 to 74 years, inclusive, were included in the mortality study for 1938–42, the same practice is adopted here for the supplementary 10-year period.

In order to estimate the mean annual population of radiologists at ages 35 to 74 years, during the entire 15-year period from 1938 to 1952, the age distribution of radiologists is required. This age distribution for the year 1940 was computed by Dublin and Spiegelman from the 1940 edition of the American Medical Directory and is shown in table 6. The age distribution for a similar group of radiologists in 1950, also shown in table 6, has been computed (34) by reference to the 1950 edition of this directory. The 1940 age distribution was based on a total of 1595 radiologists of whom 1451.5 (91.0 percent) can be inferred to have been at ages 35 to 74 years, inclusive (32). The 1950 age distribution was based on a total of 2443 radiologists of whom 2250 (92.1 percent) are calculated to have been at ages 35 to 74 years, inclusive, as of July 1, 1950. The mean number of radiologists (at ages 35 to 74) per year from 1938 to 1952, inclusive, is roughly approximated as 1850.7, which is the average of the number of such radiologists in 1940 and the corresponding number in 1950.

Deaths from leukemia occurring at ages 35 to 74 years, inclusive, among radiologists have been located in several ways with the results shown in table 5. For the period from 1938 to 1942, inclusive, five such deaths are recorded by Dublin and Spiegelman. For the period from 1943 to 1948, inclusive, the carefully documented studies of March (28, 29) record eight such deaths. For the remaining 4-year period from 1949 to 1952, inclusive, four additional deaths from leukemia have been located by reference to death notices in a medical journal (35). Thus, a minimum of 17 deaths from leukemia has been located among radiologists who died between the ages of 35 and 74 years during the 15-year period from 1938 to 1952. The upper and lower 95-percent confidence limits for this observation of 17 deaths are 25.5 and 10.8 deaths, respectively (27). Thus, a likely range of values for the average death rate from leukemia among radiologists at ages 35 to 74 years is 390 ($10.8/15\times1850.7$) to 920 ($25.5/15\times1850.7$) deaths per million per year, and the "best" estimate is 610 ($17/15\times1850.7$) deaths per million per year (of the 1938–52 period).

TABLE 5.—*Deaths and death rates from leukemia among radiologists, at ages 35 to 74 years, by 5-year periods from 1938 to 1952, inclusive*

Period	Estimated number of radiologists at mid-point of period	Number of deaths from leukemia	Observed death rates per million per year	Expected death rates per million per year	Incidence of radiation-induced deaths per million per year
1938–42	1,451.5	5	690	101	589
1943–47	[1] (1,850.7)	6	650	[1] (121)	429
1948–52	2,250.0	6	530	141	389
(1938–52)	[1] (1,850.7)	17	610	[1] (121)	489

[1] The arithmetic average of the values for the 1938–42 and 1948–52 periods.

The expected death rate from leukemia among radiologists, if they had received no occupational exposure to radiation, is estimated by first calculating the death rate they would have experienced if subject to United States white male death rates from leukemia. This calculation has been made for a 3-year period from 1939 to 1941 by first computing (36) the mean annual age-specific United States white male death rates from leukemia (table 7) and then applying them to the 1940 age distribution of radiologists (table 6), restricting the computation to the 35- to 74-year age interval. The resultant expected death rate for the latter age interval is 63 deaths per million per year.

The same type of calculation has been carried out for a 3-year period from 1949 to 1951 by computing (36) the appropriate age-specific death rates for that period (table 7) and applying them to the 1950 age distribution of radiologists. The resultant expected death rate is 88 deaths per million per year.

The average of the rates for the 1939–41 and 1949–51 periods is 76 deaths per million per year. The latter rate should roughly approximate the mean annual death rate from leukemia which radiologists would have experienced during the 1938–52 period if they had been subject to United States white male death rates for this disease. The observed death rate for this period was 610 deaths per million per year (table 5), which is 8 times the expected rate, just calculated.

It is possible, however, that reasons other than radiation exposure may account for the high death rate from leukemia among radiologists. For example, leukemia might be more likely to be diagnosed among radiologists than among the group of all United States white males. To correct for such possibilities as this, the expected death rate of 76 deaths from leukemia per million per year, calculated in the preceding paragraph, is multiplied by a correction factor of 1.6. This factor is the ratio of the observed number of deaths from leukemia among physicians who were nonradiologists to the expected number of deaths calculated on the assumption that such physicians were subject to United States white male age-specific death rates for leukemia.

This factor of 1.6 has been inferred from data for the 1938–42 period given by Dublin and Spiegelman (32) and is applied throughout the entire 15-year period from 1938 to 1952 to give the expected death rates shown in table 5. It is a conservative factor in the sense that it is possible that the increased death rate from leukemia among physicians who are nonradiologists is partly due to exposure of some of them to ionizing radiation (31). Thus, 121 (1.6×75.5) deaths per million per year is probably a conservative estimate of the expected death rate from leukemia among radiologists in the 1938–52 period, if they had received no exposure to radiation.

The expected death rate from leukemia, just calculated, would be expected to yield 3.4 ($15\times1850.7\times121\times10^{-6}$) deaths among radiologists during the 1938–52 period. It is appropriate at this point to compare this with the observed number; namely, 17 deaths (table 5). The probability of observing 17 or more when the expected number is 3.4 is readily obtained from the Poisson distribution and is found to be less than 1×10^{-6}. Hence, the observed value exceeds the expected value at a statistically highly significant level.

The difference between the expected death rate from leukemia calculated on the assumption of no occupational exposure to radiation and the observed death rate is designated the "incidence of radiation-induced leukemia," L. For a stationary population chronically irradiated at a constant dose rate, D, the incidence, L, can be approximated if it is assumed that the probability of leukemia per rad of accumulated dose per year, P_L, is a constant for all age groups in the population and for all values of the accumualted dose. On these assumptions, a sta-

tionary population exposed for a mean number of years, E, to the dose rate, D, will have an incidence of radiation-induced leukemia that can be expressed as follows:

$$L(=)(D)\cdot(E)\cdot(P_L)$$

To estimate the value of E, it is assumed that occupational exposure of radiologists starts at age 25 and ends at age 65. The value of E for individuals who were at ages 35 to 74 in 1940 can then be approximated from the age distribution of radiologists for that year (table 6) and is found to be 24.7 years. The corresponding value of E approximated from the 1950 distribution (table 6) is 24.1 years. The average of these 2 values, 24.4 years, is used as the value of E for the population of radiologists who were at ages 35 to 74 years in the 1938–52 period. The best estimate of L is 489 deaths per million per year (table 5), and a likely range of values for L is 270 to 800 deaths per million per year, based on the 95-percent confidence limits for the observed death rate of 610 deaths per million per year. For reasons discussed later, the value of D is estimated to lie between 3 and 30 rad per year.

The best estimate for the range of values of P_L is then given by the expression

$$P_L \text{ (likely range)} = \frac{489}{24.4\times(3 \text{ to } 30)} = (0.7 \text{ to } 7)\times10^{-6}$$

per individual per rad per year

A broader range, based on the confidence limits for L, is $(0.4 \text{ to } 11)\times10^{-6}$ per individual per rad per year.

TABLE 6.—*Age distribution of radiologists in 1940 (32) and 1950*

Age	Percentage distribution as of	
	1940	1950 (July 1)
Under 35	8.3	6.1
35 to 44	31.3	38.9
45 to 54	33.8	26.6
55 to 64	19.8	18.7
65 to 74	6.1	7.9
75 and over	0.7	1.8
Total	100.0	100.0

Since the dose rate, D, in the foregoing discussion, represents the average absorbed dose rate to the leucocyte-producing system, it is likely to be lower by at least a factor of 2 than the "air" dose rate to which radiologists were exposed. The recommended maximum dose rate (in air) for such workers was set at 0.2 roentgen per day in 1931 by the United States National Committee on Radiological Protection; this rate was reduced to 0.1 roentgen per day in 1936 and to 0.05 roentgen per day in 1949. Although some radiologists may well have exceeded the recommended dose rates, it seems unlikely that the average dose rate for all radiologists in the group under consideration would have exceeded the permissible limits set in 1931. Thus 30 rad per year has been taken as an upper limit for the absorbed dose to the leucocyte-producing system. The lower limit for D has arbitrarily been taken as one-tenth of this, or 3 rad per year.

This estimate that D might be much less than 30 rad per year is somewhat at variance with the following conclusions from a recent study of longevity among radiologists (37). "In comparison with nonexposed physicians, the shortening of life of radiologists is 5.2 years, or 11 percent of the adult life span (after 20 years). If extrapolation from the animal data * * * is permissible, this would be expected to result from chromic whole-body exposure to about 1.5 LD_{50} dose, or possibly 1,000 roentgens. Although this exposure was partial body and possibly less effective, it seems unlikely that the equivalent whole-body exposures differed from the above value by a factor greater than 2 or 3. Consequently, it appears that, within these limits at least, extrapolation from short-lived animals to man may be made with some confidence on the basis of percent life shortening per unit dose."

The shortening of life by 5.2 years just cited is based on the observation that during the period 1930–54 the difference between the mean age at death of physicians estimated to have had "no known contact with radiation" and the mean

age at death of radiologists was 5.2 years (37). It can be calculated (38), however, that a difference of at least 6 years would be expected in this case solely as the result of differences in age distribution (as of 1940 or 1950) between radiologists, on the one hand, and all physicians, on the other. That is, radiologists may have a slightly longer life span than physicians as a whole. Moreover, for the 1938–42 period, Dublin and Spiegelman showed that, after appropriate adjustment for differences in age distribution, the total death rate from all causes was lower for radiologists than it was for all physicians combined; however, this rate was slightly higher for radiologists than it was for all specialists combined. Thus, either a chronic whole-body exposure of 1,000 roentgens does not have a marked effect on longevity or, more probably, radiologists have averaged much less than this as a lifetime absorbed dose.

TABLE 7.—*United States white male death rates from leukemia per million per year*

Age	Period	
	1939–41	1949–51
25 to 34	18	26
35 to 44	29	34
45 to 54	55	68
55 to 64	104	149
65 to 74	154	276
75 plus	181	385

Discussion

Table 8 summarizes the various estimates of the probability of leukemia derived from the four sets of data reviewed here. For acute whole-body irradiation the best estimate of this probability will be taken as 2×10^{-6} per individual per rad per year. This value is based on the studies of leukemia among the survivors of atomic-bomb radiation. For acute partial-body irradiation, the available data are conveniently discussed in terms of a probability of leukemia "of bone-marrow origin" (ankylosing spondylitis patients) or a probability of leukemia of "lymphatic origin" (thymic-enlargement patients).

As has already been noted, granulocytic and lymphocytic leukemias may have bone-marrow and lymphatic origins, respectively. Since these two types of leukemia constitute the majority of all leukemias and occur in proportions which are, for present purposes, roughly equal, it is assumed that the best estimate of the probability of leukemia of bone-marrow origin is one-half of that for all leukemia, or 1×10^{-6} per individual per rad to the red marrow per year. Similarly, the best estimate of the probability of leukemia of lymphatic origin is taken as 1×10^{-6} per individual per rad to the lymphatic system per year.

These estimates fall within the range of values calculated for either the ankylosing spondylitis patients or the thymic-enlargement patients. Moreover, there is some evidence that leukemia following irradiation of the spinal marrow is primarily granulocytic (26). Whether lymphocytic leukemia predominates in the thymic-enlargement series (6) is uncertain on two grounds: (i) It is difficult to differentiate granulocytic and lymphocytic types in infants and children; and (ii) some irradiation of bone marrow would, in any case, be expected in this series of patients (39). Finally, the best estimate of the probability of leukemia following chronic whole-body irradiation is taken as identical with that for acute whole-body irradiation—namely, 2×10^{-6} per individual per rad (of accumulated dose) per year. This value is seen to be close to the lower limit of the range of values deduced for radiologists.

Simpson et al. (6) and Court Brown and Doll (24) point out that their studies lack a control in the form of an unirradiated series of patients. Thus, the possibility is not excluded that thymic-enlargement and ankylosing spondylitis predispose toward leukemia. However, a comparison of the various estimated ranges for the probability of leukemia (table 8) suggests that patients with the aforementioned conditions are no more prone to develope leukemia than are radiologists or the Japanese survivors.

Presently available determinations of the incidence of induced leukemia per year are based on average followup periods that are comparatively short in

terms of the normal human life span. Thus, the probability of leukemia per individual per rad per year may not be constant for an indefinite period beyond the initial time of irradiation. By choosing the lower limit for the probability of leukemia at about 0.7×10^{-6} per individual per rad per year, it is felt that adequate account is taken of the possibility that the incidence of leukemia per year following an acute dose of radiation may, as some have sugegsted on the basis of the data from Hiroshima (37), reach a peak followed by a steady decline. It is noteworthy, however, that Court Brown and Doll have concluded, from an analysis of 108 cases of leukemia among the exposed populations of Hiroshima and Nagasaki, that "the data provide no evidence of a sharp peak in incidence at any particular period after the explosion nor any clear indication that the incidence had yet begun to diminish by the end of the ninth year" (40).

The probability of leukemia per individual per rad per year is nearly constant over a rather wide range of doses in the case of the Japanese survivors (table 3) and in the case of ankylosing spondylitis patients (table 4). This is presumptive evidence that the relationship between incidence of induced leukemia and dose of radiation is either linear or approximately linear. A striking feature of the Japanese data shown in table 2 is that the incidence of leukemia in zone C—the zone with a calculated average "air" dose of 50 rem—is significantly higher than in zone D, the control zone ($P=0.02$, by the Chi-square test). Thus, these data provide no evidence for a threshold dose for the induction of leukemia. Moreover, chronic irradiation at a relatively low dose rate (perhaps 0.1 rad per day or less) appears to induce leukemia in radiologists at a rate per rad which is comparable to that observed for the Japanese survivors. This finding also fails to support the concept of a threshold dose below which leukemia will not develop.

A linear relationship between the incidence of leukemia and dose of radiation, which is suggested by the available data for man, may have its explanation in a somatic mutation hypothesis (41). Thus, radiation-induced leukemia may result from a somatic gene mutation, presumably occurring in one of the precursor cells destined to give rise to mature leucocytes. Such a mutation might cause the cell, or its descendants, to acquire an unregulated growth habit, or to release, or to respond to, viruslike or hormonal agents—to mention only a few of many possibilities. Thus, the somatic mutation hypothesis and other hypotheses for the origin of radiation-induced malignancies (42) are by no means mutually exclusive. Gene mutation has long been known to show a linear relationship with respect to dose of ionizing radiation from studies with *Drosophila*. This linearity has been extended by Spencer and Stern (43) to doses of 50 and 25 roentgens. Gene mutation is also known to be directly proportional to the accumulated dose of radiation, even when the radiation is chronically administered at a relatively low dose rate, as in the studies of Uphoff and Stern (44).

The concept of somatic mutation is also helpful in attempting to explain the long period of time which sometimes intervenes between irradiation and onset of leukemia. Thus, it may be that some of the precursor cells of leucocytes lie quiescent for years before they are brought into leucocyte production. A somatic mutation in such a cell might, therefore, be long delayed in producing its effect.

In leukemia of spontaneous origin, there is also likely to be a somatic mutation component which would be attributable to spontaneous mutation in the somatic cells. In addition, there is likely to be a hereditary component in spontaneous leukemia—that is the presence of defective genes (dominant or recessive) which are transmitted through the germ line and which result in, or predispose toward the development of, leukemia. It is well known from the work of MacDowell and associates (45) that the pronounced differences among certain strains of mice in suspectibility to leukemia have a genetic basis. In man, there is evidence for familiar factors in leukemia from the work of Videbaek (46) and others, but the type of inheritance involved is not clear (47). It should be noted that cases of leukemia which arise somatically—for example, those which are radiation induced—will tend to obscure the analysis of the hereditary component in leukemia (48).

It is likely that there will be individual differences in susceptibility to radiation-induced leukemia as well as to spontaneous leukemia. The indication of a linear relationship between dose of radiation and incidence of leukemia implies that there are some individuals in whom a single radiation-induced event (per-

haps a gene mutation) suffices to produce leukemia. There may, however, be other individuals in whom two or more such events would be required before leukemia would be manifested. Thus, the values of the probability of leukemia per individual per rad per year that have been derived here apply to the average individual in a given population, but do not necessarily apply equally to each and every individual in that population.

SPONTANEOUS LEUKEMIA AND NATURAL BACKGROUND RADIATION

The possibility that a portion of the spontaneous incidence of leukemia may be due to radiation from natural background sources is briefly considered. For this purpose, the same type of approximation procedures employed for assessing radiation-induced leukemia among radiologists is applied to the data of MacMahon and Clark on the spontaneous incidence of leukemia in the white population in the borough of Brooklyn (table 1). Thus, the incidence of leukemia, L_B, that would be attributable to irradiation of that population from natural background sources can be approximated by assuming that it is a product of the following three quantities: (i) A constant dose rate, D_n, from all natural background sources; (ii) the mean age, E_B, of the Brooklyn population, which is equivalent to the mean number of years exposed to D_n; and (ii) the probability of leukemia, P_L per individual per rad per year. The value of D_n is not known but probably is in the range of 0.1 to 0.2 rad per year (49). The value of E_B can be readily approximated from the age distribution (table 1) of the Brooklyn population, and is about 33.7 years. The value of P_L is chosen as the best estimate from the aforedescribed radiation studies, namely 2×10^{-6} per individual per rad per year. Thus, L_B can be estimated as 7 to 13 cases per million per year. The observed total spontaneous incidence in this study was 64.4 cases per million per year (13). Thus, possibly 10 to 20 percent of the spontaneous incidence of leukemia in this Brooklyn population is attributable to ionizing radiation from natural background sources.

A maximum value for the probability of radiation-induced leukemia may also be inferred from the Brooklyn data. The calculation of such a value is based on the incidence of acute leukemia, since in this form of the disease the time of onset and time of diagnosis probably nearly coincide, while in chronic leukemia some years may elapse between these two times. The observed incidence of acute leukemia has a minimum value of 12 per million per year which occurs in the 20–29 age group (table 1). By assuming that individuals in that age group had an average accumulated dose of not less than 2.5 rad (0.1 rad per year for 25 years) and by further assuming, as an artifice, that all of the acute leukemia in that age group was due to radiation, the probability of acute leukemia may be estimated to have an upper limit of 5×10^{-6} ($12\times10^{-6}/2.5$) per individual per rad per year. Since the overall ratio of acute to chronic forms was about 1/1 in the Brooklyn data, it may be inferred that the maximum value, or upper limit (table 8), of the probability of leukemia (acute and chronic) is about 10×10^{-6} per individual per rad per year.

APPLICATION TO RADIOSTRONTIUM EXPOSURE

The foregoing estimates of the probability of radiation-induced leukemia have been attempted in order to have some basis for assessing direct effects of ionizing radiations on human populations. An example of the application of these estimates to a manmade radiation exposure—namely, that from radiostrontium (Sr–89 and Sr–90)—is briefly discussed (50).

The maximum permissible concentration (MPC) of Sr–90 has been set at 1 microcurie for the total body for workers with radioisotopes (51). A level of 1 microcurie of Sr–90 per 1,000 grams of calcium (the mass of calcium in the average adult individual) has been designated as 1 "MPC" unit of Sr–90 (52). Various estimates are at hand for the level of radiostrontium that is being accumulated in the human body as the result of past testing of atomic weapons (53). The present discussion is restricted to examination of the following recent suggestion for a permissible level (presumably of Sr–90) for the population at large (54). "There seems no reason to hesitate to allow a universal human strontium (very similar chemically to calcium) burden of one-tenth of the permissible, yielding 20 rep in a lifetime, since this dose falls close to the range

of values for natural radiation background. Visible changes in the skeleton have been reported only after hundreds of reps were accumulated and tumors only after 1,500 or more."

A body level of 0.1 MPC is expected to irradiate skeletal tissue at a dose rate of about 0.25 rad per year, on the assumption of uniform distribution of Sr–90 throughout that tissue. Because of the limited range in tissue of the beta particles emitted in the decay of Sr–90 and of its daughter element, Y–90, the leucocyte-producing cells may receive somewhat less than this dose rate, depending on the exact location of such cells with respect to the surrounding calcium of the bone. This reduction factor of perhaps 2, tends to be offset by the fact that ingested Sr–90 is not uniformly distributed throughout the skeletal tissue, but appears instead to be concentrated in regions more actively concerned with red-marrow formation (55). The dose rate to the leucocyte-producing cells is estimated as 0.1 to 0.2 rad per year for a body level of 0.1 MPC of Sr–90. This irradiation will be largely restricted to the skeletal tissue, since (i) the radiation from the decay of Sr–90 is exclusively of the beta type and (ii) 70 percent of the Sr–90 in the body is estimated to lie in the skeletal tissue (51). Hence, leukemia induced by Sr–90 would be expected to be largely of bone-marrow origin (56).

TABLE 8.—*Summary of the estimates of the probability of radiation-induced leukemia per individual per rad per year*

Source of estimate	Type of radiation	Region irradiated	Types of leukemia produced	Probability of leukemia of specified type per individual per rad (or rem) to region irradiated per year		
				Estimated range		Best estimate
				Lower limit	Upper limit	
Atom-bomb survivors	Gamma rays plus neutrons	Whole body	All	0.7×10^{-6}	3×10^{-6}	2×10^{-6}
Ankylosing spondylitis patients	X-rays	Spine	Granulocytic (only?)	0.6×10^{-6}	2×10^{-6}	1×10^{-6}
Thymic enlargement patients	do	Chest	Lymphocytic (only?)	0.4×10^{-6}	6×10^{-6}	1×10^{-6}
Radiologist	X-rays, radium, etc	Partial to whole body	All (?)	0.4×10^{-6}	11×10^{-6}	2×10^{-6}
Spontaneous incidence of leukemia (Brooklyn, N. Y.).	All natural background sources	Whole body	do		10×10^{-6}	2×10^{-6}

The problem of assessing the incidence of Sr–90-induced leukemia from a constantly maintained level of Sr–90 is essentially identical with that dealt with here for determining the component of the spontaneous incidence of leukemia owing to natural background radiation. Thus, the incidence of Sr–90-induced leukemia in a stationary population maintaining a constant level of 0.1 MPC of Sr–90 is considered to be the product of (i) a dose rate of 0.1 to 0.2 rad per year to red bone marrow; (ii) a mean age for the stationary population of 31.7 years, which is that expected from the age distribution of the total United States white population as of July 1, 1955 (57); and (iii) a probability of leukemia of bone marrow origin of 1×10^{-6} per individual per rad to bone marrow per year. This computation gives an incidence of 3 to 6 cases of Sr–90-induced leukemia per million per year. For a population of 1.6×10^{8} individuals, the current population of the United States, the expected number of cases of leukemia induced by a constantly maintained level of 0.1 MPC of Sr–90 would thus be about 500 to 1,000 per year. The range for this estimate is a factor of about 3, giving 150 to 3,000 cases per year. Currently (1954), there are about 10,500 deaths from leukemia per year in the United States population (10). Thus, if Sr–90 induces leukemia of bone-marrow origin at the same rate (per rad as X-rays and radiations from atomic bombs, then a constantly maintained level of 0.1 MPC of Sr–90 would be expected to increase the present incidence of leukemia (in the United States) by about 5 to 10 percent.

SUMMARY

Leukemia in man can be induced by ionizing radiations, and also occurs spontaneously. For the average individual in a population, the probability of developing radiation-induced leukemia is estimated to be 2×10^{-6} per rad (unit of absorbed dose of radiation) per year. The available data from 4 independent sources make it likely that this estimate is valid within a factor of about 3, giving a range from 0.7×10^{-6} to 6×10^{-6} per rad per year. It is pointed out that 10 to 20 percent of the spontaneous incidence of leukemia (Brooklyn, 1943–52) may result from radiation from natural background sources. It is estimated that a 5- to 10-percent increase in the current spontaneous incidence of leukemia would occur if the population were to reach and maintain a body level of Sr–90 amounting to one-tenth of the "maximum permissible concentration."

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34. The 1950 distribution is based only on radiologists listed in the section of the 1950 American Medical Directory devoted to membership in radiological societies. Each name in that list was looked up in the main body of the directory to determine year of birth and type of specialization. Only those listed as "R*" (an asterisk signifies that practice is limited to radiology) and resident in the continental U. S. were used in compiling the final age distribution. This procedure therefore fails to include radiologists who were nonmembers of radiological societies; however, this deficiency has been taken into account by establishing that the 12 deaths from leukemia from 1943–52, inclusive, were in fact radiologists whose names were listed in the section of the 1940 or 1950 directory devoted to membership in radiological societies. (I am indebted to Alethea Miller, Janet Chaitkin, and Joan Lewis for assistance in compiling the 1950 age distribution.)

35. The procedure was to obtain a list of deaths of members of one of the leading radiological societies and to search for the cause of death in the death notices of the Journal of the American Medical Association. In the period 1949–52, inclusive, only some 70 percent of all such deaths were found to have a cause of death listed. In order to have a conservative estimate of the incidence of leukemia, no correction for this defect is attempted. The four deaths from leukemia in the 1949–52 period have the following volume and page locations in the aforementioned journal: 144, 407; 147, 1065; 148, 218, 151, 488.

36. Death rates from leukemia for the years 1939–41 have been obtained from reference 11 and computed for the 1949–51 period from Vital Statistics of the United States (Federal Security Agency, Washington, D. C., 1949–51). Sources for the living population were Current Population Repts., Series P-25, No. 98 (1954); No. 114 (1955); No. 121 (1955). (U. S. Bur. of the Census, Washington, D. C.)

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Representative HOLIFIELD. We are going to have as our next witness Dr. Shields Warren from the Deaconess Hospital in Boston, and he is going to read, as I understand it, a statement from Dr. Jacob Furth on the subject of leukemia.

Dr. Warren, it is a great privilege to have you with us, and we would like to hear from you now.

STATEMENT OF DR. SHIELDS WARREN, NEW ENGLAND DEACONESS HOSPITAL, BOSTON, MASS.[1]

Dr. WARREN. I appreciate this opportunity very very much. I have been impressed that during these useful and well-planned hearings a number of scientists who are interested in radiation are presenting their findings and views. As one who has worked on the injurious effects of radiation since 1925, I am very happy to see the great influx of enthusiastic workers into this field.

I further appreciate the kindness of the committee in allowing me to testify, and I wish to say that I am speaking as an individual, and not for the various groups of which I happen to be a member.

[1] New England Deaconess Hospital, 195 Pilgrim Rd., Boston 5, Mass. Pathology. Cambridge, Mass., Feb. 26, 1898; m. 23; c. 2. A. B., Boston, 18, hon. S. D., 49; M. D., Harvard, 23; hon. D. Sc., Western Reserve, 52; hon. LL. D., Tulane, 53. Asst. path., Boston City Hosp., 23–25; instr. Path., Harvard Med. Sch., 25–36, asst. prof., 36–48, Prof., 48–; Pathologist, New Eng. Deaconess Hosp., 2 27–; New Eng. Baptist Hosp., 28–; Huntington Mem. Hosp., 38–42; Pondville State Hosp., 28–; consulting pathologist, House of the Good Samaritan, 27–43; Channing Home, 35–; dir. State Tumor Diagnosis Serv., Mass., 28–; Trustee, Boston Univ. Chmn. atomic casualty cmn., mem. exec. cmt., & cmt. path. Nat. Research Council; mem. Nat. Advisory Cancer Council, 46–49; dir. div. biol. & med., Atomic Energy Cmn., 47–52, mem. advisory cmt., 52–. Proctor award, Sci. Research Soc. Am., 52; Banting medal, Am. Diabetes Asn., 53. Diplomate, Am. Bd. Path. Med. C., U. S. N., 42–45. A. A. (v. Pres., 48); Assn. Path. & Bact. (v. pres., 47; pres., 48); Asn. Cancer Research (v. pres., 41; pres., 42–46); Soc. Exp. Path. (secy.-treas., 34–37; v. pres., 39; pres., 40; Soc. Exp. Biol.; Soc. Clin. Path.; Am. Acad.; Col. Path.; Am. Med. Asn.; Soc. Med. Consultants W. W. II; Radiation Research Soc.; Gastroenterol. Asn.; Asn. Mil. Surg.; New Eng. Cancer Soc.; cor. mem. Asn. Path. Eng.; hon. mem. Peruvian Acad. Surg.; hon. mem. Mexican Asn. Path.; hon. mem. Indian Asn. Path. (From American Men of Science, 1955.)

Representative COLE. Mr. Chairman, I wonder if Dr. Warren would mind indicating the identity of these groups and committees of which he is a member, for the record, since it does not appear in his biography.

Dr. WARREN. Yes. I am the representative of the United States to the U. N. Scientific Committee on the Effects of Atomic Radiation. I am the Chairman of the Committee on Pathological Effects, of the National Academy of Sciences Radiation Group. I am also a member of the Committee on Genetics of the National Academy of Sciences. I am a member of the Advisory Committee on Biology and Medicine of the Atomic Energy Commission. Then there are various scientific associations with which I am also associated.

STATEMENT OF DR. JACOB FURTH,[2] PRESIDENT OF THE AMERICAN ASSOCIATION FOR CANCER RESEARCH (PRESENTED BY DR. SHIELDS WARREN)

Dr. WARREN. I think it would be appropriate in view of the fact that we have been hearing a very pertinent discussion of leukemia, to introduce at this point Dr. Jacob Furth's statement. He is an associate of Dr. Sidney Farber and myself, one whom I consider as probably the world's greatest authority on the experimental induction of leukemia. He say as follows:

FACTS

Radiation causes cancer and leukemia and other body changes, but it is also the best means of identifying and controlling many of them. Induction of neoplasms—by that meaning both leukemias and tumors—by radiations is a remote possibility and occurs rarely, while the benefits are immediate and usual; hence, radiation became a tool of medicine no physician or informed patient would want to be without. Most, if not all, increased hazard from radiations resulted from its medical use, a calculated risk well taken; it is steadily diminishing with recognition and dissemination of knowledge as to where the hazards lie.

SPECULATIONS

The statements that there is no threshold injurious dose to somatic cells, and every irradiation, no matter how small will cause cancer and leukemia, as is stated by some geneticists, are mere speculation. This applies also to the statement that even background irradiation is leukemogenic. The available facts allow argumentation of both sides. In my opinion, the statements that background irradiations will induce leukemia are contrary to observations and the reverse is more likely.

Reasons to assume that a threshold exists:

(a) All reported experiments on leukemia induction by irradiation have pointed to the existence of a threshold and none suggested the lack of it.

Dr. WARREN. I might say in addition that Dr. Furth has had access to all of these figures which you have seen on the chart here, and this is one of the conclusions he has come to.

(b) The complex mammalian host is capable of compensating for subtle damage. It has been shown that partial body irradiation is not conducive to leukemia development; the unexposed parts powerfully protect the exposed part. Thus, if direct hits cause mutation, humoral substances either counteract or reverse

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[2] Began experimental studies of leukemia in 1928; was first or second (if so in independent work) to publish induction of leukemia, ovarian, breast, lung, and pituitary tumors in mice by ionizing radiation. Presented experimental evidence that leukemia is allied to cancer and some leukemias are related to mutations. Has numerous publications 1930–57 on the subject, all essentially confirmed. Presently, is president of the American Association for Cancer Research. Recipient of high awards and fellowships in scientific organizations in recognition of scientific contributions. (From American Men of Science.)

their actions. Were it otherwise, leukemia among physicians and radiologists and others exposed to small doses of X-rays repeated over long periods of time would be manyfold that actually observed. Some radiologists receive thousands and tens of thousands of roentgens while the population at large receives a few r. of background irradiation in a lifetime. Similarly, if cancer induction is simply due to direct hit mutation with no threshold, one would expect a tremendous number of all kinds of tumors in medical personnel and others on parts exposed to radiation; for example, skin cancers on hands. The early radiologists who got such cancers had severe radiation burns with chronic ulcers in which the tumors arose. Some scientists even argue that the cancers arose from the nonirradiated adjacent skin. It deserves emphasis that cancer did not arise on the hands of tens of thousands of people receiving huge quantities in small doses over long periods.

(c) Similarly, leukemia development in experimental animals can be prevented by post irradiation infusion of marrow cells indicating that either direct radiation hit is not enough to cause leukemia or that body defense can somehow counteract this damage.

(d) The very idea that leukemia and cancers result from a direct hit mutation was never solidly proven and is being challenged recently. Newer evidence unquestionably indicates that some indirect factor plays a determining role in development of leukemias or tumors. Heavy irradiation of some organs can injure specific body regulatory mechanisms and cause cancer indirectly, not by mutation.

(e) In case of pituitary or ovarian tumor induction by irradiation, there is no such linear relationship between irradiation dose and response, as is characteristic for mutation. The reverse is true, and there is a clearly defined threshold which is that dose which markedly depresses the function of that organ—about 80 to 50 r. to the mouse's ovary, 30 μc of I–131 to mouse's thyroid.

(f) Human cells are eternally submitted to small doses of endless kinds of mutagenic agents; some are endogenous, as hormones; others are extrinsic, as chemicals in food, industry, drugs, et cetera. Even plastics and food dyes can cause cancer in animals under given experimental conditions. These, too, are believed to cause the neoplasm by somatic mutation, but I know not of a single human cancer proven to be caused by them. As to leukemia, many drugs and industrial chemicals injure blood forming organs and could be responsible for increased incidence of leukemia. We have yet to learn to what extent the endless number of potential carcinogens to which man is exposed contribute to development of neoplasia in man, alone and combined.

(g) Induction of leukemia in mice from radioactive substances as radiophosphorus and radiostrontium has been reported, as might be expected, from large doses of them, but these reports clearly show existence of a threshold.

RECOMMENDATIONS

(1) Since the medical hazards of radiation are worth taking and since these represent the bulk of radiation hazards, that thus far created by bombology, being a minor evil, should be considered as such. While it is agreed that the latter should be eliminated as expediently as possible with preservation of the safety of the free world, the burden of decision rests not with biomedical investigators, but with miltary experts. All biologists admit the potential hazards of all kinds of radiation and merely argue among themselves about the magnitude of the hazard.

(2) I wish to testify that support for long-term research has been, and still is, niggardly, and it is a disappointing struggle to undertake such research. I recommend liberal long-term support of creative scientists, and incidentally, more centers of knowledge in free institutions. Creative knowledge is our best defense.

Dr. Warren. I appreciate your allowing me to read this statement of Dr. Furth into the record, Mr. Chairman.

Representative Holifield. Thank you very much.

Dr. Warren. Then if I could go back to my own statement.

Representative Holifield. Certainly he puts the issue very plain in his presentation there.

Dr. Warren. Yes. He does this on a background of more than 30 years' experience in this field working with leukemia.

Representative COLE. Mr. Chairman, I would like to inquire of Dr. Warren if he could interpret Dr. Furth's comment with respect to the need for long-term research in which Dr. Furth says it is a disappointing struggle to undertake such research. What did he have in mind?

Dr. WARREN. What he has in mind is this: It is much easier to obtain support and much more satisfying for the scientist to work in a field where he can hope to get results in 1, 2, or 3 years, rather than to work with long-term experiments where he may spend his whole life and still come up with an unsatisfactory result at the end of that period of life. This is the sort of thing that makes it so essential to continue on a long-term basis our studies of the population in Hiroshima and Nagasaki. We may get very few results.

I would like to point out that the results at the lower end of the scale that have been used by Dr. Lewis are not considered as actually statistically significant. They may provide a guide, but I would not want to base any firm conclusions on them.

These studies must be continued. But we know that the chances of getting significant results are relatively few. This is a discouraging type of work. It is hard to get support for it because we have to be honest and say it is quite possible that we will spend funds for 20 years and then not have anything to show you. It was that that Dr. Furth was commenting on.

Representative COLE. Thank you.

STATEMENT OF DR. SHIELDS WARREN—Resumed

Dr. WARREN. I would like to make it clear that much has been learned about radiation effects. Now I am speaking for myself. There is much more still to be learned. We have a great deal of data from animal experimentation. We have in addition much data on the effect of radiation on man derived from a number of different sources. These are perhaps worth mentioning.

You have heard of the normal or background radiation to which all of us are subjected. We know that the human race not only has developed in background levels of radiation similar to those of Washington but in regions such as Denver where the radiation is greater. Thus, during 30 years in Washington, a person might receive on the average a total accumulated dose of about 3.1 roentgens. In Denver or other mountain regions, because of increased cosmic radiation, this background might go as high as 5.5 roentgens.

In India a large population has lived for many centuries in the state of Kerala on sandbanks containing monazite. Recent studies of the radiation in this area have shown it to be up to 5 or even 50 times normal background. This population will be studied very carefully medically, but it is of interest that this relatively high level of radiation has not been sufficiently obviously detrimental to the population as a whole to cause abandonment of the region. However, one cannot say what the effects have been until very careful studies have been carried out.

The misfortune of men and women in the past has been wisely utilized by scientists to gain information as to the acute and chronic effects of radiation, and we actually have, as you have heard from the

experts testifying today, a large body of information as to what occurs in man.

To review briefly, we have data on acute exposures at varying levels of radiation from Hiroshima and Nagasaki. The studies on the degree of shielding from radiation afforded by structures in which the survivors were at the time of explosion are now being carried out and will greatly sharpen the information that we now have.

We have data on acute radiation exposure from those involved in the Los Alamos accidents and the minor accident at the Argonne. Some acute radiation from the shorter lived radioactive components of fallout of the close-in type was received by the crew of the Japanese fishing vessel and the Marshallese Islanders in 1954.

Data on chronic radiation in humans derives from the early workers with X-rays and radium as well as from radiologists up to the present day. Also, a considerable body of information has been gathered from patients treated for one or another disease with radioactive isotopes, radium or X-rays.

In general, we know that exposure to acute whole body external radiation will produce death for 50 percent of those receiving about 400 to 600 r.

Second, a single dose of radiation produces life shortening at significant levels. Human beings are too variable in their responses to radiation and in their state of health to permit any direct correlation, but it is probable that an acute dose of about 300 r. or repeated small doses totaling 2 to 3 times that would produce up to 5 years' shortening of life span. It will produce an increased incidence of leukemia. At present the rate of leukemia for the few most heavily exposed survivors at Hiroshima is about 1.3 percent. Radiologists, some of whom have received chronic irradiation on the order of 1,000 r. have 7 to 10 times as much leukemia as has the general population.

If there is a large neutron component in the initial acute exposure to radiation the likelihood of development of cataract is increased.

Radiation, whether acute or chronic, has a definitely damaging hereditary effect, because, in contrast to most cells of our bodies, there is no threshold for damage to the hereditary material and there is no recovery from injury in them. In chronic radiation, this is an important difference between the effects on most cells of a person's body and the effects on his germ cells. Since there is an appreciable power of repair possible in the body cells a higher dose is required to damage them seriously than is required to damage the hereditary material that perpetuates the race.

With acute or chronic radiation there is what is called a threshold effect in body cells. In other words, because many cells can continue to function even though irradiated and many cells in the body can be repaired even though damaged, we find that at low levels of radiation there is no observable effect.

This morning you heard mention of Senator Anderson's wristwatch. My own wristwatch has a luminous dial, and I measured the radiation from this on the back of the watch, putting the measuring device in the position of the skin on the back of my wrist. Assuming that I wore this 12 hours every day—actually I wear it a little more—the skin on the back of my wrist would receive 10 milli-r, or ten-thousandths of an r, and has been receiving it for close to 20 years. Yet this skin is just as normal as is the adjoining skin. That is, I feel

there is a definite threshold effect, and that until this threshold effect is exceeded, I am not going to stop a radioactive wristwatch.

This power of the body to repair itself, other than the hereditary material, has important bearing on the amount of radiation that man can withstand without demonstrable evidence of harm.

The present rate of testing of atomic weapons is such that the radiation from worldwide fallout is appreciably less than the background radiation. From the standpoint of heredity we should watch closely the levels of radiation.

The National Academy of Sciences report on radiation indicates that the doubling dose for mutations probably is in the range of 30 to 80 r, but may be as low as 10 r; it has been suggested that it could possibly go even as low as 5. Many geneticists believe that 30 to 50 r may be the doubling dose.

Representative COLE. Would you explain what you mean by a dou bling dose for mutations?

Dr. WARREN. Yes. There are a certain number of mutations that occur in the race quite naturally at the present time. You have heard of infants that have been born with imperfectly formed digestive tracts, for example. You have seen people who have 1 blue eye and 1 brown eye. These are the extremes of the sorts of mutations, some insignificant and some significant. We have hundreds of thousands of genes, and a change, a mutation in any one of these will produce changes under appropriate circumstances in the cells that are derived wholly or in part from that.

Senator JACKSON. Mr. Chairman—at that point, how can you tell whether it is due to the inevitable process of genetics and how can you tell when it is due to outside influence? How can you trace it?

Dr. WARREN. Only by very careful experimentation. These estimates are based on the best experimental data that we have available at the present time. There is some evidence derived from the eighty-thousand-odd births that have been studied in Hiroshima and Nagasaki as well.

So I would rather not answer that question in detail, because there are others who are geneticists who will be speaking. But in general I feel that we have reasonably sound foundations to emphasize that probably 30 to 80 r is a pretty good estimate for a level of radiation that will bring about twice as many mutations as now occur in the population.

Senator JACKSON. But all mutations are not due to radiation.

Dr. WARREN. No, indeed; not all congenital effects are due to mutations. For example, mutations can be simulated very closely by injury done to a fetus in utero, if the mother has had an attack of German measles or certain of the other types of virus diseases.

Senator JACKSON. While you do not want to go into this, I take it, because this is more a problem for the geneticists, you feel that they can tag and differentiate between mutations that are a natural result—the inevitable mathematical conclusion out of so many births—and mutations due to the outside influence of radiation?

Dr. WARREN. Yes. There is a very large-scale experiment that Dr. Russell, who is carrying on that experiment at Oak Ridge, will go into for you in the course of these hearings.

Senator JACKSON. I think it would be very important because this goes to the heart of the problem and unless you can tag them and

associate them with the problem that we are reviewing here, it would not be meaningful.

Dr. WARREN. Yes. Although not a geneticist, but as a scientist I am firmly convinced that radiation will produce mutations. The estimates that have been made by the majority of geneticists appeal to me as reasonable and sound estimates.

Senator JACKSON. In that connection, Dr. Warren, have there been and studies made of the situation as in Denver where people live at 5,000 feet as distinguished from people living at sea level? I was told that these mutations do not occur as anticipated.

Dr. WARREN. One would have to get a much higher level than occurs in Denver to reach the doubling dose that we have spoken of.

Senator JACKSON. What about in the Andes?

Dr. WARREN. The difficulty in the Andes—I have been at Moracoacha and a number of the other high altitude villages in the Andes—is that the population there is so short lived from other causes—public health is so poor—that it is very difficult to get any satisfactory statistics. I think that we can hope to get much more valuable data from the studies in the monazite areas and these studies are being carried forward by the Indian Government at the present time.

Representative HOLIFIELD. Of course, the length of life in India is much shorter than it is here.

Dr. WARREN. That is quite true.

Representative HOLIFIELD. There are a lot of factors that might enter into it, and not only the comparison of their longevity and ours, but also there would have to be a comparison of the average length of life in India, and those who live on these monazite sands.

Dr. WARREN. Very fortunately there is a very similar population of the same ethnic character and the same social status who live about 10 to 20 miles away. There has been no significant intermarriage between the two groups. So we hope that the Indian Government will have a good built-in control.

I have been speaking of this possible level of the doubling dose of 30 to 50 r. Since there is uncertainty in these figures and since many years of observations will have to be made before they can be firmed up we should take no chances but use a conservative figure such as 10 r for all types of added radiation, of which medical diagnostic X-rays will use a portion.

Representative HOLIFIELD. At this time, in order to get a realization of what a chest X-ray would expose a person to, how many roentgens would you say a person would receive from a chest lung X-ray?

Dr. WARREN. This would depend on the type of X-ray, Mr. Holifield. If it were one of the photoroentgen type, it would be higher than a full chest. We are speaking here not of the direct X-ray, but the scatter from that direct X-ray to the gonads. You heard this morning from a very competent radiologist, Dr. Friedell, and since he is still in the room, I believe, I wonder if he could tell you what he uses. That would make the point even stronger and more real.

Representative HOLIFIELD. Dr. Friedell, I suggest that you come forward. You do not need to leave your chair, sir.

My question, to make it more direct, would be this: What would be the exposure of a chest X-ray—as long as you are here, I will add another—and a fluoroscoping examination of the chest, and what would be the scatter to the gonads?

Dr. FRIEDELL. I am glad you make this distinction. First of all, there is a difference between radiation on the thorax and radiation to the gonads. The radiation to the thorax is considerably larger than to the gonads. As Dr. Warren pointed out, that makes a difference whether you have the miniature kind of chest examination which is really a photograph of a fluoroscopic image or whether you have an ordinary X-ray film that many of you have had for various studies.

Somewhere of the order of six hundredths to one tenth of a roentgen is given to the thorax for an exposure to get a satisfactory chest film. Depending on the various methods that are used for protection of the gonads and the possible protective devices which may be placed over the gonads, the dose to the gonads is considerably reduced. From the scatter alone, it may be as low as one one-hundredth of the dose given to the thorax. I would not want to put a firm figure on it because it is a function of how it is done.

From the point of view of fluoroscopy, there is not any comparison between the amount of radiation delivered to the chest and to the gonads, because of scatter when fluoroscopy is used, because at the present time the fluoroscopic methods require a large dose of radiation to be visible on the fluoroscopic screen. Depending on the time, I would say that a chest could easily receive as much as 5 to 15 roentgens in one examination.

Representative HOLIFIELD. In the case of exploring for a swallowed safety pin by a child, for instance, where you have to probe with instruments, and you follow it with your fluoroscope, what would be the exposure?

Dr. FRIEDELL. That is difficult to estimate, but I think this would help you. Most fluoroscopic machines will turn out somewhere in the order of 5 to 10 roentgens a minute. Some will turn out much more, but they are not really carefully controlled. Generally the lower limit is about 5 roentgens a minute. This determines in effect how much radiation will be received by the body in general, and is generally fairly easy to calculate what might be received by the gonads. If the radiation is directed to the gonads for various reasons, they receive much more.

Representative HOLIFIELD. Do you think there is a comprehension on the part of most radiologists of the importance of the damaging effects of this scatter from a genetics standpoint?

Dr. FRIEDELL. I think this is a difficult question for me to answer. I know that people in whose circle I move are very concerned with the problem and are examining it very carefully. I would say that the radiologists in general are now very acutely aware of this problem. It is conceivable that they were not aware of it 10 or 15 years ago, and are now beginning to institute all the necessary measures to get as much protection as we can.

Representative HOLIFIELD. Certainly when they are utilizing a machine with such potentially damaging effects, they should from a professional standpoint guard the people as much as possible.

Dr. FRIEDELL. I think I would agree with this, but I would also like to add to this that you are always faced with the problem of measuring the value of this medically as compared with the possible hazard that is introduced. This is a very difficult thing to measure sometimes. It is conceivable that much error can be introduced, but

I think that most physicians are acutely aware of weighing these two things and must do the best for the patient.

Senator HICKENLOOPER. I would like to ask Dr. Friedell or Dr. Warren a question or two.

I wonder if you ever knew Dr. Erskine?

Dr. FRIEDELL. Yes, in Iowa.

Senator HICKENLOOPER. He was an old friend of mine who died a few years ago. He died without doubt from radiation which he got in the early days from his experimental work. He did some pioneering work, especially on the mechanics of measurement of radiation in those days.

The question I want to ask is somewhat along the line of Congressman Holifield's question. From a statistical standpoint, I think manifestly years ago—20, 30, 40 years ago—when the average small or large town physician's office did not seem complete unless he bought an X-ray machine, and without doubt used it with great frequency without realizing the potentials of this machine, without the ability to measure quantities or absorption or anything of that kind, and with little or no schooling in it, I wonder if there is any statistical background that would tell us how many cases of leukemia or perhaps induced cancer or something of that kind might have occurred in the American population during those periods when there was very little appreciated as to the long range possible effects of radiation of this kind.

Dr. WARREN. I think I might be able to answer, if I might, Senator Hickenlooper.

Senator HICKENLOOPER. Yes.

Dr. WARREN. I had been interested in the problem of the life span of both radiologists, general practitioners and certain specialists. We find that the life span of the general practitioner is not significantly at variance with the life span of white males over 25 in the United States. The average doctor starts his practice somewhat around 25 years of age, so that is what we took.

This means, then, that the average doctor, not a specialist in radiation or not in the specialties using radiation a great deal, such as orthopedic surgery, urology and some of the other specialties, has about the same life span. There is evidence that he has slightly more leukemia, but not as much as the radiologist who has 7 to 10 times as much as the males in the general population. He has possibly half again as much. It is rather hard to pin it down exactly.

I think it should be remembered that there are relatively few of the general practitioners who used their X-ray machines all day long. They would use them from time to time on their patients, and had appreciable rest periods during which their body cells could recover from the radiation injury done.

Senator HICKENLOOPER. I either heard or read some testimony with respect to the data on physicians, but the real point of my question I was directing at the use of X-ray in treatment years ago on patients when the effects of those X-rays were not so well known, and there was a period of time some years ago when it was really quite widespread, and there is no telling what the strength of the treatment would be that many patients received at that time. I wonder if there would be any statistical data that could indicate malignancies of various types from that treatment, rather than from natural causes.

Dr. WARREN. You saw on the chart an estimate as to X-rayed adults here. I think that the best data on this are the group with so-called ankylosis spondylitis—a form of arthritis of the spine—an X-ray treatment gives some relief to the pain and may help the course of the disease somewhat. A group under the direction of Dr. Court Brown in England studied this very carefully. I have here the white paper on radiation effects issued by the United Kingdom approximately a year ago. It gives an indication that the dose ranges ran from as little as 500 *r*, or possibly a little under that to the spine, up to more than 2,750 *r*; that this caused an increase in the crude incidence of leukemia—these are uncorrected figures—ranging from 4.1 per 10,000 people treated at the lower dose level, or 2.2—which might be sheer chance, at less than 500 *r*—up to 17.6. So arguing from this, I think it might be said that there were probably a scattering of cases of leukemia induced in the way you spoke of.

Senator HICKENLOOPER. Would you have an estimate at this time as to how long a period of time it has been since you feel that you can have some reliable data on leukemia, and many other ailments which people undoubtedly had many years ago, but which were not diagnosed by the physicians? I remember when they used to say people died of acute inflammation of the bowels, when it was probably a burst appendix, and that sort of thing.

Dr. WARREN. Yes. I think you pointed out a very important thing, Senator Hickenlooper, that medical diagnosis is steadily improving. I think in certain areas of the country in the larger medical centers, leukemia has been pretty well recognized from 1930 on; for the bulk of the country, leukemia has been very well recognized from 1945 on. I think our statistics from 1900 to 1910 may have caught perhaps half of the leukemia cases or something of that order. This is only a wild estimate, however.

Senator HICKENLOOPER. Thank you very much.

Representative HOLIFIELD. Thank you very much. You may proceed with your statement.

Dr. WARREN. At present the rate of radiation from fallout gives a probable 30-year dose of 0.1 roentgen. The data on chronic radiation to our bodies and those of animals indicates that rather more than the acute lethal dose of radiation can be withstood, though not without harm, if it is given over a protracted period of time. The effect of protracted radiation may be half or less as great as radiation given at a single time. If significant damage is done to body cells there is never complete repair, but rather atrophy persists and eventually cancer may develop.

The ill effects known to come from chronic radiation, are, as you have heard, damage to various body tissues ranging from the destruction of cells to undue or cancerous proliferation of cells. Thus, in the skin of the early radiologists, we saw atrophy occur, finally ulceration, and in some instances even skin cancer. The blood responds at first to radiation at low levels by minute and insignificant changes in some cells. For example, the lymphocytes may show a rare cell with double nuclei, the meaning of which has not yet been established. Continued exposure to radiation leads in some people to the failure of formation of adequate blood cells condition known as anemia or agranulocytosis, or, at times, to an overly enthusiastic reparative response which leads to the development of leukemia. Chronic exposure

from radium, particularly radium absorbed internally, has been shown to be injurious and radiation changes in bone can be detected with levels of radium in bone as determined years afterwards on the order of 1 microgram. Of course, these levels were initially appreciably higher.

Since one of the radioactive fission products, strontium 90, is deposited in bone, there is much concern to advance our knowledge of radioactive strontium, the amount that enters our bones, and the effect that it may have there. Strontium 90, at fallout levels or at levels many times higher, has no significant genetic effect. Neither is there firm evidence that it has a leukemia producing effect. If we assume that the radiation effect from strontium 90 or from other sources has no threshold (and this assumption is contrary to most existing information with regard to somatic effect) if we assume this, I say, it would follow that there would be a small statistical increase in bone tumors. I doubt very much that it would cause any increase in leukemia. It is striking that in those persons who have had radium deposited in their bones there has been no evidence of leukemia, even though they have developed bone sarcoma. The evidence for the possible development of leukemia from strontium 90 rests on mice treated with radioactive strontium that showed leukemia. However, leukemia is so common a disease spontaneously in mice that I hesitate to accept this observation as contradicting the information we have from experience with humans and with a number of animal experiments at the present time.

Let us, however, make the worst assumption, that there is no threshold and that we might be concerned with a linear increase in both leukemia and bone sarcoma. On this basis, as you have heard, the average level to be expected from uptake of strontium 90 already produced by weapons testing may be about five so-called sunshine units. While there is no evidence that even 10 times this level is harmful, if we assume that there is no threshold, I would be reluctant to see the average strontium 90 content of bones, particularly in children, go much above 10 times the present level. It is possible that additional experimental work will enable us to go safely beyond this tenfold increase.

Representative HOLIFIELD. Thank you very much, Dr. Warren.

Senator Jackson, do you have any questions?

Senator JACKSON. I have no further questions. I am very happy to see Dr. Warren back with us. We are very proud of his great contribution while he served as Director of the Division of Biology and Medicine of the Atomic Energy Commission.

Dr. WARREN. Thank you very much, Senator Jackson.

Representative HOLIFIELD. Dr. Warren, will you be back with us in a few minutes for our discussion period?

Dr. WARREN. Thank you.

Representative HOLIFIELD. At this point I would like to say that it is my understanding that Dr. L. H. Hempelmann, of the University of Rochester, Strong Memorial Hospital, will deliver a paper in Pittsburgh on June 11, called Irradiation-Induced Cancer in Man. When we receive a copy of this paper I would like to insert it into the record at this point.

(The material referred to follows:)

IRRADIATION-INDUCED CANCER IN MAN

L. H. Hempelmann

The possible development of cancer, and this includes leukemia, is an occupational hazard known to persons working with X-rays or nuclear radiation. Recently, this type of radiation carcinogenesis has become a matter of increasing concern to the scientific world and to the world at large. There are several reasons for this. The first is the exposure of ever increasing numbers of people to man-made radiation. (This includes not only persons working in the field of atomic energy, but also people receiving the benefits of Western-style medicine, and indeed the entire world population now that the radioactive fallout from the hydrogen bomb test programs is worldwide.) The second reason for concern is in the observation that the radiation doses necessary to induce cancer are smaller than they were believed to be formerly. The third reason for the current interest in radiation carcinogenesis is the recent evidence which challenges the concept that all somatic effects of radiation are threshold reactions.

For more than 50 years, it has been known that X-rays and gamma rays can cause cancer. The first case of skin cancer in an X-ray worker was reported in 1902. Gamma rays were the first carcinogenic agent used in the laboratory to produce experimental cancer in animals. In the past half century, the literature concerned with radiation-induced cancers has become voluminous. Review of the literature shows that almost any tissue of the body will undergo malignant change under proper conditions of exposure. In most instances, these exposure conditions involve repeated or chonic irradiation of a small volume of tissue with doses totaling several thousand roentgens. This is not the kind of radiation-carcinogenesis I will consider today. Instead I will confine my attention to the incidence of malignant disease in four human populations in which the total body of each individual, or at least a substantial portion of the body, was exposed to ionizing radiation. I will also mention a retrospective study in which the history of previous radiation exposure has been determined for a group of children with leukemia and other forms of cancer.

The first group of exposed individuals in whom the incidence of malignancy has been studied is composed of the radiologists. Since the first recorded case of leukemia in a radiologist in 1912, it has been suspected that there might be an association between this disease and prior radiation exposure. This suspicion was supported by experiments in animals which showed that leukemia could be produced by irradiation with X-rays. In 1942, Henshaw and Hawkins made a systematic study of the causes of death in physicians. They observed that the incidence of leukemia in physicians was slightly higher than it was in the general male population. Several surveys have been carried out since this time to determine the incidence of leukemia in radiologists. Probably the best figure illustrating the increased leukemia rate is that of E. B. Lewis who has calculated the age-specific death rate from leukemia for radiologists and for the adult male population. He estimates that the death rate from leukemia in radiologists is approximately eight times the expected rate for the general male population. While this figure of eight-times-normal is strikingly high, it is important to emphasize the fact that leukemia is a rare disease and that the actual number of people who contract it, even among the radiologists, is not great. Up to 1948, only 37 cases of leukemia were reported in the medical literature among the thousands of people who had worked with X-rays during the preceding 50 years. I should also like to mention the fact that the cases of leukemia usually occurred late in the life of the radiologist. The average age of death of all radiologists dying from leukemia was almost 59 years. This is essentially the same as the 60-year average age at death from all causes. I should like to emphasize the fact that aside from leukemia and skin cancers, the incidence of other forms of interval cancer in radiologists is not increased. Incidentally, I should like to point out that the exposure of the radiologists is partial body rather than total body and is protracted over a period of many years.

The second group showing an association between leukemia and radiation exposure is the Japanese people exposed to the radiations from the nuclear detona-

tions in 1945. Table 1 shows how the incidence of leukemia can be correlated with the distance of the exposed individuals from the hypocenter under the explosion. In the last column, you can see the ratio between the observed and expected incidence of leukemia. With regard to the interpretation of these data, I would like to point out that the irradiation of these individuals, unlike that of the radiologists, usually involved exposure of the entire body to a single dose of mixed irradiation, primarily consisting of gamma rays. Dosage data are uncertain but the dose certainly falls off with increasing distance from the hypocenter. The latest unclassified figures that I have seen indicate that the mean dose for each of the exposure zones is 1,500 rem for the first zone, 500 for the second and 50 for the third. These are not firm figures and do not take shielding among other factors into account. To show how uncertain they are, I would like to point out that almost all the cases of leukemia occurring in persons in the 1,500 to 1,999 meter zone had severe radiation complaints; it is difficult for me to believe that they did not receive considerably more than 50 rem.

TABLE 1.—*W. M. Court Brown, R. Doll, Hazards of Nuclear and Allied Radiations (table 2A, p. 85): A comparison between the observed and the expected incidence of leukemia among survivors of the Hiroshima atomic bomb explosion exposed at various distances from the hypocenter; persons subsequently resident in Hiroshima City only*

Distance from hypocenter at time of explosion (m.)	Number of cases with onset in the 8-year period 1947–54		Number of deaths expected among the survivors in an 8-year period [1]	Ratio of total cases observed to expected
	Confirmed	Suspected		
Less than 1,000	15	0	0.15	100.0 : 1
1,000 to 1,499	28	1	1.32	22.0 : 1
1,500 to 1,999	6	1	2.33	2.6 : 1
2,000 to 2,999	6	0	3.96	1.5 : 1
3,000 or more	4	2	4.83	1.2 : 1
All distances	[2] 59	4	12.59	4.7 : 1

[1] Calculated from the Japanese mortality data for 1952. In calculating the numbers of expected deaths, certain assumptions had to be made about the rate of change of the numbers of survivors in the different age groups, and the figures must be regarded as approximate estimates.

[2] Two cases referred to in table 1A are omitted, since the onset of symptoms in one patient was in 1955 and in another patient, who died in April 1955, the date of onset is unknown; the latter patient was exposed at a distance of 2,400 meters from the hypocenter.

The third exposed population is a series of patients with a severe form of rheumatic disease of the spine known as ankylosing spondylitis. This is a painful, crippling disease occurring mainly in young men. Intensive X-ray therapy has often been used with considerable success in treating this illness. X-ray doses of 2,000 r. given to the entire spinal column through ports 10 cms. wide are not unusual. Such treatments are not given all at once but are usually fractionated over a period of weeks. Such a series of treatments is often repeated once and possibly twice. In a group of 15,000 patients in Great Britain, the incidence of leukemia has been determined. Table 2 shows how the 37 cases found in this group were distributed according to the total dose administered to the bone marrow. These data show a linear relationship between leukemia rate and dosage. If we extrapolate to the lower dose range, it is observed that the dose of radiation to the spine necessary to double the incidence of leukemia is of the order of 100 roentgens. If the dosage data is expressed not in terms of roentgens to the bone marrow but, rather, in terms of megagram-roentgens to the body, the relationship between dose and incidence is curvilinear rather than linear. One criticism usually directed at this type of clinical studies on X-ray therapy patients is the lack of a really good control group with which to compare the treated patients. The British were able to collect as controls only 400 patients with spondylitis not treated by X-rays. Another criticism that I would like to point out is the small number of cases in the low-dose range, only 2 cases of leukemia having occurred in patients receiving less than 500 r. to the spine.

TABLE 2.—*W. M. Court Brown, R. Doll, Hazards of Nuclear and Allied Radiations (table 2B, p. 89): The numbers of male patients developing leukemia and the crude incidence rates after different doses of radiation (measured by the maximum amount received at a point in the spinal marrow)*

	Amount of treatment, maximum dose to the spinal marrow (r.)						
	0	Less than 500	500 to 969	1,000 to 1,499	1,500 to 1,999	2,000 to 2,749	2,750 or more
Number of men developing leukemia	----------	2	8	8	8	6	5
Crude incidence per 10,000 men per year	[1] 0·5	2·2	4·1	4·2	11·3	13·0	17·6

[1] The rate given for "no treatment" has been estimated from the national vital statistics for all forms of leukemia, and weighted to allow for the fact that not all the patients in the series were certified as dying from leukemia. If lymphatic leukemia is excluded (as may be more appropriate) the rate is 0·3.

The fourth population I would like to discuss consists of 1,700 children treated with X-rays in infancy. They were treated for a condition known as enlargement of the thymus gland which in the past has been alleged to be associated with sudden death of a previously healthy child or with severe and sometimes fatal respiratory distress in young children. When a diagnosis of thymic enlargement was made in a sick child, it was customary to treat the child with a beam of X-rays to the region of the chest. In the 1920's and 1930's, doses of 500 or 600 roentgens or more have frequently been administered through ports which covered the entire chest of the child. Fear of the consequences of thymic enlargement became so intense that asymptomatic children who were suspected of having this condition were often given prophylactic X-ray treatment to prevent symptoms. In one city in upstate New York it has been found that approximately 1 percent of the children born between 1925 and 1950 have been treated with X-rays for thymic enlargement. This form of treatment is still used at the present time but the practice is less common and the port size is considerably smaller now.

Table 3 shows a comparison of the observed and expected incidence of cancer in children given X-ray treatment for thymic enlargement. You can see that whereas 2.6 cases of cancer would be expected to occur in a normal group of children of this size and age distribution, 17 or probably 19 were found; 0.6 of a case of leukemia should have occurred but instead 7 or probably 8 have been found. The most striking increase of all is found in the case of thyroid cancer where 0.09 case was expected and 6, or more recently 10, cases have been observed. Now as controls we have 2,000 untreated siblings of the children which, I admit, are not good controls. Nevertheless, they have 5 cases of cancer rather than the 2.7 expected cases and no cases of leukemia or thyroid cancer. (It does seem clear that this group of children with thymic enlargement treated with X-rays has an increased incidence of cancer.) Table 4 illustrates how these cases of malignant disease were distributed among the children who received more or less than 200 roentgens. In the case of leukemia, you can see that there were 2 cases among the 600 children receiving less than 200 roentgens and 5 cases among the 800 receiving more than 200 r. Although the number of cases is small, it seems likely that there is a relationship between the size of the dose and leukemia incidence. No other cases of cancer, however, were observed in children receiving less than 200 r.

TABLE 3.—*Expected and observed rates for malignant neoplasia* [1]

	Treated children		Untreated siblings	
	Expected	Observed	Expected	Observed
All cancers	2.6	17 (?19)	2.7	5
Leukemia	.6	7 (?8)	.6	0
Thyroid cancer	.08	6	.08	0

[1] From study by Simpson, Hempelmann, and Fuller on 1,722 children treated with X-rays for thymic enlargement from 1926 to 1951.

TABLE 4.—*Distribution of neoplasia according to amount of radiation*[1]

	Under 200 r.	Over 200 r.	Unknown
Number treated	604	804	313
Cases of leukemia	2	5	(?1)
Other cancers	0	4	0
Carcinoma of thyroid	0	6	0
Adenoma of thyroid	0	6	3

[1] From study by Simpson, Hempelmann, and Fuller on 1,722 children treated with X-rays for thymic enlargement from 1926 to 1951.

The last study that I would like to mention is a so-called retrospective study of the history of previous X-ray exposure of 547 British children who died before the age of 10 from leukemia and other forms of cancer. The controls in this study consisted of the best friend of the child at the time of his death. Information was obtained from the parents as to exposure of the mother as well as the child and table 5 shows the information so obtained. I will mention the only category in which a significant difference was found in the history of X-ray exposure of the two groups of children. This category involved X-ray examination of the abdomen of the child's mother during pregnancy and this is seen in the first row of figures. You can see that whereas only 24 mothers of the control children had this form of examination, 42 or almost twice as many mothers with children with leukemia were examined in this way. This is 42 out of 269. A comparable difference also hold the exposure of the mothers of children with other forms of cancer and of the control children. There were 21 such examinations in the controls and 43 in the mothers of children with cancer. Now if you assume a cause-and-effect relationship here, you can see that all cases of childhood leukemia in this series cannot be explained on the basis of X-ray exposure of the fetus. Only about 8 percent of the total cases had this type of X-ray exposure. I would like to point out here that the doses involved in this type of X-ray examination are small. The fetus received about 2 roentgens per X-ray film and usually 4 to 6 films were taken per examination.

TABLE 5.—*Lancet, Sept. 1, 1956: Past histories*[1] *of X-ray examinations and antibiotics in 547 children with malignant disease and 547 controls matched for age, sex, and locality*

Number of mothers and children X-rayed		Leukemia		Other malignant diseases		All malignant diseases	
Period	Type of exposure	269 cases	269 controls	278 cases	278 controls	547 cases	547 controls
Antenatal	Diagnostic:						
	Abdomen	42	24	43	21	85	45
	Other	25	23	33	32	58	55
Before conception of survey child	Therapeutic	------	------	------	1	------	1
	Diagnostic:						
	Abdomen	17	24	28	30	45	54
	Other	103	88	108	119	211	207
Postnatal (children only)	Therapeutic	1	------	1	------	2	------
	Diagnostic	45	49	46	50	91	99
	Shoe fittings	55	52	40	46	95	98
Total number of mothers[2]		140	130	160	154	300	284
Total number of children		89	91	75	84	164	175
Either mother or child, X-rayed		179	172	194	198	373	370
Postnatal medication (children):							
Sulfonamides		51	45	42	42	93	87
Antibiotics		68	52	50	58	118	110

[1] I. e., before the onset of the fatal illness in the affected child or equivalent period in the control child.
[2] Since a mother or child may appear in more than 1 X-ray category, the totals in this category are less than the sum of totals in the 3 preceding ones.

In summary, I would like to show (table 6) a recent paper published in Science by E. B. Lewis, on the relationship between leukemia and radiation exposure. He has taken the data from the first four surveys that I discussed and has calculated the probability of any single individual developing leukemia per rad absorbed in the bone marrow. The figures in the last column give his best estimate of probability turn out to be $1-2\times10^{-6}$ per year. It could be chance, of course, that these calculations turn out to be so close, and it must be admitted that the dosage data in the first two groups are not accurately known. If it is not coincidence that these probabilities are practically identical, then the data suggest that the amount of blood-forming tissue exposed and the time during which the exposure takes place are not matters of great importance in the induction of leukemia. In this respect, then, the leukemogenic effect of radiation would seem to be cumulative.

In conclusion, I would like to say that the data obtained from surveys of exposed human populations indicate that there is a clear association between leukemia and previous radiation exposure. The incidence of cancer, particularly thyroid cancer, may be increased in children irradiated before or after birth. But there is no evidence that other forms of cancer are more frequent in adult populations exposed to total body radiation. In the case of leukemia there seems to be a definite relationship between the incidence of the disease and the dose of radiation provided the exposures are high. The data at hand is insufficient to allow us to conclude that this relationship also holds for low-dose levels. I would like to emphasize the fact that the risk of any given individual developing leukemia is small even if he has received considerable exposure, but when large populations are involved the absolute number of people affected may be large.

TABLE 6.—*E. B. Lewis, Science: Summary of the estimates of the probability of radiation-induced leukemia per individual per rad per year*

Source of estimate	Type of radiation	Region irradiated	Types of leukemia produced	Probability of leukemia of specified type per individual per rad (or rem) to region irradiated per year		
				Estimated range		Best estimate
				Lower limit	Upper limit	
Atom-bomb survivors	Gamma rays plus neutrons	Whole body	All	0.7×10^{-6}	3×10^{-6}	2×10^{-6}
Ankylosing spondylitis patients	X-rays	Spine	Granulocytic (only?)	0.6×10^{-6}	2×10^{-6}	1×10^{-6}
Thymic enlargement patients	do	Chest	Lymphocytic (only?)	0.4×10^{-6}	6×10^{-6}	1×10^{-6}
Radiologists	X-rays, radium, etc	Partial to whole body	All (?)	0.4×10^{-6}	11×10^{-6}	2×10^{-6}
Spontaneous incidence of leukemia (Brooklyn, N. Y.).	All natural background sources	Whole body	do		10×10^{-6}	2×10^{-6}

Representative HOLIFIELD. In the meantime the staff has made arrangements for Dr. Ernest Pollard of Yale University to speak now instead of tomorrow.

Proceed, please, Dr. Pollard.

STATEMENT OF DR. ERNEST POLLARD, BIOPHYSICS DEPARTMENT, YALE UNIVERSITY [3]

Dr. POLLARD. Thank you. As a professor, I am a little panicky away from the blackboard, so may I use it?

Representative HOLIFIELD. You certainly may.

Dr. POLLARD. Mr. Chairman, I am appreciative of the privilege of being able to testify before this committee. I think I should first of all give an indication of my field of competence, because since I have never worked on anything larger than a bacterium, and this is approximately one million millionth of the size of a human being, it might very well be questioned as to whether I have any right to come up here at all. I am a biophysicist, which means I need to know biology and physics. I am one of the very small number of the nuclear physicists who at the time of the explosion of the atom bomb decided to do something about living things. I have devoted my study to biology since 1947, and in particular I and a very strong group that work for me, have been trying to find out the fundamental nature of the action of radiation on living things.

One must realize that there is throughout biology a considerable similarity. The same molecules which are concerned with animal cells are also concerned with plant and bacterial cells. We do learn the fact that there is a great importance to these large molecules, and this brings me perhaps to the first point I would like to make and that is that we really do need a much more fundamental understanding of biology.

If I might, without showing disrespect, make a short remark about the difference between physics and biology; in our atomic energy establishments great attention is paid to fundamental physics. There must be at least $25 million a year going into research on high energy machines. These are not related to the immediate problem of nuclear energy at all. They are very definitely of an esoteric character concerned with high energy problems. There is no analogy in biology.

[3] Professor of biophysics and chairman of the department of biophysics at Yale, is one of the Nation's outstanding scientists in this relatively new field of biophysics. Under a $3 million grant from the John A. Hartford Foundation, the new department of biophysics at Yale, under Professor Pollard's direction, will apply the methods of atomic and nuclear physics to a purely humanitarian and constructive end. For example, Professor Pollard is using a cyclotron to study disease-producing viruses. The son of Emma H. and the late Samuel Pollard, the Yale professor was born in Chaotong Fu Yunnan, China, where his father was a missionary for the British Methodists. He received his B. A. degree from Caius College (Cambridge, England) in 1928, and his Ph. D. in 1932 from Cavendish Laboratory at Cambridge. He was an assistant lecturer at Leeds University in England from 1930 to 1933, when he came to Yale as a Sterling Fellow. He was appointed an instructor in physics at Yale in 1936, was named an assistant professor in 1939, an associate professor in 1942, and was promoted to the rank of professor in 1950. He was chairman of Yale's biophysics committee from 1946 until 1955, when he was named chairman of the department of biophysics. Mr. Pollard helped design and build Yale's first atom-smashing cyclotron in 1939. During World War II he worked on radar problems at the radiation laboratory of the Massachusetts Institute of Technology. For his research, which was instrumental in establishing radar systems in Europe and the Pacific to detect the approach of enemy planes and to guide lost Allied aircraft back to their home bases, he was awarded a Presidential Certificate of Merit. He is the author of Applied Nuclear Physics, published originally in 1942 and again in a second edition in 1949. He also is the author of Microwaves and Radar Electronics (1947); Physics of Viruses (1953); and of numerous papers published in scientific and technical journals. From 1951 to 1954 he was a member of the Scientific Advisory Board to the U. S. Air Force. (Submitted by witness.)

There we could use the fundamental biological research, and it would be a fine thing to see this disparity removed. I think it could be removed in part within the atomic energy operations. I do need to say that.

The nature of radiation action is worth a moment's thought. It is very energetic. I could not help wondering whether an analogy to the question as to whether there be a threshold effect would not be something like this. If you notice that a thousand shafts of lightning knock down a thousand trees in a town whether you would argue that one shaft of lightning would knock down one tree. I cannot help thinking that there is something like that. The energy released by ionizing energy is enormous, far in excess of any chemical process, in excess by a factor of between 10 and 100 always.

In view of this very energetic release, the large molecules which are very important in biology, are inactivated. We have in our group studied over 30 of these since the war, and as for the cases where we observed a recovery of the actual effect which is received from the radiation damage, I cannot recall one such case occurring.

When we say there is recovery—unquestionably there is no doubt that a human being irradiated does recover—he is calling up either some other mechanism in a cell or he is calling on a new cell. The problem we have really to ask ourselves is, therefore, the question as to whether this recovery mechanism is going to be adequate for the rest of that person's life. Whether we are going to let the cell which has been damaged go through the 30 or so more divisions it may yet have to undergo in the lifetime of the person, and say we know it has recovered adequately, or whether we take a rather careful viewpoint on that, and say we are not so sure.

Representative HOLIFIELD. Are you talking about the somatic cells?

Dr. POLLARD. Yes; I am speaking about somatic cells. I believe the germ cells undergo forty-odd divisions, and a run-of-the-mill cell, not all—the peripheral cells in the skin are not going to be used any more—the ones that are deeper down will have to be used again, and a human lives a long time, and many divisions have to occur.

We are stating as an act of faith that when we say recovery is complete, that that recovery is adequate for something like 60 years, and many divisions. This is an act of faith which I as a biophysicst am not ready to undertake. I would definitely not want in the record any statement that I thought such recovery would last throughout the lifetime of an individual. I would prefer to be very much more conservative.

The energetic events that occur in the cell are very far apart anyway. If we take 100 r. as a dose, these events then occur on the average about as far apart as the cells are themselves far apart. So if we are saying that there is a threshold of an kind, what we are saying is that the events have to be close together. That is, 100 r. already has them far apart. If there is a threshold then the events must be close together. That is why there is a threshold. They must occur close together in time or in space. Even for 100 r. they are already far apart, or the order of distance apart of two cells. Therefore, we have a right to say that if 2 or more are needed in 1 cell, we would expect not a linear increase in radiation effect—we would never see a linear increase—we would always see an increase which would

look something like this. This is observed [drawing on the blackboard].

The effect would appear in this parabolic way showing a curve of this form. For the case of certain types of chromosome aberrations this is observed.

If we see something which looks like this, the tipoff is that it is aiming at a point out on the dose axis which does look like a threshold. If we do not see that, then I think we have to presume as a hypothesis to work on and guide ourselves that it will indeed go straight down and will be linear. If it looks like it is linear up high, the probability is that it is linear down low. It is a very strange thing to explain a curve of the following form (drawing). I know of no interpretation of a curve which is linear in the upper part and ceases to be linear below. It is very queer. I know nothing about fundamental radiology that leads me to expect an effect of that kind. So if I see a linear effect up high, then I suspect it is indeed linear all the way down.

I notice that the usual effect in a class is taking place and a little diversion will not be harmful. I have what is getting to be the trademark of the biophysicist. These are on the market now as poppet beads. I can represent here one form of the key molecules of biology. This is a molecule of nucleic acid. It occurs in a long chain and it is doubled. On this chain are different colored beads indicated. They are the markers which determine our heredity and combinations in some manner of these are responsible for what conditions the nature of a cell and its future existence will be.

What radiation can do is either break one of these in which case we have a partial chain, or it can break both. If both are broken, and this is a somewhat unusual event, and it may account for a little difference between cosmic rays and internally contained radiation, there is no simple agency of repair, not for a molecule. They may indeed repair and randomly perhaps they do. But if a break like this occurs and is perpetuated, then just as I will have trouble putting this back together—and I do not want to take the committee's time to do so—so will the cell have trouble putting it back together. The damage will therefore be in some measure permanent.

The lack of our seeing permanent damage is partly the lack of our knowledge today. We do not know how to look. I would like to make an analogy between a person whose car has a convoy run into the side of it and scratches the paint. I can easily see that the people in charge of the convoy might say that the motor is not affected and the tires are all right, but if he did not know about the phenomenon of the paint rusting, he would not come to accurate estimate of the damage to the owner. The owner might in fact have sustained considerable damage to the car.

It is the same today. We don't yet understand how the cell works and how it is tied into the whole body. In fact, in the things that concern degenerative disease, we have almost no knowledge. We have very good knowledge on communicable disease, but in degenerative disease we are not so sure. I would like to submit that radiation does produce a scratch, and it always produces a scratch, and it would be a conservative and wise thing to say that we were not sure whether that scratch were important or not, and to then treat it as though it was, because it might be like the paint on the automobile, it can run into a hundred dollars.

I have been asked by the committee to answer one specific question. This is the question as to whether radiation from materials such as strontium, which is inside the body, differs in any way from radiation which is outside, such as cosmic rays. I have put this to my group, and we have thought about it. It is a very difficult question. It is not a simple question to answer. I think for the guidance of the committee, I would like to give an answer as follows:

The only reason why the effect could be less would be that some radiation were buried in the bone, and it would have to be buried deep, and in a part that does not matter. I do not think that even bone can be considered as inert. Certainly if a bone breaks and heals, it means it is not inert. I would therefore be very surprised if there was ever any case in which internal radiation were less effective than external radiation. I would expect it by all likelihood to be the same.

There are, however, reasons that might make it more. If it should turn out, as been said by two people, Dr. Mazia and Dr. Steffensen, that calcium binds together the units of the chromosome and strontium could replace calcium, then indeed internal radiation of that type might be extremely effective.

Therefore, one might get a higher proportion.

Personally, I would be surprised if this effect exceeded 2, and I would be very surprised if it exceeded 10. I would suggest for a working operation for the committee that it treat external and internal radiation, as equal as regards its physics only—I am not speaking about whether irradiating the bone marrow or the bone is more important.

Specifically, for example, I would say that internal potassium which competes with strontium is probably far less effective and less dangerous because it is primarily in the muscle and the strontium is in the bone. I am not concerned with such matters of being near vulnerable material. I am speaking solely of the physical nature of the radiation. I would suggest that it be approximately the same. Thank you.

Representative HOLIFIELD. Thank you very much, Dr. Pollard. Are there any questions?

(A supplementary statement by Dr. Pollard follows:)

FIELD OF COMPETENCE

My field of competence has been (1) in the nature of the physical character of radiation in which I have worked since my graduation from Cambridge in 1928, (2) the action of radiation on large molecules, viruses and bacteria, a field in which I have worked since 1947 with the strong support of a group of able people, (3) in this work we have followed a fundamental approach aimed at the understanding of the basic character of radiation action.

RADIATION AND BIOLOGY

Understanding radiation action requires understanding of biology. It is here that we lack certainty and a large part of this lack is due to the low priority which has been accorded biological research in the national scheme. A great deal of effort has gone into guided missiles, weapons, and into fundamental research in high energy physics and reactor physics. In the process, until very recently, the fundamental biological studies have not had the research support which they should have had. Some of this is apparent in the fact that a biology doctor of philosophy can expect to earn rather less than a physics doctor of philosophy, even though his field of work is definitely of more human value and should therefore be paid more highly. It is hoped that the committee will look into the use of Atomic Energy Commission funds for the support of research,

and to see whether there is a disparity between the physical and the biological sciences.

GENERAL STATEMENT OF RADIATION AND CELLULAR PROCESSES

With the limited understanding that we have, we can say that no living cell which has received the effects of penetrating radiation is the same as before. By this I mean that if a group of ionizations are released within a cell they will cause an alteration at the point where they were produced which will either damage a molecule of importance to the cell, or produce effects which will have the equivalent of poisoning a molecule not far away.

Once this alteration has been produced it may well be that the cell appears to recover, but if it does so it is solely because it has not been put in circumstances requiring its full capability. There is no evidence from our work that any appreciable part of radiation damage is restored fully.

Therefore, radiation puts a heavy demand on the replacement of cells. This replacement may not be perfectly achieved; by which I mean that a new cell which is grown from a healthy cell may not be exactly the same as the one which was damaged, and which had to be replaced. In this case, as time goes on and these cells divide, more and more there may occur long-delayed effects. These are due to the fact that the replacement cell did not contain exactly the right complement of parts for some function later in life. In this case late effects are to be expected, and they are found.

RADIATION WITHIN AND WITHOUT THE ORGANISM

Radiation produced from within the organism could only be less effective for one reason—burial in nonactive material. The burial would have to be deep and the material would have to be quite definitely nonactive. This to my mind is highly doubtful because there seems to be little or no nonactive material in a living human being and also the depth to which the burial would have to occur exceeds that of the thickness of most bone. So I would feel that it would be highly doubtful that radiation from within the organism could ever be less effective than that from without. Otherwise one would expect it to be equal. It could, however, be more effective. This would be the case if the radioactive element were part of a chromosome or any essential cellular element. This increased effectiveness has been observed for P–32 in viruses and bacteria. No work has yet been done on Sr–90 or Cs–137 in simple organisms. It should be done.

To summarize, I would be surprised if the effectiveness of internally applied radiation exceeded external by twofold, very surprised if it exceeded by tenfold, and I would find it even more surprising if it proved to be less effective than external.

BEARING ON PRESENT TOLERANCES

The presences of a microcurie of Sr–90 could in 1 year cause serious damage to at least 10^{12} cells. This is an appreciable fraction of the cells invaded or approached by Sr–90. It would seem unwise to consider such a stress on cell replacement as "tolerance." K–40, which is tenfold less in amount and spread far more widely, does less damage and yet it may be a significant factor in ultimate aging.

It would seem very unlikely to me that it would indeed be true that the effects of 1 microcurie of Sr–90 in even a small fraction of the population would not be clearly observed after 30 years of observation.

It seems to me that there is too great a tendency to apply laboratory tolerances to the whole world population. Pollution in the air of a chemical laboratory which is borne cheerfully by chemists would be intolerable in the atmosphere of a city. Therefore I suggest a reduced maximum tolerance and the policy of regarding all radiation with suspicion.

Representative HOLIFIELD. Now we will invite the following persons foward to the conference table to discuss today's testimony: Dr. Friedell, Dr. Brues, Dr. Lewis, Dr. Hardin Jones, Dr. Ernest Pollard, and Dr. Shields Warren.

There has been some conflicting testimony today and I am not just sure how to start, but certainly Dr. Lewis' testimony and that of Dr.

Furth are in contradiction to each other. As long as Dr. Warren had the last say on Dr. Lewis' testimony, I would like to ask Dr. Lewis—would you like to make a comment on the criticism of your statement?

DISCUSSION BY DRS. H. L. FRIEDELL, AUSTIN BRUES, EDWARD LEWIS, HARDIN JONES, ERNEST POLLARD, AND SHIELDS WARREN

Dr. Lewis. Thank you, Mr. Chairman. I made the point near the end of my testimony that the conclusions that I arrived at did not depend in any way upon assuming that leukemia results from a somatic mutation. I believe that one of the objections was that I had assumed that leukemia was due to such mutation.

Another possible criticism, I believe, was the implication that radiologists have received tremendous doses of radiation as evidenced by claims in some quarters that they have experienced a 5-year reduction in life span. I would estimate that the average radiologist did not receive more than about 10 roentgens per year of occupational exposure, judging from the place on the chart where the slant line intersected the green, dashed line. This suggests that radiologists as a group should not experience the marked decrease in life span that some have claimed.

In my opinion, radiologists do not suffer more than a 1-year reduction in life span. This conclusion is supported by the studies of Dublin and Spiegelman for the period 1938 to 1942, which indicate that radiologists live longer than physicians.

Representative Holifield. You used 1938 to 1942?

Dr. Lewis. That is correct, but I have also extended the observation to 1952. I should hasten to add that radiologists do not live as long as medical specialists. Medical specialists are a select group. They have a life expectancy greater than the general practitioner. Radiologists belong to the group of medical specialists, but they do not live so long as the average medical specialist. So, radiologists do suffer some reduction in life span, but probably much less than a 5-year reduction. This means that radiologists have probably averaged much less than 1,000 r. as a lifetime absorbed dose.

I do not remember other points now that I would care to comment on. I hesitate to do so, in any case, since Dr. Furth is not present today.

Representative Holifield. Dr. Jones, would you care to comment on what Dr. Lewis has said, and then we will ask Dr. Friedell.

Dr. Jones. Yes; I would. The radiologists in my opinion do show a life span shortening effect. The analysis is somewhat complicated. It began with Dr. Shields Warren's study of the average age at death of radiologists, which showed it was approximately 5 to 6 years less than for physicians in general and indicated radiologists would live less long. There is also Dr. Lewis' own study, in which he arrived at a different conclusion; however, by looking at the current distribution of ages in the radiologists, one could show there is a very marked shift in the age distribution toward the younger men, because they have come into the field relatively recently. The whole population of radiologists is expanding. Over the last 20 years, it has expanded by a factor of two, at least. It means that the use of approximate

methods of analysis, such as "age at death," can lead to erroneous conclusions.

Using Dr. Warren's data, where I have a list of the ages of the radiologists who died, and also Dr. Lewis' population census of radiologists, and putting these together, we can analyze the data in one of the usual ways of looking at vital data, by finding age-specific death rates of radiologists.

We find, for radiologists up to age 50, the death rates are quite comparable to the average white male deaths in the general population, and are quite comparable to the death rates seen in physicians as a whole. Above age 50, one finds there is an increasing death rate in radiologists, which is quite pronounced at ages 65, 70, and above, where ultimately the radiologists are dying twice as fast as the general population and all physicians of the same age.

Senator JACKSON. From a scientific standpoint, would you say that the data are fairly accurate on a comparative basis? Are these people dying of heart attack and so on? Would you say that was induced by their profession or work? I am just asking the general question whether you feel that the data from a scientific standpoint are reliable.

Dr. JONES. The way life-shortening effects of harmful conditions in general work, including the effects of radiation, is that average ordinary causes of death come on sooner than they would otherwise.

Senator JACKSON. Does it follow that the work in radiology is the proximate cause?

Dr. JONES. One is looking for small absolute values, and one must be very, very careful. We do not know, in this case. It would be the thing that we would suspect as the foremost of the list of things that we could suspect.

Senator JACKSON. Are there other specialists outside the medical field who die in the same age range?

Dr. JONES. I think it would be very worthwhile to examine mortality data for all the medical specialists and do it in the same way. I would also recommend that a much more thorough study of radiologists be made.

Senator JACKSON. Unless you get accurate case histories and run down a series of them and find out exactly what they died of, how could you really determine that their work in their profession is the proximate cause of shortening of their life?

Dr. JONES. You may not be able to determine this, even after you have run down case histories. These are very difficult things to do.

Senator JACKSON. Is not this necessary if you are going to have good data from which to make your conclusions?

Dr. JONES. I am sure we would all agree we would like to have much more accurate data on all things of this sort, but significant conclusions may be possible even from the limited data available.

Dr. WARREN. If I might make a comment here, Mr. Chairman, I think that one of the striking things about the effect of radiation in shortening life is that there is no specific means by which it shortens life. You cannot pick out a particular disease and say, "This is the cause of radiation life shortening. You have to take the overall picture. Whether you deal with mice or fruit flies or rats or dogs, pigs, goats, humans, this general trend of life shortening shows up all the way through, not from any one specific cause. I think this is somewhat comparable with the mutation effect. The rate of muta-

tion is increased, but there is no mutation that is characteristic of radiation. There are a great variety of mutations that are brought about. I think this goes back to the very sound point made by Dr. Pollard and some of the earlier speakers, that the significant changes are changes that take place in the fundamental building blocks of our cells, and the manifestation of these changes may be apparent in quite a wide number of ways.

Representative HOLIFIELD. Dr. Jones, do you have something to add?

Dr. JONES. When I have my next turn, I will have something to say.

Representative HOLIFIELD. Dr. Friedell, do you have a comment?

Dr. FRIEDELL. I have no prepared comments. I think I would have to state my position as saying that I am concerned by what these gentlemen have said, but not yet fully convinced.

First of all, I think you want to make sure that the selection of cases from both groups are exactly the same. Apparently evidence is being presented which shows these may be comparable. I think it is important to show that the activity of radiology itself does not attract into it people who are likely to have a higher death rate, especially at the higher ages, because very early in radiology an individual who had one sort of illness or another was often given the advice to enter radiology, because it appeared to be a sedentary occupation. I do not know whether this in any way alters the figures at all, but I think it is well to look at this from every point of view. It is difficult trying to make this decision from the statistics alone.

An example of how this might occur is something that was presented by George Bernard Shaw many years ago. He was violently opposed to immunization as I think many of you know. Statistics were presented to him to show that as immunization increased, various communicable diseases decreased in England. He hired somebody to count up the telegraph poles erected in various years in some particular streets, and it turned out that telegraph poles were being increased in number. He said, "Therefore, this is clear evidence that the way to eliminate communicable diseases is to build a lot of telegraph poles."

All I would like to say here is that the important point is that if you really want to understand it, you have to look at the mechanism of the occurrence. I think this is where the emphasis should lie.

I am very pleased to have Dr. Pollard speak to this matter as he did this afternoon.

Representative HOLIFIELD. I will withhold my remarks until I ask Dr. Brues to comment, and then back to Dr. Jones.

Dr. BRUES. I think a lot of specific comments have been made. I would like to make a sort of general one about scientific evidence, because that seems to come up here.

If you have two experiments with the same kind of mice treated in the same way, you will expect the second one to come out the same way the first one did. You take a prediction of that sort as simply representing honesty on the part of the investigator. That is why the experiment was repeated in which the irradiated mice lived a little longer because it was difficult to believe, and needed to be confirmed. I think perhaps a lot of our experiments that came out the "right" way should be repeated also.

The next experimental evidence is where you irradiate one kind of mice, and then you infer from that that some other sort of mouse will behave in the same way. That is not quite so scientific a route, and you have to spread your work out a bit to get the answer.

Then there is the third situation where you do an experiment, having to do with a certain situation, and you transfer the results of that quite a distance. You demonstrate a certain percentage of life shortening at a high level, and you guess from this that you will see a certain amount at the low level which it would be impossible to ever detect. Or you study it in the mouse and you multiply the result by the difference in the life spans, and you come out again with a figure. When this is mixed up with mechanisms that you have no way of looking at, it becomes a little more speculative.

I find personally that Dr. Lewis' figures are extremely interesting and suggestive. I think that probably some of the warmth of the statements that have come out in relation to this are owing to what some people have perhaps done to Dr. Lewis' work, saying that so many leukemias will be produced under certain conditions. I think if we look at it that way, we are perhaps a little less in danger of overweighting our thoughts on the side of radiation hazards and forgetting a lot of other things that are important and related.

Representative HOLIFIELD. Thank you. Now Dr. Jones.

Dr. JONES. I would like to go back to the leukemia study, particularly among the Japanese. It is unfortunate but there is a great deal that is unknown about these people. We do not know the doses that go with any individual person with any certainty. When I say "any certainty," the uncertainty in this case is a very large number. Many of these cases appear to have received, on the record, doses that may have run 5 to 10,000 r., and yet some of these individuals have essentially shown no radiation symptoms, in spite of being out in the open. These are rather unbelievable upper limits of radiation doses to have produced no visible effects.

On the other hand, there are other individuals on the record who reported severe radiation symptoms under circumstances where, because of the geometry, they have received only 5 r., or less.

It is possible to do some major reconstruction of the data. I myself believe that the thesis that Dr. Lewis presents is a very good one to go by at this time, namely, that the induction of leukemia by radiation is proportional to the dose. Everything we can test critically here suggests the idea of proportionality.

In the first place, the individuals who first put this tentative data together—the most tentative, I guess, ever put together for humans under critical assessment—thought that the evidence suggested that no leukemogenic effect occurred until some threshold dose was reached. Since they are the primary individuals who put this data together, I think their conclusions should be kept in mind as we go on with other considerations. I prefer not to go along with this hypothesis of a threshold effect, but I think the scientists ought to weigh their evidence carefully.

If we do the best job we can—I have spent much of my time trying to reconstruct this data myself—and look at the individuals whose symptoms show that they got definite exposure, and we can estimate the dosage rates to go with these symptoms, then the story that Dr.

Lewis presented to you becomes quite intensified. Instead of finding the probability of leukemia to be one chance in a million per person per roentgen exposure per year of life lived after exposure, the probability would be 5 times this value, or 5 times 10 to the minus sixth chances of leukemia per roentgen exposure per person alive a year after exposure. This means that the chance of developing leukemia may be very much greater than Dr. Lewis estimated. If it is this larger value which is applicable (and it is not the upper limit one could derive from this data, but simply one plausible estimate), it would mean that the 3 to 5 roentgens of natural radiation that the Japanese ordinarily received, just as Americans ordinarily receive this amount in their lives, would account for the reported incidence of leukemia among the general population in Japan. This is something we cannot conclude today, but it is something we ought to keep in mind, because this is what the data may really mean when we finish analyzing them.

I think that this kind of information warrants development to the utmost refinement, because we are going to have to make a great many practical conclusions based on this kind of evidence.

Representative HOLIFIELD. Dr. Friedell.

Dr. FRIEDELL. I intended to make some comment about the curves that were drawn here with regard to the incidence of leukemia and dose. I feel that if we leave out other considerations that help the data, and look at the data as they exist, we find that it is not possible to make any conclusions about the low doses. This has been amply discussed by everybody who has examined this, including Dr. Lewis, who presented it this way.

I would like to point out that really one of the important decisions is not what the incidence of radiation is at high levels so much, and not whether radiation at higher levels will produce effects, but whether there really is a threshold or not. Thus when we get into the very low dose levels, we are going to talk about whether leukemia can occur. I feel that the data presented here do not permit us to make this kind of conclusion.

I would like to point out that the way that these data should be presented is to draw some kind of distribution curve about each of these points. You can estimate quickly what the chances are that the lowest point would be at that level. It could be easily 6 plus 2½ or 6 minus 2½ two-thirds of the time. The same thing is true of the other point listed as six. This is one standard deviation. Therefore, the way to draw the number would be a spread in which you could draw several kinds of curves. I might add that this is perhaps a good first approximation, and it is reasonable to examine it this way, but I would also submit that it is not possible from these data alone to conclude at very low levels or small doses that you would necessarily have some biological effects, such as leukemia, specifically, in this case.

This kind of argument could be used for one molecule of cyanide. You could argue that 1 molecule of cyanide introduced into the body would find 1 cell on which the organism really depends, and therefore there is no threshold for cyanide. The truth of the matter is that toxicologists cannot come out with this kind of explanation unless they use the reasoning I proposed.

The other thing that could be said about this is that a reasonable way of writing this data is to put it on logarithmic type curves which

markedly extend the lower end of this to give you a good idea of how long this dotted line really is. This perhaps would be a fairer way of looking at the data so that people who may be uninitiated in this would not necessarily conclude that very low doses will surely result in leukemia.

On the other hand, I think there is evidence to indicate that we better look seriously at the idea that a single hit is effective. Therefore, I don't feel that we can in any way exclude this, but I think we ought to have some kind of reservations about how quickly and how rabidly we use these data to apply them to any decisions we have to make.

Representative HOLIFIELD. Dr. Pollard, would you have something to say on that?

Dr. POLLARD. I really do not have a great deal. It does seem that the linear line is very reasonable, I would personally be surprised if it were not something like this. Really, the only thing I can say is that I feel tolerably confident that the primary radiation damage is severe, and probably acts on the genetic part of the cells, somatic or not. What results from this, whether that cell becomes more sensitive to a virus or what occurs, I don't know. I would be very surprised if it took a lot of nagging away at that cell to produce an effect. It would either be "bingo," and it has got it, or not, and that would give the linear kind of relation.

I think the linear line is rational. I would like to see policy momentarily at least based on it. If later on it seems there is a threshold, then we are not too badly off. But if there is not a threshold, and we bet there is one, we are in trouble.

Representative HOLIFIELD. Dr. Lewis, we will let you have a rejoinder, and then we will have a 2- or 3-minute summation on this problem of threshold, and we will conclude the hearing.

Dr. LEWIS. Thank you, Mr. Chairman. I would like to go back to the point about the six individuals in the dotted portion of the curve. Remember, I said that in the zone from 1,500 to 2,000 meters there were 10 individuals who died of leukemia among 23,000 survivors; whereas, in the zone beyond 2,000 meters there were 26 cases among 156,400 survivors. The point has been challenged that this is not statistically significant—the difference between these two zones. I have not been able to put on the chart all of the figures. The figures are published in a paper, which by the way does discuss the statistical significance of the results.

Representative HOLIFIELD. Are you referring to the article in the Science Magazine of May 15?

Dr. LEWIS. Yes. I want to go back and point out that no survivor in the zone from 1,500 to 2,000 meters could have received more than approximately 100 rem. That is, 100 rem is the maximum dose in either city at 1,500 meters.

Beyond 2,000 meters there was esentially no dose at all, so there was a "control" zone. "Control" meaning here was a zone in which the people got essentially no radiation. It was much less probably than 10 r. There were 26 cases among 156,000 in this control zone, compared to 10 cases among 23,000 in the 1,500- to 2,000-meter zone. If you apply the statistical test for comparing the difference between two such frequencies, it is significant at a level that is called 2 percent. That means there are 2 chances in 100 that this could happen.

That is something that we ordinarily operate on to mean that it is not due to chance. In other words, the two zones we are considering differ significantly with respect to incidence of leukemia.

We would like to see the chance one in a million.

Representative HOLIFIELD. How reliable do you consider the Hiroshima dose curves?

Dr. LEWIS. As an upper limit they are very reliable since the physics of the atom bomb burst is what sets these so-called air doses which are upper limits. Where the uncertainty comes in is how much the survivors absorbed and whether the people were evenly distributed or not. There is still, I think, a need for a breakdown on the census figures. I think that the census material is available. I have assumed that the survivors were pretty randomly distributed throughout the zone.

Representative HOLIFIELD. Dr. Warren.

Dr. WARREN. I would not like to make any added comments on the threshold matter, but I would like to speak to the reliability of the data at Hiroshima and Nagasaki. These are as reliable human data as we have. Fortunately, there are careful studies now being made of reconstructing as nearly as possible the conditions under which the various survivors found themselves. This is a little hard to do a number of years afterward. But fortunately, in some we had fair detail quite early. I would feel that the dose levels of these individuals eventually can be calculated within plus or minus 25 percent. This is a very difficult and tedious job, because the geometry varies for each of the people involved. For example, in one building in which we studied in Hiroshima, which was within the 1,000-meter zone, there were 88 who died, 3 who survived. In subsequent study of the building, it was found that the three who survived were heavily protected by earth and cement, and approximate reconstruction can be very soundly worked out for them.

I think there is this important thing to remember, that in this whole field of radiation biology we are working on a relatively new type of research in which very few people were interested originally. When I was first interested in 1925, there were not more than 10 or 12 other people in the world interested in it. This interest and importance has been built up very rapidly. However, there were so many things to be learned that the picture had to be roughed in in very broad sketches at first, and then more and more of the detail filled in.

Our initial studies at Hiroshima and Nagasaki in September 1945 and from then on were very crude studies, indeed. They had to be. We were working vitrually without facilities, with thousands of injured patients for whom we had to care with the aid of our Japanese colleagues, who did a very fine and wonderful job. We had to learn what we could. At the same time we were trying to impart to our Japanese colleagues the knowledge that had been gained of antibiotics and other things.

That is simply to illustrate that many of these fine points will have to be worked out. There is work here for 25, 30, and 50 years before all these points can be answered. I think that we are now at the stage when many of the essential landmarks, at least, have been recognized, and we can go to a much more detailed and accurate survey.

Representative HOLIFIELD. Thank you. Dr. Warren, one of the staff members has told me that you have to be excused to catch a plane

very soon. Do you care to make any further comment, and then the Chair will excuse you if you wish to leave.

Dr. WARREN. Thank you very much, indeed, sir. I have about 15 minutes more I can spare before I go, and, because I am deeply interested in this, I would like to stay.

Representative HOLIFIELD. That is fine. Would you start the summary, then, on the question of the threshold?

Dr. WARREN. Yes. I have favored the concept of a threshold for most carcinogenic agents for a number of reasons. First, that in our experiments with carcinogenic hydrocarbons, which are known to be derived from such substances as coal tar, we find that a threshold exists for them. We find that, with many of the medicines that are commonly used for one or another effect on cells, there is a threshold effect to these medicines. We know, by analogy with simple things in physics, there is a threshold effect. For example, I can push very lightly against this stand of the microphone, and it will not move until I reach the threshold of where that push is greater than the friction which tends to hold it still.

I like to think of this reparative force, these agents and others which Dr. Furth mentioned, as being things which counteract the effect of very low level radiation. As I said, it is hard for me to believe that the cells that I know have been irradiated to a significant degree in this part of my wrist are any different from the adjacent cells. Of course, in that area, small as it is, there are tens and hundreds of thousands of cells involved.

I quite agree that it is entirely possible that I am wrong and that others are wrong. In fact, physicists were positive for many years that there could be no such thing as a solution of the atomic-energy problem, and that the atom was the ultimate extent to which matter could be subdivided. That has been proved wrong. I could be wrong. I think here, where we are dealing with matters of national policy, you need to be concerned not with possibilities—and I will admit that the threshold idea is a perfectly good possibility—but, rather, to weigh relative probabilities. I do not regard the complete linearity of the induction of leukemia as in the range of a reasonable probability. I would not rule it out as a possibility. Thank you very much.

Representative HOLIFIELD. Thank you. Dr. Brues, would you like to summarize?

Dr. BRUES. I might add just a brief word gained in justification of the possibility that there is a threshold for the production of cancer. I think, as we see experimental cancer and a lot of cancer occurring in human beings, we are impressed with the fact not that it is just something which strikes like lightning, although, indeed, many times it appears to, but that it frequently appears in a tissue which for some reason or other is already somewhat disarranged, and the lightning tends to strike in those places.

I think this concept might perhaps bring a little compatibility between the gene type of notion and the notion that some of us who have been in clinical work seem to adhere to, that there may not be a complete threshold, but there is something apparently close to being a threshold.

I would also say that although someone mentioned that if we don't take this no-threshold business very seriously, we may be in real trouble, I think we have to consider how much trouble we are in on

the basis of the natural environment in any case. We don't worry about the trouble we are in because someone invented fire.

Representative HOLIFIELD. Thank you. Dr. Jones.

Dr. JONES. This shows how much disagreement there is among us, because I think most of the evidence which exists shows that apparent thresholds are better explained in terms of the continuous probability of these effects occurring. We are dealing with the same body of facts in arriving at these varied conclusions.

Part of the difference is in the way people look at small quantities. In very small doses, you get very small effects. It is very easy to say that very small effects are zero, and then you have the threshold concept. If very small effects are just that—"very small"—then you do not have a threshold phenomenon. It is troubles of this sort that we get into when we have to multiply huge populations by very small numbers approximating zero.

Tomorrow, when I have a chance to talk at further length, I will show you much more evidence to establish the principle of proportionality in some of these effects.

Representative HOLIFIELD. Dr. Friedell.

Dr. FRIEDELL. I think I have expressed myself. I feel that the data at the very low levels are perhaps presumptive and even highly presumptive. I like the eternal consistency of the data on mutations better than any other purely from an examination of these data. I feel that the situation is not as yet established and not clear, and not conclusive. I don't really know how to suggest that this will be clarified except what I said earlier. I feel strongly that understanding the mechanism rather than looking at the statistics of the problem will give us a much better picture of the whole thing.

Representative HOLIFIELD. Dr. Pollard.

Dr. POLLARD. I would agree with Dr. Friedell's last remark. I would like to say that whereas from my point of view there is always damage to the cell, the question is really how many cells does the body have available to repair this and to what extent do these vary with lifetime, with the condition of a person, and so on. This is the broad realm of ignorance which I think we all agree we are in, and to escape which we have to do a great deal of work. I merely suggest again that the conservative thing to do in obtaining that knowledge is to assume linearity and therefore no threshold.

Representative HOLIFIELD. Dr. Lewis.

Dr. LEWIS. I would like to point out one thing more and that is that if we assume this straight line, this linearity as it is called, this absence of threshold, all these things being essentially the same thing, then we come up with these figures which I am afraid can be twisted to alarm the public unduly. I do think that the danger comes in legislating a dose that is said to be permissible for the public.

Representative HOLIFIELD. You used the word "legislation." You did not refer to the work of the committee, did you?

Dr. LEWIS. I beg your pardon. I meant standards set by Government agencies for the public. It would be better to state to the public that there is a distinct possibility that the so-called permissible dose will hurt a definite number of people. The number damaged will be relatively small we think for small doses, such as from fallout which is what we are talking about. But the percentage or the number who are expected to be damaged should be stated, instead of implying that

there is no danger from fallout or that the permissible dose will cause no damage.

Representative HOLIFIELD. The chairman will not try to summarize the summaries, but could the Chair ask this question? In referring to these incidents of leukemia or the mutation of genes you are all agreed, are you not, that you are referring to a low level of radiation which has occurred to date as a result of bomb testing, and that you are not referring in your statements today to a drastically increased rate of release of fissionable material into the atmosphere, nor are you referring to the incident of a nuclear warhead.

Dr. WARREN. Yes, that is our understanding, I believe, Mr. Chairman.

Representative HOLIFIELD. That is the general understanding of the panel. I see you nodding in assent and as the nods do not appear on the record, the Chair will state you are in assent.

The committee will resume its hearings tomorrow in the Old Supreme Court Chamber in the Capitol, room P–63, at 10 a. m. We will lead off in the morning with Dr. Crow of the University of Wisconsin. He will be followed by Dr. Glass, of Johns Hopkins; Dr. Sturtevant, of the California Institute of Technology; and Dr. Muller, of the University of Indiana. A general discussion will follow to wind up the morning session. In the afternoon we will have Dr. Russell, of Oak Ridge; Dr. Jones, of the University of California Radiation Laboratory; and Dr. William Looney, of the Massachusetts General Hospital.

Thank you, gentlemen, for being our witnesses and participating in the discussion.

(Thereupon at 5:05 p. m., Monday, June 3, 1957, a recess was taken until Tuesday, June 4, 1957, at 10 a. m.)

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THE NATURE OF RADIOACTIVE FALL-OUT AND ITS EFFECTS ON MAN

HEARINGS

BEFORE THE

SPECIAL SUBCOMMITTEE ON RADIATION

OF THE

JOINT COMMITTEE ON ATOMIC ENERGY

CONGRESS OF THE UNITED STATES

EIGHTY-FIFTH CONGRESS

FIRST SESSION

ON

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

JUNE 4, 5, 6, AND 7, 1957

PART 2

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CONTENTS

(Part 1 also contains statements by leading scientists, together with statements and papers submitted for the record.)

(A detailed subject index is now being prepared and will be published at a later date.)

[1] Each witness was asked to submit a short professional biography. Most, but not all, of the witnesses did, or, as in the case of some Government witnesses, their departments did. In a few cases the biographies came from American Men of Science.

In some cases the JCAE staff has abbreviated or deleted from the biography, for brevity, no matter what the original source. These biographies are for information only and should not be regarded as a formal statement.

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

TUESDAY, JUNE 4, 1957

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION OF THE
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The special subcommittee met, pursuant to recess, at 10 a. m., in room P–63 of the Capitol, Hon. Chet Holifield, chairman of the subcommittee, presiding.

Present: Representatives Holifield, Durham (chairman of the Joint Committee), Cole, Van Zandt; Senators Anderson, Hickenlooper, and Bricker.

Also present: Professional staff members: James T. Ramey, executive director; George E. Brown, Jr., Hal Hollister, staff technical adviser, and Paul C. Tompkins, consultant.

Representative HOLIFIELD. The committee will be in order.

This morning we continue our hearings on the effects of radiation on man, and we get into the area of genetics this morning.

Our first witness, Dr. Crow, is professor of genetics and zoology at the University of Wisconsin.

STATEMENT OF DR. JAMES F. CROW, PROFESSOR OF GENETICS AND ZOOLOGY, UNIVERSITY OF WISCONSIN [1]

Dr. CROW. Mr. Chairman and members of the committee, my intention is to summarize briefly and nontechnically the present knowledge of the genetic effects of radiations in man. I want in particular to indicate the sources of the data on which our conclusions are based, and give some idea of the reliability of quantitative estimates of genetic damage.

Previous statements before this committee have dealt mainly with effects on the person who receives the radiation. My discussion will be restricted to effects on his descendants; that is, to inherited effects.

I believe I can best summarize the general information on this question by stating four well established principles that are necessary background for any discussion of possible genetic hazards to man.

1. All high energy radiations increase the rate of mutation. A mutation is a change in the hereditary material of the cell, a change in the genes and chromosomes on which our heredity depends. An

[1] Date and place of birth: January 18, 1916, Phoenixville, Pa. Education: A. B., Friends University, 1937; Ph. D., University of Texas, 1941. Work history: Instructor and assistant professor of zoology, Dartmouth College, 1941–48; assistant professor, associate professor, and professor of genetics and zoology, University of Wisconsin, 1948; associate managing editor of Genetics, 1951–56; member of editorial board of Genetics; member of board of directors, American Society of Human Genetics; member of the council, Society for the Study of Evolution. (Submitted by witness.)

example of a multational change is that producing the bleeding disease hemophilia, characterized by abnormal blood clotting. Mutations can occur in any part of the body, but from the standpoint of heredity the important ones are those that occur in reproductive cells and are transmitted to the children. Thus, to have an effect on future generations, radiation must reach the reproductive cells sometime between conception and reproduction.

2. Almost all mutations that have been studied have been harmful.

Representative COLE. Will you repeat what you have said?

Dr. CROW. I said that almost all mutations that have been studied have been harmful.

Representative COLE. What do you mean by a "harmful" mutation?

Dr. CROW. One that in some way impairs the individual that carries this mutation. He might have his fertility reduced. He might be more prone to an early death. He might have most any kind of abnormality.

In general, most of the mutations that have been studied by geneticists have been in some way or another harmful to the individual.

Representative VAN ZANDT. After exposure to radiation?

Dr. CROW. That is right. I am talking about subsequent generations.

Representative COLE. What is a mutation?

Dr. CROW. Maybe the difficulty is that I have used mutation in two different senses. I have used the mutation as a change in the gene itself. What I should perhaps have said is that the consequence of that mutation in future generations has been harmful. I want to say, though, this is to be expected on purely theoretical grounds for the reason that a mutation is essentially a random change in an organism that is already functioning reasonably well. A random change is much more likely to be harmful than beneficial.

Consider, for example, a random rearrangement of parts in your television set. It is much more likely to make it worse than to improve its operation.

A point I want specifically to make here is that the typical mutation is not a gross abnormality.

Representative COLE. Let us be sure when you say mutation we are talking about the same change. You have indicated that you have used the word in two different concepts. To me a mutation is the consequence of some abnormality that has occurred in the ancestral genes. The result is the mutation. You have used mutation this morning as describing a damage to the parent genes themselves.

Dr. CROW. Yes.

Representative COLE. When you say mutation, let be be clear what we are talking about.

Dr. CROW. I am clear in my own mind, but whether I can make it clear in general is another question. Let me say this:

The mutation is the change that occurs in the gene, in the reproductive cell.

Representative COLE. Of the parent.

Dr. CROW. Of the parent. This changed gene is passed on to subsequent generations causing this varying amount of harm to the individuals that possess that changed gene in future generations.

Representative COLE. You mean that a changed gene inevitably passes that change on to the descendant?

Dr. Crow. That is correct.

Representative Cole. That an altered gene could not reproduce a normal child?

Dr. Crow. That is correct as far as the gene is concerned; the gene in the future that descend from the altered gene are altered.

Representative Cole. Is that always an accepted rule of genetics?

Dr. Crow. Yes.

Senator Bricker. How do you explain the fact, then, that there are always mutations going on in human regeneration, some caused, no doubt, by the normal background of radiation?

Dr. Crow. We have a certain amount of harmful mutation with us all along.

Senator Bricker. There must be some that are beneficial or there would not be the evolutionary process taking place in life.

Dr. Crow. That is correct. I did not say all were harmful. I said the great majority were harmful.

Representative Holifield. The great majority of the damaged genes are harmful, but the amount of damaged genes in relation to the total number of genes is infinitesimal, is it not?

Dr. Crow. It is small.

Let me answer Senator Bricker's point in a little detail.

Representative Cole. Before leaving this difference between infinitesimal and small; that is a tremendous area.

Dr. Crow. I agree.

Representative Cole. I would like to have you indicate on the record just what you mean by small as against infinitesimal.

Dr. Crow. Yes. Let me be a little more thorough about what you would like to know. You would like to know what proportion of the genes actually in the population are harmful versus that proportion which is beneficial?

I do not think the geneticists can answer that question in general. I do not think we have that kind of information. I can say that the proportion of sperms or eggs carrying a newly arisen mutant gene in any one generation may be of the order of 5 percent or so, perhaps as much as 15 percent.

That is what I mean by small.

Representative Cole. Newly arisen?

Dr. Crow. Yes. Newly mutated genes. By "newly" I mean having occurred in that generation.

Representative Cole. And out of that 5 percent, how many are harmful?

Dr. Crow. Practically all of them.

Representative Cole. So then your testimony is that 5 percent of the mutated genes are harmful?

Representative Holifield. Let us see if the Chair can understand.

The 5 percent that you spoke of was a rise in the rate of mutated genes. It did not refer to a 5 percent of the total number of genes. Am I right on that, or would you say that 5 percent of the genes are damaged, and the rate has risen to the point where 5 percent of the total number of genes are damaged?

Dr. Crow. I think I have confused this a great deal more than I have helped it.

Representative HOLIFIELD. No, we are the ones who have confused it, Doctor. You go ahead and straighten it out if you can. We will do our best to help you.

Senator BRICKER. Will you come to my question next?

Dr. CROW. Yes. Let me answer Senator Bricker's question.

The implication of your question is that if I say the great majority of mutants that occur are harmful, why is it that the great majority of genes that now exist in the population are beneficial?

The reason for this is natural selection. The mutant genes that have occurred in the past have been weeded out by the process of natural selection so that the genes which now are part of the normal population are those which have been retained by this process of natural selection. Therefore, even though the great majority of mutants at the time they occur are such as to cause harmful effects to the descendants, the ones which cause the most harmful effects are eliminated by natural selection. The genes left in the population are the beneficial ones.

Senator BRICKER. You would not say that all mutants are deleterious.

Dr. CROW. No. I did not say that. I said the great majority.

Senator BRICKER. There are some that are beneficial.

Dr. CROW. That is correct.

Senator BRICKER. Those are the ones naturally selected through the generations by the process of evolution.

Dr. CROW. Yes. May we go on from here? I am sure I have left the previous point in a state of considerable confusion. I think I can straighten it out as I go along.

Representative HOLIFIELD. Go ahead.

Dr. CROW. I think the things I say subsequent to this will straighten it out. So will the following speakers. I want to emphasize one point, though, and that is that the typical mutation is not a gross freak. Here I must rely largely on information from fruitflies, and this information tells us that the typical effect of a mutation is not a gross effect on the fly that can be seen but is much more likely to be something leading to the death of the fly and still more likely to be something that leads to an increased probability of early death of the fly without the certainty of it.

So the most frequent class of mutation that we can study in fruitflies is a kind that causes a slight impairment in the survival of these flies, but no obvious external effect.

I suggest by analogy that most mutations in man would produce various body impairments leading to increased susceptibility to disease, lower life expectancy, increased embryonic death rate and similar things.

All of these are occurring anyhow due to all sorts of causes and therefore it is ordinarily impossible to tell whether a particular instance of impairment or death is or is not of mutational origin.

3. My third main point is that one might perhaps think that mutations that cause only a minor impairment are unimportant, but this is not so far for the following reason: Deleterious mutant genes are eventually eliminated from the population since they generally increase the death rate or lower the fertility of the person carrying them. A mutant that causes a great deal of harm is eliminated in a few generations. But one that causes only a small amount of harm will persist much longer, and thus affect a correspondingly larger

number of persons. On the average the larger number affected by a mild mutation roughly compensates for the lesser effect on the individual.

Since minor mutations in the long run can do as much harm as more drastic ones and occur much more frequently, it follows that most of the mutational damage in a population is due to the accumulation of individual minor effects. This means that an estimate of mutational damage that considers only obvious hereditary diseases and conspicuous abnormalities is probably a gross underestimate of the total damage. The effect of minor mutations, though intangible in the sense of ordinarily being indistinguishable from the other ills that we are beset with, is probably in the aggregate much more important.

4. Evidence from experimental animals, principally Drosophila, indicates that the number of mutations produced is strictly proportional to the amount of radiation received. These are departures from this straight-line relationship at high doses, but these are too high to be likely to be encountered in any ordinary human situation. It is technically impossible to test this relationship for the very lowest doses, but the straight-line relation holds down to the smallest amounts that have been studied. Also from purely physical considerations, one would expect linearity at low doses. I have in mind the kind of analysis Dr. Pollard made yesterday.

For these reasons a simple proportionality between the amount of radiation and the number of mutations is fully accepted by geneticists.

The proportionality between dose and mutation production holds irrespective of the intensity or spacing of the dose. That is, 100 roentgens given at one dose is exactly equivalent to the same total given as a series of small doses over a long period of time. Also from the standpoint of future generations, it makes no difference whether one person receives 10 roentgens or 10 persons receive 1 roentgen; the effect on future generations is the same.

The total harm to the population, as measured by effects on future generations, is strictly proportional to the total amount of radiation received by the reproductive cells of the population.

Senator BRICKER. Regardless of the number of people involved?

Dr. CROW. Yes.

Representative HOLIFIELD. This, then, would establish as far as the majority of the geneticists are concerned the principle of linear progression in deleterious effects of radiation regardless of amount?

Dr. CROW. That is correct. A nonthreshold situation, to put this in yesterday's vocabulary.

This means that there is no such thing as a safe dose of radiation to the population. Any amount of radiation, however small, that reaches the gonads—testes or ovaries—of a person who may later reproduce, involves a risk proportional to that amount. The benefits of radiation must be considered in relation to this risk.

Senator BRICKER. I do not see how you distinguish that rational analysis with the radiation background that we already have.

Dr. CROW. I would say, Senator, that the mutations that we now have that are produced by earth background and cosmic rays are themselves harmful. It is something we have been putting up as a population for a great many generations. But I do not say it is good.

Senator BRICKER. This process of natural selection has overcome the deleterious effects?

Dr. Crow. That is correct.

Representative Holifield. Only to the extent that it has gradually by natural selection strengthened the race rather than allowing it to become weaker and weaker. To the extent that those harmful genes did produce weakened individuals, to that extent, speaking from an individual standpoint, there was no overcoming by natural selection.

Dr. Crow. Yes. Let me amplify that a bit.

The way natural selection usually operates is through death and disease and misery and the elimination of deleterious genes has been from our standpoint a cruel process. It is our concern with minimizing this that leads us to worry about the possibility of an increased mutation rate.

I would very happy if there were some way of decreasing the spontaneous mutation rate.

Representative Holifield. Let me go further, then, and ask if there is a tendency before fertilization or immediately prior to the fertilization of the ovum for a healthy gene to mate with a healthy gene, or is the tendency for a healthy gene to mate with one that is harmed by mutation?

Dr. Crow. I think I would say that the joining of sperm and egg is purely random and the sperm and egg is ignorant of its own content as far as the probability of fertilization is concerned.

Representative Holifield. Therefore, the fact that it is a random selection at that point results in either abnormalities or weakened individuals, is that correct?

Dr. Crow. That is correct.

Representative Holifield. You may proceed.

Dr. Crow. The statements thus far made rest on a great number of experiments on a large variety of experimental organisms—for example, viruses, bacteria, fungi, corn, insects, mice—and the results from one agree with those from others.

Furthermore, there is some direct, though fragmentary evidence for radiation effects in man from study of children of radiologists and of patients who had heavy therapeutic doses of radiation. Fortunately, biological effects of radiations have been extensively studied for many years and there is a large body of well established knowledge which we can make use of in the present situation.

Let me remark, parenthetically, that it is fortunate that the development of genetics preceded the discovery of nuclear energy. Two of the men who have contributed most of this development, Doctor Sturtevant and Doctor Muller, will follow me on this stand.

It was almost exactly 30 years ago that Muller first discovered that X-rays enhance the mutation rate in fruit flies. We now have a sufficient backlog of knowledge of radiation genetics to proceed into the atomic era with due caution for possible harmful genetic effects.

I want to say a little bit about the distribution in time of the effects of mutations that might be induced at the present time.

In the first generation after radiation there would be an increase in hereditary diseases. But, as emphasized earlier, most of the harm would be of a generalized sort, not overtly distinguishable from other human weaknesses. The total damage would be spread out very thinly over many generations, with the greatest amount the first generation and slowly diminishing amounts in succeeding generations.

Senator BRICKER. Does the same thing hold true with mental development as with the physical?

Dr. CROW. As far as we know.

Senator BRICKER. The same ratio?

Dr. CROW. I think so, sir, though nobody knows for sure.

Representative VAN ZANDT. Will this hold true regardless of the amount of the dose?

Dr. CROW. Yes.

Fruit fly data suggest that the mutations would probably persist for a long enough time, that half of the total damage would occur in 30 to 50 generations. I state this to give some sort of perspective in time as to what we are dealing with. In other words, I am dealing with something that is occurring over dozens of generations and therefore thousands of years.

Despite some uncertainty as to details, we can safely say this: The genetic damage from radiation is spread over a very long time in the future, with only a small fraction appearing in the first generation.

Representative HOLIFIELD. At this pont may I ask you this: If a person is damaged by radiation it will be entirely possible for that person to have normal children and those children not be damaged by the radiation, but the damage might appear in a grandson or great grandson?

Dr. CROW. Yes; or a great-great-great-grandson. Not only is that possible, I think that is the rule.

Representative HOLIFIELD. So then we cannot draw the conclusion that a person who has received a substantial dose of radiation, even though his children are normal, that there has not been damage done to the genetic pool.

Dr. CROW. That is correct.

Senator BRICKER. There has to be some radiation effect upon the genes entering into the first generation or it does not carry on.

Dr. CROW. That is correct, too. The genes themselves are damaged but they would not necessarily cause any damage in the person.

Senator BRICKER. The personality or the physical attributes of the first generation. If those genes are not affected in the first crossing then there is no deleterious effect?

Dr. CROW. In that particular generation?

Senator BRICKER. In that lot.

Dr. CROW. It is a little different point. We have gotten confused on the same point again. An individual that received from his parents a defective gene may or may not and usually will not shown any consequence of that particular gene. At the same time he has a certain probability of the effect showing up and some one of his descendants on the average will show the effect.

Senator BRICKER. But it is only a small minority of the genes in the parent that are affected.

Dr. CROW. That is correct.

Senator BRICKER. If the particular gene entering into the first generation is not affected deleteriously there is no effect upon subsequent generations.

Dr. CROW. That is correct.

Representative HOLIFIELD. But is it not true in the case of a female that at birth the female child has all of the supply of ovum that she will have for her whole lifetime?

Dr. Crow. That is correct, too.

Representative Holifield. So therefore somewhere in that chain of ova would be a possible harmed gene.

Dr. Crow. That is correct.

Representative Holifield. That is where it would come up in future generations. It would be that transmission of the total supply to each descendant which would carry forward the mutated genes as well as the normal genes.

Dr. Crow. Yes. What may appear as a contradiction between what Senator Bricker said and what the chairman has said is this point: Each time sperm and egg are produced there is a sampling process in which some of the genes are transmitted; some others are not. That means it is perfectly possible that a harmful gene a person carries will not be transmitted to the next generation. It is just as likely by chance that it will be transmitted twice as often to the next generation. I have ignored this fact since these chance effects cancel out in the long run.

Representative Van Zandt. Will you describe the nature of the effects on the succeeding generations?

Dr. Crow. I think the most important thing to say of the nature of such effects is that they are not of a specific sort. They are of the same sort of ills that you and I are already beset with. Decreased life expectancy and increased susceptibility to all sorts of diseases.

I want to make the point that genetic harm is not distinguishable in any way from other kinds of harm and that is what makes our problem so difficult.

Representative Van Zandt. Would it have a tendency to break down the resistance of the body to various types of diseases?

Dr. Crow. I think so.

Representative Holifield. For the benefit of the lay reader and the committee, could you not give us now on the board the chronological progression of the germ so that we could understand the different steps?

Senator Hickenlooper. Mr. Chairman, before he gets to that very complicated situation, I wonder if I could ask just one more question about this matter, which the chairman raised a moment ago, about damage to the ovum in the female.

Assuming that there is a radiation effect which might or might not damage the ova supply, would that tend to damage all of the ova or would it be selective after a fashion and only damage some?

Dr. Crow. I would say that the radiation acts completely blindly. Which particular future egg or sperm is affected as a consequence of radiation is entirely random.

Senator Hickenlooper. That is purely one of hazard or chance?

Dr. Crow. Yes.

Senator Hickenlooper. Do I understand you to suggest that under ordinary circumstances of exposure to radiation, not all of the ova or all of the sperm would suffer damage?

Dr. Crow. Yes.

Senator Hickenlooper. Let me ask you this as far as a female is concerned.

Dr. Crow. Let me make one statement first. This is where my first figure that I was mentioning a while ago comes in. I would suspect that in any particular generation from spontaneous normal causes

about 5 percent, perhaps more, of the sperms or eggs produced by an individual will carry a new mutation.

Senator HICKENLOOPER. Now we get down a little further to the practicalities of the situation. I presume your statistics and figures with regard to possible mutation or change or something of the kind that might enter in there would be based on an assumption that every ovum or every egg would be fertilized.

Dr. CROW. No.

Senator HICKENLOOPER. Out of that complete fertilization a certain percentage would be bad?

Dr. CROW. No.

Senator HICKENLOOPER. Which leads me down to the question of the whole supply of ova which a female may be born with and which she may produce during a life and how many of those in the normal course of human life are actually fertilized.

Dr. CROW. Only a small fraction, as you are pointing out.

Senator HICKENLOOPER. Does that alter or change your percentage?

Dr. CROW. No, I do not think it changes a thing, Senator. The particular ovum or sperm that succeeds in being fertilized or fertilizing is a random sample of all that are produced. The numbers are very large and I assume that we can use the kind of statistical analysis that one always uses with large numbers.

Senator HICKENLOOPER. Do I understand that you are suggesting that while a very small amount, percentagewise, of the ova that are produced are fertilized, that the percentage of injury to that small percentage that would be fertilized, would that be consistent as proportionate to the whole?

Dr. CROW. That is correct. The injury among those fertilized is exactly the same as those not fertilized.

Senator HICKENLOOPER. On the whole supply?

Dr. CROW. Yes.

Senator HICKENLOOPER. Thank you.

Dr. CROW. It should be emphasized that not all spontaneous mutations are due to radiations. Part are due to natural radiations—cosmic rays, ground radiations, radioactive isotopes in the body, et cetera—but many mutations, probably the majority, have other, mostly unknown, causes.

I am saying that most mutations are probably not caused by radiation at all, including natural radiation or manmade.

Spontaneous mutations that have occurred in the past probably, directly or indirectly, account for a substantial fraction of human physical and mental impairments. One way of attempting a quantitative assessment of possible radiation damage is to compare that due to manmade radiation with that from spontaneous mutation.

If I compare spontaneous mutation rates in man for those few genes that have been carefully studied with the radiation induced rate of mutation in mice, which is the best we can do because we have no quantitative data on man, one can estimate that it would require about 50 roentgens to produce by radiation a number of mutations equal to those that occur naturally.

I shall call this, as people have previously done, a doubling dose. That is, if your genes are as mutable as those of mice it would require about 50 roentgens to produce by radiation as many mutations as occur spontaneously.

Needless to say, this is subject to a considerable uncertainty. The National Academy of Sciences Committee on Genetics estimated 30 to 50 roentgens. The British Committee reached the same conclusion.

We, that is, the members of the Academy of Science Committee, have been told that the amount of radiation due to fallout at present rates would amount to something in the vicinity of one-tenth of a roentgen in a 30-year period. I shall choose a 30-year period because that is about the average age of reproduction. From a genetic standpoint this is the important aspect of the life cycle.

If a rate of one-tenth roentgen were to continue indefinitely, and the 50-roentgen doubling dose that I previously gave is assumed to be correct, this means that the population would eventually have an increase of one five-hundredth in genetic damage. Even if my estimate of 50 roentgens is much too high, the damage would still be a small fraction of the genetic damage due to other causes. The lowest possible value for this doubling dose would be of the order of 3 to 5 roentgens. That is, this would assume that only radiation can produce mutations and therefore all spontaneous mutations are due to background radiation.

Senator BRICKER. That is not always true.

Dr. CROW. No; I do not think it is true. I am trying to set a lower limit on the frequency of natural mutation or an upper limit on the consequences of fallout.

Representative COLE. Is it possible to distinguish the source of the cause of spontaneous mutations? They are not all caused by radiation.

Dr. CROW. No.

Representative COLE. Is it possible to segregate the causes of spontaneous mutations—those attributable to radiation and those due to unknown causes?

Dr. CROW. No; it is not. In any particular event one does not know whether this mutation is caused by radiation or something else.

As I said before, ordinarily one does not know whether a particular illness is caused as a consequence of mutation or not. In no case can we trace back a particular instance of disease to a radiation effect or to other kinds of mutational effect.

Representative COLE. By experimentation you can prove that radiation or induced radiation does result in mutations.

Dr. CROW. On a statistical basis; yes. Of course, in experimental animals the evidence is much more precise than this.

If I make this minimum assumption; that is, that all mutations that occur spontaneously are due to background radiation, and the fallout rate of one-tenth roentgen per 30 years, the mutational damage would eventually be increased by about 3 percent. That is, there would be a slow rise up to a point where the present damage due to mutation, whatever that be, would increase by a factor of about 3 percent.

Representative COLE. Doctor, how can you make that assumption, that all spontaneous mutations are the result of radiation?

Dr. CROW. I am trying to set a maximum on the effect of fallout. By setting a maximum on the effect of radiation I think we can set a maximum on the amount of fallout damage.

Representative COLE. In making that assumption you admit it is fallible.

Dr. CROW. Yes. It is a very unlikely assumption. I am trying to make an extreme assumption for the purpose of setting an upper limit. So I believe that we are completely safe in concluding that fallout at the present rates will increase the existing genetic damage by only a small fraction, even if continued indefinitely.

Representative VAN ZANDT. A body has 50 roentgens already.

Dr. CROW. Let me say the body already has a mutation rate equivalent to that produced by 50 r. of radiation.

Representative VAN ZANDT. Taking into consideration the present fallout, would it be increased by 3 percent over a period of 30 years?

Dr. CROW. My best guess was not 3 percent. My best guess was 1 part in 500. It might be as much as 3 percent.

Senator BRICKER. But there is nobody who believes that all the mutational changes in human life and the characteristics of body and mind are due to radiation.

Dr. CROW. I do not believe anybody believes that.

Senator HICKENLOOPER. Possibly do some of these mutations occur from chemical processes?

Dr. CROW. Very likely. I wish we knew the causes of all mutations but don't.

Although the general conclusions so far given are well established and there is no disagreement among geneticists, the quantitative assessment of human mutational damage is much less certain. There are no quantitatively reliable human data on radiation-induced mutation, and the figures from different experimental organisms are not in quantitative agreement. About the best we can do is to take the animal nearest man, in this case the mouse, as an indicator of possible human effects.

I think at this point there are two alternatives. One can say, as some geneticists prefer, that our quantitative data are so uncertain that we had better make no statements at all of a quantitative nature. Alternatively, we can say, as other geneticists do, that a poor numerical estimate is better than none at all. I belong to the latter group, so I shall, therefore, proceed to make some estimates as to the genetic consequences of the amounts of radiation involved in fallout.

Senator HICKENLOOPER. I am sorry to keep interrupting you, Doctor, but, may I ask, do some mutations have a beneficial effect occasionally?

Dr. CROW. I think a very small minority of mutations probably have a beneficial effect.

Senator HICKENLOOPER. We have had experience in mutations in grain where exposed to radiation, and I think the record shows that while in the overwhelming number of those mutations the progeny is less desirable than the ancestor, yet in a certain small percentage the mutations produced are in many ways far superior to the ancestry. Is that your understanding?

Dr. CROW. I think that is very likely true in man as well, Senator, but neither you nor I would suggest——

Senator HICKENLOOPER. I am sorry; I understand that you talked about that on the record before I came in, and I do not want to go over the same ground.

Dr. CROW. I have a sentence I want to say, anyhow. I think neither you nor I are willing to suggest that we sacrifice 999 persons with inferior mutations in order to get 1 beneficial one.

Senator HICKENLOOPER. I think we agree that is not even a 50–50, 1 horse and 1 rabbit, but I only wanted to be assured on the point that, in effect, not all mutations are bad and that if we have to have mutations there might be some modicum of benefit that would come out of it.

Dr. CROW. A very small amount.

Senator BRICKER. Would you say the percentage is 1 to 10 as to good?

Dr. CROW. No. It is much smaller than that.

Senator BRICKER. It has been proved between 5 and 10 in some of the grain experimentation.

Dr. CROW. I do not think so. I think the proportion of beneficial mutations that actually occur in grain or anything else is hardly higher than one in a thousand and may be much less than that. The reason the grainman can make use of this very rare beneficial mutation is that he can afford to throw out 999 of 1,000 grains and save the one that is superior.

Senator BRICKER. That is what they do. I thought the percentage was a little higher.

Dr. CROW. I do not think so, Senator. I think it is much lower. I do not care too much whether it is 1 percent or one-tenth of 1 percent or one one-hundredth percent. It is very small.

Senator ANDERSON. Was that the experience at Brookhaven when they were testing grain?

Dr. CROW. I do not know the exact figures.

Senator ANDERSON. Would that not be interesting to know?

Dr. CROW. I think it would be interesting, but I do not think it is too important from this particular consideration. It is very small, and that is what I am concerned about.

Representative HOLIFIELD. Is it not true that Burbank made thousands and thousands of experiments of crossbreeding in the plant field before he found a superior berry or superior fruit?

Dr. CROW. That is correct.

Representative HOLIFIELD. And that the actual cross-pollination and selection of pollen went into a thousand experiments before he found what he wanted?

Dr. CROW. That is correct. That is the way the modern plant breeder works. He grows thousands and thousands of plants, selects the one plant that shows the traits he wants, and throws all others out.

Representative HOLIFIELD. Which, again, proves the general statement that mutuations, generally speaking, are in the high proportion harmful.

Dr. CROW. That is correct.

Representative HOLIFIELD. Or degenerating rather than strengthening the strain.

Dr. CROW. That is correct.

I want to emphasize in these figures that I am about to present the large errors of measurement and the uncertainty of the assumptions involved in these estimates. To be concrete, I have prepared numerical estimates. I have prepared numerical estimates of the actual number of cases of different kinds of genetic harm that might be expected.

I shall assume a population of 2 billion children whose parents received an average of one-tenth roentgen. To repeat, one-tenth roent-

gen is roughly the amount that one might expect from fallout, according to the figures that have been published in a 30-year period. This may be too high; it may be too low. But it will give us a rough idea of the general magnitude of the risk.

Two billion is roughly the number of children that will be born in the entire world in the next generation whose parents have been exposed to fallout in this generation.

Representative VAN ZANDT. You are basing your figures on present-day test rates.

Dr. CROW. Yes. I am using the figures given to the Academy Committee and which agree with those published by Dr. Libby in his report last spring. Let me put some of these figures on the board, please. I think that is perhaps the best way to do it.

(The material on the board was as follows:)

Kind of damage	Number		Proportion of total population affected	Fraction by which existing abnormalities would be increased
	First generation only	Total for future generations		
Gross physical or mental defect	8,000	80,000	1/250,000	0.0001
Stillbirths and childhood deaths	20,000	300,000	1/100,000	
Embryonic and neonatal deaths	40,000	700,000	1/50,000	

NOTE.—Plus a larger but unknown number of minor or intangible defects.

Dr. CROW. I have written here rough guesses, which means that is what they are. I hope nobody takes the numerical values as being worth very much as numerical figures. They may be five times too high or low or more. I want only to give some rough indication, as I said before.

I am assuming that the parents had an average of one-tenth of a roentgen and there are a total of 2 billion children. I am going to give the number of various kinds of abnormalities both in the first generation and the total for all time, and I am going to give these figures in absolute figures, because these will be large, and then I am going to give them as percentage figures, and they will be small. That will make my principal point, as a matter of fact; the numbers are large, but the percentages are small.

Senator ANDERSON. Doctor, can I go back to the statement of yours a minute ago that quantitative data are so uncertain that we better make no statements at all.

Is it not possible that by taking these assumptions of yours that parents are only going to receive one-tenth and there will be 2 billion children born and so forth, that you are going to come out with some figures that are very reassuring that there is no danger in these tests, but you have no figures to back it up.

You keep making a statement that they are all right and then you say it is based on figures that do not mean anything.

Dr. CROW. I have not been sending out statements that it is all right.

Senator ANDERSON. You are about to make one as to the number of children based on assumptions. If I assume that today is Sunday I can prove it is only a 5-day week.

Dr. CROW. Let me finish my statement, Senator, and then come back.

Senator ANDERSON. It is the assumption that you start with. If there are 3 people up here and if 2 go the place is practically deserted, but there are more than that.

Dr. CROW. I say I have made what seem to me to be reasonable assumptions. That may be wrong. There is a high probability that numerically they are wrong. I still hold to the idea that we are better off estimating very crudely what the numbers involved here are than not making any numerical estimates at all. That is all I want to proceed with. I do not want anybody to take these more seriously than they deserve.

Representative VAN ZANDT. Dr. Crow, as I understand it, you are here to give us the benefit of your thinking.

Dr. CROW. Yes. I do not want to speak for geneticists as a whole or commit myself to any certainty as to actual numerical values.

The first category I want to talk about is gross genetic disease. I include in this both physical and mental disease, known or suspected to be of fairly simple genetic origin.

I would estimate something like 8,000 such cases is the first generation, and a total of some 80,000 all told. This would make up a fraction of about 1 in 250,000 in the population.

I have in mind here the kind of diseases that I referred to earlier. These may be serious mental diseases, serious physical diseases, deformities, mostly things of a rather clear hereditary origin.

In the second line we have death rates in childhood, including stillbirths. I will call this stillbirths and childhood deaths. The number I would give to this is 20,000, or a total of 300,000 for all time, making up in any one generation a fraction of about 1 in 100,000.

Then the final category I want to mention are embryonic deaths and deaths around the time of birth. I am going to call this neonatal deaths. I would guess for that 40,000 the first generation, 700,000 total, or a fraction of something like 1 in 50,000.

I have one other figure I would like to note and that is that these collectively make up about 0.0001; that is, approximately 1/10,000 of the normal incidence of these conditions.

I want to add at the bottom "Plus a larger but unknown number of minor or intangible defects."

I do not want to tell you in detail how these estimates were gotten. I would be glad if you like, to insert in the record the procedures by which there were obtained so that they could be independently checked by others.

Representative HOLIFIELD. I think you should do this.

Dr. CROW. I shall be glad to do that.

(The information referred to follows:)

PROCEDURES BY WHICH ESTIMATES OF GENETIC DAMAGE WERE MADE

These estimates ignore the effects of completely recessive factors. Such factors would have a probability of expression of much less than 1 percent in any 1 generation unless the amount of consanguineous marriage were high, or there were a high frequency of the mutant allele already in the population. The latter is not likely to be the case except for a few genes (such as thalassemia in some populations) that are maintained by some sort of selective balance. There is substantial evidence in drosophila, and some in mice and humans, that recessive genes generally have some effect as heterozygotes and that this effect is

large enough to constitute the main effect of these loci on the population. The early evidence has been summarized by Muller (American Journal on Human Genetics 2; 111–176, 1950) and there are recent confirmations. It therefore appears likely that most "recessive" mutants are eliminated as heterozygotes before they ever have an opportunity to become homozygous. My estimates are based on this assumption. I think this is reasonable as a first approximation. To the extent that the assumption is wrong the damage would be spread over hundreds rather than dozens of generations with correspondingly less effect on each, though the total effect would be about the same.

The mehods used in arriving at these estimates are explained in an article soon to appear (Eugenics Quarterly, July 1957). Briefly, they are as follows:

The number of gross physical and mental defects was obtained this way. The number of abnormalities of fairly simple genetic origin is estimated as about 2 percent of all children born (see "Genetics and Disease" by Tage Kemp, p. 190; National Academy of Sciences report, p. 25). Assuming a 50 r. doubling dose (National Academy of Sciences report, pp. 23–4; Muller, Bulletin Atomic Scientists, 11:336, 1955; Crow, Eugenics Quarterly, 3:201–208, 1957), 0.1 r. continued over many generations would lead at equilibrium to an increase of 0.1/50, or 0.002, in these types of abnormalities. The total effect over all time of 1 generation exposure is equal to the effect of 1 generation at equilibrium after repeated exposure. Thus, assuming a stable population of 2 billion births each generation, the total affected from one dose of 0.1 r. would be $0.02\times0.002\times2\times10^{9}$ or 80,000. The National Academy Committee suggests that about 10 percent of the damage would appear the first generation, making 8,000 out of 2 billion, or 1/250,000 of the population. The normal incidence of such conditions, genetic and nongenetic, is about 5 percent, so the 0.1 r. would cause an increase of 8,000/100 million=0.00008, or approximately 0.0001.

The estimates on stillbirths and childhood deaths are based on an estimate from the increased death rate in children of consanguineous marriages (Morton, Crow, and Muller. Proc. National Academy Sciences 42:855–863. 1956). This estimates about 8 percent as the proportion of stillbirths and childhood deaths at mutational equilibrium. Again assuming a 50 r. doubling rose, 0.1 r. would lead to an increase of 1/500, or 0.002. In a population of 2 billion this leads to a total number of $0.08\times0.002\times2\times10^{9}$=320,000 or approximately 300,000. Drosophila data suggest that the typical "recessive" mutant has about 4 percent dominance, and making a small allowance for inclusion of dominant factors, I estimate 6 percent of the damage the first generation. Six percent of 320,000 is 19,200 or approximately 20,000. The total death rate in these ages in the populations on which these studies were made was about 12 percent, hence the effect of 0.1 r. is 20,000/240 million=0.000083 or approximately 0.0001.

The data on embryonic and neonatal deaths come from Russell's data on radiated mice. When the father had 300 r., the litter size (counted at age 3 weeks) was reduced by about 3 percent (Proc. Intern. Conf. on Peaceful Uses of Atomic Energy. 2:382–383). If both parents received 0.1 r., the effect would be 0.2/300 as great. Hence the estimated first generation effect is $0.03\times0.2/300\times2\times10^{9}$=39,600 or approximately 40,000. Again assuming 6 percent the first generation, the total effect is 40,000/0.06=670,000 or approximately 700,000.

Dr. Crow. Let me give a rough indication.

These are based on statistical data on the frequency of such diseases and the assumption that mouse provides us a good indication as the radiation induced mutation rate. I have to use Drosophila data. The animal I am talking about here is in a sense a composite of a man, mouse, and fruitfly. To whatever the extent the information from these other organisms is relevant, these figures are reliable.

The stillbirths and childhood deaths are based on human figures for death in these ages gotten in a very indirect way that I do not want to take time to describe from the frequencies of different kinds of deaths in children of parents who were related. Once again I have to make use of fruitfly data in order to complete this estimate.

The third row of figures is based on embryonic death rates in mice in Russell's data. These are deaths that occur in mouse embryos and postnatally up to the age of 3 weeks.

Note that the second and third rows are not exclusive. Stillbirths and infant deaths are included in both groups.

I hope I have indicated that these all depend on mouse and fruitfly data and if man is different, the figures are no good. I think these are about as good an indication as one can get at this time.

Representative HOLIFIELD. There is no doubt that the mammal case would be much closer to the man than the fruitfly.

Dr. CROW. I think so.

Representative HOLIFIELD. The only reason for using, or the chief reason for using, the fruitfly is the rapid span of life.

Dr. CROW. That is correct. And the tremendous backlog of information in the fruitfly that we possess as a consequence of a great many years of work by geneticists.

Let me say in general that I have used human data when I could. When I could not, I used mouse. When neither human or mouse data is applicable, I have used fruitfly data.

I have one summarizing paragraph.

Despite the quantitative uncertainty of these estimates, I believe they have enough validity to permit some definite conclusions. One is that with the present levels of fallout, the amount of genetic damage in future generations from this cause will be a very small fraction of the total human death, disease, and misery. On the other hand, the number of persons exposed to fallout is as large as the world population, and therefore we can be sure that several hundreds, or thousands, or tens of thousands, or perhaps more persons will be diseased, or deformed, or will die prematurely, or be otherwise impaired as a consequence of fallout if the present rates of testing continue. In my opinion, even one unnecessary individual tragedy is too many, and no increase in radiation for any reason should occur unless it offers some compensating benefit for mankind.

Representative HOLIFIELD. Thank you, Dr. Crow. I am sure there will be some questions.

Senator HICKENLOOPER. Doctor Crow, perhaps you mentioned this, and I am sorry I was a little late in getting in, but did you discuss or have you studied or examined any data over a substantial period of time with regard to the Indians who live in the monazite sand district of India and the genetic data with respect to those generations that have developed in that monazite sand area?

Dr. CROW. My understanding is that a study of this is contemplated. I am not aware of any data yet coming from that.

Senator HICKENLOOPER. In the record we had yesterday, I believe Dr. Warren gave the tremendous amount of radiation activity there as compared to normal where these people have been living for no one knows how many generations. Perhaps those studies have not gone far enough to produce any results as yet.

It would seem to me that with that tremendous overplus of radiation exposure in this monazite sand area, we might get at least some significant data as to what had happened generation after generation there, if anything.

Dr. CROW. We might. On the other hand, we might not. I want to point out some of the difficulties in this kind of a study.

One is that the numbers of persons are fairly large, but they are not very large with respect to what is needed for this kind of study.

The second is that one always has some doubt when he compares one community with another community that there is not something else that is different about these two communities besides just the radiation. I think it is a difficult question.

Senator HICKENLOOPER. If we are attempting to pinpoint this for at least the purpose of these hearings on radiation, then I presume it would depend on whether there is reliable data going back a number of years or what the history of that situation is. From the evidence given yesterday that this is an area in the world where the strength of the radiation is many many times any possible radiation that might come from tests and is a number of times the radiation force of the normal background radiation on the average over the country or over the world, it would seem to me that would be a very fertile field for seeing whether mutations of significance have actually occurred there.

Dr. CROW. I certainly hope such a study will be done, but it has not been done.

Representative HOLIFIELD. Are there any further questions?

Representative COLE. Mr. Chairman, I just want to make sure my understanding of the doctor's figures is correct. That is, the figures which he placed on the blackboard.

Do those figures indicate that it is your very very rough guess that over the next generation of the 30 years of the total number of people in the world who will be born with a gross genetic disease, the number of stillbirths out of that total, 1 out of 10,000 of that gross number can be attributable to the presently produced radiation from fallout?

Dr. CROW. That is my best guess; yes. To clarify one point, I am assuming continuance for 30 years of the testing at the present rate.

Representative COLE. Based on what has occurred?

Dr. CROW. Based on the 5-year rate; yes.

Representative HOLIFIELD. These figures that you give us are based on the present rate of testing, and if that rate of testing would increase tenfold or more as it has in the past 10 years from the original rate of testing in 1946, then these figures would have to be multiplied, would they not, by the increased rate of testing? And would it be a simple multiplication or would it have more effect or less effect than the figures involved?

Dr. CROW. For all practical purposes it would be a simple multiplication. If testing were increased by tenfold each of my values here would increase by tenfold.

Representative HOLIFIELD. This is also assuming that the one-tenth roentgen is an average distribution.

Dr. CROW. Yes, sir.

Representative HOLIFIELD. If it is not average, as we have had testimony is that there is no such thing as an average distribution of fallout, but that the fallout is heavier in the temperate zone and it is uneven in the temperate zone, then there would be a higher degree of radiation received upon some groups of individuals than the one-tenth, would there not?

Dr. CROW. That is correct.

Representative HOLIFIELD. To that genetic strain possessed by those individuals, this would have to be multiplied at this time.

Dr. CROW. That is correct.

Representative HOLIFIELD. Assuming that they received a roentgen in certain areas, this would be a 10 times factor, would it not?

Dr. CROW. Yes.

Representative HOLIFIELD. And if they received 50 roentgens, it would be a 500 times factor?

Dr. CROW. Yes.

Representative HOLIFIELD. We do know that condition does exist in the world. There are peaks and valleys of distribution of fallout from the present testing rate.

Dr. CROW. Yes.

Representative HOLIFIELD. So in considering this average we should also consider the fact that it is an average and not an extreme either way, because there will be people who will receive no radiation from these tests.

Dr. CROW. Yes. I do want to say one thing, Mr. Chairman. Most of the harm from this will show up several generations in the future. By that time I suspect that within small areas the effects of peaks and valleys will in a sense have disappeared because persons from peak areas will have married descendants of persons from valley areas and the children will be mixed up by that time. On the other hand, if there are large differences in large areas, if one continent has a heavier distribution than another continent, most of the descendants of persons from one continent will stay in that continent. In this case the difference would be important.

Representative HOLIFIELD. I do not think we should leave this without pointing up that these figures again are based on peacetime testing and not upon the utilization of hundreds of weapons in a possible war.

Would you care at this time to say what you think would happen to the genetic pool of the human race if a hundred 5-megaton bombs were dropped upon any country in the world by any other nation in the world?

Dr. CROW. I have not made any such calculation, but my guess is that it would be disastrous.

Representative HOLIFIELD. That it would be disastrous?

Dr. CROW. Yes.

Representative HOLIFIELD. I am speaking about the survival. We know that a lot of strains would be eliminated immediately.

Dr. CROW. Yes.

Representative HOLIFIELD. To the survivors it would be disastrous as far as the future of mankind is concerned.

Dr. CROW. Yes. I think a major nuclear war would be a serious genetic hazard as well as a serious hazard immediately.

Representative HOLIFIELD. That is the point I wanted to bring out.

Dr. CROW. It may be, Mr. Chairman, that some of the people who will follow me have done some calculations on this point. I simply have not.

Representative VAN ZANDT. Dr. Crow, is this your best estimate?

Dr. CROW. Yes.

Representative VAN ZANDT. Have you taken into consideration the estimate of some of your colleagues in this highly specialized field?

Dr. CROW. Yes. Other people have made some of these same estimates. In particular the first estimate would agree with that one made by the National Academy of Sciences Committee.

Representative VAN ZANDT. Would you say your estimate is a general opinion of you and your colleagues?

Dr. CROW. I think so. You will have a chance to ask my colleagues later on.

Senator HICKENLOOPER. Dr. Crow, perhaps you have covered this, but have you discussed whether or not there are degrees of deleterious effects of mutations? In other words, would some mutations be by hazard or otherwise worse than others, or would some mutations merely result in a slight alteration that would in the long run be not very serious at all?

Dr. CROW. The answer is, I think, "Yes" and "No" to your two statements. Certainly mutations range from very serious to very mild. I did make the point earlier and I would like to repeat it now, that we cannot afford to ignore mild mutations because the milder mutations by virtue of being milder are less likely to cause the sterility or death of the person who possesses them and therefore more likely to persist in the population and therefore more likely to affect a larger number of persons.

So to some extent the mildness of the mutation is compensated for by the greater number of individuals it affects. For example this minor list I put at the bottom I do not regard as negligible at all. I think it may be the major part of the damage. It is so intangible that it is very difficult to measure or even to discuss.

Representative HOLIFIELD. Dr. Crow, I asked you a question a few minutes ago and it was not quite a fair question when I asked you about the number of weapons—one hundred 5-megaton weapons—so let me put it in a different way, which would possibly be more within your field of consideration.

If there was an accumulation of 500 roentgens over a period of 5 or 10 years by an average of the population, as might be expected from a massive attack such as I described, then would your answer be more explicit?

Dr. CROW. If I take the one-tenth of an r. figure that I have used here and convert that to 500 r., that means simply that every figure on this is multiplied by 5,000. I think it would be serious.

Representative VAN ZANDT. Dr. Crow, does this tear down the resistance in the body?

Dr. CROW. Yes.

Representative VAN ZANDT. I am thinking of the diseases spread over the world from time to time, like polio, and the virus situation in the Far East at the present time. Would it not add to the problem considerably from the standpoint of deaths?

Dr. CROW. Yes. I think what the nature of most genetic mutations is, as you said a few minutes ago, to lower the resistance so that the disease is more likely to be fatal than it would be otherwise.

Representative VAN ZANDT. Therefore we would be faced with additional hazards and other diseases that are taking a heavy toll today?

Dr. CROW. Yes.

Representative VAN ZANDT. It would greatly aggravate the problem.

Representative COLE. Before the doctor leaves, I am intrigued by his answer that 500 r. up in the atmosphere we would multiply all of his figures by 5,000. That would conclude that in the next 30 years, out of every 10,000 injurious effects on living persons or unborn persons, 5,000 would be due to 500 r. in the atmosphere.

Dr. CROW. One way of saying it is that I would expect that the number of such effects we now have would be increased by approximately 50 percent.

Representative COLE. What do you calculate would be the needed number of roentgens in the atmosphere to make the equation 10,000 equal 10,000? Would it be 10, or 100? It would be 1,000, would it not?

Dr. CROW. If I say one-tenth——

Representative COLE. If you say 1,000 r. is in the atmosphere then your fraction would be 10,000 out of 10,000 cases attributable to these 1,000 r.

Dr. CROW. I think that is right; 1,000 r. would, on these assumptions, about double the numbers for many generations in the future.

Representative COLE. That would be ten thousand times more radiation than presently exists?

Dr. CROW. Yes. Ten thousand times my assumed fallout level.

Representative HOLIFIELD. Thank you very much.

We are going to ask all of you gentlemen to remain until the end of today when we will have a roundtable discussion on this subject. We will do it at the end of the morning if we can. If any of the witnesses do have previous engagements they will be excused.

Our next witness is Dr. Bentley Glass. Dr. Glass is professor of biology at the Johns Hopkins University. He has a notable record. We are glad to have you with us.

STATEMENT OF DR. BENTLEY GLASS, PROFESSOR OF BIOLOGY, THE JOHNS HOPKINS UNIVERSITY [2]

Dr. GLASS. Mr. Chairman and members of the committee, like the preceding witness and those who will follow me this morning, I served as a member of the National Academy of Sciences Committee on the Genetic Effects of Radiation, and I would like to have it clear on the record that the opinions I express this morning are my own opinions and not those of the Committee as such, although I believe that the Committee would be in agreement with them.

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I hope that what I have prepared to say will answer some of the questions that have already been raised by the members of the committee, if they have not already been adequately answered to your satisfaction.

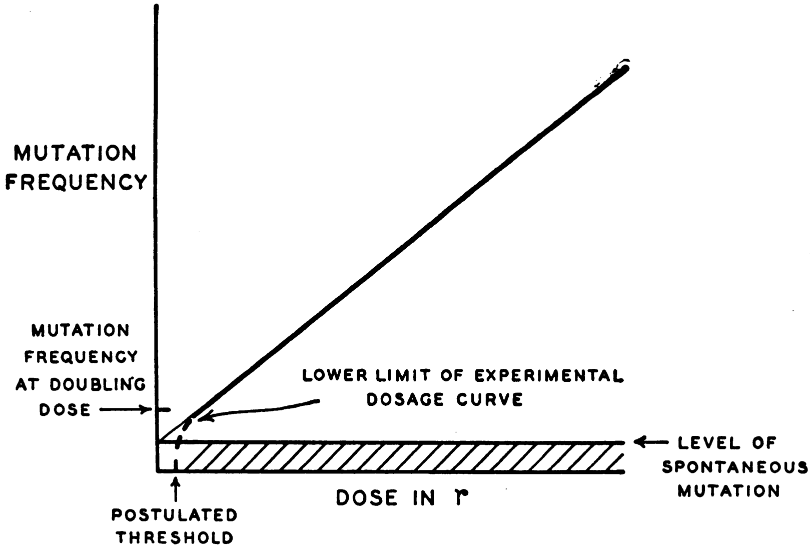
The hereditary material of all cells is located in bodies called chromosomes which are contained in the nuclei of the cells. This material is divisible into genes, perhaps 10,000, 20,000, or even 40,000 in each cell. This explains why—as I believe, Senator Hickenlooper asked—the matter that a large proportion of ova are not fertilized is not really a matter of prime concern, since we are concerned with the probability of mutation of the individual gene and there are so many of these in each cell that the probability that a mutation of some one of those genes will occur after any sizable dose of radiation is considerable. There would hardly be an ovum or a sperm cell that would not carry a mutation of some gene or other if the dose of radiation were more than a few roentgens.

Each of these genes regulates one important biochemical step in the living machinery, or controls some other significant property of protoplasm. The biochemical nature of the material is now identified as being deoxyribose nucleic acid (DNA), which forms a major part of the chromosomes. Permanent genetic changes, that is to say, mutations, consist then of modifications of the chromosomes and specifically of the DNA.

The hereditary material not only regulates the synthesis of enzymes and thus controls all life processes extrinsic to itself; it also has the unique ability to duplicate itself so that every new cell coming into existence has a replica of the genes and chromosomes of the parent cell. This at once explains why on the one hand damage done to other parts of cells can, if not too extensive, be repaired by replacing the lost or damaged machinery with newly synthesized parts, but on the contrary damage to the genes and chromosomes is irreparable. Such damage constitutes a defacement of the mold, the model, the die by means of which the new hereditary stuff is replicated, and which therefore inevitably bears the replica of every flaw in the original. Only by an exact reverse mutation may the damage done to the hereditary material by a mutation be erased. As long as it is transmitted in heredity to offspring it is permanent.

Microscopically visible fractures of chromosomes are produced by all sorts of ionizing radiations. In general, only those alterations that involve two or more breaks of chromosomes in the same nucleus, followed by reunions in new arrangements, can be inherited, for single breaks result in losses which are promptly fatal. Hence these gross mutations increase as the square or some higher power of the radiation dose.

I would like to duplicate a graph that Professor Pollard presented to the committee yesterday, plotting dose against mutations produced. We might elaborate on his diagram a bit by putting in a kind of a substratum to represent the level of spontaneous mutation. I draw it all the way across, since no matter how much of a dose of manmade radiation is applied, this would be the proportion of spontaneous mutations.



Now the gross chromosomal mutations which are microscopically visible show an increase with dosage like that first curve which Professor Pollard drew for you. These gross mutations are generally very harmful but because they occur mostly only at high doses and chiefly in spermatozoa—rather than ova—which may be eliminated if the male is rendered temporarily sterile by the high dose of the radiation, they are not very significant in the overall picture of mutation and the damage that mutation does.

Of chief importance are the so-called point mutations, in which the lesions in the hereditary material are submicroscopical in size. These increase characteristically in direct, linear proportion to the dosage, and, like the gross mutations are producible by all ionizing radiations, such as cosmic rays, gamma rays, and X-rays, and by ionizing particles such as beta particles, alpha particles, and neutrons.

These are the mutations, then, that show a linear proportionality to dosage even up to a very high dosage in all the organisms that have been studied experimentally. And although we cannot work on this region where the dose is very small the direction of that dosage curve points to this level of spontaneous mutation as its origin, which implies that even down to the very smallest doses each quantum of radiation is capable of producing a mutation.

As Professor Pollard said, it would be very difficult on any kind of physical ground to suppose that a threshold exists, say, at this level of radiation, (see figure) and you had a steeper slope to the curve right in this low dosage range than anywhere else.

Representative HOLIFIELD. Doctor, at that point, will you tell us where the amount of the dose begins in terms of roentgens?

Dr. GLASS. Yes.

Representative HOLIFIELD. I mean the amount of discernible calculation which you have in your linear field.

Dr. GLASS. The data are most extensive for the fruitfly and the lowest dose that has actually been studied is 25 r.

Representative HOLIFIELD. But the fruitfly is not as susceptible as the mammal.

Dr. GLASS. No, that is correct.

Representative HOLIFIELD. So you would have to raise that for the mouse or the man, would you not?

Senator ANDERSON. It would be lowered.

Dr. GLASS. The only part of the curve which has actually been studied—I think Dr. Russell will testify about this—is the range between 300 and 600 roentgens in the mouse. But in some plants and in bacterial studies part of this curve has been extended down to 5 roentgens or thereabouts.

Representative HOLIFIELD. In my previous question I should have said a lower dose, because I knew it took more to affect the fruitfly.

Dr. GLASS. Yes.

Because the changes in the genes are irreparable except by exact reverse mutations, the effects of doses of radiation administered at different time are cumulative; that is to say, the mutation rate is proportional to the total dose administered, irrespective of whether it is given in small doses over a long period of time or in large doses of high intensity as in the case of X-rays.

Because a mutation can be produced by a single ionization in the right place, there is no threshold below which the amount of radiation is too small to produce mutations—that is, every dose produces mutations with a probability equal to its magnitude.

This is to repeat what Dr. Crow said, that there is no safe dose of mutation. This curve continues down without any threshold until it hits the zero point at the level of the spontaneous mutation in the population. Because the genes control important, often essential biochemical steps in the living system, a mutation of any one of them is practically always harmful, and would often be lethal, were it not that each cell, being lineally descended from the union of sperm and egg at the beginning of the individual life, has two representatives of each gene, the one maternal in origin and the other paternal. We have a kind of life insurance, so to speak. Two genes of every sort, so that if something goes wrong with one of them we can still carry on. This is very important to us. It is why most of the mutations that are produced, although they would be very harmful if we had 2 doses of them, have a much milder effect when we have only 1 mutation of that kind.

Thus a mutation is often not obviously harmful at once, unless its effect is dominant over the normal gene of the same sort which is still present. But the recessive mutations are nonetheless harmful, and in many cases kill or severely handicap those persons in the population who eventually happen to inherit the same mutation from both parents. This may occur only after many generations—as Dr. Crow said, 30 or 50 generations—have elapsed since the mutation originated. Consequently, harmful mutations can accumulate and be carried in a population without visible damage until at length an equilibrium is reached at a frequency where the mutation rate that produces each particular kind of harmful gene is balanced against the elimination of that gene from the population either by the death or by the failure to reproduce of some person who gets a double dose of the gene.

There is also evidence that even in single dose most mutant genes do harm, although it is of an intangible sort, affecting general resistance, shortening the life span, or reducing the fertility of its carrier. I think Dr. Crow has spoken sufficiently to that point. From studies on the fruitfly, which are the only ones on any animal sufficiently extensive, it appears—and I believe these are the figures you were asking for a while ago—that about one-fourth of all mutations are lethal or semilethal, 15 to 20 percent produce sterility in one or both sexes, and nearly all of the remainder, whether producing visible changes or not, reduce the vitality. Less than 1 in 100 mutations—probably nearer 1 in 1,000—is definitely advantageous under existing conditions, although some of the subvital ones might become neutral or even advantageous under altered circumstances.

Two other things I would like to emphasize. First—and this is an important point which I have not seen previously emphasized in these hearings—most, if not all, of the mutations produced by radiation act as if they were truly losses of a part of the hereditary material. Some of these losses are big enough to see in the microscope. Others cannot be seen, but are probably losses, because they cannot be made to revert to the original condition. For this reason, mutations produced by radiation are probably as a class much worse in nature than those which arise spontaneously.

Secondly, there is no known way of directing mutation and of producing mutations of just the particular gene, a mutation of which may be desired. Suppose there is one mutation in a thousand that is beneficial. We cannot produce just the mutation of that one sort we desire. We have to produce a thousand mutations and pick it out. Radiation acts blindly, and that is why the deleterious nature of the vast majority of mutations is so important. By means of several hundred r. of radiation, it might indeed be possible to increase the probability of obtaining a desirable mutation in a spermatazoa or egg cell to a chance of 1 per 1,000. At the same time, the probability of getting a lethal mutation of some gene would have risen to 1 in 4, and the probability of getting a mutation with some degree of harmful effect would have become a virtual certainty. That is why we must wait for the slow processes of evolution to sort out the advantageous changes.

The breeder, of course, can produce the advantageous mutations along with the many deleterious ones and discard the latter. But in the human population we cannot do that.

It would be a grave mistake to think that mutations of the hereditary material are confined to the reproductive cells or germ line. They can unquestionably occur also in the somatic cells of any tissue—kidney, brain, liver, skin, everywhere—but at present we have too little knowledge of what consequences may follow. In case the mutations are recessive, perhaps little damage would result because of that insurance principle I spoke of.

More significant would be dominant or partially dominant effects upon essential metabolic or biochemical processes, which might as a result be impaired. The loss of damaged cells would presumably do little harm, since in most tissues undamaged cells could take their places and repair would follow. But recent suggestions made here yesterday that leukemia and shortening of the life span, when induced by radiation, may increase linearly with the dose and show

no sign of a threshold at the lower dosage rate, may imply that those effects, too, result from the induction of mutations by radiation, and their accumulation in the somatic cells.

I know that Dr. Lewis would not commit himself to that point of view, but I think he and others would agree that this is a possibility that must be recognized. Possibly cancer, in general, may arise through the same cumulative effect, which does not at all exclude the intervention of other types of agents (viruses, nutritive factors, or chemical agents) in the final outburst of malignancy.

I just want to emphasize here that contrary to what many people seem to think, mutations are not limited to the reproductive cells. They can occur to any part of the body. If they are damaging in the reproductive cells we would suppose that they were damaging in the other cells, too. It is only because the body has the ability to replace damaged cells that we can escape some of these effects.

I shall now try to appraise the current exposure of the United States population to nuclear radiations, with special reference to fallout. According to the views of most, though not all geneticists, the genetic effects of exposure to radiations can best be weighed in relation to the magnitude of the spontaneous mutation rate, which is currently responsible for a certain amount of tangible genetic defect in the population, and a certain load of wholly or partially hidden mutations in individuals who carry only a single does of any particular mutant gene.

If one could confidently assume that all spontaneous mutation was attributable to the background radiation of the environment, the problem would be fairly simple. Unfortunately, this cannot be done, since in most organisms the spontaneous mutation rate is demonstrably higher than could possibly be caused by the background. Many years ago Professor Muller, who may want to speak to this point later, pointed out that for the fruitfly not more than about one-thousandth of the spontaneous mutation could possibly be caused by the background radiation. For longer-lived animals a greater fraction may well be due to the background, since the overall mutation rate per generation in different species holds fairly constant, that is, within about one order of magnitude—10 times, say—although the exposure to background radiation increases far more than that, enormously, with length of life.

If the low-level radiation of the background in fact causes a proportionate amount of mutation, then in a species that lives a thousand times as long as the fruitfly all the spontaneous mutation would be caused by the background, and some eminent geneticists have argued that that is the case. Man lives about 365 times as long as the fruitfly, for their reproductive lifetimes are of the order of 30 days and 30 years, respectively. Thus, while it may not be very likely that for man the "doubling dose of radiation"—that is, as Dr. Crow defined it, the dose that would double the total spontaneous mutation frequency, is as small as the amount of the background radiation, it is quite possible that it may be no greater than 3 times the background, or about 10 r. The doubling dose is the dose that would raise the level of spontaneous mutation in my figure to just twice the original level. Here would be the mutation frequency caused by a doubling dose, and the doubling dose would be the corresponding dose right here [indicating

on the blackboard], that would lift the spontaneous mutation frequency to a level just twice as high.

Representative COLE. Doctor, while you are at your diagram, I do not understand why your linear line does not start at the corner of your chart, rather than upward, since there are mutations which occur from spontaneous radiation.

Dr. GLASS. Yes; it starts from the level of spontaneous mutation. This becomes effectively the corner right here. At all doses there is a certain fixed proportion of spontaneous mutation.

Representative COLE. You and Dr. Crow said that there are mutations resulting from spontaneous causes.

Dr. GLASS. Yes. Those are the mutations that are indicated in this blocked-in part at the floor of the diagram.

Representative COLE. But your diagram would indicate that mutations occur only from induced radiation and not from spontaneous.

Dr. GLASS. No; only that all mutations due to extra radiation are proportional to the added dose of radiation. For example, let us say the doubling dose represents 40 roentgens. At 40 roentgens the percentage of mutations below the horizontal line is the spontaneous frequency, and from that level up to this level is the frequency induced by the radiation. So at every dose—at large doses as well as at low doses—to the frequency of mutations induced by the radiation there must be added a small percentage of mutations that is occurring spontaneously in the population.

Senator BRICKER. Are there any data, Doctor, showing that the ratio of mutations due to background radiation is twice as much at Denver as it is at Washington, D. C.?

Dr. GLASS. No. I believe no data of that kind exist.

Senator BRICKER. You would naturally conclude that the rate would be higher?

Dr. GLASS. The increase in radiation at Denver is, of course, very small. Studies were done many years ago in an effort to see whether cosmic radiation, which is greater in amount at high elevations above sea level, produces a proportional amount of mutation. Fruitflies were placed on top of Pikes Peak and compared with fruitflies kept at sea level. This was a very large and laborious experiment, but it never added up to anything conclusive, because the difference in the amount of cosmic radiation at sea level and on top of Pikes Peak is so low in terms of roentgen units over a short period of time that you just would not get any perceptible change in the percentage of mutations.

Perhaps if the experiment could have been done with bacteria where you could work with much larger numbers, the effect could have been demonstrated. But certainly for the human population I know of no evidence at all that would show that the mutation rate varies with altitude.

Representative COLE. Doctor, would you indicate on your chart where the zero dose of radiation would occur?

Dr. GLASS. Manmade radiation? Zero would be right here [indicating on blackboard the level of spontaneous mutation].

Representative COLE. I was wondering about zero rate of radiation irrespective of source, whether spontaneous or induced, since radiation you have said does cause mutation. Therefore, mutation starts with

the occurrence of radiation. Would you indicate on your chart where is the zero of radiation irrespective of source?

Dr. GLASS. It is on this axis (the ordinate) at this particular point [indicating the intersection of the level of spontaneous mutation with the ordinate].

Representative COLE. Where is the zero mutation frequency?

Dr. GLASS. The zero mutation frequency would be at the bottom of the figure.

Representative HOLIFIELD. From radiation but not from other effects.

Dr. GLASS. From any source whatever. The zero mutation from added radiation would be approximately here [indicating the spontaneous mutation level]. It might be a little below that.

Representative COLE. Put a circle there so I can see it. Zero mutation frequency.

Dr. GLASS. Zero mutation frequency is right here.

Representative COLE. And zero radiation is there also?

Dr. GLASS. Yes.

Representative COLE. Including spontaneous radiation.

Dr. GLASS. Yes. I think the thing that is troubling is that perhaps if a sizable part of this spontaneous mutation frequency is due to background radiation, then the point at which this curve should be projected is not right here [intersection of ordinate at the level of spontaneous mutation] but at some point between the bottom and the spontaneous mutation level, at a point which corresponds to the proportion of the spontaneous mutation produced by radiation. If that is only one-thousandth of this spontaneous amount, as it is in the fruitfly, then it is so close to this point of origin that I could not draw it separately on the graph. If, on the other hand, as may be in the human species, according to Haldane's argument, perhaps all, or at least a third of the spontaneous mutation was produced by the background radiation, then the dosage curve would not point at that origin on the spontaneous mutation level, but would point at a lower origin. But we do not have the data for human population, only data from the experiments on animals.

Representative COLE. Thank you, Doctor.

Dr. GLASS. It is quite possible, then, that the amount of the doubling dose may be no more than 3 times the background or about 10 roentgens—that is, on the basis of the argument that the reproductive lifetime of man is about 365 times as long as that of the fruitfly and that a proportionally larger amount of radiation is received during that lifetime.

I have left out of this account the probability that the human genes are more sensitive to radiation than the fruitfly genes. At least we can base the argument on the analogy with the genes of the mouse. The mouse genes seem to be more sensitive to radiation than the fruitfly genes and therefore, if that should apply to human genes, it would also work toward an effective lowering of the doubling dose, the dose of radiation that would double the spontaneous radiation rate.

In the most recent estimate made by the consultants of the National Academy of Sciences committee—Drs. John Laughlin and Ira Pullman have estimated the average exposure of the population of the United States to background radiation as amounting to a dose to the reproductive organs over a 30-year period of 3.1 rem. (Cosmic radia-

tion, 0.78 r.; earth and housing, 1.59 r.; atmospheric radioactivity, 0.06 r.; internal radioactivity, mainly the beta radiation from potassium-40, 0.69 r.)

To correct something that was said yesterday, according to their data the amount from earth and housing is about double that from the cosmic radiation. This is one of the things that makes the experimental answer to the question that was posed a moment ago about the effects of cosmic rays on the mutation rate a very difficult one to work out experimentally. As Dr. Crow said, the lowest conceivable doubling dose would be 3.1 r. to the gonads over a period of 30 years.

Three times the average background would amount roughly to 10 roentgens, which is the permissible limit for the general population recommended by the National Academy Committee last year. A more probable range for the doubling dose is 30 to 50 r., but that expectation, which was adopted by both the American and the British committees reporting last year, is based on experimental evidence from fruitflies and mice, which have much shorter reproductive lives than human beings. That assumption, I feel, may not be sound.

Preliminary studies in my own laboratory with normal human cells growing in tissue culture—cells derived from kidney—and exposed to radiation of X-rays, indicate that doses of even 50 or 25 r. produce significant increases in the number of microscopically visible—gross—chromosome mutations, and from that fact it may be deduced that an even greater increase of submicroscopic mutations is to be expected.

These preliminary studies make me wonder whether our committee last year was not oversanguine in estimating that the 10 r. permissible level we recommended amounted to no more than one-third or one-fourth of the doubling dose. If it should actually constitute a doubling dose, then geneticists will certainly want to reconsider all their recommendations. For a doubling dose means that, after a lapse of generations, the frequency of all kinds of hereditary defects in the population will be doubled.

Right now this frequency amounts to at least half of all tangible defects not caused by accident or infectious disease. For example, about 5 percent of births are marked by some congenital defect, and at least 2 percent of these, according to the estimate of the National Academy Committee, are of a simple hereditary nature. Doubling just these, to say nothing of those less hereditary defects which Dr. Crow has emphasized, and those that arise later in life, would mean eventually about 2 million more defective babies per generation than now, assuming a hundred million babies per generation in the United States.

The 30-year gonadal dose from fallout amounts, according to our estimates, to about one-tenth roentgen if extrapolated on the basis of the average fallout for the past 5 years, or two-tenths roentgens at the rate of testing during the 2 most active years in that period. This is small indeed compared to the estimated background radiation or to the amount received on the average from medical and dental diagnosis and therapeutic uses of X-rays, radium, and radioisotopes, which amount to about 4.6 r. reproductive dose during a 30-year reproductive lifetime.

A safety factor here—that is, in relation to fallout—is the very fact that strontium 90 is accumulated in bones and radioiodine in the thy-

roid, and consequently they provide only a negligible amount of radiation to the gonads. We are not so sure about the localization of cesium 137, however. Nonetheless, if the gonadal dose from fallout which constitutes the genetic hazard is at the present time only 1 or 2 percent of the permissible limit for the general population, that is no reason to be complacent, in my opinion, about its rise. The increasing tempo of weapons-testing in 1957 will certainly make a reevaluation necessary.

Senator BRICKER. Is the half life of cesium 137 the same as strontium 90.

Dr. GLASS. Yes, sir, about 30 years.

Representative HOLIFIELD. It is deposited in the muscles.

Dr. GLASS. Yes, in the muscles and soft tissues, generally. It is possible that it might even concentrate in the reproductive organs. This is something that I think should be very urgently looked into in our experimental studies. I have heard a rumor to that effect. That is all I can say.

Representative HOLIFIELD. Is the power of emissions stronger than the strontium 90?

Dr. GLASS. I think it is about the same. I am not certain.

Representative VAN ZANDT. Doctor Glass, this increased tempo of weapons testing in 1957, did take into consideration the British testing?

Dr. GLASS. That is what I mean, the British and Russian testing plus our own.

Representative VAN ZANDT. Looking to 1958 and 1959, did you include any other nations participating in these tests?

Dr. GLASS. No, I do not know that any other nation is ready to begin weapons-testing, but I think that unless some international agreement to limit weapons-testing, or to eliminate it, is reached, it will be only a matter of a few years until other nations will be testing weapons, too.

Representative VAN ZANDT. In 1958–59 do you anticipate that the tempo of the schedule of 1957 tests will be stepped up?

Dr. GLASS. I certainly do, unless we can reach some international agreement.

Representative VAN ZANDT. In other words, you anticipate an annual increase of tests.

Dr. GLASS. Yes.

Senator BRICKER. The Russian tests up to the present produced about one third of the total of radiation effects through fallout.

Dr. GLASS. Others can speak about that better than I. I think that is about the correct fraction.

I must emphasize, too, that because I estimate fallout to be at present a negligible hazard to the genes we must pass on to future generations, in comparison with other factors, I by no means feel that the accumulation of strontium 90 is a negligible hazard in other respects. The evidence that has been presented to this committee would make me think otherwise. These are simply two different questions, and I am speaking to the genetic question only.

When we consider that at present our population is receiving almost half of the 10-roentgen allowance per generation from manmade sources, and that exposure is certain to increase as wastes from the development of peaceful uses of atomic energy multiply, a fivefold in-

crease in fallout from weapons testing would add to a grave problem.

There are clearly many uncertainties in the evaluation of these questions. I would therefore like to conclude by stressing something said in the final part of our National Academy of Sciences Committee report.

> The present state of advance in atomic and nuclear physics on the one hand, and in genetics on the other hand, is seriously out of balance. We badly need to know much more about genetics * * *

There are critical problems in this area about which answers are urgently needed, for example, the exact magnitude of the doubling dose.

> Our society should take prompt steps to see to it that the support of research in genetics is substantially expanded, and that it is stabilized.

I think that this remark, which I had prepared before I heard Dr. Pollard yesterday, strongly reinforces what he said to the committee.

The program of the Atomic Energy Commission exemplifies this unbalance between emphasis on the physical aspects of atomic energy and on the biological counterpart, its effects on living beings. Without wishing in any way to reflect on the competence within their own fields of our sincere and able Atomic Energy Commissioners, I think it safe to predict that this unbalance is likely to continue until the genetic and other biomedical problems and points of view are appropriately represented on the Commission itself.

Representative HOLIFIELD. Would you not say that a more desirable goal would be to have a completely independent study of this made, so that there would be no justification on the part of those making the study to attempt by the results of their study to justify an administrative policy which may or may no be correct?

Dr. GLASS. That is an interesting question, Mr. Chairman. I would say that, in my own personal opinion, if at the beginning it had been set up that way, I would answer yes. But we have a going concern, and the Atomic Energy Commission has a large program of support for research in the biological sciences at the present time. I have indicated that I don't think it is by any means adequate, but it is large. I have great fear that if there was some attempt to shift all of the biological, genetic, and medical support of research in the atomic energy area to some other agency, there would be such disruption and chaos for a period of several years, that we would lag sadly, even more than now, in our program.

Representative HOLIFIELD. Any attempt that could be made within the program to achieve complete independence by freeing of such an advisory group or research group from administrative policy would be desirable, would it not?

Dr. GLASS. Yes, I think so.

Representative HOLIFIELD. We had an Advisory Committee on Reactor Safeguards.

Dr. GLASS. Yes.

Representative HOLIFIELD. In one instance their advice was not taken, and this is the problem that you run into with all advisory groups, that is, that their advice sometimes is not taken. I am not saying that it should be taken at all times. It should be considered in relation to the other problems which the administrator has. That is why I bring up the question.

Representative COLE. On that point, is it not true that Dr. Glass himself is a member of the Advisory Committee?

Dr. GLASS. I am a member of the Advisory Committee to the Division of Biology and Medicine of the AEC, yes.

Representative COLE. With further reference to your feeling that the program of the Commission is out of balance, it was indicated to me that the effort of the Commission in the field of biology and biological aspects of atomic energy has been neglected. I do not think you intend to imply that, do you?

Dr. GLASS. No, not neglected in an absolute sense. It is a matter of emphasis. It is a relative balance that is important.

Representative COLE. Since you are on the Advisory Committee in this field, while dollars are not a true yardstick in determining the degree of effort, could you tell us the amount which the Commission is spending annually in this field in which you think there should be greater emphasis?

Dr. GLASS. I will have to rely on my memory here. I believe it is currently around $38 million. Dr. Dunham is here.

Representative HOLIFIELD. Dr. Dunham, could you respond to the question?

Dr. DUNHAM. The question was the overall biology and medicine budget. About $31 million.

Representative COLE. That represents an effort on the part of how many individuals? I am speaking of scientists.

Dr. GLASS. I am afraid I cannot answer that.

Dr. DUNHAM. This material will be available to you by tonight. We are trying to figure that out. It is very difficult to sort out the projects and add up the number of scientific man-years.

Representative COLE. I was going to say it is my recollection, Dr. Dunham, at the beginning of these hearings you indicated of the order of 500 individuals were engaged in this research.

Dr. DUNHAM. I think it is a considerably higher figure now that we have gone back and looked at the record. We will have this for you.

Senator BRICKER. Most of this work is done by contract with the various universities and research centers.

Dr. DUNHAM. It is by contract with universities, independent laboratories and with our national laboratories.

Senator BRICKER. It would be practically impossible to know exactly the number of scientists working in the various fields, because this is determined by the local organizations.

Dr. DUNHAM. It is very difficult because you get people working part time on these projects. It will be an educated guess at best as to the figure we will come up with.

Senator ANDERSON. Did you say that the general overall biology and medical budget was $31 million?

Dr. DUNHAM. That is correct.

Senator ANDERSON. That includes a whole lot of things besides genetics.

Dr. DUNHAM. That is correct, sir.

Senator ANDERSON. Could you give us any idea when you give us a report how much money is being spent to study this question of genetics which has interested Dr. Glass and so greatly interested a great many people in the United States?

Dr. DUNHAM. This figure will be broken out separately.

Senator ANDERSON. At the same time, have you any idea how much the Atomic Energy Commission is spending in research in physics?

Dr. DUNHAM. I do not recall their exact budget this year, but it is in the $40 million or $50 million range.

(NOTE.—This figure was later checked by Dr. Dunham, and the budget for current fiscal year for the Research Division is $59,523,000 excluding cost of equipment.)

Senator ANDERSON. They are spending that much on a few reactors alone.

Dr. DUNHAM. Very definitely so. That is in a different part of the budget.

Senator ANDERSON. So we have plenty of money to find out how a reactor is going to react, but we do not have too much money to find out about genetics.

Dr. DUNHAM. It is one way to look at it.

Senator ANDERSON. It is a way that a great many mothers and fathers and even some grandfathers are interested in looking at it.

Representative VAN ZANDT. Dr. Glass, you say "appropriately represented on the Commission itself." What are your recommendations?

Dr. GLASS. I would say that the balance might be restored, possibly adequately, by one representative of the biological sciences. Perhaps a geneticist, perhaps not.

Representative VAN ZANDT. Serving as an AEC Commissioner?

Dr. GLASS. Serving as a Commissioner. I would also like to see either the present Advisory Committee to the Division of Biology and Medicine serving as a general advisory committee to the Atomic Energy Commissioners, or else I would like to see the present General Advisory Committee revised to include geneticists and biologists.

Representative VAN ZANDT. At any time during the past several years, have you made these recommendations to the Commission?

Dr. GLASS. No; I have not.

Representative HOLIFIELD. Dr. Dunham is asking for permission to speak.

Dr. DUNHAM. I would like to state that the Advisory Committee on Biology and Medicine is advisory to the Commission, and it was so established by the first Chairman of the Atomic Energy Commission.

Representative HOLIFIELD. But it does not have a geneticist or biologist on it.

Dr. DUNHAM. No. The Advisory Committee on Biology and Medicine, of which Dr. Glass is a member, is advisory to the Commission, not just to the Division of Biology and Medicine.

Dr. GLASS. This is technically correct. At the same time I think it is correct, is it not, Dr. Dunham, to say that it has a ranking somewhat lower than that of the General Advisory Committee?

Dr. DUNHAM. It is different in that the exchange of correspondence between Mr. Lilienthal and the Academy at the time the committee was set up indicated a definite wish that it be the advisory group on all biomedical problems to the Commission.

Representative COLE. That would indicate that there is no biologist on the General Advisory Committee.

Dr. DUNHAM. That is correct.

Representative COLE. But there is a special committee whose responsibility is exclusively in the field of biology.

Dr. DUNHAM. That is correct.

Representative VAN ZANDT. Mr. Chairman, may I ask Dr. Glass this question? Just what advantages would be enjoyed if we had a biologist sitting as a Commissioner on the Atomic Energy Commission?

Dr. GLASS. May I read my last three sentences which I think perhaps answer that?

Representative VAN ZANDT. All right.

Dr. GLASS. The geneticist cannot help feeling frustrated when physical scientists, religious leaders, and common people everywhere raise a loud outcry for some answer to their fears of genocide by radiation, and at the same time the available funds for research are actually in danger of reduction. Some of the research required is expensive, but in comparison with the cost of a bevatron for physical research or a nuclear reactor in the program it is of course minor. It is vital to redress the balance.

Senator ANDERSON. You say you have been on this Advisory Committee. How long have you been on it?

Dr. GLASS. Two years.

Senator ANDERSON. Dr. Dunham pointed out this is an advisory committee directly to the Atomic Energy Commission. In those 2 years, how many times have you been called before the Atomic Energy Commission to give a report of what the Advisory Committee had been doing?

Dr. GLASS. There has never been any formal call before the Commission as a whole.

Senator ANDERSON. Have you ever been called into a meeting to discuss this directly with the Atomic Energy Commission?

Dr. GLASS. Not personally, no. There have been occasions when various Commissioners—Admiral Strauss, Dr. Libby, Mr. Murray, and I remember Dr. von Neumann, too, before he became too ill—sat in with our committee for one or more sessions as individuals. I do not believe there was ever a time when more than one of them was present at the same time.

Senator ANDERSON. I thought Dr. Dunham made quite a point that this is an Advisory Committee which reports directly to the Commission. How many times have you been in with the Commission for the discussion of this problem in which so many people are interested? None at all?

Dr. GLASS. None at all.

Senator HICKENLOOPER. Have you made reports to the Commission from time to time periodically as to your recommendations and findings?

Dr. GLASS. Yes. This is what I wanted to state. Our minutes, our conclusions and recommendations are always submitted to the Atomic Energy Commissioners in writing. A full report is made and often special letters by way of report are made. This has not been neglected.

Representative COLE. How does your committee operate? Is it by meetings of individuals or by exchange of views by paper?

Dr. GLASS. By meetings. We meet regularly.

Representative COLE. During the past 2 years how many times has your committee met?

Dr. GLASS. About 12 times.

Representative COLE. That would indicate once every 3 months.

Dr. GLASS. Every 2 months, except for the summer, when we sometimes skip a meeting. There are special meetings called fairly frequently, too.

Representative VAN ZANDT. Dr. Glass, you have recommended to this committee that a biologist be appointed to the Atomic Energy Commission. Would you go a step further and recommend to President Eisenhower that one of the vacancies that exist now as far as the Commission is concerned be filled by a biologist?

Dr. GLASS. This is my personal feeling of what is wise.

Senator ANDERSON. If you get as far with your recommendation as a majority of this committee got with its suggestion, you will not get very far.

Dr. GLASS. There is no harm in trying.

Senator HICKENLOOPER. May I ask Dr. Glass if he would recommend that a physician or a medical doctor be appointed to the Commission, and if he would recommend that a physicist be appointed to the Commission? Where do we stop? One of the difficulties is that when 1 very important branch of this very ramified science is appointed to the Commission, it has a tendency to create a little friction on some of the other equally important branches involved, and there are only 5 Commissioners. So while it is desirable to have the best scientific competence available in one way or another to the Commission, I believe it was the theory of the original act that the General Advisory Committee and the other advisory groups would bring the professional competence in substantial numbers in the various fields for the benefit of the Commissioners, and it would to some extent stop the friction which occasionally unfortunately may exist between the various branches as to their relative importance. So if one starts putting one branch representative on the Commission there will literally be no stopping within any reasonable numbers as to the other branches of this science which might feel they too were equally justified in having representation.

Dr. GLASS. I realize, of course, that one cannot have representation of all the sciences and the specialized branches. I do think, however, that the biological considerations here are so important and require such urgent attention that it is advisable that the Atomic Energy Commission include at least 1 physical scientist and 1 biological scientist.

Senator ANDERSON. Doctor, the fact that there is a great deal of discussion in Japan over the testing of British bombs right now, and that the British consulate has been subjected to a little activity by students in the past few days, would indicate that there is some interest in these biological effects of the bomb, would it not?

Dr. GLASS. It certainly would.

Senator ANDERSON. And the fact that 2,000 scientists have joined in suggesting that there are some biological ramifications to this story would suggest that this is an extremely important facet. We have representation of fine men from industry on the Commission who are greatly interested in the reactor program. I think it is fine that there may be people interested in that field. One of the things which touches the hearts of the people has been the possibility that this might have some future implications for our children, and our children's children, and on down through succeeding generations. Is not that the consideration that prompts your suggestion?

Dr. GLASS. It certainly is.

Chairman DURHAM. Dr. Glass, is any substantial amount of research being carried on by any other group except the Commission at the present time in the field of genetics?

Dr. GLASS. Yes, indeed. There are considerable numbers of projects in this area which are supported by research grants from the National Science Foundation and also from the National Institutes of Health. There are some, not too many, I think, which are supported by the Office of Naval Research, because their interest is mainly physiological.

Chairman DURHAM. I thought that was true and that is why I asked the question. Could you hazard a guess as to the amount of money being spent by those outside of the Commission at the present time?

Dr. GLASS. Very roughly, I would say at the present time the Atomic Energy Commission and its Division of Biology and Medicine is supporting about half the work that is receiving governmental support in genetics. It may be even more than that.

Chairman DURHAM. Would that particularly apply to radiation?

Dr. GLASS. Much of the genetic work that is supported by the National Science Foundation of the National Institutes of Health is not concerned with radiation. If you limit it to radiation genetics, I would say that the Atomic Energy Commission is supporting at least 90 percent.

Senator HICKENLOOPER. I just wanted to ask Dr. Glass this question about the availability of competent personnel. Is there a substantial reservoir of competently trained personnel in genetics that could use greatly stepped up amounts of money over and above what is being used in total or that is devoting its time total in the country in both private and public activity?

Dr. GLASS. I think it would be a mistake to try to triple or quadruple the amount going into this area in one year or overnight. But over a period of years, a stepped-up increase could be absorbed. Perhaps the principal difficulty at the present time is in the selection and training of adequate young people in this area. They are not being attracted into it in large enough numbers because of lack of support.

Senator HICKENLOOPER. In order to get down to specific cases, suppose the Atomic Energy Commission said today we will double the amount for the next year for genetics activity; would that have a significant effect in the immediate future, or would it take a substantial period of time before you could train the personnel and get the equipment and so on to utilize that extra money? Are they available in the United States at the present time?

Dr. GLASS. It would take a rather careful survey, I believe, to answer that question precisely. I am not sure that doubling it in 1 year would be at all advisable. But there are certainly some large programs which would require a large annual budget which could be initiated then and that now cannot be attempted. I believe that an increase by 50 percent could probably be absorbed in 1 year.

Senator BRICKER. Is it not true that there are many, many programs beyond those which are being conducted by the Government, either by the Atomic Energy Commission or defense research or the National Science Foundation, by private institutions in the various universities of the country?

Dr. GLASS. There is a lot of genetics of that kind going on, yes. As I said a moment ago, I believe about 90 percent of the genetic

problems relating to radiation effects is being supported by the Atomic Energy Commission.

Representative COLE. Mr. Chairman, in response to a question by Senator Anderson, Dr. Glass indicated that these activities in Japan at the British Embassy indicated an interest by the Japanese people in the biomedical aspects of the weapons testing. Similar activities occurred at the American consulate 1 or 2 weeks ago. If that was the interest which inspired these people to become active at those two consulates, why have they been silent with respect to the testing by the Russians?

Dr. GLASS. I do not know that they have been silent. Have they?

Representative COLE. So far as I am aware, there has been no appreciable degree of representation in front of the Russian Embassy in Tokyo as there has been with the American and British.

Dr. GLASS. That may be true. I know that at the time of the last Russian tests, when fallout occurred over Japan, there was a considerable stir and a good deal of reporting of the Japanese concern about this in our newspapers. I do not know that it took the form of a vocal demonstration.

Representative COLE. There must be some other motiviation on the part of the Japanese than interest in the biomedical aspects which prompts them to voice themselves with respect to weapons testing.

Dr. GLASS. Yes, I am sure there must be political motives involved in this.

Senator ANDERSON. We all recognize that the Russians may have done a little better propaganda job than we have. They have offered to stop tests over and over again. We do not like the circumstances under which they offered it. That may be one of the things which led the Japanese people to believe they will stop the tests. It is that the British and ourselves will not stop for all practical purposes, so we have gotten ourselves in a bad position internationally.

Representative COLE. I would like to have Dr. Glass indicate for the record the extent, if any, to which the genetic aspect is under constant survey by the United Nations Committee on the Radioactive Hazard.

Dr. GLASS. It is a very important part of that study. The United Nations Committee, of which Dr. Shields Warren is the Chairman, I believe, has met repeatedly over the course of the last 2 years to consider these problems. I am confident that our own Atomic Energy Commission has given it every degree of cooperation possible in supplying whatever data were requested.

Senator HICKENLOOPER. Mr. Chairman, may I ask Dr. Glass one more question?

Representative HOLIFIELD. The Chair would like to make a statement at this time.

I am sure every member of the committee thinks that this problem is so important that we are not going to curtail questioning of witnesses even though it disturbs our schedule. I hope the members will agree with me on that. This is a very important part of our hearings, and they should not be curtailed in any way as long as we have valuable testimony to be given or questions which the members wish to ask.

Senator HICKENLOOPER. Dr. Glass, this occurred to me a while ago when you were talking about desirable mutations. If a desirable mutation is had, do the desirable qualities of that mutation continue in the succeeding generations, or do they deteriorate?

Dr. GLASS. They would be just as permanent, we think, as the effects of a harmful mutation.

Senator HICKENLOOPER. So that once a desirable mutation is acquired, its characteristics would probably continue.

Dr. GLASS. Would be transmitted indefinitely; yes.

Chairman DURHAM. By radiation, you are speaking?

Dr. GLASS. Yes. There was a big "if" there, if I may answer your question, because I did earlier stress the fact that as a class of mutations produced by radiation are much more likely to be losses of the genetic material than are mutations that occur spontaneously. Consequently, I think the probability that a desirable mutation will occur as a result of radiation is even less than the probability that it might arise spontaneously.

Senator BRICKER. I do not think there is a member of this committee or anyone advised that does not realize that there is political propaganda being utilized by the Communists and the left wing groups generally in this field, and we may be giving some credence to it. On the other hand, it is an essential investigation that must be made. I think the public is entitled to all the information they can get.

Do you, as a scientist, or does your group of advisers, have any information as to what Russia is doing in this field of biological effects?

Dr. GLASS. We have very little information with regard to the state of genetics in Russia at the present time. I think Professor Muller would be better qualified to answer that question than I am. Perhaps I should postpone it.

Representative HOLIFIELD. We want to give Professor Muller plenty of time. When we get him on the stand we will ask him to testify on this point.

Thank you very much, Dr. Glass, for your very fine presentation this morning. I hope you will be able to stay for the roundtable conference, whenever we have it. I am afraid we will not be able to get at it at noon. We would like to put on this morning another notable, Dr. A. H. Sturtevant from the California Institute of Technology. Dr. Sturtevant, we are happy to have you with us this morning. You may proceed.

STATEMENT OF DR. A. H. STURTEVANT, PROFESSOR OF GENETICS, CALIFORNIA INSTITUTE OF TECHNOLOGY [8]

Dr. STURTEVANT. Mr. Chairman, the nature of the genetic effects and the quantitative estimates have been presented by Dr. Crow and Dr. Glass, so I shall not go over that ground. There is one point

[8] Date of birth: 1891. Education: Bachelor of arts, Columbia University, 1912; doctor of philosophy, 1914; honorary doctor of science, Princeton, Pennsylvania, Yale. Thomas Hunt Morgan professor of genetics, California Institute of Technology. Past president, American Society of Zoologists, Genetics Society of America, Pacific division, American Association for the the Advancement of Science. Kimber Medal for Genetics, National Academy of Sciences. Member, National Academy of Sciences Committee on the Genetic Effects of Atomic Radiation. Member, American Philosophical Society, National Academy of Sciences. (Submitted by witness.)

I should like to indicate, however, that sometimes has led to some confusion.

If there is an increase in the amount of radiation, within the range with which we are here concerned, there will be more mutations induced, but there will not be more serious mutations. Individually they will have the same range of effect as will those which were produced by the smaller amount of radiation.

Many geneticists have been disturbed by statements from the Atomic Energy Commission implying that there is no reason for any concern about possible damage to man arising from bomb tests. Recent statements by Commissioner Libby suggest that there is now an area of agreement on which discussion may be based. We are agreed that there is at least a possibility of damage to a small percentage of the population. We are, I think, also agreed that it is not now possible to present very exact estimates as to just how small that percentage is; and, further, that it is important to make studies that will improve the accuracy of such estimates.

Such efforts to improve the estimates must come largely from further research, both on the physical side (as to the nature and distribution of fallout), and from the biological side (as to the genetic and pathological effects of fallout). I should like here to express my opinion that, at least in the field of genetics—which is where I am competent to judge—the AEC has sponsored an excellent program, both in its own laboratories and in its grants to other laboratories. These projects seem to me to have been well chosen and well administered. In particular, there is, so far as I know, no pressure for the expression of conclusions or opinions that may be agreeable to the AEC.

I would like to add here that I should not like this statement read to mean that I feel that more money could not be used effectively. In other words, I am not in general in disagreement with Dr. Glass' plea for more support in this field.

However, there remain areas in which some geneticists are still disturbed by the AEC position. This, as has often been pointed out, arises in part from a difference in attitude concerning very small percentages. Various methods of calculating damage from fallout result in estimated numbers of affected individuals ranging from a few hundred to perhaps tens of thousands or even millions, depending in part on what assumptions one makes about the rate of future bomb testing. The highest of these numbers remains a very small proportion of the total population, and to some people this means that it is relatively unimportant. It is probably unimportant for the survival of the race, and it is relatively unimportant as an economic burden to society, though these could become serious matters if the rate of fallout should increase by a large amount. But hundreds or thousands or tens of thousands or more of individual human beings are involved, and to me it is not acceptable to say that they are unimportant, no matter how small a percentage of the total they make up.

Another point that some of us find disturbing is the insistence that the risk from fallout is much less than risks that we voluntarily

take repeatedly—such as those involved in riding in an automobile or going for a swim at the beach. While the risk is less from fallout, the essential point is that it is one over which the individual has no control. It has been argued that the risk from fallout is not very different from that of wearing a wristwatch with a radium painted dial. Even if this comparison is accurate, it still leaves out of account the fact that some of us do not wear such watches, and would complain loudly if anyone tried to insist that we and our children must do so.

There are biological risks from bomb testing. They are small—so small that no individual should be seriously concerned about the danger to himself or his immediate descendants. Nevertheless, when the entire population of the United States, or of the world, is exposed, it must be expected that there will be many individuals damaged, both in the exposed generation and in following ones.

I should like to lay special emphasis on the fact that every bomb test adds to the biological hazard. It follows that the most effective way to reduce future hazards would be not to test any more bombs. A geneticist cannot assume special competence in evaluating all the arguments for or against such a cessation, since the decision has to take into account many factors that lie outside his special field. But I should like to point out that biologists have made a serious effort to present an objective statement of the biological hazards, and it therefore seems to me reasonable to ask for as detailed and objective a statement of the reasons for continued testing as is consistent with security considerations. Some physicists have questioned the desirability of testing, on the ground that it gives almost as much information to other nations as it does to those making the test. Many nonscientists in this and other countries have argued that the tests worsen rather than improve, the international situation. These arguments, and those of the biologists, have never been publicly discussed seriously and in detail by those responsible for carrying out the tests.

Representative HOLIFIELD. Thank you very much, Dr. Sturtevant. Are there any questions?

Senator BRICKER. Of course, it would be impossible for us to do it unilaterally in light of what Russia is doing at the present time, and that may be the reason for your last conclusion.

Dr. STURTEVANT. That will have the same effect.

Senator BRICKER. That leads to your last conclusion that that is the reason there has not been serious discussion of cessation until we can get the international situation solved. Until that is done, there can be no effective steps taken by the United States alone.

Dr. STURTEVANT. I think the discussion should be as frank as possible in view of the security considerations involved. I was careful to add that.

Senator ANDERSON. Did you hear the statement that Dr. Langham made the other day when we were considering the possibility of limiting tests in some fashion where he suggested that we set a sort of ceiling of ten megatons of fission products as a reasonable limit of how much might be put in the atmosphere each year. Would you feel that something of that general nature is desirable until we are able to reach

some other step in this process. In other words, I think his thought was if we could limit to maybe 10 megatons the amount of fission products going into the atmosphere, there is a certain deterioration of strontium 90 and the decay of that material, and therefore we should not be putting more into the atmosphere than might decay out. Do you think it would be desirable to have some sort of limitation of that nature?

Dr. STURTEVANT. Speaking as a geneticist, anything which will decrease the amount is desirable. Speaking as a citizen, I realize that there are other considerations that have to be balanced against this.

Senator ANDERSON. His statement was based on some sort of international agreement. It was not to be done unilaterally by this country. He did feel it was desirable to make sure no more was going into the atmosphere than was falling out of the atmosphere each year. I think geneticists generally are in sympathy with that.

Dr. STURTEVANT. I would prefer to see less going in than was coming out, if possible.

Senator ANDERSON. I think he would, too.

Representative VAN ZANDT. Dr. Sturtevant, in the closing part of your statement, you say these arguments and those of the biologists have never been discussed in public seriously with those responsible for carrying out the tests. Would you recommend a public forum for this discussion, keeping it within the bounds of security?

Dr. STURTEVANT. That has been suggested recently as a possibility. I would rather see myself a statement by people who were responsible for it. In other words, by the AEC.

Representative VAN ZANDT. These hearings should constitute a forum where the biologists and those responsible for carrying out the tests can sit down and discuss the overall problem.

Dr. STURTEVANT. I think that would be very desirable.

Representative HOLIFIELD. Are there any further questions? If not, we will recess at this time until 2 o'clock.

(Thereupon at 12:35 p. m. a recess was taken until 2 p. m., the same day.)

AFTERNOON SESSION

Representative HOLIFIELD. The committee will be in order.

We are honored to have before us this afternoon Dr. Muller from the University of Indiana. It is my pleasure to announce to our audience today that Dr. Muller is a Nobel Prize winner.

As the Chair looks around and sees the group of children on our left here and realizes the importance of this testimony to them and future generations, it brings to my mind the gravity of the purpose of this committee, which is to bring out the scientific facts in this field.

Dr. Muller, you may proceed.

STATEMENT OF DR. HERMANN J. MULLER, PROFESSOR OF ZOOLOGY, UNIVERSITY OF INDIANA[4]

Dr. MULLER. Thank you, Mr. Chairman.

The subject of the genetic effects of radiation is, in my opinion, so closely bound up with that of the effects on the exposed individual himself that I feel it necessary, to begin with, to touch on that subject first.

I feel this is especially necessary because there has been a curious official silence concerning findings showing that the main damage to the exposed individuals themselves by small or moderate exposures to radioactive substances or X-rays consists of an insidious weakening of the body's resistance to the onset of infirmities and diseases of all kinds, expressing itself in a shortening of the length of life, and also consists in a long delayed production of certain specific disorders of which the most important are leukemia and some other malignant conditions.

Still less publicized has been the increasing evidence that the amount of these effects is simply proportionate to the total dose of radiation received, even when this has been given in tiny bits scattered over long periods. That is evidence for the conclusion that there is no threshold, some of which evidence you heard presented by Dr. Lewis.

I may say that I first heard of this principle of the shortening of life in amounts proportionate to the total dose received, no matter in how many small bits, in a very scholarly address given by Dr. Robert D. Boche at Oak Ridge in April 1948, and again at the Argonne Laboratories in November 1948. These were not classified meetings. The principle that he explained to them was based on work done by a number of different investigators during the war, working mainly at Rochester, N. Y., under the Manhattan project.

I mentioned these conclusions of Boche in a series of lectures given under the auspices of the Society for Sigma Xi, that were published 2 years later. But I was unable to refer to Dr. Boche's work except as addresses because I waited in vain to see publications along those lines.

Representative HOLIFIELD. Where were these addresses given?

Dr. MULLER. One was at Oak Ridge and the other at Argonne. They were open meetings. That is to say, it was not classified. They

[4] Date and place of birth: December 21, 1890, New York City, N. Y. Education: A. B., Columbia University, 1910; M. A. (1911), Ph. D. in zoology, physiology and biochemistry, (1916); D. Sc., Edinburgh University, 1940. Work history: Teaching fellow in physiology at Cornell Medical College, 1911–12; assistant in zoology at Columbia University, 1912–15; instructor in biology, Rice Institute 1915–18; instructor in zoology, Columbia University, 1918–20; associate professor (1920–25); professor of zoology, University of Texas, 1920–36; guest investigator (1937–38); lecturer in animal genetics, Institute of Animal Genetics, Edinburgh University, 1937–40; resident associate (1940–42); visiting professor of biology, Amherst College, 1940–45; professor, (1945–53); distinguished service professor of zoology, Indiana University, since 1945. Subsidiary appointments: Guggenheim Memorial Foundation fellow at Institut fur Hirnforschung, Berlin, 1932–33; senior geneticist, Institute of Genetics of Academy of Sciences of U. S. S. R., Leningrad and Moscow, 1933–37; civilian consultant to MED, 1943–44, to AEC, 1946–48; member of National Aiademy of Science, American Association of Advanced Science (fellow), American Society of Naturalists (vice president, 1932; president, 1943), American Philosophical Society, American Academy of Arts and Sciences, American Society of Zoologists, Genetics Society of America (president, 1947), American Genetics Association, Society for Study of Evolution, American Society of Human Genetics, Society of Experimental Biology and Medicine, American Humanist Association (president, 1956–57), Sigma Xi, Phi Beta Kappa, Alpha Epsilon Delta. Honors received: Annual $1,000 award of American Association of Advanced Scientists, 1937; Nobel Prize in physiology and medicine, 1946, for the production of mutations by radiation; presidency of Eighth International Congress of Genetics, Stockholm, 1948; honorary D. Sc. from Columbia University, 1949; Kimber award in genetics of National Academy of Sciences, 1955; Virchow medal of Rudolph Virchow Medical Society of New York, 1956. (Submitted by witness.)

were not published and they were not open to the press so far as I know, so I only knew about the matter verbally.

Representative HOLIFIELD. Were they in the form of prepared addresses?

Dr. MULLER. Prepared addresses, and with charts of data; yes.

Representative HOLIFIELD. Who did you expect to print those?

Dr. MULLER. I expected he would have an article on the subject. Later I found that there was a classified article of his in 1946 on the subject. It was later declassified, but I am not sure when. It did not get out into the general literature. It was not spoken about to any extent. In fact, although I do not know whether it had any relation to that silence or not, there was an attempt made to prevent the publication of that part of my own lecture which contained this material. No special reason was given. I do not want to go into the details of that because it may have been accidental, but it is a very curious incident.

Representative HOLIFIELD. You said there was an attempt made?

Dr. MULLER. The editor had agreed to publish the whole article. It was presented in 2 parts because it was too long to go in as 1 article. Both parts were to have been published. After the one part had been published I was told that they did not want to publish the other part, that that was enough. The first part concerned itself with the effects on future generations and the second part with the effects on the exposed generation itself.

Representative COLE. Mr. Chairman, would Dr. Muller give the time when this incident occurred?

Dr. MULLER. The lectures were given in 1948. They were published first in 1950. So that occurred in 1950.

Representative VAN ZANDT. Dr. Muller, did the thinking at that time fit in with the problem of today?

Dr. MULLER. Yes. It was not as acute because we did not have hydrogen bombs yet.

Representative VAN ZANDT. Were there any predictions in those papers?

Dr. MULLER. I called attention to these points; yes. We did not know much about fallout yet.

Representative HOLIFIELD. Dr. Muller, what type of a meeting was this? Who set the meeting up, and where was it?

Dr. MULLER. One of them at Oak Ridge was set up by the Director of Research, Dr. Hollaender, a very eminent biologist and geneticist. The other was called under the auspices of the Argonne Laboratory on the problem of the effects of low doses of radiation.

There were many interesting papers there. For example, one which showed the lessening of the number of white blood cells in workers at Los Alamos who had received only 25 r. of radiation over the whole year. Nevertheless, by taking accurate statistics of the whole group it was possible to show the effect. One would not have been able to see it in one individual because it fluctuates too much.

Representative COLE. Mr. Chairman, I am curious to know, Dr. Muller, is it your feeling that there was a deliberate attempt on the part of the responsible officials of the Government, whether of the Atomic Energy Commission or the Manhattan Engineering District, to suppress the expression of your views on this subject?

Dr. MULLER. I would not like to express an opinion about that. It may have been a coincidence. But I was very surprised that no one else said anything about the matter in any semipopular or popular publication. This was a semipopular publication.

Representative VAN ZANDT. Dr. Muller, do you feel that had the papers been made public at that time that it would have eased this acute problem we are faced with today?

Dr. MULLER. Yes. I think that any such knowledge is to the good because the public should be prepared to know the facts and if they find out later that something has been withheld from them—I will touch on that later—they are apt to exaggerate the importance of the facts. I think that there was a feeling probably amongst many people who felt that these facts needed further verification, that it was unwise to let the public know about them until they were proved up to the hilt.

To me it seemed that the main principles were already far enough established to warrant a public airing, since if true, they were of such grave concern.

Senator ANDERSON. Doctor, this morning there was some reference made to Russia, and it was suggested that one of the witnesses testify about the situation in Russia. Here we have the situation in this country today where the foremost geneticists of the country are testifying before an open hearing. Is that the situation in Russia, or are geneticists in somewhat different circumstances over there?

Dr. MULLER. There are few geneticists left. I would rather postpone questions on Russia until we get through this because we might go on indefinitely. I think we can draw lessons from Russia.

Senator ANDERSON. I knew you had worked in Russia.

Dr. MULLER. Yes.

Senator ANDERSON. And therefore have some opinions. I hope you give them when you finish this afternoon.

Dr. MULLER. I would be happy to answer to the best of my ability any such questions as you might have.

These facts, which I think Dr. Hardin Jones and Dr. Russell will take up in their talks, about the shortening of the length of life seems to me to be extremely important. I think it can be shown that they and the long delayed malignancies such as leukemia constitute by far the greatest damage of all the effects of radiation on the exposed individual himself. That is, of moderate and small doses. I am not speaking of the radiation sickness and death that occurs from very high exposures such as direct atomic bombing.

Therefore, I thought it was very curious that these effects were not discussed more and were not in fact investigated more. It seemed to me if there was an uncertainty then it should have been pushed to find out is it true or not.

We did finally in 1954 have papers on this work published in declassified form in a highly technical volume edited by Dr. Blair almost 10 years later. Since then the effects have been talked about to some extent. They were mentioned in the Pathology Committee report of the National Academy of Sciencies. So far as I can remember in the National Committee on Radiation Protection—I happened to belong to its Subcommittee on External Dose for some years—there was very little mention of them. I doubt very much that they were gen-

erally realized by workers in the field of radiology, for example, or by biologists in general.

In respect to the fact that probably there is no threshold, that these effects are proportional to the dose, in this respect these effects of radiation—and also the leukemia—on the exposed individual himself resemble those produced by the radiation in weakening descendants.

You have heard Dr. Glass and Dr. Crow say that geneticists are convinced that there is no threshold for the genetic effects and that others, too, now accept that principle for the genetic effects.

If this is true of these other effects, and it is certainly time we knew whether it was—I think the evidence is convincing that it is—then this important resemblance between the effects on later generations and on the exposed generation is probably not an accidental resemblance. For there is growing reason to infer that this shortening of life and the other long delayed damage done to an exposed individual have their basis in damage done to the genetic material—the chromosomes and their contained genes—of the body's ordinary cells, those of the blood, skin, glands, and so forth, similar to the damage done in his reproductive cells that is passed on to later generations.

In the body's own cells, however, we have good reason to infer that more of the damage that becomes expressed is done by breaks of chromosomes and in the reproductive cells by mutations of genes.

Let me use the blackboard a moment.

Here is a cell, and there is its nucleus, and in it there are these long threadlike bodies which can become condensed into sausage-shaped forms, called chromosomes, that consist essentially of strings of minute particles called genes, each of which, as Dr. Glass explained to you, has some specific effect in furthering the functioning of the body.

Radiation has two main kinds of effects on this genetic material. Let me again say that every cell of the trillions of cells in the body has this genetic material the same as the reproductive cells, and radiation can affect the genetic material in the body cells the same as in the reproductive cells.

There are two main kinds of effects on the genetic material. One is a break in a chromosome. Two, a change in the inner composition or arrangement of parts of the gene itself.

The breaks in the chromosomes have more effect on the exposed individual himself when they happen in the somatic cells for this reason. If this cell in the diagram is one that is going to later reproduce itself in the body to form more cells, or to multiply, as our skin cells must—as skin is always sloughing off and has to be replenished by multiplication of the cells in the lower part of the skin, and it is the same with many other parts of the body—then these chromosomes have to reproduce themselves, as Dr. Glass explained, forming duplicates of themselves. Then they are drawn apart by the process of cell division or mitosis, forming 2 groups of chromosomes in the 2 daughter cells. But if the chromosome is broken, it may kill the whole cell at the time or shortly after division by this mechanism, or it may result in very abnormal cells.

There is a point on each chromosome, which becomes attached to a fiber that pulls the 1 daughter chromosome to 1 pole and the other to the other pole, so that when you get the cell dividing into 2 daughter cells, one daughter cell gets one representative and the other gets the other representative of that duplication process, and they thereby come

to have like material. The chromosome gets pulled at that particular point. But the part beyond the break won't be pulled in because it has nothing to be pulled by. It gets left behind, and the cell is left without all those genes.

Moreover, where the chromosomes have broken, there is a sort of stickiness which causes them to join together at their raw ends if they happen to touch, which is very often the case, and then, when they get pulled apart, they form what we call a bridge between the two cells. They can't, in that case, get apart. That chromosome bridge, then, connects the two cells, which often die in consequence. Therefore, as a result of the chromosome break, we often have a cell death.

Dr. Puck, of Denver, studying human cells in tissue cultures, has found that, with doses of radiation given at a high dose rate, it takes only about 90 r. to cause the death of about 50 percent of the cells. In other words, that happens to about 50 percent of the cells in the tissue culture. It will only happen to cells which attempt to undergo division.

Your brain cells, your kidney cells, as Dr. Warren said yesterday, are not affected much by radiation. On this interpretation the reason is easy to see, because they have stopped dividing for life. You have the same cells of these kinds for life, whereas your skin cells, the cells of the intestine, et cetera, have to keep replenishing themselves by division. It is only when they come to replenish themselves by division that these tangles take place that result from the breaks in the chromosomes.

On that basis it is easy to understand the rule that Dr. Warren referred to yesterday, sometimes known as the law of Bergonié and Tribondeau, established about 1906, that it is the tissues with rapidly dividing cells that are the most damaged by radiation. The damage does not take place in the cell until division occurs. When that happens in a germ cell, those cells generally die and seldom take part in the production of offspring. That is why this is not important for the later generations. But it is important for the generation itself. It takes place both in the germ cells and body cells, but it is in the body cells that the main damage of this kind is done.

I will give you an illustration of that. In the people that were bombed a long time ago at Hiroshima you can see a lot of little opaque points in many of them in the lens of the eye where evidently cells have been badly damaged. That damage did not appear until much later when the cell probably undertook to divide. The reason you can see it there is because the lens is a transparent tissue. I think there is reason to believe that this happens in all the tissues of the body that contain cells that are subject to division. These tissues would, therefore, be weakened.

It is true that other undamaged cells would tend to replenish the damaged places. But that replenishment or regeneration, it is to be expected, will not be complete and perfect. Therefore, there is a certain amount of damage left. That damage, you can see, would be a generalized damage all over the body, wherever there are these dividing tissues. Therefore, it would be expressed as a weakening of resistance to disease and infirmities of all kinds, somewhat like what occurs in aging, since then, also, our resistance to bodily ills decreases as the functioning of the cells or tissues weakens.

I do not say by any means that is the proved explanation, though we do know this phenomenon of chromosome breakage occurs, and, to me, having examined the evidence, it seems by far the most reasonable explanation. That is, the bodily damage in general is to be explained in this manner rather than by some damage to the other materials of the cells.

There is a good deal of other evidence showing that it is this damage to the genetic material, to chromosomes and genes, that is by far the most important damage, and that you have to have the radiation strike at or in the close neighborhood of the chromosome or gene before you can damage it. For example, experiments done in Chicago by Zirkle and Bloom show that when they were able to direct a minute beam of radiation (protons) through the cell, it was only when they actually hit the chromosome or right close to it that you then got chromosomal damage. If you hit right there, when the cell divided you had the bridges form.

Other experiments similarly show that if you only hit the rest of the protoplasm you do relatively minor damage compared to what happens when you hit the chromosomes or genes.

You can readily see, I think, that since we know through experiments in geentics that the frequency of these breaks, like the frequency of the mutations of the genes, is linearly proportional to the dose of radiation used, no matter in how small bits it is divided, then you might expect a derived effect, such as the decreased resistance to disease and consequent shortening of the life span also to be linearly proportional.

It is true that at high dose rates of radiation you sometimes have two chromosomal breaks near together and then you can get entanglements which would not happen if you have low dose rates. At low dose rates you therefore expect the effect to be proportional but at the high dose rates to go up even more steeply.

I think it is also very probable that even the very pronounced effects of heavy exposure, such as you find in radiation sickness, such as the nausea, the drastic lowering of the count of white blood cells, the bleeding internally, are also due to this genetic damage to the somatic cells.

For example, in a very careful investigation, Dr. Quastler showed that the intestinal injury that leads to death after high doses is caused by the failure on the part of the cells of the intestine that normally replenish the rest every 3 or 4 days to carry out this task. Owing to being so badly damaged, they fail to survive the process of cell division in normal condition.

These effects of heavy doses do have thresholds, not because there is a threshold in the production of chromosome breaks, but only because you do not see clinical symptoms unless you have damaged a certain number of cells.

If this point of view is correct, then I think it is to be expected that even the damage to the exposed individual could be better investigated by people who have the genetic point of view about it. They would be the ones who would be more likely to look for an effect that has no threshold. I think it is not at all surprising, therefore, in view of the probable mechanism of these effects, that it has taken geneticists, notably Boche, in the case of life span, and Lewis, in the case of the

effect of leukemia—both, by the way, *Drosophila* geneticists originally—to uncover the evidence for the cumulative nature of the damage to exposed individuals. Moreover, I think the research should be continued along those lines.

Doctor Friedell said yesterday that the important thing to know is the mechanism involved. I do not say we know it. But I think we have more than a pretty good hunch. And we have to follow these hunches, even if they lead to conclusions that are distasteful to some people, such as that there is no threshold. But if we calculate (as Dr. Hardin Jones will calculate with you) the amount of effect of a given dose on the length of life, we find that the dose which is now the maximum permissible dose for occupationally exposed workers, namely, 50 r per 10 years, would lead in 40 years of their work to 200 r. and would thereby deprive them—we may want to change this estimate; we can't now say exactly—of some 4 years of their life. Perhaps 1 year of their life lost for each 4 years they work, or something like that.

My main point here is that if there is no threshold, then the loss is of a sizable amount and we had better pretty soon find out how much. We meanwhile have to act on the supposition that it exists. Nevertheless, this effect on the length of life of the exposed generation is not as great as the effect in damaging future generations.

Representative HOLIFIELD. Why is that, Doctor? Will you explain that?

Dr. MULLER. Yes. Let me now speak of the damage to future generations. I said that not much of it was caused by the chromosome breaks but most of it by the mutations of the genes. The reason the mutations of the genes do not cause much damage usually to the exposed individual is because most of them are what we call recessive. There is a normal gene from the other parent that has a dominating effect, although it may very well be the case that in leukemia we have a dominant gene. But mutant genes that are decidedly dominant are not the usual ones.

As for later generations, the chromosome breakage cases are largely weeded out by the cells dying before they get to the next generation. The mutations of the genes, however, persist and are handed down as mutant genes. These mutant genes also are usually recessive, as Dr. Glass explained. The person usually gets a normal gene from the other parent and that has the dominating effect. But the dominance is not quite complete usually, and that slight deviation from completeness is very important.

Suppose it only reduces by 5 percent the chance of an individual surviving to maturity; that is a chance of death of 1 in 20. It does so by handicapping him in some way. It is usually a slight handicap that he hardly realizes is there. He takes it in his stride because he has had it perhaps from birth, though it may not have expressed itself always, and it is mixed in with his other infirmities. All of us have some. No one is perfect because there is no such thing. But by it his biological survivability is reduced by just that much, and he hands that weakness on down to the next generation, and after awhile it will take its toll by happening to come in a combination of circumstances where it will kill or prevent reproduction. So the thing will finally die out.

As Dr. Crow explained, if it gives a chance of 1 in 20 of causing death in 1 individual, it tends to pass down to 20 individuals before it takes its toll. So it hampers correspondingly more persons than a gene that killed outright. Therefore, as Dr. Crow explained, the slight mutation is as bad in the end as the mutation with the big effect. Moreover, we are all of us full of these defects that come from the past. A hundred things, each of which does a one-hundredth of as much harm, are together as bad as one thing that does that much harm.

Representative HOLIFIELD. Before you leave that, is it not true that if a general population receives a dose of radiation, as would occur in the general raising of the rate in the atmosphere, that there would be a greater chance for the mating of recessive genes?

Dr. MULLER. If both people had been exposed, no, the increase in the chance would be insignificant, contrary to a common misconception. Because the chance is so slight that it should have been the same gene that was effected in both parents. There are more than 10,000 genes, we believe, and if each parent had an affected gene, therefore, the chance would be only 1 in 10,000. So that is virtually ruled out.

Senator ANDERSON. May I go back one step? Did I understand you to say that if, for example, the exposure was 50 r. every 10 years, and a man worked for 40 years, making a total of 200 r., his life might be shortened as much as 1 year for every 4 years of work?

Dr. MULLER. No; I am sorry. I made a mistake. I means 1 for every 10. Dr. Hardin Jones will give you the latest information.

Senator ANDERSON. You said 1 for every 4.

Dr. MULLER. Yes. I was thinking of 4 times 10, and I got the 4 instead of 10.

Senator ANDERSON. So that would be 4 years shortening.

Dr. MULLER. Yes. That would be the provisional estimate I made a long time ago and we may have better data now. I think this is probably a conservative estimate.

Dr. JONES. A very reasonable estimate.

Senator ANDERSON. Thank you.

Dr. MULLER. These effects then, being slight, are usually not recognized as such. Certainly you could not tell which mutation was caused by radiation, if radiation had been received by the parent, and which was not. The induced mutations are like those already existing in the population but are added to them. The only way you can tell they have been produced is by means of a very exact statistical study on large groups, comparing those that have been irradiated and those that have not.

Through work on the fruitflies where we have the most exact knowledge to date, unless Dr. Russell has more exact knowledge on mice now, we can get a kind of minimum estimate of the amount of damage to the children by a given amount of irradition of the parents. Although there would not be time to show you here the way the calculations are done—and they do have a considerable error—I think it is possible to show that the amount of damage to the offspring of parents that had received a certain amount of radiation to their whole body, in the case of fruitflies, would be something like the amount of damage in the parents themselves.

A comparison, however, would show that, when, X-rays or gamma rays were used on fruitflies, the damage to the offspring would be somewhat less than to the parents. You may think that is contradicted

by what I said before when I said the damage to future generations is greater than to the parents. There is no real contradiction because we are here referring only to the first generation of offspring. But they hand the damage down to their offspring, and so on.

As Dr. Crow explained to you, the damage does not die out to half its value for probably scores of generations. So you have to multiply this damage to the first generation of offspring by scores, maybe by 50, to find out what the total damage to future generations is of just the 1 exposure to 1 generation. When you have done that you have obtained a figure of damage to future generations that is far greater than what is done to the exposed generation itself.

I think most people would not be impressed with the weaknesses caused in future generations, even though future generations would feel them. Therefore, it is my contention that it is a very good thing that people's own life span is shortened; that is, that there is a demonstrable effect on the generation itself that is exposed, because they will take notice of this effect on themselves, if they are allowed to know it. They will then take precautions that will save future generations from a lot more damage than it saves themselves.

The prolonged official reluctance, at least until a year or two ago, to give information in popular form regarding these major types of radiation damage, that in the exposed individual himself expressed in the shortening of life and long delayed malignancies and that expressed in the descendants, and the reluctance to give information regarding the conclusion of some of those who have worked most directly in the field that even the tiniest doses add up accurately to determine the amount of these effects without any threshold, has, I think, undermined the confidence of large numbers of well-intentioned people in the judgment and the intentions of the responsible governmental authorities, because the facts have, after all, leaked out or have been suspected by the public, and they wonder why nothing has been said about it.

As I said before the National Academy of Sciences 2 years ago:

> So many of the public are already aware of the genetic damage produced by radiation that their morale is weakened and their apprehensions are increased when they see that the damage is denied by prominent sponsors of our national defense. Thus the door is opened for their acceptance of the defeatist propaganda which alleges that even the tests are seriously undermining the biological integrity of mankind. In this situation the only defensible or effective course for our democratic society is to recognize the truth, to admit the damage, and to base our case for continuance of the tests on a weighing of the alternative consequences.

Now when we do this, we conclude that the number of lives that will be seriously curtailed or injured throughout the world in future generations, as a result of the tests already held—supposing that they continue at this rate for perhaps 10 years longer—is in all probability in the hundreds of thousands or millions, and is therefore enormous. We should recognize that.

Despite all the uncertainties in regard to the exact figures, I think it was not possible to make clear to you how much careful work these estimates were based on, and the fact that although there may be an error of what we call a factor of 2 or 3, that is, that the true figures may be 3 times as much or only a third as much, nevertheless it is very unlikely that they should be less than, I would say, a third as much. In other words, the values given to you by Dr. Crow are those which are most likely in the light of present knowledge.

So I think we must recognize that the number of lives that would be seriously curtailed or injured will be in the hundreds of thousands or millions, and is therefore, enormous.

Senator ANDERSON. Doctor, you say that a number of lives seriously curtailed or injured from tests already held?

Dr. MULLER. Yes. I should modify that. I should say from tests held at the rate at which they have been held.

Senator ANDERSON. As they are now going?

Dr. MULLER. Yes.

Senator ANDERSON. That does make some difference.

Dr. MULLER. Yes.

Senator ANDERSON. That would damage, you think, hundreds of thousands or perhaps millions, and you say that is within a probability factor of 3?

Dr. MULLER. Yes.

Senator ANDERSON. A third as much or three times as much?

Dr. MULLER. I would put it at that.

Senator ANDERSON. Then it would be a very substantial number of lives if it is on the smaller side and an enormous number of lives on the larger side.

Dr. MULLER. Yes.

Senator ANDERSON. Doctor, do you believe that a number of geneticists agree with you in that point of view?

Dr. MULLER. I do, yes. They might differ as to where to put the factor. Some might say 2. Some might say 5 or 6.

Senator ANDERSON. This would sort of imply that there is no threshold.

Dr. MULLER. Yes.

Senator ANDERSON. That it starts immediately.

Dr. MULLER. Yes. That is because of the mechanism that Dr. Pollard explained. The thing strikes or it doesn't strike. If it strikes it does the damage even if there was only that one strike. He made the comparison with a lightning strike occurring in a large space and a long time. in which however that strike would be just as effective.

Senator ANDERSON. I ask that question because of the discussion we had this morning of having some group to come in and discuss these figures with a responsible group such as the Commission itself. You do feel that the geneticists could make a strong case in support of that?

Dr. MULLER. Yes, especially those who have worked in the field.

Senator HICKENLOOPER. Dr. Muller, in your prepared statement I notice you said with respect to that, and I quote from the prepared statement as follows:

> As I stated before the National Academy of Sciences 2 years ago, it has caused—

that is, certain conceptions or misconceptions—

> people to lend too ready an ear to the alarmists who declare that the genetic material of the human race is seriously endangered by the fallout from the test explosions themselves.

I do not know whether you got to that or whether you intended to do it or not.

Dr. MULLER. No. I read a more detailed statement on the subject, taken from the discussion that I gave before the National Academy

of Sciences. I read a part of that discussion. But I also stand by the statement as I have it in the text that you have just read.

Senator HICKENLOOPER. With respect to these numbers which you refer to as hundreds of thousands or millions, do I understand the connotation of that to be that that in fact is extremely minute as compared to all the human beings for ensuing years?

Dr. MULLER. Yes.

Senator HICKENLOOPER. Although it is large in numerical value standing by itself.

Dr. MULLER. That comes in the next sentence, you will see, where it says:

Nevertheless these injuries, being scattered over the whole earth and through hundreds of years—

I should have said thousands—

are relatively very few, in comparison with those due to other causes, including natural mutations. Moreover, the suffering to be entailed, although enormous in absolute terms, must be very small relatively to that which might follow from any serious mistake in the conduct of international relations.

Senator HICKENLOOPER. Therefore, Doctor, it becomes a matter of relative value in this particular field.

Dr. MULLER. Yes.

Senator HICKENLOOPER. Some things we may have to do, such as getting into war where we do not want to kill people, whether by bullet or disease or anything else, but we have to balance security and necessity against the hazard and strike a balance as to our conduct.

Dr. MULLER. Yes. I do not mean that when we strike that balance—I am not trying here to say which way the balance will be struck.

Senator HICKENLOOPER. I understand. You are making the point that someone must exercise judgment and determination in the light of all the circumstances.

Dr. MULLER. Yes. I would agree with Doctor Sturtevant that one life is a serious matter.

Senator HICKENLOOPER. Yes, without doubt.

Representative HOLIFIELD. You are really making a plea that all the facts be known before the decisions on policy are made.

Dr. MULLER. Yes.

Representative HOLIFIELD. And no facts be repressed.

Dr. MULLER. Yes.

On the other hand, the consequences of a full-fledged war, with its heavy irradiation of large numbers of people on both sides, would be inordinately more serious in its effects on the human genetic heritage as well as in its more direct effects. It is this consideration which, in my opinion, makes a continuation of test explosions a monstrous mistake of policy for both sides. Of course it would be absurd to expect one side to stop without the other. But a continuance by both sides would tend to lead the world nearer to a war that even with present techniques would result in the cataclysmic ruination of humanity in general.

May I add that the means of destruction are now so advanced on both sides that further advances by one side alone could not save it in the case of a war from becoming itself destroyed. By war I mean an atomic war, because I do not think you can have a world war any-

more without a thermonuclear or atomic war. I do not think it is realistic to suppose that you can.

Senator HICKENLOOPER. This illustration, I realize, is not exactly on all fours with the situation we have here today, but we are concerned with the danger and propriety of continued tests in the world.

Dr. MULLER. Yes.

Senator HICKENLOOPER. I presume all of us would earnestly hope that we never had to test atomic weapons. That perhaps would be the ideal. There are certain political factors that enter into those decisions, but by the same token I presume that we want to save thousands of lives in this country every year and we could just abolish the manufacture of automobiles and go back to riding horses. It seems to have struck a balance in the minds of people that transportation is important and we keep making automobiles, people keep getting killed by the thousands on the highways every year. We are all sad about that.

The point I was attempting to understand in my own mind is that there is a balance which someone must determine as to the ultimate good to either us or the world in this atomic field and whether or not we continue.

Dr. MULLER. I would accept that, except that I would rather say, "which every one has to help determine."

Senator HICKENLOOPER. Yes, indeed.

Dr. MULLER. I might add in this connection that in my talk to the National Academy 2 years ago I stated it to be my belief at that time that a continuation of the nuclear tests was necessary. I still think that this was the case at that time but I think the situation has changed since then. But there I am not speaking as a geneticist.

Senator HICKENLOOPER. I do not know whether I understand or not, but may I ask you, are you advocating that the United States stop testing weapons unless we get reliable agreement that other nations in the world would stop also?

Dr. MULLER. Of course not; no. The more that we can get people of the world to recognize the terrific damage that nuclear war will bring to them all, I think the more they will see the light on that point.

Senator BRICKER. Are you going to discuss later, Doctor, the answer to the question I asked this morning in regard to the experimentation that is going on in Russia?

Dr. MULLER. Yes. Just a little more here—I am on the last page on a somewhat different topic, but it is all related.

In order that the grave biological effects of radiation may receive due recognition and study—and I have tried to show you that they have not received due recognition and study—and may duly influence our policies and procedures, it is important that persons with a systematic background in genetics be placed in positions in which the decisions involving these matters are made. Truly this should be the case if there is any chance that genetic processes lie behind all the major damage done by radiation to man. Yet this is not the case at present.

For example—and let me not be misunderstood here—I have the highest regard for Dr. Shields Warren and for Dr. Brues and their associates, but I think it is important in this connection to point out that the official delegates of our country on the United Nations Scien-

tific Committee on the Effects of Atomic Radiation—and those are the gentlemen I have just mentioned—are neither of them geneticists. I am sure neither of them would wish to claim to be. I mean by that they would disclaim it.

Representative COLE. On that point, Doctor, do you know whether other members or official delegates from other countries on this committee are geneticists?

Dr. MULLER. There is one geneticist of bacteria, Dr. Appleyard of Canada. There is Dr. Caspersson from Sweden who studies chromosomes and other cell materials through the microscope. He is not exactly a geneticist. There is an alternate, Dr. Gopal Ayengar, from India who is a geneticist. Dr. Bacq of Belgium is in fields related to genetics.

So far as I know, those are the only ones that come near the subject of genetics who are on that committee. I stand to be corrected. They can, of course, and have, at least at the last meeting, I understand, had some geneticists present as consultants. Yet the chief discussions of that committee to date, so I have been told, have been on directly genetic matters, on these very questions here, especially on the effects on future generations.

You may remember that this country insisted that the delegates be chosen not by sciences but by countries and that the delegates by countries be chosen by the government and not by the scientific bodies. Some other countries put up strong resistance against that, but finally accepted it.

It may be noticed that most of the nongeneticists who deal with these matters, as I think is clear from the discussions of yesterday, are on the same side. They are on the other side from geneticists in regard to the major question of whether there is a threshold or whether the major effects on the body are linearly proportional to the dose all the way down to zero. That is an important issue in assessing the effects not only of the tests, but also of the peacetime uses of atomic energy.

I might say that when it comes to another body that is very important in this connection, the National Committee on Radiation Protection, we are in a better situation. Dr. Glass here, a very good geneticist, if I may say so, is a member of that committee, who was newly appointed just a few days ago. I was a member of one of the subcommittees for some years, although I am not sure whether I still am or not.

Representative COLE. Mr. Chairman, may I clarify the record? I understand Dr. Glass had been a member of this committee for 3 years.

Dr. MULLER. No, that is another committee. That is the Atomic Energy Advisory Committee on Biology and Medicine. I am now speaking of the National Committee on Radiation Protection which is under the auspices of the National Bureau of Standards and which is the one promulgating the permissible dose which we had explained to us the other day on the blackboard.

The latter committee do have, as I have said, a geneticist here and there. However, they have official representation from about 15 different organizations, mostly of a medical or governmental nature. Yet they do not have one official representative from any of the professional genetic organizations, such as the Genetics Society of America,

the American Society of Human Genetics, the American Genetic Association, or the Society for the Study of Evolution, all of which are in my opinion as closely concerned with this matter as for example the Radiological Society. Consequently, there is not sufficient representation among them of that genetic point of view of the mechanism which leads us to expect no threshold and to take the matter more seriously at small doses. This circumstance, I think, provides the reason why the record of this committee's decisions on the permissible dose, which as Dr. Taylor presented the matter yesterday appeared to show that they were so cautious, in actuality showed that the first dose they set was far too high, so that they had to set it lower. Then they found the second limit also was far too high and again they came down. And recently they found that the third limit in turn was too high and they came down once more. This does not indicate that they have been so cautious. It means they have not been cautious enough. The geneticists would not have set so high a permissible dose in the first place, on the basis of what we knew 30 years ago.

The grounds for the reduction in permissible dose that was made by the committee a few years ago, prior to the issuance of the National Academy's report, did not lie in considerations of genetic damage. For the permissible dose handbook specifically stated that this dose (of 0.3 roentgens per week) was set without regard to genetic effects. The geneticist members objected to that but it was carried anyway. In other words, it was known that the dose was considered too high on genetic grounds but it was adopted in spite of this, although it was acknowledged that it might be reduced again later.

Now that the Academy has made its report it has in fact been reduced a great deal more. I agree however with Dr. Glass, that it is probably due for even further reduction.

A similar attitude is reflected in the omission of any mention of the genetic effects of radiation in the courses on radiation in relation to health that are given both for our own people and for foreign selectees under AEC auspices at Oak Ridge. It is not enough to have biologists of some sort, or medical men, to reach decisions on these matters, unless they include a strong contingent of geneticists and of those who have a genetic point of view. Others are not likely to admit the danger from small doses.

In view of this situation and of the notorious resistance to the acceptance of genetic principles on the part of so many, not only of governmental appointees in the policymaking positions, but also of so many of the medical profession, a resistance that has prevented the medical profession for 30 years from duly protecting themselves, their technicians, and their patients when X-rays are used medically, and that has thereby subjected the reproductive cells of our population to very much more radiation than that from fallout—it is my opinion highly important that a National Radiation Health Institute be established as a part of the United States National Institutes of Health, but only if it contains a solid core of competent and versatile geneticists as one of its major features.

It is true that there is excellent research on the genetic and other effects of radiation being carried out in our country and that a considerable amount of it is made possible by the support or is done under the auspices of the AEC. This research, however, does not sufficiently insure the all-around consideration and study of these matters in rela-

tion to public health and well-being, and the promotion of adequate measures in application of the conclusions reached.

If anyone wishes to ask some specific questions on the Russian aspect of the situation, I would be glad to take them up.

Senator BRICKER. I just wanted him to discuss the situation and whether they are conscious of the conclusions you have come to and are presenting to us and what research is being done there with regard to the effects of fallout.

Dr. MULLER. I have to infer from my knowledge of Russia derived from various sources, including firsthand information gained 20 years ago, that they are and will be having to follow our lead. We can't look to them for useful information at present along these lines. For, as I think most people realize, there was a purge of geneticists and an expurgation of the subject of genetics from teaching in the school and universities, from the boards of publication of journals, and from research institutes. It has not been taught to students for about 20 years. Most of the leading geneticists were somehow done to death, and I say this advisedly.

Chairman DURHAM. You say they will have to follow us. Do you think they would follow us?

Dr. MULLER. Yes.

Chairman DURHAM. Do you think so?

Dr. MULLER. Yes. There are a few geneticists left, of course. I think that some of the politicians in leading positions, since Stalin died, realize the folly of their old ways in regard to the subject of genetics. We have solid evidence that it is now possible to advocate the principles of genetics and to do some research in it.

A few of the old research workers are left, and there are said to be plans to give them positions in which they can resume their genetic investigations. However, the quacks have not by any means been disestablished yet, although they do not hold as commanding positions as they had before. One of the older geneticists, Dubinin (he was not old 20 years ago), was even rumored to have been selected as one of their delegates on the U. N. Scientific Committee on the Effects of Atomic Radiation. If so, Russia did better than most of the other countries in regard to that committee because Dubinin was a real "honest-to-goodness" geneticist.

Chairman DURHAM. You do think that Government officials are getting advice on genetics?

Dr. MULLER. They are beginning to get advice on genetics again. But I also note that the quack group are still strong. We have proof of that in publications and in the fact that in the Conference on Genetics that was held in Japan last September the Russians sent, I believe, four delegates, and all of them belonged to this quack school. So there is a division on the matter in Russia now.

Chairman DURHAM. What do you think is the reason for the Russians not permitting the genetic scientists to take part in the United Nations?

Dr. MULLER. The Russian, Dubinin, is a geneticist. Most of the other countries didn't seem to realize that geneticists were needed, for this committee seemed to assume that physicians would know about the subject. However, you will find very few physicians in this country that have an education amounting to anything in genetics.

Chairman DURHAM. Did I misunderstand you? I thought you said they would not permit him to go to the United Nations panel.

Dr. MULLER. No. I said I had heard, but I cannot verify, that Dubinin was to go to that meeting as an official Russian delegate. Whether he went or not I do not know. Perhaps someone here knows. I would be interested to know if he did.

Representative HOLIFIELD. Dr. Muller, you have spoken in two instances in your presentation here of the prolonged official reluctance and the curious official silence. Is it not true that you were invited to give a paper at the Geneva Conference a couple of years ago?

Dr. MULLER. Yes.

Representative HOLIFIELD. The Geneva Conference on Atomic Energy?

Dr. MULLER. Yes.

Representative HOLIFIELD. Did you give it?

Dr. MULLER. It was printed in the proceedings.

Representative HOLIFIELD. That is not quite an answer to my question.

Dr. MULLER. No, I did not give it there. I was prevented from giving it.

Representative HOLIFIELD. Who prevented you from giving it?

Dr. MULLER. The story has some complications of detail, but the essential thing is that it was called off by higher echelons of the AEC. That was actually shortly after this article of mine had appeared in Science that I quoted from earlier, in which I said there had not been enough airing of the matter.

Representative HOLIFIELD. Were you given any reasons as to why you were not allowed to give your paper?

Dr. MULLER. Yes; there was not room for me. Also they were sorry they had to notify me so late because they had only just received word so late from the International Committee. It was afterward found that it was not the International Conmmittee that had asked to have my paper excluded. They had approved of having it given. That expression was used, however, in the official letter written to me by the AEC authority.

Representative HOLIFIELD. Are there any further questions?

Representative COLE. Mr. Chairman, it had been my understanding that the committee was going to allow our staff specialists in this field to interrogate the witness in any area in which they felt there was some need for further amplification. I would like to inquire if Mr. Hollister might not have some questions of Dr. Muller. I would suggest that hereafter Mr. Hollister would be invited to interrogate. He is reluctant to inject himself into the interrogation.

Representative HOLIFIELD. The Chair has informed Dr. Tompkins and Mr. Hollister that they have the privilege of touching me on the shoulder and asking questions of any witness. They certainly do have that privilege. It was announced at the beginning of the meeting, and we certainly intend to allow them that privilege.

Mr. Hollister, would you like to ask some questions of Dr. Muller before he leaves the stand?

Representative PRICE. Before he does, is Dr. Muller's paper before the National Academy of Sciences included as part of the record?

Representative HOLIFIELD. Dr. Muller, Mr. Price's question was,

have you presented your paper that you were to give at the Geneva Conference as part of the record of your presentation?

Dr. MULLER. I had not intended to do so. I would like to present something more recent—two things in fact—one short paper, Potential Hazards of Radiation, which is now in press in the journal Excerpta Medica published in Amsterdam. It is six typewritten pages with some references.

Then a little statement that I gave out last October 20, 1956, that I think I could read. It is very short.

So devastating would be the damage done to both present and future generations by the nuclear explosions which any global war is likely to bring, that the great issue of today is not that of the relatively minor damage produced by mere tests of H-bombs, but that of taking all steps we safely can, such as the mutual discontinuance of these tests, which will tend to lessen international tensions and bring us nearer to all-round armament control. Unless this control is achieved in the short time open to us before thermonuclear weapons have become available to more countries still, and before intercontinental missiles have become a reality, we will find ourselves in a situation even more ungovernable and menacing than that of today.

I also wish to introduce a paper in the report by the World Health Organization study group on the effect of radiation on human genetics (see appendix, p. 1728). This report of the World Health Organization is being presented to the U. N. Committee on Radiation Damage and will be published next month sometime, it is expected. I have a copy of the paper of mine of that report here to include.

Representative HOLIFIELD. Thank you very much. Without objection they will be received.

(The document referred to, together with an article entitled "How Radiation Changes the Genetic Constitution" by Dr. Muller, follow:)

POTENTIAL HAZARDS OF RADIATION [1]

(By H. J. Muller, Indiana University)

Evidence has in recent years been accumulating for the broad conclusion that the great majority, if not all, of the damaging effects on life and health evoked by ionizing radiation are results of permanent changes produced in the genetic material: the chromosomes or their contained genes. These changes, when occurring in the genetic material of the germ cells, reach expression through reduction in the number of functional germ cells (infertility), increased mortality of zygotes of the first and subsequent generations (dominant lethals and detrimentals), and, in general, the alteration of one or more hereditary characteristics, that thereafter are transmitted in their new form (mutations).

Our knowledge of the nature of these effects, and of their manner of production, has been derived in the first place from studies of the descendants of exposed individuals, checked by cytological observations of cells derived from exposed germ cells. However, there is increasing ground for the inference that ionising radiation produces changes in the genetic material of the somatic cells like those in the germ cells, and that it is these chromosomal and gene changes in the somatic cells that form the basis of most of the damage to the exposed individual himself, such as erythema and the various other aspects of radiation sickness, shortening of the life span, diverse malignancies, etc.

The primary changes in the genetic material, as disclosed by cytogenetic investigations on widely different forms of life ranging from viruses and bacteria to fungi, flowering plants, insects and mammals, may for convenience be divided into two groups. These are the *chromosome breaks*, that result in fragments the broken ends of which tend to unite with one another either in the old or in new arrangements, and the *point mutations*, that involve alterations at very localized positions on the chromosomes and are inherited according to Mendelian principles. It would take us too far afield here to discuss the proposi-

[1] Slightly modified version of article in press in June 1957 issue of Excerpta Medica (Amsterdam).

tions that there may be a fundamental similarity between the changes of these two groups, that they may integrade with and even overlap one another, and that, at any rate, the classification into one or the other group is in many cases uncertain. However that may be, the distinction remains of practical importance.

It is the point mutations which, although the more elusive of the two types of genetic changes, do the most harm in the long run. Even though evolution has come about by the natural multiplication of the very infrequent advantageous point mutations, the vast majority of them, whether arising naturally or induced by radiation, are of a detrimental nature, as is only to be expected of "blind" changes. Yet, contrary to popular opinion, mutations giving rise to conspicuous abnormalities, monstrosities, or freaks, are a great rarity, even after heavy doses of radiation. The point mutations induced in animal material by ionizing radiation have been found to be similar in their range of types and in the relative frequencies of those having different kinds of visible expression ("phenotype") to the naturally arising mutations, although the distribution of relative frequencies from gene to gene may be somewhat different for the two groups.

In considering the expression of a mutant gene we must distinguish between that which it has when homozygous, i. e. when inherited from both parents alike, and when heterozygous, i. e. from but one parent (the other having supplied a normal gene). Many mutant genes (possibly as many as one-fifth, as indicated by work on fruit flies) have such drastic effects when homozygous as to unconditionally kill the individual prior to maturity: these are the "lethals." The great majority of the remainder, when homozygous, cause some degree of impairment, even though relatively few of them give rise to readily visible abnormalities. In heterozygous condition their expression is usually much less pronounced than in homozygous condition and is very seldom recognizable; hence most mutant genes are termed "recessive." Yet even when heterozygous there is usually some slight, statistically important impairment of the capacity to live and reproduce. Hence, since the mutant gene is regularly transmitted to subsequent generations, it nearly always results, eventually, in the extinction (genetic death) of the line of descent carrying it, and this usually happens before the gene has had an opportunity to become homozygous.

These "genetic deaths" are seldom identifiable as such because the heterozygous individuals that suffer them show so little recognizable impairment. They represent the price paid by any population in preventing an unlimited accumulation of mutant genes within it. Studies on the frequencies of natural mutations in Drosophila, the mouse, and man, and of the effects of inbreeding in man, agree in indicating that each person carries on the average, mainly in heterozygous condition, at least four times as many mutant genes as would have been enough to kill him outright if they had been homozygous. It is further indicated that, in the mainly heterozygous condition in which the mutant genes actually occur, they tend, by their cumulative action, to cause the genetic death of at least 1 person in 5.

When the mutation rate is raised by exposure to radiation the frequency of genetic death is correspondingly raised, over a number of generations that is inversely proportional to the degree to which the mutational damage is expressed in the individual's of any given generation. Russell's studies on the mutations of 7 genes in mice show that some 30 r. delivered to the immature germ cells constitutes the "doubling dose," in that it induces in them as many mutations as arise naturally per generation. It is unlikely that in man the doubling dose is more than twice as high, and it may even be somewhat lower than in the mouse. At any rate, the correctness for man of this order of magnitude is indicated by studies of Turpin and Lejeune and of Macht and Lawrence, and is not contradicted by the lack of statistically significant findings in the studies made in Japan by Neel and Schull.

Since there is much evidence indicating a linear relation between the radiation dose and the frequency of the induced point mutations, even at extremely low doses, and the exactly cumulative nature of these radiation effects, it becomes possible to arrive at probable estimates of the minimum damage done to subsequent generations by any given chronic or acute exposure of parents. In view of the dearth of conspicuous abnormalities, one of the best overall measures of

this damage lies in a measurement of mortality, expressed for instance as length of life. The criterion has recently been used by Russell in his demonstration that neutrons applied to male mice cause a shortening of the average life-span of their progency that is about as great as that caused in the directly exposed individuals: nearly 0.1 percent of the life-span per rep. Since, however, the effects on the progeny are likely to reappear throughout scores of generations before being terminated by means of genetic deaths, the total damage to the descendants is many times greater than to the exposed individuals.

Chromosome breaks, like point mutations, are induced with a frequency directly proportional to the total dose of radiation, regardless of how concentrated or dispersed the treatment has been. Some of the rearrangements of chromosome parts result from single breaks, and some from two or more breaks lying in the course of the same ionizing particle. The frequency of either of these types of rearrangements, like that of the individual breaks, varies directly as the total dose, regardless of its distribution in time. Other rearrangements, that result from a combination of fragments derived from independent breaks occurring within the same few minutes, have in consequence of this mode of origin a frequency that is insignificant when the radiation has been of low intensity but that rises rapidly with the intensity, approximately as its square, and that becomes of major importance at high dose rates. Thus chronic or repeated low exposures give rearrangements that are linearly related in frequency to the total accumulated dose, while higher dose-rates give disproportionately numerous effects.

A cell in which one or more chromosomes have been structurally changed by breakage will continue to function normally until cell division occurs. At that time the altered chromosomes often give rise to chromatin bridges that connect the daughter nuclei, interfere with their further multiplication, and ultimately result in death of the affected cell line. In the absence of a bridge, the daughter or descendant cells may come to lack parts of chromosomes and/or to have other parts in excess, and the resulting unbalance of gene proportions ("aneuploidy") tends to impair and even kill the descendant cells. Happening in the germinal line, these phenomena are expressed as infertility of the exposed individuals, and as lethality among embryos of later generations.

The chromosome damage, leading to postmitotic cell impairment and death, is also induced in somatic (body) cells. It provides an interpretation of such phenomena as the "law of Bergonie and Tribondeau" (relating the degree of tissue damage to multiplicational activity), the radiation death of individual somatic cells in tissue cultures (as in work of Puck and Marcus), and the delayed production of minute cataracts in irradiated lenses of the eyes. That it is the chromosomes rather than the protoplasm of cells which are ordinarily the seat of the more significant radiation changes, leading to cell death, has been shown in numerous studies, among them those of A. R. Whiting, Zirkle and Bloom, and Ulrich. It is only when a given number of cells has been destroyed within a given space and time that certain visible symptoms appear, such as reddening of the skin, reduction in number of white blood cells, intestinal hemorrhage, etc. However, there is no threshold for the individual cell effects, and analyses such as that of Quastler are increasingly implicating them as the basis of the clinical manifestations.

It is almost certainly through the individual cell deaths and impairments that minute doses of radiation, long continued or repeated, exert their action in shortening the life-span of the exposed individual. This effect, first analyzed by Boche and then by Sacher, has been calculated to cause a reduction in length of life of the order of several days for every roentgen unit received by the body as a whole during a person's lifetime.

On the other hand, leukemia and some other malignancies, the induction of which may also be linearly dependent upon radiation dose, are considered by geneticists as being more probably results of point mutations in somatic cells than of chromosome breaks. From the conclusions of Lewis it may be calculated that for a population of 160,000,000 with a lifespan like that in the United States each absorbed roentgen of whole-body radiation would result in some 10,000 cases of leukemia during their lifetime, while one-tenth the "maximum permissible dose" of strontium 90 would result in some 55,000 cases.

The present population of the United States has been reckoned to receive, on the average, some 5 r. of radiation to the gonads from medical exposures alone before the age of 30 (see Laughlin and Pullman) but the amount from all diagnoses and treatments may well be double this (see Schubert and Lapp). About 3 r. are received from the natural background radiation. The amount

from atomic test fallout is as yet much less, and is said to be of the order of .1 r., although atomic warfare, or insufficient precautions in the peacetime use of atomic energy, could raise it enormously. The present exposure of Western populations, caused largely by fluoroscopy and by roentgenograms of the lower trunk, is not enough to cause concern in regard to shortening of life of the exposed generation, but its effect on future generations must be a good deal greater. The same consideration applies to occupational exposures. It has led to the recent recommendations for intensification of radiation precautions promulgated by the National and International Committees on Radiation Protection, and the committees on the genetic effects of radiation of the National Academy of Sciences (United States), the Medical Research Council (Great Britain), the World Health Organization, and the United Nations. It has recently been reflected in increasing activity in this direction in medical and dental circles.

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How Radiation Changes the Genetic Constitution

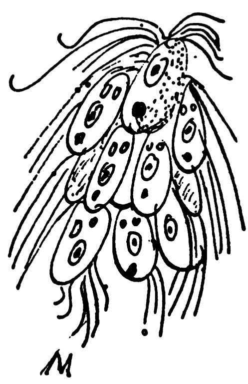
H. J. Muller

Dr. Muller, professor of genetics at Indiana University, prepared the following paper for presentation at the United Nations Conference on the Peacetime Uses of Atomic Energy, at Geneva. Although the paper was not given orally, it will appear in the published proceedings of the Conference.

The changes in the genetic constitution produced by ionizing radiation may for convenience be classified into two major groups: *chromosome aberrations* and *point mutations*.

The chromosome aberrations consist of losses and additions of whole chromosomes or chromosome parts and/or alterations, called structural changes, in the alinement of chromosome parts. Structural changes are caused by the breakage of one or more chromosomes at two or more points, followed by the junction of the fragments at their broken ends, so as to form a new arrangement; that is, a new linear sequence of their component hereditary particles or genes.[1]

Point mutations are changes confined to regions of the chromosomes so small that no loss or addition or change in arrangement of genes can be demonstrated by microscopic examinations or breeding tests. Since structural changes range from "gross" to those so minute as to be at the limit of being detectable as such, there are doubtless other cases of substantially the same kind, but below that limit of size, which become included among the point mutations. However, there is reason to infer that many of the point mutations produced in animals by radiation are not of this kind, but involve changes within the individual genes, and are therefore to be considered as "gene mutations." By this it is meant that these changes are restricted to genetic elements too small to be divided either by the process of normal hereditary recombination (crossing over) or by that of gross structural change. This seems to be true also of the great majority of genetic differences that exist naturally between individuals of the same species; that is, they appear to have arisen as gene mutations.



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[1] H. J. Muller, J. Genet, 40: 1-66 (1940).

CHROMOSOME ABERRATIONS

The chromosome aberrations produced by radiation in the cells of somatic tissues that replenish themselves by proliferation cause necrosis in much of the tissue descended from these cells and abnormality in much of the surviving descendant tissue. This constitutes a major source of delayed radiation damage, some of it never repaired, in the exposed individual himself. The same series of events, occurring among the immature germ cells of the exposed individual, can result in his or her partial or complete sterility. Among mature and nearly mature germ cells, especially spermatozoa, there is a much higher incidence of induction of these chromosome changes, for any given dose of radiation, than among immature germ cells or somatic cells.[2] Recent evidence[3] confirms the inference[1] that this peculiarity depends upon the chromosomes being in a condensed (tightly spiralized) condition and that it therefore applies also to cells that are in mitosis at the time of irradiation.

Mature sperm or eggs in which chromosome aberrations (actual or potential) have been induced, function in fertilization, but many of the resulting embryos die in consequence of their abnormal chromosome content. Other embryos, in which there has been gross structural change without excess or deficiency of chromosome parts, develop into normal adult individuals. However, when these seeming normals reproduce, recombination occurs between the structurally changed chromosomes derived from one parent and their normal homologues derived from the other parent. In consequence of the nonmatching linear arrangement of the genes from the two parents, about 50 percent of the germ cells now produced have excesses and/or deficiences of chromosome content. These germ cells usually function, but give rise to embryos (of the second generation after exposure) which die in utero at an early stage.[4] This mortality of embryos tends to be repeated over an indefinitely long series of generations. For half of the surviving embryos of such a line of descent, although not themselves containing the lethal excesses or deficiencies, have the grossly changed linear arrangement of genes that, by recombination, again gives rise to these effects.

In modern human populations, there is a tendency to compensate or even overcompensate for reductions in the frequency of viable births, by purposely increasing the number of pregnancies.[5] Hence damage of this kind, once induced, does not tend to die out rapidly but may even spread.

Fortunately there are several factors which serve to limit the frequency with which these cases of inherited abortions are produced. One is the fact that the period spent by male germ cells in a mature or nearly mature state averages, at the very most, a few months, whereas they usually spend some 25 years or more—well over 100 times as long—as immature germ cells, relatively insusceptible to the induction of chromosome aberrations. Although the relative lengths of the corresponding periods for female germ cells are not well established, the germ cells are, even when nearly mature, much less susceptible than spermatozoa to the induction of the gross aberrations that cause inherited abortion.[6] It may be concluded that more than 99 percent of the germ cells which function after a given exposure of limited duration (comprising only a few days or weeks) were at the time of that exposure in an immature stage, relatively insusceptible to the induction of chromosome aberrations. In them, aberrations of all kinds were induced with far lower frequency than point mutations.

Even in the less than 1 percent of germ cells that are exposed to radiation of beta or gamma type during their susceptible stage, gross structural changes of chromosomes will be produced at a low frequency, relatively to point mutations, unless the total dose of radiation received in that period is fairly high, of the order of a hundred or more r. (roentgen) units. This is because the

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[1] See footnote, p. 1069.

[2] However, some immature germ cell stages are much more susceptible to chromosome alteration than they appear to be when judged by the frequency with which such alterations are found later, on analysis of offspring derived from the cells that had been exposed while immature. This is because the descendant cells derived from those immature germ cells in which the chromosomes had been altered so often die out, and have their places taken by compensatory multiplication of descendant cells derived from those immature germ cells in which the chromosomes had not been altered. This consideration does not apply in the case of point mutations. (Note added October 5, 1955.)

[3] I. I. Oster, Excerpta Medica (8th Internat. Cong. for Cell Biol. No. 8: 406 (1954).

[4] P. Hertwig, Z. indukt. Abstammungs-u. Vererbungslehre 79: 1–27 (1940). P. C. Koller, Genetics 29: 247–63 (1944). G. D. Snell, Amer. Naturalist 68: 178 (1934).

[5] B. Glass, Amer. J. of Human Genetics 2: 269–78 (1950).

[6] G. D. Snell and F. B. Ames, Am. J. Roentgenol. Radium Therapy 41: 248–55 (1939).

production of these aberrations requires at least two chromosome breaks, and these are usually produced independently, by the tracks of different fast particles. On account of being in this sense double or multiple events, these aberrations vary in their frequency according to an exponent of the dose of radiation higher than one (commonly, about 1.5).[1] On the other hand, the point mutations vary as single events, according to the dose itself. Thus, as the dose is diminished, they do not drop off as fast as the structural changes do, and the latter become rare, relatively to the former.

It follows from the above considerations that inherited abortion caused by structural change is a relatively insignificant danger even in the case of a large dose of beta or gamma radiation that has been received in small fractions of not more than a few r. per month. If the amount received in any month is higher than this, however, measures should be taken to avoid this damage. These measures would consist in the prevention or avoidance of conception until the passage of several months after the high exposure. With a very high dose, however, all but the first month of this period would be sterile anyway.

When the exposure has been to alpha or neutron radiation, the production of gross structural changes tends to vary with the dose itself instead of with a higher exponent.[7] This is because both the breaks participating in such an aberration are usually produced by activations arising from the track of the same fast atomic nucleus. In consequence, both of this proportionality of the frequency of structural change to dose with this type of radiation, and of the fact that the more densely crowded activations from such radiation are actually more efficient in breaking the chromosomes, much lower doses, in reps, of neutrons or alpha rays than of gamma, beta, or X-rays give significant numbers of structural changes. Hence, the rule of not reproducing within some months after exposure should be applied in this case of much lower doses when the radiation has been of these types. In order to gage how low this limit should be placed, it should be taken into consideration that even 5 reps of neutrons, applied to spermatozoa, may be estimated to induce inherited abortion, based on gross structural change in some 1 to 6 among every thousand viable individuals derived from these spermatozoa.[8]

The frequency of natural occurrence of gross structural changes giving inherited abortion has not been studied extensively in mammalian populations, but it is known to be low. The highest recorded figure[9] is about 6 percent. Among offspring from spermatozoa treated with 500 r. X-rays, 25 percent have been reported by 2 observers.[10]

CHARACTERISTICS OF NATURAL POINT MUTATIONS

Among the genetic changes induced by exposure to radiation from artificial sources the point mutations are far more frequent and significant that the chromosome aberrations. Among the genetic changes that arise from natural causes (those somewhat misleadingly referred to as "spontaneous") the point mutations are still more frequent and important, as compared with the chromosome aberrations. Any ordinary population contains a large accumulation, or "load," of these natural point mutations, which have arisen in the course of many past generations. If any new point mutations are induced by radiation these are added to this already existing load of mutations. They thereupon become lost to view among the latter, in the sense that, with rare exceptions, the origin of any individual point mutation cannot be traced to the radiation. Thus, in order that radiation mutations may be viewed in due perspective, certain salient facts about the natural mutations should first be passed in review.

Natural point mutations occur sporadically. They are not individually controllable. Any such mutation may be thought of as resulting from an accidental ultramicroscopic encounter between a gene and some atom group, particle, or photon to which the gene happens, under the circumstances, to be vulnerable. It is probable that, on occasion, instead of the original or "mother-gene" becoming

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[1] See footnote, p. 1069.

[7] A. Catsch, O. Peter, and P. Welt, Naturwissenschaften, 32: 230–31 (1944). N. H. Giles, Jr., Proc. Natl. Acad. Sci. U. S. 26: 567–75 (1940). N. H. Giles, Jr., Genetics 28: 398–418 (1943). H. J. Muller, Amer. Naturalist 88: 437–59 (1954). H. J. Muller and J. I. Valencia, Records Genet. Soc. Am. 20: 115–16 (1951); Genetics 36: 567–68 (1951).

[8] G. D. Snell, Proc. Natl. Acad. Sci. U. S. 25: 11–14 (1939).

[9] P. Hertwig, Biol. Zentr. 58: 273–301 (1938). P. Hertwig, Z. indukt. Abstammungs-u. Vererbungslehre 79: 1–27 (1940).

[10] W. L. Russell, "Genetic Effects of Radiation in Mammals," Radiation Biology (edit. by A. Hollaender), McGraw-Hill Co., New York, Vol. 1, Pt. II, Chapt. 12, pp. 825–59 (1954).

altered, the accident causes a misstep in the construction of the "daughter-gene," but the effect is much the same as if the old gene had itselm mutated. In either case, point-mutational changes are permanent. This implies that the changed gene tends to be very stable, as the original gene was, and that in reproducing it continues to give rise to daughter genes like itself, that is, in this case, of the new type. Thus it "copies itself" through an indefinite succession of generations.[11]

The frequency of mutations in general is influenced, however, by many conditions. Thus, cells in certain developmental stages have mutations occurring more frequently in them, in other stages less frequently.[12] There is some evidence that markedly detrimental disturbances in the cellular biochemistry, of whatever nature, tend to favor the occurrence of mutations, while the functioning of the cell within its normal range is associated with a low mutation frequency. Certain special substances, such as the mustard gas series, some organic peroxides and expoxides, and triazine are so conducive to mutation that they have been termed "mutagens."[13] Some of them can in fact be used to induce mutations at about as high a frequency as with radiation. When the distribution of relative frequencies of the different types of mutations induced by one mutagenic agent is compared with that induced by another, or with that of spontaneous mutations, considerable differences are often found, even though most types of mutations produced by one agent are also produced to some extent by any other, and also arise spontaneously but at a lower rate.[14]

The partial selectivity of action of mutagens does not give evidence of being of such a nature as to result in the mutations produced by a given agent, or under given conditions, being better adapted, as a group, for life in the presence of that agent, or under those conditions, than are the mutations which arise under other circumstances. That is, mutations arising independently of radiation like those produced by radiation are, so far as the organism is concerned, accidents, not adaptive responses. There is evidence indicating that the organism has, through a long period of evolution, been selected for the maintenance of biochemical operations which give it as low a frequency of "natural" mutations as can practicably be attained just as it has been also selected to react in such ways as to minimize the occurrence of other accidents.[15]

It is entirely in line with the accidental nature of natural mutations that extensive tests have agreed in showing the vast majority of them to be detrimental to the organism in its job of surviving and reproducing, just as changes accidentally introduced into any artificial mechanism are predominantly harmful to its useful operation.[11] According to the conception of evolution based on the studies of modern genetics, the whole organism has its basis in its genes. Of these there are thousands of different kinds, interacting with great nicety in the production and maintenance of the complicated organization of the given type of organism. Accordingly, by the mutation of one of these genes or another, in one way or another, any component structure or function, and in many cases combinations of these components, may become diversely altered. Yet in all except very rare cases the change will be disadvantageous, involving an impairment of function.

It is nevertheless to be inferred that all the superbly interadapted genes of any present-day organism arose through just this process of accidental natural mutation. This could take place only because of the Darwinian principle of natural selection, applying to the genes. That is, on the rare occasions when an accidental mutation did happen to effect an advantageous change, the resultant individual, just because it was aided by that mutation, tended to multiply

[11] H. J. Muller, Second Internat. Cong. Eugenics, N. Y., Abstracts, p. 7–8 (1921). H. J. Muller, Genetics 3: 442–99 (1918). H. J. Muller, J. Exp. Zool. 31: 443–73 (1920). H. J. Muller, Amer. Naturalist 56: 32–50 (1922). H. J. Muller, "Mutation," Eugenics, Genetics, and the Family, Williams and Wilkins, Baltimore, Vol. I, pp. 106–12 (1923). H. J. Muller, Genetics 13: 279–357 (1928).

[12] H. J. Muller, Yrbk. Amer. Philos. Soc. for 1945: 150–53 (1946). H. J. Muller, Genetics 31: 225 (1946). H. J. Muller, unpublished data.

[13] C. Auerbach, Cold Spring Harbor Symposia Quant. Biol. 16: 199–213 (1952). C. Auerbach and J. M. Robson, Nature 157: 302 (1946). M. J. Bird, J. Genetics 50: 480–85 (1952). M. Demerec, G. Bertani, and J. Flint, Amer. Naturalist 85: 119–36 (1951). K. A. Jensen, I. Kirk, G. Kolmark, and M. Westergaard, Cold Spring Harbor Symposia Quant. Biol. 16: 245–62 (1952). J. A. Rapoport, Compt. Rend. Acad. Sci. U. R. S. S. 54: 65–67 (1946). J. A. Rapoport, Bull. Biol. Med. Exp. U. R. S. S. 23: 198–201 (1946). J. A. Rapoport, Compt. Rend. Acad. Sci. U. R. S. S. 61: 713–15 (1948) O. Wyss, J. B. Clark, F. Haas, and W. S. Stone, J. Bact. 56: 51–57 (1948)

[14] M. Demerec, Proc. Amer. Philos. Soc. 98: 318–22 (1954).

[15] H. J. Muller, Genetics 3: 422–99 (1918). A. H. Sturtevant, Quart. Rev. Biol. 12: 464–67 (1937).

more than the others. By the continuance and repetition of this process, the type that had been normal became supplanted by other types, that were at least better adapted for life in certain particular environments, in in certain ways. Thus, the mutant gene of the previous era became the normal gene of today, and the whole system of genes of the species tended to become even more differentiated and highly organized. Yet at each stage the great majority of new mutations, if examined before being put through the sieve of selection, must have been detrimental to life or to reproduction, as they are today in all species studied, no matter what the degree of advancement of the species.

As important for the survival of a species as the differential multiplication of the few better adapted mutants is the reduction in number and eventual dying out, in competition with the "normal" type, of the much more numerous mutants that are less fit than the normals. Since each generation supplies a fresh crop of these mutations, to be added to those inherited from earlier generations, it is obvious that without this negative selection the system of genes would undergo continued decay. Thus after a time it would become completely heterogeneous, disorganized, and degenerate.[10] In the past, only natural selection has saved it. This selection makes it practically inevitable that any detrimental mutation, no matter how small its harmful effect, will in the long run become limited by tipping the scales against some descendant who carries it, causing his premature death or failure to reproduce.

However, this dying out of the unfit mutants is in most cases rather long delayed. One reason for this delay is the fact that mutant genes are in the great majority of cases heterozygous, that is, present in individuals who have received the corresponding normal gene from their other parent, and that in such a situation the normal gene usually produces most of the effect. The normal gene is for this reason said to be "dominant," and the mutant gene "recessive," even though the mutant is seldom completely without expression when heterozygous.

Another reason for the delay in the dying out of mutant genes lies in the fact that even in those relatively rare individuals who are "homozygous" for a given mutant gene, by reason of having inherited that same gene from both parents, the amount of abnormality is often not very great. Hence, even in this situation the gene usually confers a much less than 100 percent risk of premature death, or of failure to reproduce. It may be noted in this connection that the idea that most mutations are monstrosities or freaks is a popular misconception. In fact, only a tiny minority of mutations cause very conspicuous visible abnormalities.

CALCULATION OF NATURAL MUTATIONS PRESENT IN A POPULATION

The total number of point mutations (or, more correctly, of point-mutant genetic conditions) present in any population at a given time is a product of two interacting numerical factors. The first factor, *a*, is the total number of new point mutations that arise in one generation. The second factor, *b*, to be multiplied by the first, is termed the persistence. It represents the total number of individuals of successive generations by whom, on the average, any given mutation, present at first in one individual, comes to be inherited.[16] This same relation holds for mutations of any particular type as well as for the totality of mutations.

Obviously *b*, the persistence, depends upon the ability of the individuals carrying the mutation to live and breed, as compared with normal individuals. If, for simplicity, we assume the whole population to be of stable size, then *b*, for the average mutation, or for any given type of mutation, is the reciprocal of *c*, the average chance that an individual who has inherited it will be killed prematurely, or will fail to reproduce, as a result of the one or more functional impairments occasioned in him by that mutation. In getting this average chance of elimination, *c*, we must estimate the relative frequencies of individuals heterozygous and homozygous for the mutation, and multiply the chance of elimination of each of these types, taken separately, by its relative frequency. When this is done it is found that usually, despite the much smaller

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[10] See footnote, p. 1071.

[16] H. J. Muller, Amer. J. Human Genet. 2: 111–76 (1950). H. J. Muller and S. L. Campbell, unpublished data. (This reference was omitted from original. Note added October 5, 1955).

detrimental effect in the heterozygous individuals, their relatively large numbers cause most of the eliminations, and most of the total genetic damage to the population, to occur in this group. Thus, in most cases, the homozygous group can for practical purposes be ignored.[16]

In order to apply this method of calculation to human populations we must first have estimates of *a* and *b*. At present such estimates are very indirect, and serve only to indicate a broad range, within which, somewhere, the actual value is probably located. The fruitfly *Drosophila* has thus far been the only organism in which anything like a direct approach has been made to an observed value for either *a* or *b*, and even here the results are subject to very large errors. In this material it can be estimated that, in a population of 100 million, *a*, the number of new mutations arising naturally per generation that becomes transmitted to the next generation, is on the average at least 8 million, and that *b*, the persistence or average number of individuals of successive generations which finally come to inherit any given mutation, is considerably more than 20 and probably more than 40. This makes *ab*, the number of mutations carried by the population of a hundred million in any given generation, probably more than 320 million, that is, probably more than three per individual.

The estimate of *a* for *Drosophila* was obtained by first taking the observed frequency, 0.18 percent, with which "recessive" fully lethal mutations (those that invariably kill homozygous individuals) usually arise in the X chromosome per germ cell per generation when no mutagenic treatment is used.[12] This figure was then multiplied by 6, the ratio of recessive lethals in all the chromosomes to those in the X chromosome. This figure had to be obtained from experiments in which radiation was applied to spermatozoa.[17] The product, 1.08 percent, representing all lethals, was in turn multiplied by 4, the ratio which all mutations detrimental enough to have been detected by a given technique were found to bear to fully lethal mutations. This figure 4 also was based on radiation mutations.[18] Finally, the second product, 4.3 percent, was multiplied by 2, because each individual results from 2 germ cells, and the resultant percent, 8.6, was multiplied by 100,000,000, the number assumed to exist in the population.

That the application of the ratio 6, derived from radiation work, to natural mutations is legitimate has been shown by special tests. However, among natural mutations as a group, the ratio of all mutations to lethals is probably a good deal higher than 4, the ratio found among mutations produced by irradiating spermatozoa. For the radiation mutations include a greater proportion of structural changes and these are more often lethal. This is one reason why the final figure for *a* is very conservative. The other reason is that the methods of detection used failed to find mutations that produced less than about 10 percent risk of premature death, even if they caused considerable infertility, and such mutations may have been relatively numerous.

The figure for *b* is based on tests, carried out independently by two groups of investigators,[16, 19] to determine how much risk of premature death is conferred by a "recessive" lethal mutation when it is heterozygous. In both cases an average figure of about 3 percent to 5 percent risk of death was obtained. This would result in only 1 heterozygous individual among some 25 being killed and would hence allow the average lethal a persistence of 25. That is, it would tend to be passed on to some 25 individuals, on the average, before it died out. Since, however, a considerable majority of mutations are not so detrimental as to be fully lethal when homozygous, and most of them are probably not even 50 percent lethal when in that condition, the figure of 1 in 25 (4 percent) for the risk of death when heterozygous must be considerably higher than that holding for the average mutation, and the persistence, being the reciprocal of this, would be considerably higher than 25. That is why 40 was used as a better guess for *b*, but the observed distribution of mortalities indicates that even it is likely to be too low.

Before we can convert our figure *a* for newly arising mutations in *Drosophila* into a corresponding figure *a* for man we must obtain some indication of the

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[12] See footnote, p. 1072.
[16] See footnote, p. 1073.
[17] R. L. Berg, Genetics 22 : 225–40 ; 241–48 (1937).
[18] J. J. Kerkis, Summ. Commun. XV. Int. Physiol. Congr. 198–200 (1935). J. J. Kerkis, Izv. Akad. Nauk SSSR 75–96 (1938). H. J. Muller, Verh. 4, Int. Kongr. Radiol. 2 : 100–02 (1934). N. W. Timofeeff-Ressovsky, Strahlentherapie 51 : 658–63 (1934). N. W. Timofeeff-Ressovsky, Nachr. Ges. Wiss. Gottingen N. F. 1 : 163–80 (1935).
[19] C. Stern, G. Carson, M. Kinst, E. Novitski, and D. Uphoff, Genetics 37 : 413–49 (1952). C. Stern and E. Novitski, Science 108 : 538–39 (1948).

ratio of mutation frequency in *Drosophila* to that in man. As yet the only line of approach to this problem lies in a comparison of the frequencies, in the two species, of natural mutations that produce certain specific effects, and that may be inferred to occur at given highly limited positions in the chromosomes. Although the evidence of this kind is meager and imperfect, there is enough of it to show that in *Drosophila* a mutation of any one specific type, located in a specific chromosomal position (so as to give rise to what is technically known as an "allele" or "pseudo-allele" of some preexisting mutation) arises, on the average, with a natural frequency of between 1 in 100,000 and 1 in 300,000 germ cells, the most likely figure being about 1 in 200,000.[20] In mice there is little published data of this kind as yet but it would indicate a figure in the range between 1 in 40,000 and 1 in 400,000 or, most likely, about 1 in 140,000.[21] In man, an estimate of between 1 in 50,000 and 1 in 100,000 has been arrived at, on the basis of a much larger amount of data than in either mice or *Drosophila*, but the uncertainties of the methods used in man are much greater.[22] These apparent differences in mutation frequency between the three species may well correspond to the different numbers of cell divisions which take place in their respective reproductive cycles, since these numbers for flies, mice, and men are related about as 1: 1.5: 2. At any rate, it is likely that the average frequency of mutations of any specific type in man is higher than in *Drosophila*, probably from 2 to 4 times as high. To be conservative, we will adopt the lower figure, 2.

It is, however, likely that the ratio of frequencies of specific mutations in man to those in the fly would not be nearly as high as the ratio of total mutation frequencies in man to those in the fly. For man, and mammals in general, give evidence of having a more complicated organization, all told, than the fly, especially when the complications of the nervous system are taken into account. Mammals may therefore be expected to have a more complex germ plasm than flies, one in which a larger number of different kinds of mutations of specific types can occur. This agrees with existence of a larger amount of the genetic substance, polymerized deoxyribonucleic acid, in mammalian than in fly chromosome sets. Therefore we are in all probability obtaining a low minimal figure for a in man if we multiply the *Drosophila* a by only two.

For the value of b in man or other mammals there is as yet little basis for a decision. The existing indications point strongly to the conclusion that natural mutations in mammals in general, including man, are, as in *Drosophila*, prevailingly recessive, yet not completely so. Moreover, they certainly include a fairly abundant group of "recessive" lethals, but it is probable that mutations having a lesser degree of detriment are more frequent than lethals. At this preliminary stage of our knowledge of the subject, then, we have little ground for using a markedly different value of b for mammals than for *Drosophila*.

The figure of about 6.5 is thereby arrived at as a minimal one for the content of recessive, definitely detrimental mutations (including lethals) per individual human being. In a preliminary calculation using related methods, the figure 8 was arrived at.[16] These estimates, as recently shown by Slatis,[23] can be checked in a more direct way. The method consists in observations of the frequency with which homozygous individuals, showing the more definite abnormality often associated with a homozygous mutation, appear among the offspring of marriages between close relatives. Application of this technique has led Slatis, very tentatively as yet, to the figure 8 as the most probable present approximation to the number of natural mutations of the kind in question for which a person is, on the average, heterozygous. This method now needs to be applied on a much larger scale but the present result is enough to be reassuring, in indicating that our mode of calculation is giving figures of the right order of magnitude.

It should be emphasized that in these calculations we are dealing only with these mutations which are detrimental enough to give a "sizable" risk of genetic extinction by way of premature death; that is, one as great as about 0.5 percent in the case of the heterozygous individual, or 10 percent in the case of the homozygous one. We do not know how many mutations arise which are less harmful than this, or which cause extinction mainly by their interference

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[16] See footnote, p. 1073.
[20] H. J. Muller, J. I. Valencia, and R. M. Valencia, Rec. Genet. Soc. Amer. 18: 105–06; and Genetics 35: 125–26 (1950).
[21] W. L. Russell, Cold Spring Harbor Symposia Quant. Biol. 16: 327–35 (1952).
[22] J. B. S. Haldane, Proc. 8th Internat. Cong. Genet. (1948); Hereditas 35 (Suppl.): 266–78 (1949). J. V. Neel, and H. F. Falls, Science 114: 419–22 ((1951).
[23] H. M. Slatis, Amer. J. of Human Genet. 6: 412–18 (1954).

with reproduction, not with life itself. However, even if there are relatively few, those few which have an average grade of detriment within the same order of magnitude as the frequency of their origination by mutation, will accumulate so as to be inordinately numerous in the population. They will provide a very considerable proportion of the superficially observable genetic variability. Moreover, the frequency of the different types of mutations of this group will differ greatly from region to region, in response to differences in the conditions of selection, as well as to random influences.

Since the frequency with which mutant genes of any given degree of detrimental effect exist in the population at any one time is the product *ab*, where *b* is inversely proportional to *c*, the degree of detrimental effect, it is evident that the existing mutant genes have a distribution, with respect to their harmfulness, very different from the distribution to be found on examining mutations as they arise. For, among the mutant genes as they exist in the population as compared with them at their origination, the less harmful ones are (in inverse proportion to their harmful effect) more numerous than the more harmful ones. For that very reason each slightly harmful mutation that arises tends to cause as much detriment to the population as a whole in the end as each drastically harmful or lethal mutation does, since it compensates for its relatively small degree of harm by afflicting correspondingly more individuals. In consequence, the total amount of genetic damage done to a population by mutations is much more closely proportionate to the total frequency of mutations arising per generation (a/N, where N is the number of individuals in the population) than to the frequency of mutations existing in the population (ab/N).[24] If a is raised or lowered, however, it may take scores of generations before its changed value becomes proportionately reflected in the altered average fitness or mutational load of the population. A similar lag occurs if b is altered, as happens when the rigor of selection is increased or decreased.

CHARACTERISTICS OF POINT MUTATIONS PRODUCED BY RADIATION

In the plant material studied by Stadler,[25] evidence was obtained, based on the intensive study of a few types of mutations, that the great majority of apparent point mutations induced by radiation probably consisted of losses of a small section of a chromosome including more than one gene, unlike what was usually true of the natural mutations. In the animal material best studied with reference to this question, that of *Drosophila*,[26] there is evidence that such "sectional deficiencies" do comprise a good deal larger proportion of the point mutations obtained by irradiation of the mature germ cells than of the point mutations arising naturally. However, the apparent point mutations produced by irradiation of immature germ cells of *Drosophila* do not include substantially more that on further analysis prove be be demonstrable "sectional deficiencies" than are found among the natural mutations. Moreover, the characteristics of the effects produced on the individual, both in *Drosophila* and mice,[10, 27] also indicate that a large proportion of these radiation-mutations are as truly changes within the genes as are the mutations of natural origin.

In general, then, in the animal material, the radiation-mutations strongly resemble the natural ones. Practically all types of natural point mutations that have been looked for in extensive irradiation experiments have been found to be produced by radiation also. Like natural mutations, of course, the great majority, although not quite all, of those produced by radiation, are detrimental. Moreover, the great majority have far less dominance (i. e., less expression in the heterozygous individual) than the normal genes from which they arose. Once arisen, the radiation mutations, like the natural ones, are permanent, reproducing themselves as such.[28]

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[10] See footnote, p. 1071.

[24] J. B. S. Haldane, Amer. Naturalist 71: 337–49 (1937). (Volume and year given through typographical errors as 11 and 1949 in original. Note added Oct. 5, 1955.)

[25] L. J. Stadler, Cold Spring Harbor Symposia Quant. Biol. 9: 168–77 (1941). L. J. Stadler, Science 120: 811–19 (1954). L. J. Stadler and H. Roman, Genetics 33: 273–303 (1948).

[26] H. J. Muller, J. I. Valencia, and R. M. Valencia, Genetics 35: 126 (1950).

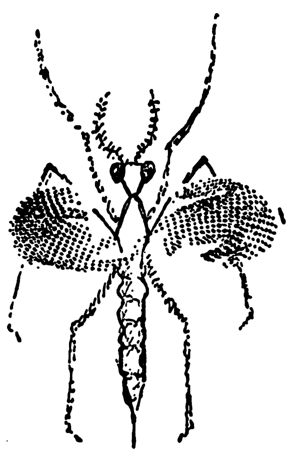
[27] H. J. Muller, "Gene Mutations Caused by Radiation," Symposium on Radiobiology, John Wiley & Sons, New York, Chap. 17, pp. 296–332 (1952).

[28] H. J. Muller, Cold Spring Harbor Symposia Quant. Biol. 9: 151–65 (1941).

Just which mutation is produced by radiation on a given occasion is of course a matter of "accident," as is true of natural mutations. However, the total frequency of the mutations produced by a given dose of radiation varies to some extent with the accompanying conditions, as in the case of natural mutations, although the conditions in question are to some extent different ones in the two cases. The conditions which influence the production of point mutations by radiation include genetic differences, differences in cell type or stage, differences in metabolic reactions, and (a category overlapping the previous one) differences caused by the application of special chemical or physical treatments.

For the most part, the same influences have been found to promote or hinder the action of radiation in causing point mutations as in causing structural changes of chromosomes. For example, chromosomes in condensed stages are more susceptible to the induction of changes of both types. Some findings of interesting differences in this respect have been reported, however. Among these are the observations that sperm cells of *Drosophila* several days prior to their release, and therefore perhaps in the spermatid stage, are much more susceptible than mature spermatozoa to the production of radiation of structural changes, but not of point mutations.[29]

In accordance with the view, first proposed by Rapoport[30] on the basis of chemical work by Fricke,[31] that the mutagenic action of radiation is exerted via the production of actively oxidizing radicals or molecules, it is found that radiation mutagenesis of both major types is positively correlated with the amount of free oxygen present at irradiation. Physical or chemical influences which appear directly or indirectly to increase or decrease the abundance of oxygen available for conversion into mutagenic radicals influence correspondingly the frequency of mutations produced. There is, to be sure, evidence indicating that not all the mutagenic action of radiation takes the same pathway, and that some of it may be quite unconnected with oxidation. But, however that may be, the above and other findings, by demonstrating the conditional nature of radiation mutagenesis, constitute a disproof of the target hypothesis of such mutagenesis, at least in the simplified form in which it had sometimes been applied.[32] Moreover, these findings are of considerable practical value in having led to the working out of treatments, described in other sections of this conference,[33] which give hope of affording significant protection against the mutagenic action of radiation. The fact that certain treatments, even when given after irradiation aid in such protection, is especially noteworthy, both from a theoretical and from a practical standpoint.



[29] K. G. Lüning, Acta Zool. 33 : 193–207 (1952).
[30] J. A. Rapoport, Zhur, Obshchei Biol. 4 : 65–72 (1943).
[31] H. Fricke, J. Chem. Phys. 2 : 556–57 (1934). H. Fricke, Cold Spring Harbor Symposia Quant. Biol. 3 : 55–63 (1935).
[32] H. J. Muller, J. Cellular Comp. Physiol. 35 (Suppl. 1) : 9–70 (1950).
[33] A. Hollaender, Science 121 : 624 (1955). A. Hollaender, W. K. Baker, and E. H. Anderson, Cold Spring Harbor Symposia Quant. Biol. 16 : 315–26 (1952).

In material of varied kinds, but more especially in *Drosophila*, there is good evidence that over a considerable range of dose (in *Drosophila*, from some 50 r. to more than 1,000 r., a more than twentyfold range) the frequency of point mutations (like that of chromosome breaks) is directly proportional to dose.[34] Moreover, they are independent of the timing of the dose, over an enormous range, provided cellular conditions are held constant.[35] Below 25 to 50 r. the mutation frequency is so low that it has hitherto been impossible to obtain sufficient data, and above 1,000 or 2,000 r. the determination of frequency may be interfered with by a selective elimination (through chromosome aberations) of the cells that happened at irradiation to be in a more susceptible state.[36] Since, however, in the work with low doses and low time-rates of delivery of gamma radiation the germ cells of some series were traversed by only one electron track in a period of a half hour or more, on the average, and still showed a frequency of mutations proportional to the total dose, there is reason to infer that no dose or intensity of such radiation is without its proportionate production of point mutations. Moreover, if this is true of gamma radiation it must be at least as true of radiation producing tracks more densely crowded with ionizations.

Despite the equal mutagenic efficiency of different doses and dose rates of ionizing radiation, it is not necessary to infer that a point mutation or a break is ordinarily the consequence, direct or indirect, of a single activation or even of a single ionization. For all the ionizing radiation studied, has some of its ionizations produced in clusters of minute diameter. If two or more ions commonly cooperate mutagenically, however, it might be thought that this would become evident by causing the frequency of mutations to vary as the square or some higher power of the dose. Yet this would not be true if those ions had to be as near together as the ones in a natural cluster, for such close juxtaposition as this would not be brought about with appreciable frequency by raising the dose and the dose rate within toleration limits.

That such cooperation in mutagenesis does occur is indicated by recent observations to the effect that fast neutrons appear to be approximately twice as efficient as X or gamma rays in inducing point mutations in the chromosomes of *Drosophila* spermatozoa,[37] and are probably a good deal more efficient still, relative to X or gamma rays, in inducing chromosome breaks.[38] Presumably alpha rays likewise would be more efficient than X or gamma rays in these respects. One possible interpretation of this higher effectiveness of fast neutrons would be provided, on the Watson-Crick hypothesis of the structure of the genetic material, by the doubleness of the fibers in which the rearrangements are produced, if we suppose that the occurrence of the mutation or break is much facilitated when both fibers are simultaneously affected.

The effectiveness of fast neutrons in inducing point mutations is actually higher than it appears to be, because intensive studies of given cases of these seeming point mutations have shown that in fact a considerable proportion of them involve a double or multiple effect within a very localized chromosome region.[39][40] This greater clustering of effects with neutrons than with X or gamma rays is to be expected, in view of the greater concentration of the ionizations in the tracks of the ionizing particles released by neutrons, provided that the mutational effects arise in close proximity with the activations that induce them. Since this clustering of effects causes many of them to be lost to

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[34] C. P. Oliver, Z. indukt. Abstamm. u. Vererbungslehre 61; 447–88 (1932). W. P. Spencer and C. Stern. Genetics 33: 43–74 (1948). N. W. Timofeeff-Ressovsky, Experimentelle Mutationsforschung in der Vererbungslehre. T. Steinkopf, Leipzig (1937).

[35] K. G. Lüning, B. Lindell, and R. Falk, Acta Radiol. 43: 89–92 (1955). J. T. Patterson, Biol. Bull. 61; 133–38 (1931). S. P. Ray-Chaudhuri, Proc. Roy Soc. Edinburgh B62; 66–72 (1944). N. W. Timofeeff-Ressovsky and K. G. Zimmer, Strahlentherapie 53: 134–38 (1935). D. E. Uphoff and C. Stern, Science 109: 609–10 (1949).

[36] H. J. Muller, I. H. Herskowitz. S. Abrahamson, and I. I. Oster, Genetics 39: 741–49 (1954).

[37] P. T. Ives, R. P. Levine, and H. T. Yost, Jr., Proc. Nat. Acad. Sci. USA 40: 165-71 (1954). G. H. Mickey, Amer. Naturalist 88: 241–55 (1954). H. J. Muller, Rec. Genet. Soc. Amer. 23: 58 and Genetics 39: 985 (1954).

[38] W. K. Baker and E. Von Halle, Science 119: 46–49 (1954). A. D. Conger, Science 119: 36–42 (1954). J. S. Kirby-Smith and C. P. Swanson, Science 119: 42–44 (1954). W. L. Russell, Liane B. Russell and A. W. Kimball, Amer. Naturalist 88: 268–86 (1954).

[39] H. J. Muller and J. I. Valencia, Rec. Genet. Soc. Amer. 20: 115–16; and Genetics 86: 567–68 (1951).

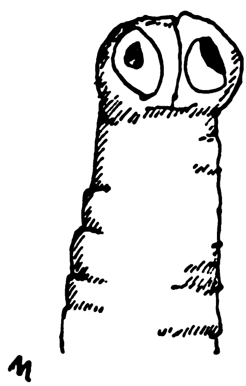
[40] H. J. Muller, "The Manner of Production of Mutations by Radiation," Radiation Biology (edited by A. Hollaender), McGraw-Hill Co., New York, Vol. I, Chap. 8, pp. 475–626 (1954).

view by reason of their proximity to each other (except when special techniques of analysis are used), the mutagenic potentiality of the fast neutrons is correspondingly underrated in most experiments. So far as genetic damage to the population is concerned, however, a double or multiple effect of the given kind adds no more to the mutational load than does a single effect. Hence for present purposes fast neutrons may be regarded as no more than twice as effective as X or gamma rays in producing point mutations.

ESTIMATION OF THE TOTAL POINT MUTATIONAL DAMAGE FROM A GIVEN AMOUNT OF RADIATION

It has been noted that the important quantity in the determination of the total amount of genetic damage is not the amount of harm done to the individuals who have inherited the mutations in question but only the total number of these mutations. For a mutation doing less harm to an individual will, as if in compensation, be passed down to a correspondingly larger number of descendant individuals. It has also been noted that an approach to a direct estimation of the total number of mutations arising has thus far been made only in *Drosophila*, and that this calculation has involved the use of data from radiation experiments. This work can therefore be applied to the estimation of the total damage arising from a given dose.

The principles have already been explained whereby a minimum value for the total number of mutations is obtained by getting the number of lethals in the X chromosome and then multiplying this by 6, to get the number of lethals in all the chromosomes, and again by 4, to get the total number of mutations causing at least 10 percent detriment to life, when homozygous. (A correction is made in this calculation, based on certain tests, in order to estimate the number of point mutations without including the structural changes.) When this calculation is carried out, using the results obtained at any given dose, the resulting number can then be expressed in terms of the total number of point mutations produced by a single r. unit, by using the principle of proportionality of point-mutation frequency to dose. It is then found that this number turns out to be about 1 mutation among 2,000 germ cells per r. (that is 5×10^{-4}/r.) for X or gamma rays applied in the usual way to mature spermatozoa.[41] The more important figure, representing the result of irradiation of the more prevalent stages (gonia) of immature germs cells of adult *Drosophila*, is only a fourth to a half of this, according to the conditions. It is probable that there are even lower values for certain other immature stages of *Drosophila* germ cells, as for instance those in the embryonic polar cap.[42]



In order to obtain a figure for the total number of mutations produced by a given dose in mammalian material we may follow the procedure which we

[41] H. J. Muller, "Radiation Damage to the Genetic Material," Science in Progress, Yale University Press, New Haven, pp. 93–165, 481–93 (1951). H. J. Muller, "The Nature of the Genetic Effects Produced by Radiation," Radiation Biology (edited by A. Hollaender), McGraw-Hill Co., New York, Vol. I, Chap. 7, pp. 351–473 (1954).

[42] Z. I. Berman, Isvest. Akad. Nauk, SSSR, pp. 645–78 (1939). Helen U. Meyer, unpublished data.

adopted in the calculation of natural mutations. This involved a comparison of the mutation frequencies involving particular types of mutations, located in given positions on the chromosomes, in *Drosophila* and in mammals, and then applying the ratio thus found to the figure for total mutation frequency in *Drosophila* so as to convert it into the presumed corresponding value (a minimal one) for mammals.

Fortunately, there is available for this comparison a much more reliable body of data, for both groups of organisms, than that which we had recourse to in the case of natural mutations. The average frequency of point mutations of the kind in question in *Drosophila*, based on a study of 10 types (loci), was found to be about 1.4×10^{-8}/r. for any given type, when the radiation was applied to inactive immature germ cells (oögonia).[43] The different types seldom varied from one another in frequency by a factor of more than 2 in material abundant enough for judging this matter (that in which spermatozoa had been irradiated.

In the mammalian material, comprising irradiated spermatogonia of mice, Russell[30] has reported an average mutation frequency of about 25×10^{-8}/r., based upon 7 specific types of mutations (loci). Here the range of variation between the different types was greater than in the above *Drosophila* material, but their mean agreed well with their mode, and 4 of the 7 types conformed fairly closely with this mean. It is clear on comparison of the two sets of results that the susceptibility of the mammalian material is at least an order of magnitude higher than that of the flies, the observed factorial difference in results being 18. To obtain a minimum estimate of the total frequency of mutation in the mice we must therefore multiply by 18 the figure arrived at for gonial cells of flies. (It is of no consequence that in the flies oögonia were studied and in the mice spermatogonia, since special comparisons[44] have shown these two cell types to be alike in mutagenic susceptibility as they are expected to be.) Since the figure for the gonia of flies had a lower limiting value of 1.25×10^{-4}/r. the minimum value for mice becomes 2.25×10^{-3}/r. This is the frequency for a germ cell, not for an offspring derived from two such germ cells; for the offspring it would be 4.5×10^{-3}/r.

In performing this calculation we are, as in the case of the natural mutations, assuming that the hereditary material of mammals is no more compound than that of flies; i. e., that there is not a greater number of different specific types of mutations in mammals than in flies, despite their seemingly more complicated organisms and their larger amount of deoyribonucleic acid. The total frequency of mutations per r. may be a good deal higher than here calculated not only because of the inadequacy of this assumption but also because weakly detrimental mutations and those mainly affecting fertility rather than individual survival have not been included. Moreover, only the lower limiting value for the somewhat variable mutation frequency of fly oögonia was used. All this emphasizes the fact that our estimate is decidedly on the "conservative" side.

At the same time, it is true that the value is one for mice, not human beings. All that can be said to this is that, so long as we lack data on an organism still closer to man, it is necessary, provisionally, to base our judgments on this result, and that, since mice are so much closer to men than flies are in almost every other important respect, it would be strange if they were not closer in their mutagenic properties as well. Moreover, the factors which might be expected to cause a significant difference in the natural mutation frequencies of mice and men—their great discrepancies in length of life, size, and number of cell generations in the reproductive cycle—would not be expected to exert significant influences on the frequency with which mutations are produced in them by radiation.

The minimum figure of 4.5×10^{-3}/r. point mutations for the offspring of parents both of whom were exposed can be expressed in the form: "at least 1 induced point mutation per offspring, on the average, for each 220 r. of exposure to both parents. From this it is evident that many of the children who were conceived by Hiroshima survivors at any time after their exposure must have contained one or more mutations induced by the radiation. Similarly, children conceived

[30] See footnote, p. 1075.
[43] H. J. Muller, Seventh Annual Report to Amer. Cancer Soc., Inc., pp. 120–21 (1952).
[44] K. V. Kossikov, Genetics 22: 213–24 (1937). R. I. Serebrovskaya and N. I. Shapiro, Compt. Rend. Acad. Sci. U. R. S. S. 2: 421–28 (1935).
NOTE.—Only when these two papers are taken in conjunction with one another does the equal mutagenic susceptibility of gonial cells of the two sexes become evident.—H. J. M.

by parents both of whom have been exposed to the so-called "permissible dose" of 0.3 r. per week (15 r. per year) for as long as 15 years would on the average contain at least 1 induced mutation. It is probable that the same is also true of the children of many radiologists, dermatologists, and dentists.[45]

The recent study of Macht and Lawrence,[46] gives direct evidence of genetic damage in such cases and is in this respect superior to the studies made in Japan. Moreover, studies of Moeller et al.[47] show that the population in general is already receiving significant amounts of radiation from medical diagnoses. Sonnenblick [48] finds that exposures of this kind are seldom adequately controlled.

When it is considered that practically every mutation must eventually become eliminated from the population, after having—even if imperceptibly—hampered enough descendants so as finally to be a deciding cause, in the last of the line, of his premature death or failure to reproduce, then it becomes evident that practically every mutation represents a postponed disaster. Thus the genetic damage, that to later generations, caused by a given total dose is seen to be far greater than the damage to the exposed individual himself. In view of this, measures and regulations concerned with radiation protection should be based, at least in the case of persons who may later reproduce, primarily on the risk of genetic damage or, more specifically, of point mutations in their germ cells, rather than on the risk of damage to their own bodies. This would cause such measures and regulations to be far more stringent than they are at present.[49]

THE INDUCED MUTATIONS IN RELATION TO THE NATURAL LOAD

On our conservative estimate of 16 million natural mutations arising per generation in a population of 100 million, a frequency of 0.16, it would take only about 37 roentgens of gamma radiation delivered to the population to produce a quantity of new mutations equal to the new natural ones, and thus to double the mutation frequency. Our conservative estimate, however, was based on the assumption of only 1×10^{-5} as the average freqeuncy of a mutation of some specific type, involving a given chromosomal position. According to this assumption the actual data, which indicate about 2×10^{-5} as the frequency of mutations of a specific type, are misleadingly high, because of certain sources of technical error. Since, however, this is a matter by no means proved as yet it remains quite possible that the amount of radiation necessary to double the mutation frequency is 75 roentgens or higher. This is approximately the value that we used in our earlier treatments of the subject,[49] [50] in which it was assumed that the observed 2×10^{-5} frequency for mutations of a specific type was approximately correct. These considerations illustrate the considerable margins of error in any present quantitative treatments, and the need for greater exactitude of knowledge.[51]

The present uncertainty regarding the natural mutation frequency carries with it a corresponding uncertainty regarding what proportion of the natural mutations in man are contributed by natural radiation. There is also uncertainty regarding this question based on variation in the amount of natural radiation. If we suppose that in some typical regions as much as 6 roentgens are accumulated, on the average, in the span of one human reproductive generation (25 to 30 years), then, on the more conservative estimate that the natural mutation frequency is equal to what would be induced by 37 roentgens, it turns out that some 16 percent of the natural mutations in man are produced by natural radiation. On the higher estimate for natural mutations, some 8 percent of them would be radiation-induced. In either case, the figure must be far higher than for short-lived organisms, such as mice or flies. On the other hand, in some

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[45] H. J. Muller, Bulletin of the Atomic Scientists 11: 210–12, 230 (1955) and Science 121: 837–40 (1955).

[46] S. H. Macht and P. S. Lawrence, Amer. J. Roent. and Rad. Ther. 73: 442–66 (1955).

[47] D. W. Moeller, J. G. Terrill, Jr., and S. C. Ingraham, II, Public Health Rep., U. S. Publ. Health Serv. 68: 57–65 (1953).

[48] B. P. Sonnenblick, J. Newark Beth Israel Hosp. 6: 31–42 (1955).

[49] H. J. Muller, Acta Radiol. 41: 5–19 (1954).

[50] H. J. Muller, Amer. J. Obstet, and Gynec. 67: 467–83 (1954).

[51] Much of the information concerning mutation frequencies in Drosophila cited in the present report was derived from work by the present writer and his associates that had been supported by grants from the Atomic Energy Commission (contract AT (11–1)–195), the American Cancer Society (given on recommendation of the Committee on Growth of the National Research Council, United States of America), and the Rockefeller Foundation.

human populations living at a high altitude, with its greater cosmic ray intensity, the contribution of radiation to the natural mutation rate must be twice as high as here estimated. Still higher values must obtain for some populations living in regions where radioactive minerals are abundant.

Many persons unfamiliar with genetics have regarded the seeming normality of the children born to survivors of the Hiroshima and Nagasaki bombings as evidence against the conclusion that the amount of radiation there received produced a significant amount of genetic damage. This misunderstanding arises from their lack of realization of the following points.

1. Few mutations are sufficiently dominant to give readily perceptible effects when inherited from only one parent, as they are in the vast majority of cases.

2. Even though these effects are not perceptible they are nearly always sufficient to hamper the individual somewhat, and finally, usually in a very distant descendant, to cause the extinction of that line of descent.

3. In any heterogenously breeding population, such as is found anywhere outside of the geneticists' fields and laboratories, there is already so much natural genetic variation, representing an accumulation of many generations of natural mutations, that the additional mutations caused by the radiation would become lost to view among them even if they were as abundant as those that would arise naturally in the course of a number of generations. Thus, the genetically damaged population will eventually have to pay the costs, but these will be spread out over so many small installments, and so intermingled with the greater weight of other payments, as hardly to be recognizable. All this was of course well known to geneticists before the observations on the children at Hiroshima and Nagasaki were conducted, and led them to express serious doubts that any genetic effects would be demonstrable there, even though they had no doubts that they had actually been produced.[41]

These points may be better appreciated if it is realized that in *Drosophila* also it had not been possible to demonstrate the mutagenic action of radiation by mere inspection of the individuals of the first, second, or third generations after exposure. Exact genetic methods had first to be worked out[11] and these are of course unavailable in man. Even following an exposure of fly spermatozoa to some 5,000 roentgens, which we today know causes each offspring, on the average, to receive at least three induced mutations, hardly one abnormal offspring is usually to be found among 100 examined, yet the damage is there, and it will be exerted if the population is allowed to continue.

At the same time, it is true, unlike what many nongeneticists suppose, that the effects of the genetic damage are more strongly exerted in the first generation of offspring than in any subsequent generation. They very gradually subside, in the course of many generations, as the population is purged by the dying out of the unfit. Even the recessive effects, those present in individuals homozygous for the given mutations, are found most frequently in the first generation, and then less and less frequently if the population breeds naturally rather than being subjected to a geneticist's controlled inbreeding manipulations. Moreover, there is a much higher chance that a given induced mutation will become homozygous by meeting, at fertilization, a gene of the same type derived from the great accumulated store of natural mutations, than one of that type which, like itself, had been induced by the radiation.

Since the worst effects are already exerted in F_1, what the Hiroshima observations do demonstrate clearly is that the genetic damage to posterity caused by exposure to between one hundred and several hundred roentgens is not *conspicuously* detrimental, and is well within limits consistent with the survival and self-perpetuation of the population. This might have been reckoned as probable without the direct evidence. For, according to the conclusion that the average individual is already heterozygous for some 6, 8, or even more mutations which when homozygous would be fairly conspicuous and/or detrimental, it does not seem likely that the addition in heterozygous condition of just one more, induced by some 200 roentgens, would result in any very evident change in the picture. This remains true even when we take into consideration the fact that the already existing mutations have already passed, to varying extents, through the sieve of selection and are therefore not, on the average, as detrimental as the newly induced ones.

The apparent contradiction between the fact that a really serious amount of genetic damage was produced and the fact that none is evident even in the most

[41] See footnote, p. 1079.
[11] See footnote, p. 1072.

afflicted generation (the first), is reconciled by the manner in which the damage is spread out, thinner and thinner, over a great number of generations. There is a kind of buffering or dilution of the damaging effects, by the normal genes that dominate over them and thus delay their elimination. Thus the effects are spread out in time, in inverse proportion to their dilution in any one generation, but the total damage remains just as great as if concentrated. Moreover, even though the induced mutations may be many times the number that would arise naturally in one one generation, they are nevertheless few in relation to the accumulated natural "load." Hence they can raise by only a rather small percent the number of genetic shortcomings already present in the population.

Despite these buffering influences, it would be impossible for a population to tolerate, generation after generation, an exposure which, given to only one generation, would cause no perceptible deterioration. Gradually, as elimination rose enough to balance the new mutations, an equilibrium level of accumulation would be approached, and at this new level the then existing accumulated load would be as many times greater than the original accumulated load as the then existing mutation rate was greater than the original mutation rate. Thus, if 37 roentgens doubles the mutation rate, a population which had received this dose for many generations would at last have twice as many ills of genetic origin as we have. Yet we already have more than enough for comfort.

Not to be neglected in the picture is the other end of the balance mechanism; the rate at which elimination of mutations goes on. Under modern civilization we interfere so much with this that we are probably raising the load of accumulated mutations as fast as by applying some tens of roentgens to everyone's reproductive organs.[45] Under these circumstances the raising of mutation frequency at the same time, by exposure to radiation, might tend to bring us to a genetic situation that it would be difficult to cope with.

All these questions need to be not only discussed but actually investigated far more realistically than they have been in the past. Otherwise we may at last find ourselves, genetically, facing a parallel to already accomplished deforestation and erosion, on an even grander scale. This problem is not only one that is concerned with the possible aftermaths of atomic war. It must be faced equally by the proponents of peace if we are to have an atomic age, with its risks of prolonged "permissible" exposures arising from industrial uses and radioactive waste products.

For peace will, we hope, go on and on through a great series of generations. Under these circumstances, it will be the more necessary to control and limit the radiation received by the population at large in every generation. For, given enough generations, the equilibrium level of damage will be reached, at which that damage will no longer be buffered, but will accurately correspond with the existing mutation frequency. Then, a relatively small number of roentgens per generation will exert an inordinately larger effect than it seems to now. At our present juncture, before that process has more than begun, far-seeing policies should be established. These must guard us against the dangerous fallacy that what cannot be seen or felt need not be bothered with.

This subject of protection of human beings against the genetic damage produced by radiation must, until suitable policies are established, far overshadow in its importance that of the utilization of radiation in the genetic improvement, for human purposes, of organisms potentially useful to man, or in the elimination or reduction of noxious organisms. However, these constructive uses of radiation in biological engineering will come increasingly to the fore as the more menacing aspects of radiation are brought under control. There is already abundant evidence of the possibility of such beneficial applications on a considerable scale.[41, 53]

At the same time, the dangerous mistake should not be made of considering man as a species who would himself undergo a long-term benefit from the application of radiation to his germ plasm. His own reproductive material is his most invaluable, irretrievable possession. It is already subject to an amount of variation which, in relation to his present reproductive practices, borders on the excessive. Under these circumstances, man's first concern in dealing with radiation must be his own protection.

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[41] See footnote, p. 1079.
[53] A. Gustafsson, Cold Spring Harbor Symposia Quant. Biol. 16: 263–281 (1952). A. Hollaender, Ann. Missouri Botan. Garden 32: 165–178 (1945).

Representative HOLIFIELD. Are there any questions by members?

Representative VAN ZANDT. Dr. Muller, in regard to Russia, can you estimate the number of years it will take Russia to catch up with our progress in this field of radiation?

Dr. MULLER. I believe it will probably take them a long time. One would have to assess political factors there which are essentially unpredictable. Personalities play such a large role there, much larger than here, despite their theory of economic determination. Anyway, my estimate would be 10 or 15 years.

Representative HOLIFIELD. Thank you very much, Dr. Muller. Mr. Hollister wishes to ask you some questions.

Mr. HOLLISTER. Dr. Muller, I understand that you are familiar with the work of the Atomic Bomb Casualty Commission, and I wonder if you would comment on it and on the quality of and the conclusions from the data.

Dr. MULLER. I think it was evident to geneticists from the beginning that it was very doubtful whether any positive or essentially negative evidence could be obtained from a study of that kind, because human populations are so variable and it is so next to impossible to obtain two groups to compare, one irradiated and the other not, which are essentially similar in other respects. In view of that, I think that the present lack of results is not surprising even to most of those who took part in the investigation. I do not think they are to be blamed for it. There was some slight chance—because of the uncertainty of the estimates of the human mutation rate—that if the induced mutation rate was exceptionally high then some evidence of it could be obtained.

I think that it is very unfortunate to cite the lack of results from the study as indicating that there is no effect. I think that those reporting on the matter for the AEC in a December 1956 publication that I recently saw were quite right in saying that the data obtained there are not at all out of line with the expectation based on results with mice, that is, the frequency of induced mutations could well have been just as high among the human beings for the dose received as it would have been for mice, which is a matter we know a lot more about through Dr. Russell's experiments. We can go a little further than that and say that it could not have been a great many times higher than it is in mice, otherwise there would have been demonstrable results.

But we need not feel at all secure or relieved that no effects were found.

I remember that in a meeting of a committee called to decide whether these studies should continue one of the persons on the committee said, after I had remarked that I thought no effects would be found and that that would not prove anything, "That will be a very good thing because it will tend to allay the public fear on the matter."

Chairman DURHAM. Did you participate in writing the report?

Dr. MULLER. No; I did not. I may say this: I do disagree with a lot of the discussion in the last chapter where exception is taken to drawing conclusions from the results obtained in lower organisms. Certain criticisms are leveled against the work on flies and mice, for example, that I think are demonstrably unjustified and have a mistaken position. But that is not the major part of the work. When it

comes to the work on humans, I think that was done as well as could be expected under the very difficult circumstances.

Representative HOLIFIELD. Are there any further questions?

Representative VAN ZANDT. Dr. Muller, in the December 3, 1956, issue of the Federation of American Scientists you are quoted as saying this:

> It is reckless to increase the risk of war by continuing H-bomb tests. It is not the fallout from these tests that is at issue at this time but the war feeling. The first step for peace open to us is a discontinuance of tests by both sides. If breached by either side it can be detected by the other.

Will you comment on that statement?

Dr. MULLER. I was not speaking there primarily as a geneticist, but I think physicists are pretty generally agreed that the setting off of a test in the megaton range can be detected with certainty by the other side. So that a breach would be known and in that sense we already have 100-percent-effective inspection for tests of that kind.

Therefore, it is in that sense safe if both sides agree to discontinue the tests for them to act upon that agreement, since as soon as one side breaks it, the other side will know it.

Senator ANDERSON. As a matter of fact, Doctor, the first times that we detected tests we detected them with instruments that are primitive compared to what we now have; is that not true?

Dr. MULLER. And we detected ordinary atomic bomb tests of the other side.

Senator ANDERSON. So that a megaton test would be easily detectable, and there is no possibility of deception.

Dr. MULLER. Yes.

Representative HOLIFIELD. Thank you very much.

The next witness is Dr. W. L. Russell.

Dr. Russell comes from Oak Ridge, and has done important work at the Laboratory. We are glad to have you here and to have your statement.

STATEMENT OF DR. W. L. RUSSELL, OAK RIDGE NATIONAL LABORATORY [5]

Dr. RUSSELL. Mr. Chairman, members, the testimony to be given here is presented in response to one of the requests by this committee for scientific results on the biological effects of radiation caused by events other than fallout. Assuming the present estimates of radiation from fallout to be approximately correct, it would in fact be virtually impossible at the present time to measure the genetic effects of fallout in mammals. In estimating the genetic hazards of fallout, we are, therefore, forced into using the information obtained from experiments, such as those to be described, in which much higher levels of radiation were used.

[5] B. A., Oxford University, 1932; Sherman Pratt fellow, Amherst College, 1932–33; fellow, University of Chicago, 1933–34; asistant, department of zoology, University of Chicago, 1934–36; Ph. D., University of Chicago, 1937; research associate, Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine, 1937–47; principal geneticist, Oak Ridge National Laboratory, 1947 to present; research and publications on the genetic effects of radiation in mice. In charge of the Mammalian Genetics and Development Section of the Biology Division of Oak Ridge National Laboratory. Member of the United States delegation to the 1955 Geneva Conference on the Peaceful Uses of Atomic Energy. Member of the Committee on Genetics Effects of Atomic Radiation, National Academy of Sciences. (Submitted by witness.)

The results described here were obtained from a series of experiments conducted in the Mammalian Genetics and Development Section of the Biology Division of the Oak Ridge National Laboratory. This program was started in 1947, the year the Atomic Energy Commission was founded. The preliminary work involved the construction of animal rooms and laboratories, the development and building up of special stocks of mice necessary for the experiments, and certain pilot experiments, including studies on the effects of radiation on fertility. The major experiments were started in 1949.

I was very happy that Dr. Sturtevant said what he did this morning about the lack of suppression of information and the desire to do good work in the national laboratories. I heartily concur, but such a statement comes more forcefully from a person outside the AEC, and one of Dr. Sturtevant's standing. I will say no more on this, other than that neither I nor any of my colleagues would attempt to do scientific work under conditions that were not free.

I think I might also add, and it would be ungrateful of me if I did not, that in addition to working under free conditions and not having our information suppressed, we have received personal encouragement, especially from the Division of Biology and Medicine of the AEC, including its Directors, from Dr. Warren, the first one, to the present one, Dr. Dunham.

Before the results of these experiments with mice were obtained, estimates of genetic hazards of radiation in man were based primarily on data from experiments with the fruitfly, *Drosophila*. Some information on the genetic effects of radiation in mammals was available, but most of this dealt with major chromosomal aberrations which are probably not an important hazard in human exposures. There was virtually no information on radiation-induced gene mutation rates in any mammal. The information now available from our experiments has therefore played a basic role in the new estimates of genetic radiation hazards made and published last year by the National Academy of Sciences committee in the United States and by the Medical Research Council committee in Great Britain. The United Nations Scientific Committee has also used this information.

In response to the request by this committee, I should like to present here a simplified up-to-date summary of our results, emphasizing the aspects which are useful in estimating human hazards. More detailed technical accounts have been submitted for the record to supplement this oral presentation. Again it must be kept in mind that these data were not obtained from fallout radiation. However, they can be used to estimate the genetic hazard from fallout, provided the radiation dose to the gonads is known. I shall describe two types of experiment. One of these measures the rates at which genes mutate by studying the effect of radiation on a particular selected sample of genes. The other measures damage in a population as a result of the total amount of mutation induced.

Most of our work up until recent times has been concerned with the former method, and so I shall describe the results obtained by it in more detail. The measurement of gene mutation rates is important, both for absolute and for comparative purposes. The particular method that we have used was chosen in the hope that it would be a fine-edged tool for making comparisons. One of these that was ob-

viously badly needed was a comparison of the mutation rates in any mammal with those in Drosophila. The mutation rate method chosen by us seemed likely to be the one which would give the most meaningful information on such a species comparison. Our present information on this point indicates that mouse genes are on the average approximately 15 times as sensitive to radiation as Drosophila genes. The earlier estimates of genetic hazards in man based on results in Drosophila have, therefore, been revised.

Another question on which information was badly needed was whether or not there is any recovery from genetic damage with time after irradiation. Some geneticists believed that there would be no such recovery. Others thought that some recovery might occur. Critical evidence on this point had not been obtained. Most of the information along these lines came from experiments with Drosophila sperm, whereas the evidence needed was for immature sex cells. Let me elaborate for a moment on the importance of the cell stage, i. e., immature versus mature germ cells, before we continue with the question of recovery.

In the testis, the immature germ cells—called spermatogonia—persist throughout life. Cells are constantly budded off from them and, after further multiplication, develop into the mature sperm cells. The time required for the development of a mature sperm from an immature spermatogonium is only a few weeks. When we are exposed to continuous or intermittent radiation, the dose received by a cell up to and including the spermatogonium stage is the total dose received over the whole period from conception of the individual up to the time when the spermatogonium starts its final development. This is obviously going to be much greater than the dose received during the few weeks of its final development into a sperm cell. Even with acute radiation received as a single dose, the chance of a fertile mating occurring within a few weeks after exposure is small compared with fertile matings that will occur at later intervals. Thus again it is usually the dose received by the immature cells which will count. Therefore, the problem of genetic hazard of radiation in man relates primarily to the immature germ cells. It is on these cells that our studies of induced mutations in mice have been conducted. This has many important implications, one of which is on the question of recovery, to which we can now return.

How might the question of cell stage affect recovery following irradiation? It is possible to imagine that an immature germ cell in which a mutation has been induced might multiply at a slower rate than a normal cell. This is one example of a possible mechanism by which some recovery from genetic damage might occur. It is obvious that experimental data from mature germ cells in Drosophila could not answer such a problem. Considering the possibility that I have just mentioned, along with others, it was clear to us that the question of recovery with time had to be investigated for immature germ cells. It also seemed important that the problem be examined in an organism with a generation time much longer than that of the fruitfly, which is only 10 days, and in mammalian gonads, which are quite different in their makeup and function from those of insects. Having now obtained this information, we find that in spite of the possible mechanisms by which recovery might have occurred, there

is in fact no evidence of any significant recovery with time after irradiation. The offspring of a mating made a long time after irradiation is just as likely to contain a gene mutation as is the offspring of a mating made shortly after irradiation. I might interject here that I am talking only about irradiated spermatogonia. If we included the sperm, there would be some recovery within a short time after radiation. In human hazards, however, we are concerned, as I have already explained, primarily with the immature germ cells. Thus it would appear that it is the cumulative dose which is important, and this principle is now established for the immature germ cells of a mammal. I have dwelt on this point at some length because I have found it to be one that is often raised in discussion. We are so used to the body being able to recover from various types of damage that it is quite reasonable to require rigorous proof that this does not occur with genetic damage.

Another problem that was investigated by our method for measuring gene mutation rates is the relation between mutation rate and dose. On the basis of experiments with other organisms, a linear relation was expected. In other words, it was expected that mutation rate would be directly proportional to dose, that, for example, doubling the dose would double the mutation rate.

Dr. Crow this morning assumed this principle and stated that he was basing his assumption on Drosophila. Therefore, I think it appropriate to state what information we have in this respect in the mouse.

Our first data came from males exposed to 600 roentgens. As the data started to come in from males exposed to 1,000 roentgens it became clear that the mutation rate was significantly lower than expected on the basis of the 600 roentgen results. It seemed likely that this result, which was unexpected on the basis of Drosophila results obtained up to that time, might be due to the fact, already emphasized, that we were dealing with immature germ cells (spermatogonia) in the mice, whereas Drosophila results had been obtained from mature germ cells. If, for example, there were differences in sensitivity to mutation induction correlated with sensitivity to killing of the cells, then at the higher doses the mutation rate observed might represent only the mutation rate for the surviving and more resistant cells. Careful studies in our laboratory by Dr. Oakberg on the amount of killing of spermatogonia with various doses of radiation support this possibility.

Whatever the explanation might be, it was clear that it was important to obtain data on mutation rates at doses lower than 600 roentgens. Since the relation between mutation rate and dose has been found not to be directly proportional above 600 roentgens, it was possible that it would also not be proportional below 600 roentgens. We have therefore begun experiements at lower doses. The data so far obtained from a 300-roentgen experiment show no significant departures from proportionality with the 600-roentgen results. More data are, however, needed.

Representative Holifield. At that point, Dr. Russell, could you tell this committee how near you think the experiments on mice would correspond to the radiation of human beings? You have made the statement that the mouse is 15 times more sensitive than the fruitfly. Can you give us a comparison between the mice and the human being?

Dr. RUSSELL. I have prepared a short statement involving the whole problem of extrapolating.

Representative HOLIFIELD. Is that contained in the statement?

Dr. RUSSELL. It is not in the typed copy you have, but I shall be glad to read it.

Representative HOLIFIELD. Or could you give me a summary right now?

Dr. RUSSELL. I believe a summary is the best we can do at the present time. I might explain a little more about this dose relation, if I may, on the board.

If we put the 600-roentgen mutation rate at this point here, the 1,000-roentgen result instead of being up here, as expected from the linear relation found in Drosophila, turned out to be down here [pointing on blackboard].

Representative COLE. Doctor, would you go back to the microphone and repeat what you said, because I do not understand it.

Dr. RUSSELL. The first result we got was with the 600-roentgen experiment and is shown here. The points immediately above and below it that I have connected with the line represent the 95-percent confidence interval for this result, that it, with 95 percent probability the true value should lie within that range. The 1,000-roentgen result was expected, from the linearity found in Drosophila experiments, to be correspondingly higher than the 600-roentgen, but actually came out lower. In other words, the relation is not linear. This raises the question of why there is linearity in Drosophila and not in the mouse.

As I have said, we felt that perhaps this lack of linearity in the mouse was due to the killing of the spermatogonial cells themselves, and that at the higher dose the result represents the mutation rate of more resistant cells which were not killed. At the lower dose you get a mutation rate for more sensitive cells, some of which have not been killed.

The question arises what is the curve going to do below 600 roentgens? You can no longer predict that it will be linear between 600 roentgens and zero. There is now some likelihood that it will be higher than expected on a linear basis. Up to this time in the hearings we have had a good deal of discussion of whether the mutation rate is linear and if whether some somatic effects are linear or whether they go up this way, that is, concave upward. These mouse mutation results represent something going this way, that is, convex upward. In other words, the effect at lower doses might be higher than expected on a linear basis. It was obviously important to obtain data at the lower doses. The 300-roentgen result looks like this. At the present time it falls close to the straight line drawn between the results for the 600 roentgens and the zero doses, but as I say, I think we need more data on this point.

Representative HOLIFIELD. This would indicate, then, if this theory becomes established, that the lower dose rates would cause more mutations in the germ cells than the higher rate.

Dr. RUSSELL. This is a possibility raised by this departure from linearity at high doses.

Representative HOLIFIELD. That is because the higher dose kills the cell.

Dr. RUSSELL. That is right. The amount of killing of cells is quite high so it is still possible to get some departure from linearity at lower doses. So we feel that this point needs further experimentation.

Representative HOLIFIELD. But you are not yet ready to set the rate?

Dr. RUSSELL. Three hundred roentgens was the first attempt to provide some information between 600 roentgens and zero. As we go down in the dose, it is much harder to obtain the data because fewer mutations are obtained.

Chairman DURHAM. Are you using only flies?

Dr. RUSSELL. This is all on mice.

I might also interject at this point another comment. A good deal has been said about there being no Drosophila data on mutation rates below 25 roentgens and about there being no mouse data below 300 roentgens. Yet, again referring to Dr. Oakberg's work in our laboratory, he can see and actually measure the killing of spermatogonial cells with doses as low as 2 or 3 roentgens, and even with 1 rep of neutrons. A dose of 22 roentgens kills half of the sensitive spermatogonia. So it seems to me that if we can actually see cells that have been killed by doses as low as 2 or 3 roentgens, and can put this on a quantitative basis, if cells are actually killed at these dose levels, there is no question in my mind that there will be genetic effects from doses as low as this.

Representative HOLIFIELD. This has a tremendous impact on the theory of threshold, does it not?

Dr. RUSSELL. It certainly helps to answer the question of threshold for genetic effects. Of course, the question of threshold for some somatic effects is not answered by this, because killed cells might be replaced by normal cells. So it does not conclusively prove anything about the question of a threshold for somatic effects. I think it supports the already quite well established point of there being no threshold for genetic effects, because here is direct experimental evidence on mammals that cells can be killed measurably by doses as low as 2 or 3 roentgens.

Representative HOLIFIELD. You have been able to observe those?

Dr. RUSSELL. The killed ones can be observed, yes.

Another comparative study that can be made with the gene mutation rate method used by us is on the variation in mutation rates of different genes. Extensive information on seven different genes in the mouse shows a wide range in their mutation rates. The difference between the lowest and the highest rates is more than thirtyfold. This finding is of interest in many respects. For example, it raises the possibility that the so-called rate-doubling dose, the dose required to double the spontaneous rate, might be quite different for different genes.

The results from our gene mutation rate method that I have so far described have been useful primarily for comparative purposes. Thus we have been able to compare mutation rates in mice and fruitflies; we have been able to compare mutation rates at short and long intervals after irradiation; we have compared mutation rates at different doses; and so on. Additional work now going on with this method will, in addition to measuring mutation rates at lower doses, also give information on other comparisons—for example, a comparison between the mutation rates in females and males, a comparison of the

results from long-continued low-level irradiation with those from single dose acute radiation, and so on.

Although designed primarily to provide information for the comparisons that I have already described, the gene mutation study has given some information on the nature and amount of total damage to be expected. First with regard to the nature of the damage, considerably more than one-half of all the radiation-induced mutations obtained from the seven genes studied in detail have proved to be recessive lethals—that is, when an offspring inherits the mutation from both parents, it will die. It seems unlikely that this result is biased in the unfavorable direction, because before the experiments were started it was not known whether or not any of the seven genes chosen for the study could mutate to lethals.

Representative HOLIFIELD. Will you explain that term "recessive lethal"?

Dr. RUSSELL. That is when an offspring inherits the mutation from both parents, it will die. When the organism inherits it from one parent, it won't die. It seems unlikely that this result is biased in the unfavorable direction, because before the experiments were started it was not known whether or not any of the seven genes chosen for the study could mutate to lethals. In other words, the sample of genes chosen was not chosen for production of this type of mutation at all. It was not known whether they could even mutate to lethals.

Some information has been obtained on the time at which these lethals kill the individuals who inherit them from both parents. All the lethals that have occurred at one gene locus kill the offspring at about weaning age. The lethals obtained from another gene locus apparently vary considerably in their time of killing. More information is needed on this point, but it is already clear that a large proportion of the lethals already studied kill at times that would be considered tragedies in human experience. Most of the lethals found do not belong to the category of lethals in which death occurs so early in development of the embryo as to pass unnoticed.

It was anticipated that some, perhaps most, of these lethals would have some deleterious effect when inherited from only one parent. Such deleterious effects have already been observed for some of the lethals. It was expected that these deleterious effects might be observable only in a statistical sense in populations. However, in some cases the deleterious effect of a lethal when inherited from only one parent is large enough to be detectable in the individual. It should be mentioned that a lethal which has a deleterious effect when inherited from only one parent will express its damage in the population far more frequently in this way than in the more drastic effect that occurs when it is inherited from both parents.

As has been mentioned, the results of the gene mutation study can be used to estimate total mutation rate. One way of doing this is to take the information from Drosophila on the ratio of total mutation rate to mutation rate at specific genes and use this to make the calculation of total rate on the basis of mutation rates at specific genes in the mouse. This method of estimation was used in the report of the Genetics Committee of the National Academy of Sciences. Dr. Muller used it before, and I think Dr. Crow incorporated this idea in his calculation this morning.

More direct methods of measuring the overall genetic damage are desirable and one of these is the second of the two methods I should like to describe in this testimony. This method was suggested by the evidence that had begun to accumulate that there are slight dominant deleterious effects of mutations formerly regarded as recessive. Two lines of such evidence came from our work on mice. One has already been mentioned, namely, that at least some of the recessive lethals obtained have some dominant effects. The second line of evidence came from the large populations of animals examined for gene mutations. In that study, the offspring were kept to 3 weeks of age, and it was found that survival to this age was slightly lower in the offspring of irradiated males than in the controls. As a result of these findings, and of evidence from other organisms, it began to seem probable that other effects might be detectable in the first generation offspring of irradiated mice. Shortening of life was chosen as an effect that might reveal, as a statistic of a population, the presence of a variety of minor weaknesses which individually could not be easily detected. Pilot studies on longevity in the descendants of irradiated mice were accordingly started. The results from the first of these were presented before the National Academy of Sciences a few weeks ago, and have just been published.

Dr. Crow did not discuss these this morning. I asked him whether be did not believe them. He said "No," he had left them for me to discuss, and I think he felt it was more appropriate as they are quite recent data.

Representative COLE. When you say that he responded to the question of whether he believed them or not, by saying "No," did he mean he did not believe them, or that was not the reason?

Dr. RUSSELL. That was not the reason.

Senator ANDERSON. Are you going to discuss them?

Dr. RUSSELL. I have submitted for the record the detailed account of this which has been published. I shall be glad to read from this or cite any parts.

Senator ANDERSON. You were here the other day when I read briefly from the news story.

Dr. RUSSELL. Yes.

Senator ANDERSON. Is that what it is based on?

Dr. RUSSELL. Yes.

Senator ANDERSON (reading):

> Neutron radiation from atomic bomb can shorten the life of a man's children.

This is quoting from you.

> Offspring from a man exposed to such radiation will have their lives shortened on the average of 20 days for each unit of radiation their father has received.

Dr. RUSSELL. Yes.

Senator ANDERSON. We have been talking about receiving up to 400 roentgens. We multiplied that out and came to 8,000 days, and that was 22 years.

Dr. RUSSELL. There are two major qualifications which I believe were included in some of the news reports. I remember that they were in the full Science Service report. These qualifications are first, that we were dealing with neutrons which are probably more effective than X-ray or gamma rays. The other is that we were not dealing with the spermatogonia. In this experiment we deliberately tried to

maximize everything to get the effect on scale so that if there were any such effect, we would detect it. In this we used the maturing germ cells which for mutation rates are at least, shall we say, 2 to 4 times as sensitive as the spermatogonia for straight gene mutation rates. It is possible they are even more sensitive for this effect, but this we don't know yet. A more conservative conclusion from the results is the one I put in the discussion. I think at the present time we would be justified in estimating the effect on the first generation of offspring to be between one-tenth and equal to the effect on the shortening of life of the exposed individuals.

I think it is perhaps fairer to talk about this than the 20 days which, in several respects, is maximized.

Senator ANDERSON. I know. Apparently this article in the Science Service was quoting you. It said, "Dr. Russell found, however, that 'There was a significant effect of radiation on the length of life of the offspring.' In the male mice"—I am sorry, the quotation marks were dropped there. It says the life of the offspring was shortened 0.61 days, I assume, for each unit of radiation.

Dr. RUSSELL. Yes.

Senator ANDERSON. Does that refer to mice?

Dr. RUSSELL. Yes; that refers to mice. A unit of radiation in this case was the rep of neutrons rather than the r. for X-rays or gamma rays. There should be some reduction factor. This is not known. But perhaps it is a factor of about 2.

Senator ANDERSON. Quoting further from this article—

> Using this information for human beings, Dr. Russell has figured out that a man's life will be shortened from 5 to 35 days for each unit of radiation received by the father.

Is that a correct statement?

Dr. RUSSELL. That is correct. This again refers to the conditions of this experiment, which are not the conditions of human exposure.

Representative PRICE. What was the unit of radiation?

Dr. RUSSELL. The unit of radiation was a rep.

Representative COLE. A rep is what fraction of a roentgen?

Dr. RUSSELL. A rep is equivalent to the roentgen in its physical effect on tissue, but is more effective in its biological effect for most types of biological effect.

Representative HOLIFIELD. Are you drawing the line between the neutrons which would be received close to a bomb explosion, and the gamma and other types of rays that would be received far away?

Dr. RUSSELL. That is partly correct. There would be a mixture of neutrons and gamma rays. Our experiment was set up only to measure the neutron effects. The animals were shielded behind lead to filter out the gamma radiation. The experiment, as I emphasized in the paper, was not conducted primarily for this particular study. We got the data which showed this rather striking effect, so reported it. We are now conducting experiments designed deliberately to measure the shortening of life. But we felt we should not wait for the completion of these before reporting the effect already observed. However, the qualifications are important, and I must insist that they be understood. Otherwise if you just take the 20-day figure and do not put the qualifications on it, it is probably a much larger effect than would be obtained under normal conditions of human exposure.

Senator ANDERSON. It says—

This shortening of life in the immediate offspring, Dr. Russell warns, in the Proceedings of the National Academy of Sciences, will turn out to be of a magnitude that will warrant serious consideration as a genetic hazard in man.

Is that a correct quotation?

Dr. RUSSELL. That is a correct quotation. I have the same statement in this testimony I was about to read. I do believe that, in spite of the qualifications, this is still a serious effect. I think the 20-day figure is too high. For conditions of human exposure, at least by our present estimates, it should be somewhat lower, but I still think it would be serious.

Senator ANDERSON. From 5 to 35 is a safe figure.

Dr. RUSSELL. No; that would still be under these conditions. If you want a definite figure, I would say at the present time something more like 1 to 10. These are guesses. It is much safer to publish the data you have and put the qualifications on them, than to make guesses about the quantitative values of the qualifications.

Senator ANDERSON. It was pointed out that it could take up to 10 and with 400 r, that would produce 4,000 days which would be 11 years, which would be quite a shortening of human life.

Dr. RUSSELL. Yes.

Senator ANDERSON. Which would be serious.

Dr. RUSSELL. Yes, 400 r. would be serious. To conclude the statement on this, some of which we have already discussed: Our data on this effect are not yet as extensive as we should like. However, though the first sample studied was small, it was sufficient to yield a statistically significant effect which appears to be large and, therefore, of general importance. It should be kept in mind that some of the conditions of this experiment were set up deliberately to increase the chance of obtaining a detectable effect. However, since the effect observed appears to be so large, it seems likely that, even when allowance is made for the conditions of human radiation exposure, shortening of life in the immediate descendants will turn out to be of a magnitude that will warrant serious consideration as a genetic hazard in man.

In conclusion, I should like to emphasize one point in addition to those already expressed in this presentation, a point that Dr. Crow mentioned this morning. The layman tends to think of mutations in dramatic terms as gross monstrosities occurring in the first generation following radiation. Our studies on mice confirm the results obtained from other organisms showing that these are exceedingly rare types of genetic damage. Mutations which cause slight deleterious effects are far commoner.

Before summarizing these points, I should like to say a word or two about extrapolation from mouse to man, the question which the chairman raised earlier. There are, of course, risks in this. I think some of the earlier testimony, from medical workers and others, overemphasized the difficulty. Yet, applying animal results to man is exactly what is done all the time in medicine in the testing of drugs and so on. Others who objected to the extrapolation of mouse mutation data to man had no qualms about extrapolating the rate-doubling dose. I personally would feel safer about extrapolating the induced mutation

rate in mammals from mouse to man than about extrapolating the rate-doubling dose itself.

I will certainly agree that there are some risks in extrapolating on the quantitative points, that is, the actual mutation rates. I think the risks are very much less in extrapolating with regard to the principles or relations between various points. Such principles as the lack of recovery with time, the shape of the dose curve, the relative frequencies of different types of mutations and so on, it seems to me, can be extrapolated with fair confidence.

Some of these are principles that cannot be extrapolated from fruit-flies, because of the biological difference between flies and mammals. Other principles, of course, which were very well established in flies can be extrapolated to mammals. I have been stressing in this report those which I think needed work directly on mammals.

I have listed nine points of this testimony in summary.

1. Present data indicate that mouse genes are approximately 15 times as sensitive to the induction of mutation by radiation as fruit-fly (*Drosophila*) genes.

2. There is no recovery from genetic damage with time after irradiation. This principle has now been established by direct experimental evidence on the material in which investigation was badly needed, namely, the immature germ cells of a mammal.

3. Mutation rates following a dose of 1,000 r. show a significant departure from proportionality with the rates obtained at lower doses. Mutation rates at 300 r. and 600 r. do not as yet show any significant difference from proportionality. However, more data are needed, especially in view of the observed departure from proportionality at the higher dose.

4. Different genes show widely different radiation induced mutation rates.

5. More than one half of the radiation-induced mutations obtained have proved to be recessive lethals—that is, when an individual inherits the mutation from both parents it will die.

6. Most of these lethals do not belong to the category in which death occurs so early in development of the embryo as to pass unnoticed. On the contrary, they kill at times that would be considered tragedies in human experience.

7. The deleterious effect of some lethals when inherited from only one parent is large enough to be detectable in the individual.

8. Rough estimates of the total mutation rate expected from a given dose of radiation have been made from the sample of mouse genes studied.

9. More direct methods of measuring overall genetic damage are being used. The first results from one of these show a significant shortening of life in the first generation offspring of irradiated male mice.

In presenting this statement today, I hope I have given the committee a useful picture of at least a part of the basis on which conclusions on the genetic hazards in man have been reached. Thank you.

Representative HOLIFIELD. Thank you very much, Dr. Russell. I think you have given a very important statement here. It certainly seems to prove that in the case of genetics, at least, any radiation is harmful to the genes.

Are there any questions?

Representative COLE. Mr. Chairman, I do not recall that the doctor responded to your inquiry with respect to the relative sensitivity of human cells in comparison with mice.

Dr. RUSSELL. I think there is some question about extrapolating the exact mutation rate from mice to man. I think there is less risk in extrapolating the principles that we have established in our experiments. However, I think the best we can do at the present time is to use the organism closest to man on which we have data, and that is the mouse. I personally would feel less worried about extrapolating the induced mutation rate from mouse to man than I would about some other extrapolations that have been made.

Representative COLE. Then you are unable to give us any estimate of the ratio between fruitfly, mice, and human being?

Dr. RUSSELL. We have the ratio between mouse and fruitfly. This is 15 to 1. With regard to man and mouse, we have no information other than such as has been discussed. It appears from the Hiroshima and Nagasaki data that man is probably not greatly more sensitive than the mouse. Otherwise more damage would have been observed. However, the results on the Japanese study are fully consistent with a rate as high as has been found in the mouse. I would, like Dr. Muller, quarrel with some of the final conclusions of the report on the study in Japan. I would quarrel with one statement there about this particular point. Even so, in the report on this study, the authors do not raise any question about man being less sensitive than the mouse to radiation-induced mutation. I think the data are not extensive enough to rule out the possibility that he could be more sensitive than the mouse.

Chairman DURHAM. Your second summary point you base on how many years of research, that is, there is no recovery from genetic damage?

Dr. Russell. The first indication came after, I would say, about 2 years' work after the basic experiments were started. This has been further confirmed by more extensive data as time has gone on.

Representative HOLIFIELD. This refers to your experiments on mice.

Dr. RUSSELL. Yes.

Representative HOLIFIELD. Thank you very much, Dr. Russell. We will place your article, Shortening of Life in the Offspring of Male Mice Exposed to Neutron Radiation From an Atomic Bomb, in the record at this point.

(The material referred to follows:)

[Reprinted from the Proceedings of the National Academy of Sciences, vol. 43, No. 4, pp. 324–329, April 1957]

SHORTENING OF LIFE IN THE OFFSPRING OF MALE MICE EXPOSED TO NEUTRON RADIATION FROM AN ATOMIC BOMB[1]

By W. L. Russell, Biology Division, Oak Ridge National Laboratory, Oak Ridge, Tenn.

Communicated by Sewall Wright, January 31, 1957

Introduction.—Only in recent years has evidence begun to accumulate that there are slight dominant deleterious effects of mutations formerly regarded as

[1] Work performed under Contract No. W-7405-Eng-26 for the United States Atomic Energy Commission.

recessive.[2-5] The results to be reported here, and our earlier work on mice,[4, 6, 7] indicate that such effects may add up to an important part, perhaps the most important part, of the genetic hazards of radiation in man. The evidence from the earlier work on mice is that appreciable deleterious effects of radiation become manifest in the first-generation offspring. This evidence is of two kinds. First, work on radiation-induced mutations at specific loci in spermatogonia has shown that among the recessive lethals, which comprise more than one-half of all the mutations recovered, many have dominant deleterious effects which, even for individual mutations, are sometimes large enough to be detected easily. Second, overall population damage was found in the large numbers of animals that are raised as far as 3 weeks of age in the specific loci studies. In all such experiments carried out, the survival to 3 weeks of age is significantly lower in the offspring of irradiated males than it is in the controls. (It should perhaps be pointed out that neither of the above effects, nor the effect reported in this paper, is the result of what the geneticist usually refers to as "dominant lethals," which are major chomosomal aberrations that cause early death of embryos and which, as has been pointed out elsewhere,[6] are probably not an important hazard.)

Our earlier work that showed a significant effect on survival to 3 weeks of age in the offspring of irradiated males led us to expect that there would be measurable deleterious effects later in life. The data reported here show that such is indeed the case. These data furnish a third kind of evidence of first-generation damage and perhaps the most striking one. They were obtained as a byproduct of another investigation, and they are not as extensive as we should like. However, they are the only data we have on this subject that were collected under the expensive and difficult conditions of a field test of a nuclear detonation. Furthermore, although the sample was small, it was sufficient to yield a statistically significant effect which appears to be large and, therefore, of general importance.

Materials and methods.—The material used in the present longevity study was the byproduct of an investigation of the relative effectiveness of neutrons from a nuclear detonation and from a cyclotron in inducing dominant lethals in the mouse.[8] In order to reduce the gamma component of the radiation to a proportion that would not appreciably interfere with the estimation of neutron effects, the animals were shielded with lead. The exposure chambers available were lead hemispheres of 7-inch wall thickness and 14-inch inside diameter. Young adult hybrid males, obtained by crossing inbred 101 strain females with inbred C3H strain males, were exposed inside the hemispheres placed at various distances from the detonation. Control males were placed in hemispheres 2 days before the detonation and for a length of time approximately the same as that required for the exposed animals. Further experimental details are described in the report of the earlier work.[8] One day and a half after the detonation, each male was placed with four adult untreated females of the same hybrid strain. At 18½ days after irradiation each surviving male was placed with a new group of 4 females. Most of the females that became pregnant were killed at a late stage of gestation for the dominant-lethal study. However, since the number of pregnancies turned out to be more than adequate for the dominant-lethal experiment, several of the females were allowed to come to term. It was the offspring of some of these females that were saved for the longevity study described here. All these animals came from matings made from 19 to 23 days after irradiation. A few animals died before weaning, and these were not included in the data reported here. At weaning age the sexes were separated and the animals grouped, so far as possible, six to a cage. They were kept in the same grouping throughout their life span. They were checked at least twice weekly for deaths. Only one animal died at less than 1 year of age, indicating that the conditions under which the animals were kept were good.

The total (neutron plus gamma radiation) dose inside each lead hemisphere was measured, as described in the earlier publication,[8] by means of tissue-

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[2] C. Stern and E. Novitski, Science, 108, 538–539, 1948.
[3] H. J. Muller, J. Cellular Comp. Physiol., 35, suppl. 1, 205–210, 1950.
[4] W. L. Russell, Cold Spring Harbor Symposia Quant. Biol., 16, 327–336, 1951.
[5] C. Stern, G. Carson, M. Kinst, E. Novitski, and D. Uphoff, Genetics, 37, 413–449, 1952.
[6] W. L. Russell, in Radiation Biology, Vol. I, ed. A. Hollaender (New York: McGraw-Hill Book Co., 1954), chap. xii.
[7] W. L. Russell, Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, 11 (New York: United Nations, 1956), 382–383, 401–402.
[8] W. L. Russell, L. B. Russell, and A. W. Kimball, Am. Naturalist, 88, 269–286, 1954.

equivalent ion chambers designed for this purpose at short notice.[9] Subsequently, extensive testing and recalibration of these chambers[10] has led to a revision of the original dose estimates. The doses reported in the present publication are the revised estimates. As was reported earlier,[8] attempts to measure the gamma component of the radiation by means of film dosimeters left a large uncertainty as to the size of this factor. Later tests have been made in which both ionization chambers and chemical dosimeters were used to measure the gamma component inside the lead hemispheres when these were exposed to fission neutrons. According to the latest information,[11] these tests indicate that the gamma-radiation exposure in our experiment was almost certainly less than 10 percent of the total dose.

Results.—The median and mean lengths of life, together with the number of animals, for each dose group are given in table 1. An analysis of variance showed that neither grouping in cages nor sex had a significant effect on length of life. It seems likely that larger samples would show some effect of both of these factors, but as there was no significant effect of them in the present experiment, the data were pooled.

TABLE 1.—*Length of life in the offspring of male mice exposed to neutron radiation 19–23 days before mating (deaths before weaning age excluded)*

Total dose to parent (rep) [1]	Number of offspring	Median length of life of offspring (days)	Mean length of life of offspring (days)
0	103	823	792
31	50	741	754
71	5	717	699
118	22	739	723
136	8	666	688
186	2	756	756

[1] Includes some gamma radiation, estimated to be less than 10 percent of the total dose.

To test whether there was a significant effect of radiation on the length of life of the offspring, the means were fitted to a straight line by the method of weighted least squares. This gives an intercept of 786 days and a slope of -0.609 ± 0.238. Since the residual variance is less than the within-subclass mean square, there is no evidence of nonlinearity over the dose range tested. Even if the true shape of the curve is nonlinear, it will be conservative, in making the test of significance, to assume linearity. The larger mean square was used to compute the variance of the slope, and a two-sided t-test shows that the slope differs significantly from zero at the 1-percent level. If one is willing to accept a one-sided t-test as more appropriate, the significance level is 0.5 percent. Thus there is strong evidence of shortening of life in the offspring of the exposed males.

Discussion.—It is noteworthy that a significant shortening of life was detected in spite of the small sample and the considerable genetic variability that must have been present in a population that was the F_2 of a cross between inbred strains. Furthermore, the weighted mean dose received by the exposed fathers was only moderate, being less than one-sixth of the 30-day median lethal dose as measured from other animals of the same strain exposed under the same conditions at distances closer to the same detonation. While it is true that certain features of the experiment, which will be discussed later, tended to maximize the shortening of life, nevertheless the result observed appears to be so large that it seems quite possible that shortening of life is an effect that might be detectable in studies of the offspring of exposed parents in human populations.

[8] See footnotes, p. 1097.
[9] C. W. Sheppard and E. B. Darden, appendix to J. S. Kirby-Smith and C. P. Swanson, Science, 119. 42–45, 1954.
[10] C. W. Sheppard, M. Slater, E. B. Darden, Jr., A. W. Kimball, G. J. Atta, C. W. Edington, and W. K. Baker, Radiation Research (in press).
[11] G. S. Hurst, personal communication.

TABLE 2.—*Shortening of life in the offspring of fathers exposed to neutron radiation 19 to 23 days before mating—Observed result in the mouse and extrapolation to man (deaths before weaning age excluded)*

	Mouse	Man
Point estimate	0.61 day/r. e. p. to father	20 days/r. e. p. to father.
Lower 95-percent confidence limit	0.14 day/r. e. p. to father	5 days/r. e. p. to father.
Upper 95-percent confidence limit	1.07 days/r. e. p. to father	35 days/r. e. p. to father.

In view of the lack of information on this subject, and specifically the fact that no data of this nature were ready for consideration prior to the writing of the 1956 report of the National Academy of Sciences Committee on Genetic Effects,[12] it is desirable to consider what the present data might indicate when they are extrapolated to man. Taking the estimate obtained from the curve fitted to the mouse data, and assuming that the shortening of life in man would be proportional to this, gives, on the basis of a 70-year length of life in man, the figures shown in table 2. It should be kept in mind that the results were obtained from neutron irradiation. The relative biological effectiveness of neutrons for this effect is not known, but it seems likely, from other data on mutations, that gamma and X-radiation would be less effective than neutrons. It should also be emphasized that the effect observed here is probably a maximum one, since the offspring were obtained from matings made between 19 and 23 days after irradiation. Our data from experiments on mutations at specific loci [13] indicate that the sperm utilized in matings made within this time interval would have been derived from cells in a sensitive stage of gametogenesis at the time of irradiation. From approximately 2 to 4 times as many mutations are recovered from this stage as from the spermatogonial stage, which is the important one so far as radiation hazards in man are concerned.[6] It is also possible that the spectrum of mutations from irradiated spermatogonia would be qualitatively different and, conceivably, less effective in shortening life. However, there is no direct evidence of this, whereas there is evidence from our specific loci studies that some mutations induced in spermatogonia have, even individually, a dominant effect on length of life that is detectable. To summarize this paragraph, it should be remembered that the estimates given in table 2 are based on neutron irradiation of a postspermatogonial and sensitive stage in gametogenesis and that X- or gamma irradiation of spermatogonia would almost certainly produce a smaller effect.

Another way of considering the magnitude of the observed results, so far as its human implications are concerned, is to compare the shortening of life in the offspring of irradiated fathers with that in the irradiated individuals themselves. The data on shortening of life of the males exposed to this same detonation will be presented in detail elsewhere. Briefly, the percentage shortening of life of these animals, based on 24 controls and 128 exposed animals, is 0.078 percent per r. e. p.

The present data, expressed in the same form, give 0.077 percent shortening of life in the offspring for each r. e. p. received by the father; that is, approximately as much effect as on the exposed individuals. Thus the best estimate from our present data is that, for neutron irradiation of the sensitive stages in spermatogenesis, the shortening of life in the offspring of irradiated males will be similar in magnitude to that in the exposed individuals. Again, the effect from irradiation of spermatogonial stages would probably be less. Whether the *ratio* of effect in offspring to effect in exposed individuals will be different for X- and gamma rays from that observed for neutrons will, of course, depend on whether the relative biological effectiveness of neutrons is different for the effect on the offspring and the effect on the exposed individuals. Present, incomplete data on these points give no grounds for expecting that the ratio of effect in offspring to effect in exposed individuals will be less for X-rays than for neutrons. Weighing the evidence reported here, and making

[12] The Biological Effects of Atomic Radiation: Summary Reports (Washington: National Academy of Sciences, National Research Council, 1956).

[13] W. L. Russell, USAEC Unclassified Report ORNL-2155 (Washington; Office of Technical Services, Department of Commerce, 1956).

some allowance for the many uncertainties, it seems reasonable to predict that, even under the conditions of radiation exposure in man, shortening of life in the offspring of irradiated fathers will be between 10 and 100 percent of the shortening of life in the exposed individuals themselves. It should be remembered that this excludes an additional effect on the offspring; namely as measured in the mouse, death before weaning age. Also, and more important, since the shortening of life is probably the result of mutations with slight dominant effects, the damage would not end with the first-generation offspring but would, to a certain, and probably large, degree, be transmitted to later generations.

Summary.—Length of life in the offspring of male mice exposed to moderate doses of neutron radiation from a nuclear detonation is shortened by 0.61 day for each r. e. p. received by the father over the dose range tested. This figure excludes death before weaning age. The 95-percent confidence limits are 0.14 and 1.07 days per r. e. p. Extrapolating to a proportional shortening of life in man gives 20 days per r. e. p.received by the father as the point estimate and 5 and 35 days as the 95-percent confidence limits. The offspring were obtained from matings made from 19 to 23 days after irradiation and, therefore, represent the effect of irradiation on germ cells in a postpermatogonial and sensitive stage of gametogenesis. It is probable that irradiation of spermatogonia (the stage that is important from the point of view of human hazards) would give a somewhat smaller effect. However, since the present data show an effect on the offspring which is as large as the shortening of life in the exposed individuals themselves, it seems likely that, even when allowance is made for the conditions of human radiation exposure, shortening of life in the immediate descendants will turn out to be of a magnitude that will warrant serious consideration as a genetic hazard in man.

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Representative HOLIFIELD. Before we have our discussion, we are going to ask Dr. Hardin Jones, to give his presentation.

STATEMENT OF DR. HARDIN JONES, UNIVERSITY OF CALIFORNIA RADIATION LABORATORY[a]

Dr. JONES. Thank you.

Representative HOLIFIELD. Dr. Jones, this is your prepared statement?

Dr. JONES. Yes.

Representative HOLIFIELD. It will be accepted for the record. Are you going to summarize it?

Dr. JONES. Yes.

(The statement referred to follows:)

STATEMENT OF HARDIN B. JONES, PROFESSOR OF MEDICAL PHYSICS, PHYSIOLOGY; ASSISTANT DIRECTOR, DONNER LABORATORY, UNIVERSITY OF CALIFORNIA

My field is the physiological basis of human health problems. In research I have contributed appreciably to: (1) an understanding of some metabolic disturbances associated with heart and vascular disease; (2) evaluations of the

[a] Donner laboratory, division of medical physics, University of California, Berkeley 4, Calif. Physiology, Los Angeles, Calif., June 11, 1914. Bachelor of arts degree, California, at Los Angeles, 1937; master of arts degree, California, 1939; fellow, 1940–45; doctor of philosophy degree (physiology), 1944. Instructor, medical physics and physiology, California, 1946–47; assistant professor, 1947–49; associate professor, 1949–54; professor, 1954; assistant director, division of medical physics, 1948; research associate, radiation laboratory, 1947–. With Atomic Energy Commission, 1944. Cancer Society; Historical Science Society; Physiology Society. Radiobiology; metabolism, physiology of gas exchange; regional blood perfusion; biological effects of radiation; eipopistein metabolism and physiological change with age (from American Men of Science).

cancer problem and especially human cancer therapy; (3) the study of aging in that I have been able to construct a general explanation of aging that can be subjected to experimental study and which has been useful in evaluating factors contributing to improvement or deterioration of health and lifespan. I have been especially interested in estimation of effects of radiation in man.

SUMMARY

In natural radiation exposure and the extent to which it is increased by fallout, we are dealing with effects that are minute compared to other factors of importance to the health of man. Estimation of the magnitude of these small effects depends on determination of the dosages to be expected and the biological responses associated with these doses. This paper deals with the latter factor.

There is no direct evidence that doses as small as these produce any harmful effects; some have therefore jumped to the conclusion that they produce none. There is much evidence that a variety of undesirable effects occurs in cells, tissues, and whole organisms when exposed to doses large enough to establish either a positive or a negative result with reasonable confidence. Some of that evidence is presented here. Since it indicates an effect proportional to dose in the known range, the gap in the unknown range with which we are concerned is filled by arbitrarily assuming that the same proportionality holds. The principal justification for that assumption as a working hypothesis is that to ignore a small factor of risk, if it is real, may be costly if that small risk is applied to a very large population.

The similarity of the effects of all harmful processes, including aging, disease, and irradiation, has been expressed in terms of a concept involving an equivalence between units of the damaging agency and effective increase in physiological age over the chronological age. Factors obtained in this way may be used to estimate the cost in health and lifespan of any of the circumstances tabulated. As a present step, while recognizing the uncertainties of the assumptions on which it is based, the factor for irradiation may be used to compare costs and gains with respect to the atomic energy program. Much more effort should be devoted to the accurate determination of the quantities involved.

COMMENTS TO CONGRESSIONAL COMMITTEE, JUNE 1957

I am trying to evaluate every circumstance which can add up to or subtract from average human health and useful life span. Some of my colleagues jokingly refer to me as a prophet-of-doom because many of my estimates are best explained in measures of morbidity or mortality risks. This is how we must quantify human experience in order to evaluate factors that may truly be worth accepting or avoiding. It is not enough to know something is good or bad for us; we must express this knowledge in quantitative terms in order to be able to compare costs in health or life span with gains in other directions. Very fbrequently we accept circumstances having a known and measurable risk, such as using the bathtub, crossing the street, overeating, or riding a device for transportation, simply because doing so gives us some definite pleasure or gain that is reasonably more than the estimated cost in risk of harm. However, we are conscious of risk, in that bathtubs now are made with nonslip bottoms, streets have crosswalks and traffic signals, and our mechanical devices are made as safe as is thought to be warranted economically.

Since we are concerned here with the evaluation of radiation effects upon humans, I would like to tell you in some detail about interpretations I believe are reasonable estimates of this problem. I would like to separate information that is generally accepted from that based upon reasonable but debatable arguments; and I shall also use the evident uncertainty regarding answers to critical problems both as a caution concerning current interpretation of results and to indicate needed critical information. My concern for estimation of even minute effects places me at times in awkward situations—when quoted out of context, I may appear to be constructing "scare" stories. This I am not doing. Effects such as I have estimated are too small to be measured directly in individual humans affect estimated numbers of people only when large populations are subjected to the risks in question. Better estimates should eventually be made for each problem we can now discuss, but any estimate will have to depend on statistical studies of large populations.

I state definitely my summary belief that estimated effects of radiation from recommended occupational exposures and from fallout are minute costs compared to the gain to man of abundant useful energy and the extraordinary advantage we have gained for free nations by the awesome presence of effective atomic weapons. I would not, however, assume that, this formula for peace may continue to work indefinitely. Just as defense strategy, economic costs, possibilities for international agreement and other political, economic, and military factors are constantly reevaluated as circumstances change, the biological costs of the atomic age must be reappraised continually.

We all recognize that the world must find more energy to be used for human betterment. Many facts attest to this. Ten percent of the world's population is now using 90 percent of available energy and everyone—whether he already has much or little—wishes to have more of the material comforts made possible through the use of mechanical power. Populations everywhere are increasing, knowledge is increasing, need is increasing. In a world of dwindling available energy, humans very shortly would face a crisis of relative poverty. The availability of atomic energy promises instead a new age for man, marked by extraordinary technical progress that can be the result of both growth of knowledge and increase of efficiency. These asset values are well known; the liabilities are less definitely established. At the moment, we need to find out with much greater precision many basic facts of effects of radiation in man, the biologic costs of radiation exposure, how its effect can be minimized or avoided. It is quite evident that the problems that the atomic age has brought us are more complex than had originally been visualized. At the same time, it is true that the gain through atomic energy is considerably greater than was foreseen even a few years ago. While the physics, chemistry, and engineering of development of atomic energy needed and deserved the great attention they received, it is now evident that the factor of human tolerance of radiation needs further attention because of lack of precise knowledge regarding critical radiation effects. In my opinion, the next stage should emphasize a greater relative effort in this part of the problem, because it is imperative that we reduce speculation and establish estimates of biologic effects with more certainty. At the moment, in spite of the shortcomings of current information concerning these important problems, there seems to be no difficulty in continuing the development of atomic energy and at the same time accepting very cautious limits of radiation exposure.

Much of the information concerning radiation exposure as a problem to man developed almost wholly from the great sponsorship of this field of scientific inquiry by the Atomic Energy Commission. Many of the points currently under critical discussion, such as mutation rate associated with very low radiation doses or the quantitative estimation of fallout, might have been neglected had not those in authority shown a responsible comprehension of these problems and undertaken to support unusual costs as a part of the overhead of development of useful energy.

Now, I would like to present to you an outline of some of the facts and arguments that seem to be of special importance in current estimation of the effects of radiation in man. I wish once again to caution that, in these discussions, we are considering radiation risks that are very much like other commonplace risks to which we have long been exposed, such as driving automobiles—indeed, these newer risks are usually very much smaller.

Everyone wishes that risks to health and life might be reduced to zero. If that could be done, we would all live forever without growing old! Every moment of life, we face certain average risks of mishap—rarely of improvement. These average risks are the sum of many contributing factors, some large, some small. In individual affairs, very small risks are frequently regarded as no risk at all. On the other hand, in a popular presentation of hazards with a view toward emphasizing their importance, any risk that is greater than zero can be presented in a way that attracts concern, even though it may affect only one person in the entire world. A widespread exaggeration of hazards would, of course, have prevented any technical progress. I believe it would be an equally great mistake to dismiss, as being equivalent to zero, those small radiation effects we are now considering. It is true that most of these estimated effects are exceedingly small, and I can give many examples of ordinarily accepted circumstances that modify health to a much greater extent. The problem of differences of opinion derived from the same body of facts, is really the problem of deciding "how small is small," in the light of whatever is to be gained.

Similarity of radiation effect in mammals

In estimating radiation effects in humans, there is a striking similarity to observations established in experimental studies with animals. Cancer induction, radiation sickness, and genetic change occur at equivalent exposures. The average lethal doses are within the same range. Exposures causing burns or tissue damage are comparable. In general, this similarity would be expected from our understanding of the chemistry and physics of radiation effects and from the very similar structure of body cells of mammals of widely varying species. As an approximate truth, damage that is incurred by radiation exposure becomes more apparent as time elapses, so that for many effects, such as the induction of cancer, there is a latent period during which the effect of radiation is rarely observed. Roughly, the latent period for the induction of cancer is relatively long or short, depending upon the relative life span of the species. Thus, greatly simplified, the problem of making comparisons of radiation effect between, say, the mouse and man is a question of the relative time scale. Very similar relations in development of other diseases with time are observed between man and other mammals when time is expressed as a fraction of the life span. In such a comparison, the biology of aging in the mouse is remarkably similar to that of man, if one estimates 1 day's life of the mouse to be equivalent to 1 month's life of man.

Life-span-shortening effects of radiation

The conclusion from all studies of animal populations exposed to radiation in the range of 100–1,200 roentgens of whole-body exposure is that these dosages shorten life span. The shortening of life span of laboratory animals by these exposures is an established fact for either acute of chronic exposure. It is, however, distinctly unproven whether these effects apply on a relative basis to man, though the consistency for the many species of mammals tested lends great plausibility to this hypothesis. If we do make such an assumption, as I have done on several occasions since 1953, one obtains a number that suggests that the average human life span loss at a dose of 100 roentgens may be about 500 days, or about 1⅓ years. We can be certain that some number exists defining radiation induced life-shortening in man, but we cannot state the number with confidence. It can at best only be approximated. It is very important that we do learn what its value is, even though great effort and cost are attached to finding it. In the absence of definite knowledge, we must do the best we can to estimate this quantity.

In discussing this question, several additional considerations arise. Is life span loss proportional to radiation exposure? Most evidence can be interpreted in this sense, but not exclusively. It appears possible that life span loss may be somewhat greater than the above estimate at very large single doses, even though proportionality may hold in the small-to-moderate dose range. For very small chronic exposures, observation of the effect becomes very difficult and it is possible that no effect on life span may exist; however, current data are truly inadequate to test either hypothesis. Nevertheless, in mice, for example, a significantly enhanced and early appearance of tumors has been caused by exposure to 0.1 roentgen per day, the lowest dose yet studied for life span effects; and although no comparison with man can be made on this basis, it may be inferred that some effects occur at low doses. Even though the studies that enable us to speculate upon effects in this dose range have been extraordinary undertakings of technique and labor, we still need to have much more information of this kind to establish the bearing of radiation exposure upon life span loss. One of the most important points concerns whether exposure rate is a factor in the lower dose ranges, since it determines whether one may express this radiation effect in terms of loss per roentgen of radiation exposure. I feel that, with respect to large doses of radiation, the majority of evidence supports a concept that life-span loss is proportional to exposure; and, in the absence of human data on low doses and low rates of accumulation of dosage, it seems safest to assume that the same relation holds at these levels. On this basis, we might say that 1 r. of whole body radiation is perhaps equal to becoming 5 days older. In the absence of definitive information on radiation-induced aging in man, I believe it is reasonable to use this tentative number, even though it may be subject to revision as more certain information is acquired. Study of radiologists and atom bomb survivors may provide a better answer. In this regard there are conflicting opinions and reports. It has been evident for several years that radiologists have 6 to 10 times more leukemia than their expected rates.

Some additional studies of life expectation suggested shortened life span for radiologists. Dr. Shields Warren recently estimated that average age at death was 6 years less for radiologists than for other physicians. Thus, one might infer that radiologists have about 6 years shorter life span. Lewis has recently reviewed this conclusion and reports that, when the distribution of ages among radiologists is considered, the death rate for that group as a whole is no worse—and possibly better—than for physicians in general. However, by using both the individual ages of deaths provided to me by Dr. Warren and the number of registered radiologists, my colleague, Grendon, and I have constructed the age-specific death rates for radiologists. They are found to have the same death rate risk as the general population at ages under 60; but over 60, the death rate is about twice as high as expected. I had come to similar tentative conclusions several years ago by estimating approximate death rates of radiologists from obituary notices in the professional journals. I think it is reasonable to conclude tentatively that radiologists have a higher than expected death risk at older adult ages. This kind of evidence of radiation effect upon man is so important that a relatively great effort should be employed to make a precise study of individuals with known occupational exposures. It is true that the effects are probably very small but they are effects we need to know with relative accuracy, and such study is directly to the joint of estimating the effect of accumulating radiation exposures in humans.

What about dosage measurements in human studies of radiation exposures

Another crucial element in estimation of radiation effect in man is determination of exposure dose. Even though I believe that radiologists may have slightly shortened life span at older ages, there is considerable difficulty in correlating this estimate with a measure of exposure radiologists may have experienced to produce this aging. We simply do not know the average or individual exposures, and direct estimates of exposure can be stated only within rather absurd limits such as 100 r. to 5,000 r. of accumulated whole-body irradiation. There is the possibility, also that only a few greatly exposed individuals may be responsible for the extra deaths, giving a falsely high death rate to the entire group. Such difficulties also plague current attempts to evaluate findings among the Japanese survivors of atomic bombing. It is absolutely necessary to refine estimates of exposure of individuals in order to arrive at a proper evaluation of human life-span effects and to be able to test such data for proportionality or lack of proportionality of induced effects of exposure. A truly valid assessment of radiation effect in man has not been made and would require for its accomplishment the upmost in competent personnel and financial support, on a scale commensurate with its complexity. A great part of the potential and useful evaluation of effects of radiation in Hiroshima and Nagasaki is in jeopardy because too little is known concerning true exposure. The several evaluations of leukemia risk in these Japanese are in doubt because exposure is unknown and evidence for exposure is quite inconsistent with distance from the blast and shielding. Thus, the true magnitude of the leukemia-inducing effect of radiation may be very much smaller or larger than it is now estimated to be. With some effort, the dose might be estimated for accurately for those who developed leukemia and those who were in certain exposure categories. Many critical decisions concerning effects of radiation will need to be made, and it is important to have a proper number to apply in hte construction of these estimates. If such a number can be obtained by making an additional effort, I believe we should do so.

Proportional effects versus threshold effects in man

Attention to effects of radiation first established that there are acute effects, especially marked by tissue destruction, from which recovery subsequently takes place. Recovery in this instance is of the same quality as recovery from any usual kind of injury. As the amount of radiation given in a single dose declines, acute effects decrease more rapidly than the dose, because acute effects are due to tissue destruction and too little damage is done at any one site at doses of about 100 r. and below to evoke measurable systemic responses. Thus, there are some kinds of radiation effect that are not seen at doses of about 100 r. or less. These include immediate radiation sickness, death, burns, ulceration, hair loss, severe anemia, or sterility. All of these symptoms are largely the result of destructive effects of radiation upon rapidly dividing cells. Such cells for the most part are being replaced at a rapid rate, so that the loss of a small number may be quickly compensated. When radiation damage is considered in

terms of acute effects developing soon after radiation exposure, there is general and convincing evidence of at least partial recovery from the effects of exposure. Attention exclusively to this phase of the problem leads some to the opinion that human can tolerate single intense exposures to several hundred roentgens and subsequently recover all of the gross features of normal health.

As we direct our attention to injury of tissues or cells instead of confining it to whether an individual lives or dies, we find in general that effects of radiation are much more nearly proportional to radiation exposure. For whole categories of effects, such as genetic effects, destruction of cells, artifical aging of individuals, and induction of cancer, there is evidence—some completely convincing, some only a reasonable argument—that radiation effects at the cellular level are proportional to exposure to radiation. It is quite possible that some of the effects, such as the induction of cancers or of artificial aging, may not be as likely to occur per roentgen of radiation exposure at low exposures as at higher exposures. These points will be settled only by additional study of the problem experimentally in various animal species and by utilizing every opportunity to study chance human exposures. As a working estimate, it seems reasonable to postulate that effects at low dosage and low rates of dosage accumulation are proportional to know effects at higher doses. First of all, such a working hypothesis seems plausible in the light of the experimental evidence previously mentioned, and secondly, it is a cautious position to take to protect human health.

I believe that the general evidence relating biological effects to exposure shows a remarkable similarity between genetic effects and changes in cell numbers and cell quality in the other body tissues (somatic tissues). Muller, for example, argued as long ago as 1939 that the somatic tissue effects of induced aging and carcinogenic change were quite comparable to genetic effects induced by radiation, and that both effects upon the somatic cells were probably of the same kind as the genetic changes in cells measured directly by the geneticist. Radiation effects on various cells of mammals, such as genetic effects and the survival of cells, including germinal cells, blood-cell-forming cells, and embryo cells, roughly fall into a category of proportional effects of radiation in which the chance of effect is 1 to 3 cells affected out of a thousand, per roentgen exposure.

Radiation effects such as upon growth are quite in keeping with other effects at the cellular level, and human growth change comparable with experimental values of radiation growth suppression in mice. The sum of such evidence suggests that life-span changes and induction of cancers are accelerations of normal aging produced by decreases in cell numbers and cell qualities in amounts proportional to radiation exposure. The effect is frequently made more complicated, it is true, by the ability of the body to repair tissue damage through replacement of injured cells with functional ones; but, fortunately, this modification helps us to escape acute radiation effects.

Were it not for the simultaneous development of atomic energy and broadly supported studies applying to health safety, we would not be in today's position of understanding radiation induction of cancer and other effects to the point of realizing that we should estimate these effects at very low exposures. Only a few years ago, we would have assumed that such effects could not exist. Attention to the effects of radiation in the small-dose range means that these efforts will be extended and that the problem becomes much more difficult technically. It is important to emphasize, in addition to life-span-shortening and cancer-inducing effects, the effect of radiation from generation to generation and upon embryologic development, both of which may be effects that are proportional to radiation exposure into small-dose ranges; if they are, they need to be estimated in terms of human costs.

I would like to turn attention to some of the other environmental effects with which radiation exposure may be compared. For several years, I have been evaluating a large number of environmental circumstances collected from various sources in terms of effects upon the life span and death rate. A summary tabulation is given in tables I and II. The exact numbers attached to some of these effects are in dispute, because there are different choices concerning the estimated magnitude of the effect. Some of the effects are related to reversible circumstances as, for example, marital status, occupational hazards, metabolic disease; other factors, such as radiation exposure, are permanent. The latter class includes traumatic injury, childhood disease, and country of origin. Some of these estimated effects are additive; others are not additive because they are partial measures of the same problem. For example, if one estimates the effect of fat

metabolism on life span from consideration of fat-carrying molecules in the blood, this estimate will already contain the evaluation to life risk that can be obtained from obesity status; and, similarly, the cigarette-smoking effect is also evaluated by blood fats, so that these are not additive. Differences between males and females, urban versus rural dwelling, and national differences appear to be entirely additive.

Differences in life span in this list are given in terms of change of physiologic age. An individual in a category listed as +3 years—as, for example, when a person's mother lived to be 90 while his father lived the average life expectancy—is considered to have the death risk at any adult age of a person 3 years younger chronologically.

Such effects on life span can also be established for, say, automobile driving or employment in industry. The effect corresponding to recommended limits for human radiation exposure (as, for example, 50 roentgens of accumulated exposure in occupations) is very much less than any of these other estimates, perhaps being —0.7 year. The effect of fallout estimated by these methods to be about one one-hundredth of this value. It is quite obvious that each of these effects can be worth our attention, although the average person is not aware at any time that change is taking place. Even such effects as a difference of 10 years in physiologic age are quite unlikely to be noticed by the casual observer watching the tide of life from day to day. Detection of these effects is possible only by employing statistical tools, and the conclusions apply only to averages—not to any one specified individual, except in terms of the statistical concept of risk.

The additive nature of disease categories and factors underlying development of disease suggests a very important point with regard to radiation effects, namely that, even though we cannot reverse the effect of radiation damage itself, it should be possible to counter this effect by enhancing health in other ways. The general recession of disease and improvement of health over this century are clear evidence that great gains can be made in the direction of better health and longer useful life. Some of these gains may more than offset the adverse effects of radiation, and some of these gains may be expected to arise as the direct result of the AEC program in research.

TABLE I.—*Relative displacements of physiologic age by factors that accentuate aging or loss of life span (minus time) or retard aging (plus time)*

REVERSIBLE

	Years
Country versus city dwelling [1]	+5.0
Married status versus single, widowed, divorced [1]	+5.0
Overweight: [2]	
25 percent overweight group	−3.6
35 percent overweight group	−4.3
45 percent overweight group	−6.6
55 percent overweight group	−11.4
67 percent overweight group	−15.1
Or an average effect of 1 percent overweight	−.17
Occupational exercise versus sedentary occupation [3]	+5.0
Smoking: [4]	
1 pack cigarettes per day	−7.0
2 packs cigarettes per day	−10.0
Atherosclerosis: [5]	
Fat metabolism from consideration of cholesterol or lipoprotein concentrations in human serum:	
In 25 percentile of population having "ideal" lipoprotein concentrations	+10.0
Having average lipoprotein concentrations	0.0
In 25 percentil of population having elevated lipoproteins	−7.0
In 5 percentile of population having highest elevation of lipoproteins [9]	−15.0
Diabetes: [6]	
Uncontrolled before insulin—1900	−35.0
Controlled with insulin:	
1920 Joslin Clinic record	−20.0
1940 Joslin Clinic record	−15.0
1950 Joslin Clinic record	−10.0
Antibiotics	+

PERMANENT

	Years
Female versus male sex [1]	+3.0
Familial constitutions: [7 8 9]	
2 grandparents lived to age 80	+2.0
4 grandparents lived to age 80	+4.0
Mother lived to age 90	+3.0
Father lived to age 90	+4.4
Both mother and father lived to age 90	+7.4
Mother lived to age 80	+1.5
Father lived to age 80	+2.2
Both mother and father lived to age 80	+3.7
Mother died at 60	−.7
Father died at 60	−1.1
Both mother and father died at 60	−1.8
Recession of childhood and infectious disease over past century in western countries	+15.0
Life insurance impairment study: [7]	
Rheumatic heart disease, evidenced by—	
Heart murmur	−11.0
Heart murmur plus tonsillitis	−18.0
Heart murmur plus strep infection	−13.0
Rapid pulse	−3.5
Phlebitis	−3.5
Varicose veins	−.2
Epilepsy	−20.0
Skull fracture	−2.9
Tuberculosis	−1.8
Nephrectomy	−2.0
Trace of albumin in urine	−5.0
Moderate albumin in urine	−13.5

[1] Vital Statistics of Denmark, Netherlands, Sweden.

[2] L. I. Dublin and H. H. Marks, Mortality Among Insured Overweights in Recent Years, presented at 6th annual meeting, Association of Life Insurance Medical Directors (Metropolitan Life Insurance Co., October 1951).

[3] The Registrar General's Decennial Supplement, England and Wales Occupational Mortality, pt. I, 1951 (Her Majesty's Stationery Office, London, 1954).

[4] E. C. Hammond and D. Horn, The Relationship Between Human Smoking Habits and Death Rates, Journal American Medical Association, in press, presented at annual meeting, American Medical Association, New York, June 4, 1957.

[5] J. W. Gofman and H. B. Jones, Obesity, Fat Metabolism, and Cardiovascular Disease, Circulation 5, 514 (1952).

[6] E. P. Joslin, H. F. Root, P. White, and A. Marble, The Treatment of Diabetes Mellitus, ninth edition (Lea and Febiger, Philadelphia, 1952).

[7] Society of Actuaries, Impairment Study (Peter F. Mallone, Inc., New York, 1951).

[8] As measured in 1900 (Beeton and Pearson). These effects may be measurably less now, as environment is changing to produce greater differences between parents and progeny. Also, in 1900, it was a greater feat than now to live to be 80 or 90.

[9] This 70 percent difference in distribution of lipoproteins, between 25 percent lowest and 5 percent highest, is equivalent to a total of 25 years in relative displacement of physiologic age.

TABLE II.—*Statistical distribution of lifetime shortening by travel and industrial accidents*[1]

[Calculation based on Vital Statistics of 1949, values for adult white males 20 years and older

All accidental deaths	−2.3 years per individual in United States of America.
Travel accidents:	
Accidents involving railways	−0.06 year per individual in United States of America.
Accidents involving ships	−0.04 year per individual in United States of America.
Motor-vehicle accidents involving driver and passengers.	−0.67 year per individual in United States of America.
Assuming only half of population spends appreciable time in automobiles.	−1.3 years per individual at risk.
Pedestrian motor-vehicle accidents	−0.2 year per individual in United States of America.
Assuming this effect largely involves the urban portion of the population.	−0.4 year per individual at risk.
Aircraft accidents	−0.05 year per individual in United States of America.
Assuming that ¼ of the population (actually, probably much less) uses airplanes.	−0.2 year per individual at risk.
Accidents involving industrial machinery	−0.04 year per individual in United States of America.
Assuming only 30 percent of males are employed using industrial machines.	−0.27 year per individual at risk.

[1] These values are based upon numbers of deaths attributed to accidents; the estimates of life span lost are actually perhaps slightly low because survivors who are maimed, and hence have reduced life expectancy, are not included in these estimates.

Dr. JONES. I will deviate from the prepared statement to save time. I can use the blackboard and anyone reading my ad lib remarks can fall back back on the prepared statement if he has difficulty following me.

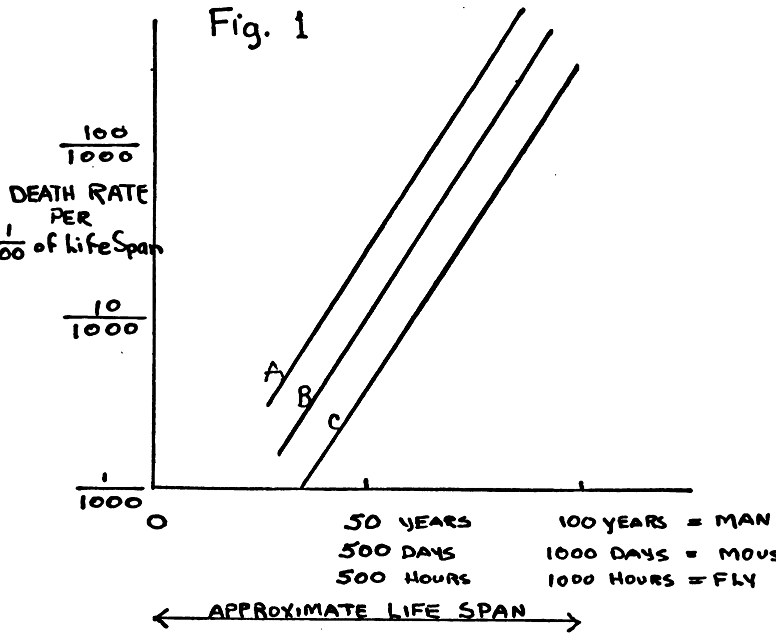
Representative HOLIFIELD. That will be fine.

Dr. JONES. My general problem in health research is to try to evaluate as many things as we can that have some bearing on human biology, human biology from the standpoint of the degree of good health we might achieve and how long we can maintain our life in relatively good health. These things can be expressed in terms of length of life, they also have to do with measures of death rate, because this is how we measure the risks of failing to remain alive.

I would like to present a diagram of this (fig. 1) because I think it helps to understand the problems of human biology. Ordinarily vital statistics for man are presented in units of death rates, that is, deaths per thousand of population per year. This level would be 1 death per thousand per year, this would be 10 deaths per thousand per year, 100 deaths per thousand per year, and so on.

I regret giving information in these terms because they are exceedingly morbid values to deal with. Sometime in the future I would like to turn them around and express them in units of vitality.

We are interested in the problem of vitality and not the problem of morbidity. If we draw this function (line B, fig. 1), this could essentially represent the aging of men or women of the United States. These are approximately the age-specific death rates. If we consider individuals at the age of 30, they have a risk of approximately 2



deaths per thousand individuals per year. At age 50, we find the death risk per year is 10 per thousand and so on.

We find this is a very regular sequence of events. It was described over 130 years ago by Benjamin Gompertz.

The very interesting thing about this is that if the horizontal distance represents life span of man in years, we can convert this equation to the biology of the mouse by changing the time scale to make the life span equal 1,000 days and then our line represents the aging of the mouse. The longer he lives, the larger the risk of dying.

One can also interpret the increasing death risk on the basis that physiologic degeneration is an accumulative affair. The more degeneration accumulates, the more likelihood there is of further decay; and the more decay in toto, the more the death risk. These concepts are relatively important to us because they may explain the very great differences in death rate between populations, either of humans or mice. If we plot the death rates for several populations of mice, we might get marked differences in death rate at each age (lines A and C, fig. 1). The interesting thing is that the biological equation always gives us the same slope. That is, the rate of progression of the death rate tendency is always the same for that species.

In this case, the death rate doubles for each age increase of approximately 80 days for the mouse, or approximately 8½ years for man. Otherwise, the equations are very much the same. The range of variations shown (a factor of 4) in death rate at any age occurs under average circumstances.

I can illustrate some of the differences among human populations. If we say this line represents death rates in the United States today, the Scandinavian countries are in a much better position, with lower death rate risk on the average. We can separate out some population

groups in the United States and find intrinsically low death rates, as good as those of the Scandinavian countries.

What has been happening? If we go back at least 100 years, say, to 1850, we find high death rates; and we find that over the course of the last century, throughout the Western World, death rates have been shifting in the direction toward lower and lower values, even though the rate of change of the death rate with age has remained characteristically the rate of change for man. This helps us to identify many factors that are probably of importance to us in a public health sort of way. I do not think that some of these things can be established with great certainty, but at least the effects of certain conditions have been identified as existing or not, which is quite important to us. One of the main things that has caused a shift from very high death rates in adult life to very low death rates at the same age, has been the recession of the childhood diseases. As childhood diseases have been eliminated, due to progress of nutrition and public health and medicine over the past century, we find that health in adult life has also been better and better, as evidenced by lower death rates.

We can identify a number of factors that have an effect on health. I will put some of them down. The recession of childhood diseases during the last century has added about 15 years of useful adult life. We can tentatively estimate differences associated with exercise—a deplorable thing to discuss when we do not get enough of it. The gain due to exercise might be of the order of 5 years. The effect of obesity, based on life-insurance statistics, is apparently proportional to extent of obesity, where 1 pound of overweightness is equal to 1 month of lifespan.

If you think of it the other way, if you are 1 pound leaner, it works in reverse or you have a 1 month gain.

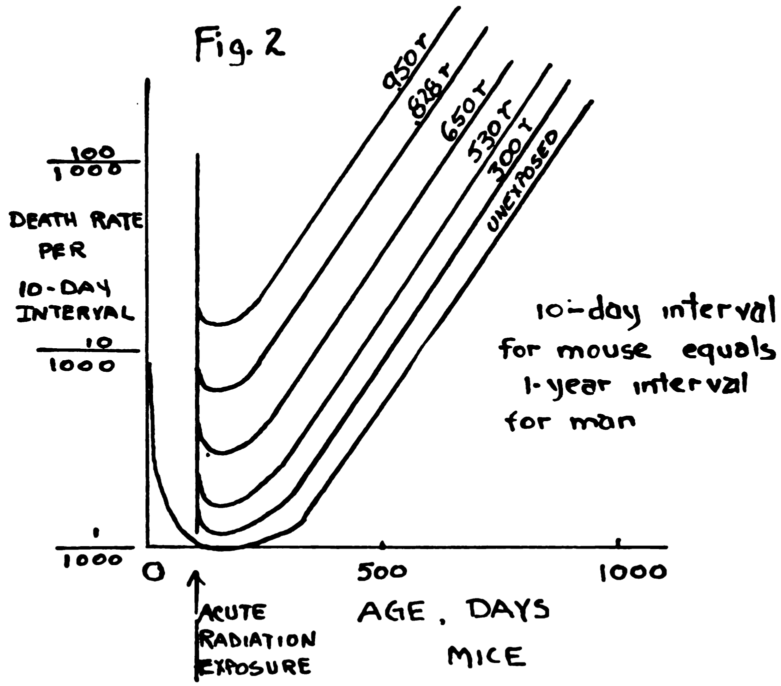
There are differences between living in the country and the city. There are very great differences between various population groups by nation of origin, not related to politics but to human environmental factors.

This type of analysis is important to us because it gives us a background to use with respect to the irradiation problem; particularly, if we look at the effects of irradiation on the mouse, we can use these same equations.. We will be talking in terms of a lifespan of a thousand days instead of the human lifespan of 100 years. The biology of aging of the mouse is like this [drawing on blackboard] (fig. 2). If we give single doses of radiation early in life, what we find is that the death rate goes up momentarily, due to the acute effects, and then, among the mice that recover, there is a displacement of the death rate just as though these mice were already older than they were before.

This apparent aging is the primary thing that characterizes the radiation effect on mammals; the general things that cause death are just about like the things that would have caused death anyway if the animals had been a little older. There are certain things that might really be specific for radiation toxicity, but in general radiation injury represents an increase in natural tendencies toward death. If we give twice as much radiation, this line would move up twice as far.

This brings us, then, to the concept of proportionality. As far as this kind of increase in the death rate is concerned, we seem to have a linear displacement that will move the line toward higher death

Fig. 2



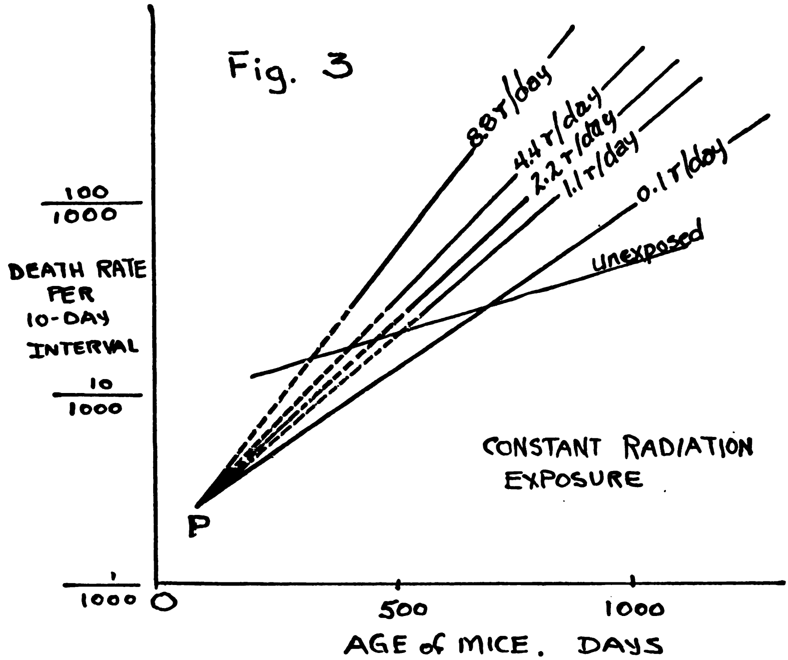
rates in amount proportional to exposure. The amount of injury, which is measured by the increase in death rates, is proportional to the amount of single-dose exposure between 100 and 1,200 roentgens. We can't go to higher exposures because the best techniques we have available will not enable these animals to live beyond the acute phase.

So this gives us one of the best illustrations of proportionality of radiation effect. Even so, there is a great deal of debate among my colleagues as to whether we should really conclude that these effects on life span are still proportional at very low doses. The reason for that is that the basic information we have to go on is relatively scanty. In spite of the fact that we would like to have better data, it has taken a great deal of effort on the part of most of our great laboratories to accumulate the information we now have.

Some of the experiments have been rather great undertakings in terms of numbers of animals, housing of animals, and the numbers of scientific investigators that have been a part of these studies.

A very large study was done in Operation Greenhouse, one of the atomic blasts in the Pacific, where large numbers of mice were used, and where we have good tests of the proportionality. Even in this case there was a difficulty in estimating the physical doses of these animals, and in the higher dose ranges it looked as though the effects on life span were relatively greater than at the lower dose rates. However, at the lower dose rates, the concept of proportionality still seems to hold in that the curve becomes a straight line which seems to go through zero effect at zero dose.

Some of the other difficulties in establishing proportionality of radiation damage are evidenced in terms of another effect that was demonstrated in mice, and that is the chronic radiation exposure ex-



periment of the late Dr. Lorenz. Fairly large numbers of mice were used in this study. In this case, when radiation is given constantly, one does not find that these lines are displaced in a parallel fashion; but, because the little increments of radiation damage are constantly being given, the line has a steeper slope, and the slope of this line will increase in proportion to the amount of radiation given.

So then, if radiation began at this point (point P, fig. 3) and we have varying dosages—maybe this is a tenth of a roentgen per day, this might be 1 roentgen per day, 2, 4, and 8—one can see that the change of death rate with age becomes steeper and steeper as we go to the higher chronic exposures. Unfortunately, with respect to interpretation of the results at the lowest dosages, the control mice were housed separately.

As far as the direct comparison of slope of the death rate curves is concerned, the unirradiated controls seemed to have a lesser slope than even the 0.1-roentgen-per-day group. But when the curves are put together, the control mice intersect in this fashion, (fig. 3), so that early in life the control mice had a higher death rate than the mice irradiated at 0.1 r. per day, and this variation essentially happened twice. So that the direct observation is that the animals that had a tenth of an r. per day lived slightly longer than those that had no irradiation; but the former group did have twice as many tumors as the latter.

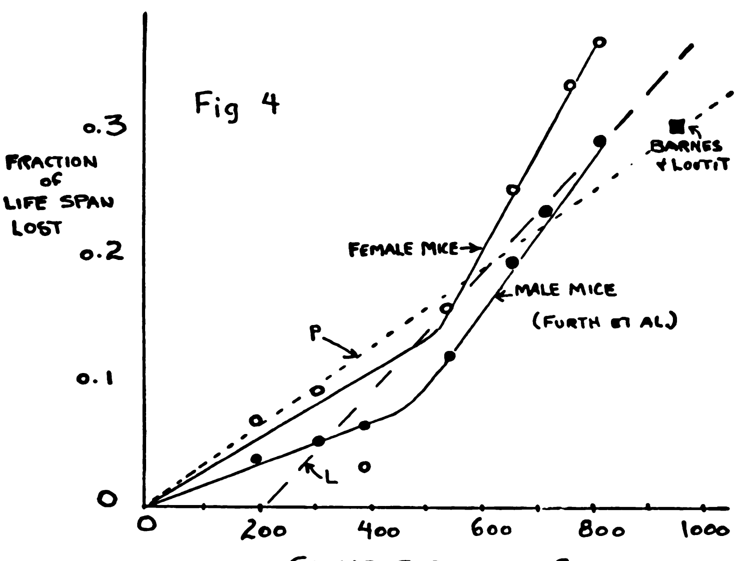
Such differences in death rate are seen quite commonly among animals that are housed separately in slightly different environments, in spite of every effort to make conditions identical. In such cases, however, the slope of the line has remained the same, but the position of the line has been displaced.

I don't know any way out of this particular dilemma except to get further information from repetition of these experiments. The anomaly has given some people a bulwark to fall back upon in arguing that perhaps there is a threshold in terms of radiation effect on the life span. On the other hand, I would place little confidence in this argument, because these animals were housed separately.

Representative HOLIFIELD. What do you mean housed separately and controlled?

Dr. JONES. The experimental mice were chronically irradiated, so they could not be kept in the same room or region of the building as the control mice, which are kept in a normal environment for comparison. They had to be housed in a different room. These particular control animals were even raised at a different time period because the animals that were contemporary with the mice that were irradiated happened to have an infection and the colony was lost. This limits the interpretations one can place upon these experiments.

Even so, expected effects of 0.1 r. per day are so slight, and the numbers of animals are so small, that the results do not let the investigator test significantly either way whether the effects of irradiation on life span are subject to a threshold or are continuous in the small-dose range (fig. 4).



Solid lines are best fit, aging proportional to dose
LINE-P, possible fit, aging proportional to dose
LINE-L, possible fit, aging beginning at a threshold of 200 roentgens

So we have to conclude in general that there is no direct evidence of life span effects in the small-dose range. We can only draw inferences from effects at higher doses. But I believe if we add everything we have available in the higher dose range, of the order of 100 r. up to 1,000 r., that the effects seem to be reasonably proportional to

radiation exposure. Just as the geneticists have trouble in going below 25 roentgens in testing proportionality, I think we have trouble going below 100 r. in establishing that life span effects can occur at very low doses. It is certainly possible to test for radiation effects in this region, but it will be difficult and the testing will not be completed for some time to come.

I also think that everywhere possible we should make an effort to obtain such information directly from man. Of course, there is the study of the Japanese. There is also the study of the radiologists that we discussed briefly yesterday. These studies have some limitations. If one makes the best estimation possible of the death rate for radiologists and compares it with the white male population, it does appear that up to the age 50 the population of radiologists and the white male population have similar death rates. Above this age, the death rate of radiologists is increased above the general population, so that the displacement in death rate, which corresponds to age, makes radiologists effectively at least 10 years older than nonradiologists of the same chronological age. This really only tells us that a life span effect of radiation can occur in man. It is very difficult to say what the ratio of life-shortening to radiation dose is.

It is very difficult to say what the exact number is because the numbers of radiologists have been increasing in each year's time. Over the last 20 years, the number of radiologists has increased by a factor of 3 or 4. With this increase, there has been an increase not only in the younger ages but in all ages. So we have no idea as to what the average dose of radiologists may have been. We can only guess that this 10-year difference in aging is associated with an average dose of somewhere from 200 r. to perhaps as much as 1,000 r. I personally doubt whether this 1,000-r. value taken as an upper limit could really be expected, because I think the effects of 1,000 roentgens of whole body irradiation would be more severe than we see it in the average radiologist.

There is, however, very little doubt that many of the radiologists who are responsible for this higher death rate are really groups that had much greater than average irradiation, and that the main population of radiologists having much less exposure than these few are really subject to a lower death rate.

It is also important at this time to point out that we really need to have a value that we can say represents the effect of radiation on the life span of man. The best thing we can do directly in the absence of such a number, I believe, is to estimate it from the mouse. Unfortunately, we know so much about the biological equations of aging, how diseases develop in both the mouse and man, that we can use this system of simply converting 100 years to 1,000 days to give us this information directly, and use it with whatever confidence limit needs to be applied, remembering that the information is from the mouse.

When we do that, we will find that, taking the mouse data directly, we might conclude that 1 roentgen perhaps—and the "perhaps" is big—is equal to minus 5 days in terms of individual life span. On the average, the individual exposed to 1 roentgen would be like an individual without this amount of radiation who was 5 days older. You see, for all practical purposes within the model as constructed, radiation exposure simulates the ravages of time. So we are justified from the standpoint of the general logic of these events in making this conversion between roentgens of exposure and units of life span loss.

But this is not all. This value, within the confidence limits of the data, might truly range from 1 day to greater than 15 days. We simply don't know. I would advise us all to look at the problem as carefully as possible and use whatever means we can to get such information.

While I cannot be certain where this value may truly lie, the sum of all information I have available to work with would lead me to trust tentatively as a direct average without weighting it for any factor at all this value of 5 days. If we look at the Japanese data in terms of death rate, it is possible that these will fall somewhere within the liimts of 1 day to perhaps greater than minus 15 days per roentgen. We simply do not know yet.

I had occasion to make some very rough estimates some time back and all I could say was that it looked as though there was a life span loss on the Japanese. As we know, there is quite a leukemia rate associated with it, similar to the increase in leukemia rate in radiologists.

Very briefly, I might examine a few factors that are perhaps related to the shortening of life span. They may help to explain radiation effects. I think this particular result of radiation is possibly due to some combination of two effects: the tendencies for the cells to undergo mutations, such as Dr. Muller and others described as somatic mutations, and also the reduction in numbers of functional cells in the animal. If we look at the numbers of functional cells in the animal, or the effects of radiation on mutation rate, we would be looking in effect at a corresponding increment of the death rate. If we look at the cells directly in terms of the response to radiation. for many cells in mammals we will find that there is proportionality in numbers of cells left alive, and number killed per roentgen.

From a lower value of 10 roentgens up to values of 2,000 roentgens in the rat or in the mouse or human blood cell estimates, it appears that, per roentgen in this range, you would have between 0.3 percent and 1 percent of the cells killed. It looks as though the effect of radiation in mammals under a variety of circumstances, for cells like the marrow cells or lymphatic cells, is within this range.

Also, the cells in the developing embryo, which also have a high rate of cell division, have approximately this same range of radiation sensitivity. We can show that the small effect of radiation on the growth of children irradiated before birth in Japan is about the same rate as this, 0.3 percent per roentgen, and this turns out also to be equivalent to the data that Dr. Russell described a moment ago on his irradiated mice. So we have some general idea that in the various mammals we can study, the effect upon cell numbers seems to be relatively the same among the various mammalian species, and also the life span effects seem to be roughly the same as far as we can test; but it does leave us with the great desire to know much more about these effects, and also to know what kind of number we should estimate for the life-subtracting effect of radiation in man.

Senator HICKENLOOPER. Do I understand that range you put on the board a moment ago from 10 roentgens to 2,000 roentgens, that an exposure of 10 roentgens would kill 0.3 percent of the cells and exposure of 2,000 roentgens would kill 1 percent?

Dr. JONES. The range of 0.3 percent to 1 percent of cells killed per roentgen applies throughout the range from 10 roentgens to 2,000 roentgens. One has to interpret the proportionality in what we call

the hit equation. You cannot kill the organism twice. So that in upper doses very few cells are left untouched.

Senator HICKENLOOPER. That is the thing that confuses on that statement. I would interpret what I understand you to mean that as compared to an exposure of 10 roentgens and 2,000 roentgens that the 2,000 roentgens would kill only about 3 times as many cells percentagewise as the 10 roentgens. That is my understanding of your statement.

Dr. JONES. No. If we test for this range where we have available data in the mouse, regardless of whether we give 10 roentgens or give 2,000 roentgens, we will find that we have 0.3 percent of the cells affected per roentgen. So that, per roentgen, as far as the effect on rapidly dividing cells is concerned, it will be three cells of each thousand cells affected.

Senator HICKENLOOPER. I understand. I missed the equation to the single roentgen.

Dr. JONES. Yes. The remarkable thing is that this is such a good proportionality effect.

Representative HOLIFIELD. Dr. Jones, we want to adjourn at 5 because the committee has to go into executive session on another matter.

Dr. JONES. I can summarize this section. I think life-span effects do exist. I have no reason to doubt this at all. I have some reason to believe that we should look with caution on the argument that a threshold effect exists, although we cannot be absolutely certain that a threshold effect might not exist. But as far as my opinion is concerned, on the basis of having examined all the facts at my disposal, I do not believe a threshold effect is very likely to exist, I think we should try to get better information, not only for this point, but also to find out directly in man how much each unit of radiation subtracts from life span, because in an atomic age everyone is going to need to know this number with a great deal of certainty. At least, it should be determined to 1 or 2 significant figures and not merely within the range of perhaps a factor of 10.

Representative HOLIFIELD. Thank you. Your summary is very interesting, and the statement that you have submitted will be studied carefully.

I have two papers by Dr. Jones that I would like to place in the record at this point.

(The material referred to follows:)

A SUMMARY AND EVALUATION OF THE PROBLEM WITH REFERENCE TO HUMANS OF RADIOACTIVE FALLOUT FROM NUCLEAR DETONATIONS

Hardin B. Jones,[1] Donner Labortory of Biophysics and Medical Physics, University of California, Berkeley, Calif., January 14, 1957

ABSTRACT

The tolerable amount of radiation exposure to humans is probably less than formerly estimated. It is shown, however, that accumulated effects of the low-level worldwide exposure to radiation from fallout to date is relatively small. The genetic effects are not large enough to be statistically detectable. The health effects, as expressed in life expectancy, are much smaller than those of such factors as infectious or chronic disease, metabolic disturbances, smoking, obesity, lack of exercise, and environment and marital status. Predictions of strontium 90 levels to be expected in the next 2 decades indicates, however, that bone irradiation may become detectably harmful.

[1] With suggestions and critical review gratefully acknowledged to R. Lowry Dobson, John W. Gofman, John H. Lawrence, Burton J. Moyer, William Siri, Curt Stern, and Edward Teller.

INTRODUCTION

Nuclear detonations form radioactive isotopes in quantities so enormous that they must be reckoned in terms of many thousands of curies, the equivalent of many pounds of the element radium. A portion of this radioactivity is dispersed into the atmosphere and subsequently falls upon the land and sea. However, the vastness of the land, air, and water of the earth provides a means of dilution so great that even these large quantities of radioactive materials are soon reduced by distance and time to exceedingly small concentrations of radioactivity. The problem of radioactive fallout in relation to human beings involves the need to know the quantity that becomes a part of human environment, and to know the effect upon man of ionizing radiation from fallout.

This summary concerns low-level worldwide fallout. It must be recognized, however, that very intense fallout may be experienced in the vicinity of an atomic detonation. For example: [3]

"On March 1, 1954, an experimental theromonuclear device was exploded at the United States Atomic Energy Commission's Eniwetok Proving Grounds in the Marshall Islands. Following the detonation, unexpected changes in the wind structure deposited radioactive materials on inhabited atolls and on ships of Joint Task Force 7, which was conducting the tests. Radiation surveys of the areas revealed injurious radiation levels; therefore, evacuation was ordered, and was carried out as quickly as possible with the facilities available to the task force.

"Although the calculated accumulated doses to the exposed human beings were believed to be well below levels that would produce serious injury or any mortality (267 Marshallese received 14 r. to 175 r.) * * * All of the exposed individuals have recovered from the immediate effects (burns, loss of hair, anemia) without serious sequelæ. Nevertheless it is planned to evaluate the medical and genetic status of the group at appropriate intervals with a view to learning what if any of the known late effects of radiation exposure may be observed. Obviously and indeed fortunately the number of persons (92 Marshallese) receiving 75 roentgens exposure and greater is too small to make it possible to determine with any degree of accuracy the effect on life span."

EFFECTS OF RADIATION EXPOSURE IN HUMANS

The reasonable and superficial evaluation of radiation hazard is that humans can obviously tolerate exposure to several hundred roentgens, recover from immediate effects, and remain in "normal" health and functional capacity. Recently, however, we have become aware of deleterious long-term effects of radiation which, however subtle, appear to be proportional to the total quantity of radiation exposure and may be assumed to act even at very low levels of irradiation. Except for these long-term changes, our understanding of radiation effect usually has been concerned with two important facts that dominated our thinking about these problems:

(*a*) For certain kinds of radiation damage and injury, there is recovery. Individuals recover from acute symptoms produced by sublethal radiation exposures even though they may show general sickness, burns, loss of hair, anemia, etc. Recovery from acute radiation effects is analogous to recovery from any other acute injury or infectious process in which damaged tissue is healed and repaired.

(*b*) These obvious signs of radiation effect are associated with relatively large single doses of radiation (greater than 100 r.). As dose size is decreased, detectable acute effects decline, becoming disproportionately small, so that there is a true threshold of dose of irradiation, at about 100 r., below which these particular acute manifestations of radiation do not occur.

THE PROPORTIONAL-EFFECT CONCEPT OF IRRADIATION

Recent evidence that long-range effects of radiation simulate aging effects comes from a variety of sources and is consistent with information relating radiation effects with genetic change and changes in cell-population numbers and quality. Evidence and logic support an argument that small increments of radiation-induced morbidity persist as small permanent changes in body functional struc-

[3] Quoted from Charles Dunham, A Report on the Marshallese and Americans Accidentally Exposed to Radiation From Fallout and a Discussion of Radiation Injury in the Human Being (United States Atomic Energy Commission, July 1956).

tures, which become detectable as aging, neoplastic disease, and genetic change (see appendix A). However, it remains to be proven experimentally that these effects do occur as the result of small irradiation exposures. The testing of this question is not likely because it would involve great technical difficulties. Attempts to procure some evaluation of the problem relating small-doze (5 r. to 10 r.) effect to life span would involve study under uniform conditions of perhaps several million mice. It is pointed out that in order to establish the effect of the smallest doses as yet measured for genetic effect, namely 25 and 50 r., the geneticist Curt Stern and his associates worked for 6 years and examined approximately 50 million individual flies. They came to the acceptable conclusion that these small exposures have the same effect per roentgen upon gene mutation as at higher exposures to radiation. Ionizing-radiation effect, in the depression of blood-forming function and blood-forming cells, is proportional to radiation dose even down to 5 r. (Hennessy and Huff). Other effects upon blood cells, leading to abnormal doubling of the cell nucleus, are now reasonably established by Dobson in the range of 0.1 to 0.3 r. of single exposure, but are not yet tested for proportionality.

There is no reason to doubt the general evidence of a proportional effect of radiation; but it is also possible that linear extrapolations of higher doses to the small-dose range may not give a true representation of the problem. It is known for some kinds of cellular response to radiation that there can be no effective change in function until two or more similar critical entities within the cell are affected. Some kinds of observed injury, however, appear to depend upon the effect on one critical entity per cell; and other observed injuries may be the result of damage to any one of a number of critical functional parts. The response that depends upon a chain of two or more detrimental changes shows a lesser apparent effect in proportion to radiation exposure at the low-dose ranges. Although this kind of irradiation response does not argue against extrapolation of radiation effect, it may explain a factor of —2 or —4 buffering against detectable radiation effect in the lowest exposure ranges; or it may even have the opposite effect, because radiation effect can add onto partially initiated dysfunctional changes in structures that otherwise would have remained functional.

Radiation effect is most frequently estimated in animals that are rather uniformly irradiated over the whole body. Thus, we are usually generalizing from the observed result of whole-body exposure. In some studies (Kaplan), shielding a relatively small portion of the bone marrow from radiation may protect the animal from generation of thymic tumors. In others, local irradiation is associated with induction of cancer in that region, quite independently of exposure or shielding of the remainder of the body.

The problem of estimating radiation effect and making recommendations concerning it is not the simple problem of avoiding exposure at levels at which there is a detectable or predictable response. This is especially true when considering radiation effect through systems that allow proportional extrapolation to very small radiation exposures. It is always important to keep radiation exposure to a minimum; but it is also important in the understanding and evaluation of the relative importance of radiation effect to establish its place in the entire climate of factors that can modify health. Similar—and, at times, greater—effects upon health can be shown to result from a large number of common environmental factors.

Also, the problem is not simply that of effect of body irradiation upon health. It is necessary to evaluate the effect upon human beings of all known phenomena resulting from the onset of the atomic age, including general socio-economic factors related to our well-being, which are dependent upon progress and the development of useful energy.

ADVANTAGES MINUS COST EQUALS NET GAIN

The sum of evidence would lead to the conclusion that radiation probably does affect man's health subtly, and—like money and time—it should be exchanged for equivalent advantages.

Since the usefulness of atomic energy—including material and energy gain and defense measures of prime importance—is a positive result, and the radiation effect upon humans generally is a negative result of the atomic age, atomic-energy usefulness minus harmful radiation effect must be equated to the net gain. Therefore it is critically important to estimate hazard quantitatively, and to be mindful of other factors while doing so. However, there is no unanimity of opinion at this time as to the precise balance that should be achieved between advantages and disadvantages of use of atomic energy, because certain

qualifying factors are still too poorly known. Uncertainties exist which can mean either underestimation or overestimation of the effect of radiation. This brief synopsis roughly appraises the biological costs of exposure to radiation and presents information which must largely guide decisions in the interim until more precise information on radiation effect is available.

A summary of current knowledge of radiation tolerance or hazard and fallout is provided in several major public documents that have appeared in 1956 in broad survey of the problems to man of atomic radiation and fallout.[3 4 5 6 7]

THE CONCEPT OF MAXIMUM PERMISSIBLE DOSE

Early estimates of that amount of exposure to ionizing radiation which constitutes a permissible occupational hazard placed the upper limit at 0.1 r. per day. Such a value was exceedingly conservative in view of information available at the time it was established. It is lower by a factor of 50 than chronic exposures leading to physiological disturbances and radiation sickness, and by a factor of 1,000 to 5,000 than the dose which, in a single exposure, might threaten life. Also, at the time it was proposed, 0.1 r. was the lower limit of radiation exposure dose known to elicit any biological response. Evidence on the magnitude of physiologic response of the individual to radiation in the range of a few hundred roentgens has not changed; but extensive information on effects of lower levels of radiation has recently appeared. This knowledge requires a reevaluation of the cost to humans of radiation exposure in terms of (*a*) genetic effects, (*b*) shortening of lifespan, (*c*) induction of cancers, (*d*) destruction of tissue, (*e*) congenital malformation, and (*f*) effects upon young individuals. All these effects appear to be proportional to the exposure to radiation, and have been largely responsible for a recent downward revision in maximum allowable exposure to radiation.

THE GENETIC EVALUATION OF RADIATION EFFECT

Up to 1946 estimations of the genetic effects of radiation had placed the quantity necessary to double the mutation rate per generation in the fruitfly at about 50 r. (Muller, Stern), but with some uncertainty, so that the true value might have been 80 r. or 35 r. At that time, there had been relatively little comprehensive evaluation of the range of genetic sensitivity to radiation in mammals or man. At the present time, the mutation rate per generation for the fruitfly is known to be doubled over the natural rate by about 50 r. (Stern). Through genetic study of irradiated mice (Russell), the amount required to double mutation rate per generation in the mouse is partially established also at approximately 35 to 80 r. Wright has estimated from evidence now available that the mammalian mutation rate may be doubled by as little as 3 r. or as much as 300 r. The best current estimates place the mammalian mutation-doubling dose of radiation at about 50 r. (4).

As an approximation, each species appears to form in natural circumstances about one new mutation in a generation time. The fruitfly lives a short time in about the same radiation environment (estimated roughly at 0.1 r. per year) as man. In its life span of 20 or 30 days, it can accumulate only the minute quantity of 0.008 r. Thus, if 50 r. in the fly produces an additional number of mutations equal to those which occur naturally, radiation can account for only a part of the natural mutation frequency, namely, the fraction

$$\frac{0.008}{50}=\frac{1}{6,000}.$$

Hence, at background radiation, only 1 observed mutant in 6,000 is suspect of being induced by radiation. In humans, the life span up to average reproduction age is about 30 years, lived in the same environment of 0.1 r. per year, or a total of about 3 r. by average reproduction age. Thus, if 50 r. is estimated to double the human mutation rate, radiation from natural sources may be expected to account for

$$\frac{3}{50}=\frac{1}{17},$$

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[3] National Academy of Sciences, The Biological Effects of Atomic Radiation—Summary report, 1956.

[4] National Academy of Sciences, The Biological Effects of Atomic Radiation—Report to the Public, 1956.

[5] British report, Radiation Hazards to Man, Cmd 9780.

[6] Willard F. Libby, Current Research Findings on Radioactive Fallout, Proc. National Academy, December 1956.

[7] M. Eisenbud and J. H. Harley, Radioactive Fallout Through September 1955, Science 124, 8215 (Aug. 10, 1956).

or approximately 6 percent of the naturally occurring mutations. If we accept the lowest possible value of 3 r. for the mutation-doubling dose, we would have as the fraction attributable to radiation

$$\frac{3}{3}=1,$$

and radiation could account for the entirety of mutation changes in humans.

The fallout of radioactive materials through 1956 has increased the radiation exposure of gonadal tissue by an amount estimated as approximately 0.004 r. per year (see table V–D) (largely from ingested cesium–137 (4) and deposits on the ground (6)). This is an increase of approximately 3 percent over natural radiation exposure.

The recommended limits of radiation exposure in man will be affected by information on the quantitative relationship between ionization and mutation and the understanding of the natural mutation burden. Should we estimate the level of radiation likely to double the natural mutation frequency in man as 25 r. or 3 r., we will be at least 2 to 20 times as concerned about the genetic problems associated with radiation exposure as we are under the current assumption that the human mutation rate is doubled to 50 r.

Genetic studies of irradiated Japanese have been carried out by the Atomic Bomb Casualty Commission at Hiroshima and Nagasaki. A 10-year study has been analyzed by S. V. Neel and W. S. Schull.[8] The principal result is that no measurable increase in mutation rates was observed. They measured biological characteristics that could reflect genetic state and genetic change, such as stillbirths, male-female birth ratios, and congenital malformations. The results of all observations of this kind can be interpreted either as demonstrating no measurable increase in these events, which are associated with mutations, or as showing that, had the true congenital malformation rate been doubled, there would be only 90 percent probability of discovering even this increase. Thus a small increase in congenital evidence of genetic change would not have been detected.

The results of the study of the Japanese indicate that the human genetic effect of radiation is acceptably consistent with the range of response estimated from mammalian genetic experiments; and it establishes with certainty that there are no catastrophic genetic effects at low to medium range of radiation exposure in human beings, although catastrophic effects are predicted at high levels of accumulated radiation exposure to whole populations. Many new mutations were probably produced in the Japanese exposed to the atomic bombs; but many of these may have been unobserved because of early lethality, and the rest are overwhelmingly diluted by the vast number of normal genes. This dilution was expected; and the statistical odds are known to be very greatly against the appearance of unfavorable and detectable combinations of mutant genes in any one generation of offspring.

Genetic change is, of course, basic to the concept of the Darwin principle of evolution. For this reason, it is possible that some increase in the mutation rate might be to human advantage in the long run by providing a greater pool of variance from which selection could take place, to our final advantage some thousands of years from now. Some brief speculation regarding the extreme limits of variation may be offered:

(*a*) Humans and other long-lived animals have, as a corollary of their longevity, a less frequent natural mutation rate per gene per unit of time than short-lived species. As an approximate rule, each species appears to have about the same mutation rate per generation time. Thus, it appears that species are in some balance between generation time (or lifespan) and stability of genetic structure.

(*b*) Testing biological capacity for survival under circumstances that increase genetic variation is possible only with species having relatively short generation times. They have a common feature of a potentially great ratio of progeny per parent. These species can therefore survive even if a relatively high proportion of conceptions are incapable of survival and reproduction. Humans in natural selection are at some disadvantage in comparison with species producing a large number of offspring per generation, such as the mouse or the fly. Thus, human

[8] Reported at the First International Congress on Human Genetics, in Copenhagen in August 1956.

genetic tolerance should not be judged from effects of radiation exposures on these more fertile populations.

(c) For survival of a species, the ratio

$$\frac{\text{Reproducing offspring}}{\text{Individual}} = \text{must exceed } 1.$$

In humans, lowering of infectious disease toll has brought this ratio to approximately 2. As a consequence, the human population is now doubling in numbers approximately every 40 years. Thus, humans have already achieved some protective reserve against genetic changes toward lower fertility.

(d) On a scale of catastrophic genetic misfortune, humans also have the protection of vast numbers of individuals. There are 2.6 billion inhabitants of the world. While this number is small compared with numbers of insects and small mammals, it is still a very large number compared with that of any previous age in the history of man. Radiation exposure, as a cause of genetic change and increase of genetic variance, would be expected to produce that change in a random way. Thus, even with a large increase in genetic variance induced by radiation exposure, if population numbers are sufficiently great, some individuals will remain relatively unchanged. If these individuals were favored in selection, they might replace the less fit, less fertile fraction of the population. Thus, if survival of mankind were the only consideration, population numbers might, through reduction and segregation, achieve selective retention of adequately functional humans.

Several approaches to evaluation of extreme tolerance of human populations to radiation exposure with respect to health and genetic constitution are presented in appendix B. These methods of estimation are difficult and speculative; but they indicate that an additional 2 r./per year (or 50 r. per generation time) of chronic radiation exposure to the average individual in the human population would eventually cancel health and lifespan gains we have achieved recently. Such estimations of impairment of health and estimations of the cost of increasing the genetic variance suggest that human population cannot afford the biological cost of this intensity of chronic radiation exposure, and that there should be extreme caution at this time against increasing the radiation exposure to all people by 10 times over its natural level.

Evolutionary benefits?

It seems possible that human evolution is occurring in some optimal balance between mutation tendency and genetic stability. Fertility, length of life, death rate, and individual usefulness may be highly affected by the number of accumulated new genes,[9] which both add to favorable evolutionary drift in average human vigor and add to the pool of undesirable genes to be selected against. At low radiation levels, such as 10 percent or 1 percent above the natural radiation background (the range of fallout effect), it seems unlikely that long-range genetic disturbances can become an appreciable problem, since the natural radiation background appears to account for only 10 percent of the change in genetic structure per generation. One may speculate further that, in the long run, man may be beneficially affected by good genes yet to be formed, so that increasing radiation exposure and the mutation rate may operate to human advantage. Such an argument is unlikely to convince men who understand some of the dangers of too great a burden of undesirable mutants; it is analogous to an attempt to convince the experienced cook that the baking of her prize cake would be accomplished in half the time at higher oven temperature.

Penrose has evidence of indirect beneficial effect of some recessive lethal genes, which appear to enhance the effect of the functioning gene with which they are matched in individual combinations. This effect is one in which mutation may beneficially add some variance to genetic functional characteristics. On the whole, however, there is a strikingly large mass of information indicating that any genes that can disturb function should be kept to an absolute minimum.

Unfortunately, there are still many unanswered questions facing geneticists on the topic, "What is the effect of undesirable genetic burden on the quality of humans?" Fully satisfactory experimental measures have yet to be applied to this problem. One approach that has led to considerable speculation is through

[9] Transformed genes are, with rare exception, nonfunctional, lethal, or undesirable.

estimations of the numbers of undesirable mutations carried by the average person. Estimations of this burden place it within the small range of 5 to 15 undesirable genes per average individual[10] (4). This value is the equilibrium resulting from approximately one such gene gained and one lost in each generation.[11] Thus it has been pointed out that if, through increase in radiation exposure, the genetic gain of undesirable genes increased from 1 per generation to 2 per generation, there would be a relatively great reduction in the quality of the best 25 percent of individuals (assuming that reduction in quality of offspring is proportional to the number of undesirable mutations per individual). Because of speculative—but reasonable and cautious—arguments of this nature, geneticists have uniformly cautioned against allowing any major proportion of the population to accumulate radiation as high as 50 r., which is the amount estimated to double the human mutation rate.

LIFE-SHORTENING EFFECTS

Life-shortening effects of radiation have been observed under a variety of experimental conditions. An experiment of particular significance because of the large numbers of animals and the range of exposure was the exposure of mice to nuclear detonation at "Operation Greenhouse" (Furth et al.). The fraction of life span lost per unit of radiation exposure appears to be essentially the same for a number of species, including the mouse, the rat, the guinea pig, the rabbit, and man. The largest number of experimental observations concerns the mouse. In the mouse, the fraction of the life span lost per unit of whole-body radiation exposure is acceptably constant over a wide range of variation in radiation exposure. The tentative conclusion is that radiation effect simulates aging itself, and that a unit of radiation exposure, regardless of the intensity and duration of exposure, produces approximately the same relative disturbance to body structure in adults of all mammalian species. On the human life span scale, these effects of radiation summarized from small-animal data suggest that 1 r. of radiation exposure is equivalent to 5 to 15 days of physiologic aging. This prediction is confirmed directly in man (with reasonable technical reservations) by Dr. Shields Warren's recent investigation of life span of radiologists compared with physicians not using radiation in their practice of medicine. The average age at death is approximately 6 years less for radiologists than for physicians in general practice or for pathologists, both selected as being relatively unexposed to radiation. The estimation of accumulated radiation exposure in radiologists is uncertain, but has been approximated as 300 to 500 r. Thus, the life span loss, if attributed to radiation, is

$$\frac{-6 \text{ years} \times 365 \text{ days/year}}{300 \text{ r to } 500 \text{ r}} = -7 \text{ to } -4 \text{ days per r of whole-body exposure}$$

Such a number is still subject to considerable possible revision; but many different estimates give values of 1 to 30 days lost per roentgen of radiation exposure, and the probable value for humans is in the range of 5 to 10 days lost per roentgen.

A question exists whether we can justifiably extrapolate effects such as life-span lost per roentgen from measures that are mostly determined in the range of 100 to 1,000 r. The evidence is that, over the range that can be tested, the effect is linearly proportional to the radiation exposure; and the information fits an extrapolation to zero shortening of life span at zero artificial radiation exposure. There is additional evidence in the effects of radiation upon cells (as distinguished from entire organisms), in which lethal damage to cells per roentgen also appears to be proportional to total radiation exposure. Such estimates agree for cells in the mouse, the rat, the rabbit, the guinea pig, and man. This

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[10] Some individuals may have none. The fraction having none or very few diminishes steeply with increasing average numbers of undesirable mutations. Thus, doubling the burden of mutations may reduce the numbers of individuals having desirable genetic combinations to rare events.

[11] The average mutation frequency of 1.5 spontaneous mutations of human genes per generation, as summarized by Penrose, corresponds to 30 mutations per million genes per generation, assuming that humans have about 50 ;000 genes:

$$\frac{30 \text{ mutations per generation} \times 50{,}000 \text{ genes per individual}}{1{,}000{,}000 \text{ genes}} = 1.5 \text{ mutations per individual}$$

per generation.

The average mutation rate may be less than this estimate, since one may suspect that the genes usually observed to mutate are perhaps 10 times as mutable as the average gene.

experimental evidence that effect of radiation on cells is in linear proportion to radiation exposure of from 15 to several thousand roentgens provides a reasonable basis for understanding the life-shortening effects of radiation.

Furthermore, the life-shortening effects are consistent in order of magnitude with the genetic effects of radiation upon cells (2 to 3 cells affected per 1,000 cells per roentgen). The genetic effect of radiation has been shown to be acceptably proportional to radiation exposure from 25 r to 8,000 r.

The sum of systematic evaluations of such effects of radiation as mutation induction, cell destruction, and life-span shortening indicates that these effects are permanent and represent the quantum interactions of radiation randomly affecting body cellular structure. The concept of quantum interactions with matter justifies extrapolation to the probability that a single quantum of radiation reacts with an individual molecule.

Although all recent evidence suggests that radiation effects is proportional to radiation exposure, such effects must be viewed together with other common environmental factors that modify health. A scheme is used here in which the effects upon health is expressed as an induction of aging (this is expressed as loss in physiologic lifetime, or minus time, written "—*n* years") or as a postponement of aging (expressed conversely as lifetime gained, or plus time, "+*n* years").[12] These factors all appear to have a general action upon disease tendency, and the effect is about the same at any adult age. The list of relative displacements of physiologic age (table I) is given for factors that accentuate aging or loss of life span (expressed as minus time) or retard aging (expressed as plus time). These measures are derived directly from human records. They are grouped according to whether they appear to be reversible or permanent. Most of the effects that are not partial measures of the same state are apparently additive, in the few instances that can be tested for this property. (See p. 1107.)

Certain of these circumstances that modify health are partially interrelated, others may be independent of one another. Estimates of effect upon physiologic age may be additive, depending upon the extent to which they are independent. Thus country against city dwelling may be suspected to include the factor estimated as exercise benefit. The lipoprotein test already contains information that can be estimated partially by relative overweightness, and the lipoprotein tests already accounts for a portion of the smoking effect. Familiar inheritance is independently estimated from each ancestor; male against female differences are equally added to city against country effects, and presumably each separate disease sign in the impairment study is additive.

In further support of the additive nature of effects upon health, each morbidity circumstance that can be quantitatively estimated produces an effect proportional to the intensity of the circumstance. Examples of proportional change in mortality risk with morbidity severity are:

(*a*) Overweight: —0.17 year for each percent overweight
(*b*) Smoking: —0.45 year per cigarette used per day
(*c*) Radiation: —5 to 10 —days per r. of whole body radiation
3 cells killed per 1,000 cells per r. (marrow and lymphatic tissue)
4 cells with chromosome breaks per 10,000 cells per r.
1.4 percent increase in lukemia per r.
(*d*) Atherosclerosis, diabetes, nephritis: End effects are proportional to severity of metabolic error
(*e*) Accidents are proportional to exposure risks

A somewhat similar tabulation can be made of an estimation of the cost of industrial and transportation progress in this century in terms of years of life span lost by accident death, distributed to the average individual in the population of the United States (table II). These values are approximately comparable to the preceding values based upon changes in physiologic age. (See p. 1108.)

In about the same way, we can tabulate the effects on life span of radiation received (table III).

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[12] This estimation of life span lost or gained is in terms of relative physiologic age change. Change in life expectancy may be estimated by determining life expectancy at a given age in terms of a given age+*n* years' change in apparent age. Thus, a person of age 40 has a normal life expectancy of 31.1 years. If his physiologic age is 50 (because of a sum of factors predicting —10 years age over the average), his life expectancy (from life tables) is 22.8 years, or an average loss of life span of 8.3 years. Thus the life expectancy lost in somewhat less than physiologic time lost.

TABLE III.—*Estimation of radiation effect upon health and life span*

Radiation received	Life shortening (in years)	
	If l. r.=−5 days [1]	If l. r.=−10 days [1]
(r.)		
50	−0.7	−1.4
100	−1.4	−2.7
200	−2.7	−5.5
400	−5.5	−11.0

[1] 2 columns are given because of uncertainty whether l. r.=−5 days or −10 days.

Thus it is observed that, although the estimated effect of radiation upon life span is a number worth attention, its magnitude of effect at low accumulated dosage is slight compared with many public health problems. It must be remembered that major problems such as smoking and overweight and fat metabolism are so subtle that they are estimated and established not by clinical methods but rather by statistical (actuarial) researches involving large population samples. The effect of smoking 1 pack of cigarettes per day, for example, appears equivalent in reduction of health and life span to the effect of between 200 and 400 r. of accumulated whole-body radiation. This is several times as great as the 50-r. limit currently recommended for occupational exposure; and 50 r., in turn, is on the order of 10 times as much as the individual would accumulate through fallout. If the life-span loss is estimated as 5 to 10 days per r. of whole-body exposure, the loss due to 50 r. falls within the range of —0.7 year to —1.4 years of life span. This effect is greatly exceeded by the magnitude of the smoking problem; the obesity problem; the problems of atherosclerosis, diabetes, and all the chronic diseases; the benefits of marital status; etc. The effect of 50 r. of whole-body exposure to the general populace can also be viewed as being in the same category of life-span loss as that which results in the population of the United States from use of the automobile. This estimation, however, does not include the problem of the mutation burden in the next generations following such radiation exposure.

SUMMARY OF THE FALLOUT PROBLEMS ON A GLOBAL BASIS

On a global basis, the fallout intensity of radioactive materials is no more than one-millionth of the high-level fallout that occurred by mishap in the vicinity of a thermonuclear explosion in the Marshall Islands in Ooctober 1954. Current estimations made directly in humans throughout 1953–56 place the fallout exposure from strontium 90 as being, on the average; sufficient to produce an irradiation effect of approximately 0.004 r./year to human bones. This is a small quantity of radiation—2 percent of naturally occurring bone radiation—and estimates of effects derived from this additional tissue burden will be correspondingly small compared with other human problems.

At the present time, according to the Libby report (October 1956), there is in the stratosphere about 2.2 megacuries of Sr–90,[13] and a similar quantity of cesium 137.[14] If all the material in the stratosphere (in the fall of 1956) were to descend upon the surface of the earth uniformly, the amount of either Sr–90 or Cs–137 would be about 12 millicuries per square mile. The time of retention by the stratosphere of highly dispersed fission products is on the order of many years. Measurements indicate approximately 10 percent fallout per year and 2.5 percent radioactive decay. As about 25 percent has been added to the stratospheric reservoir of dispersed fission products during the past 2 years, the level in the stratosphere has remained nearly constant over that time. The quantity of Sr–90 in the soil of the United States is somewhat greater than expected from the fallout estimated on an average global basis; in the far west it is

[13] Strontium 90 has a half life of 25 years and decays by emission of a β particle of 0.54 Mev. maximum energy to produce yttrium 90. Yttrium 90 is short lived (half life 65 hours) it decays to the stable zirconium by emission of a β particle having a maximum energy of 2.24 Mev. Because of the short half life of the daughter product and the probable insoluble chemical form of yttrium, the radioactivity of strontium 90 is equivalent to both its own beta decay and that of yttrium 90.

[14] Cesium 137 has a half life of 33 years and decays by β emission (0.52 Mev. maximum energy) with associated γ emission (0.66 Mev. energy.

23 m C of Sr–90 per square mile. This is due to the heavier fallout in the near vicinity of a nuclear exposion.

Strontium 90 distribution to September 1955

	mC/square mile
Worldwide except the United States and Pacific Islands	3. 4[6]
United States, except Utah, Colorado, New Mexico, and bordering regions	4. 9[6] / 12. 5[5]
Utah, Colorado, and New Mexico	20–23

The specific ratio of Sr–90 to normal calcium is a convenient way of expressing the Sr–90 problem.[15] This is because strontium closely follows calcium in chemical behavior. The levels of Sr–90 directly measured in young human bones during the period up to October 1956 are in the vicinity of 0.0038 r/year to the bone. Strontium 90 is deposited preferentially in the bone by a factor of more than 100 over the soft tissues, so that only the bones need be considered with regard to this isotope.

The Libby report estimates, on the basis of a balance between accumulated fallout of Sr–90 into the soil and uptake by cattle and man, that in America the human ratio of Sr–90 to calcium may eventually become 10 to 30 percent of that observed in the topsoil. The report estimates that Sr–90 now held by the stratosphere, in descending to the earth over the next 4 years, will produce a human Sr–90 concentration of from 0.016 to 0.038 r./yr. (0.004 to 0.010 MPC[16]), assuming that no further Sr–90 is added. The range of this expected gain of radiation exposure is equivalent to the extra cosmic radiation exposure experienced by individuals dwelling at altitudes of 5,000 feet (e. g., Denver, Colo.) compared with individuals at sea level. The estimation assumes that there is a selection factor[17] favoring calcium over strontium in uptake from soils into the plant and into the cow and into the human bones, so that 70 to 90 percent of the soil strontium is rejected in favor of calcium.

Both human adults and stillborn babies have similar concentrations of Sr–90 (i. e., similar Sr–90/Ca ratios). This is to be expected, since the developing child draws its calcium from the maternal calcium pool, which is in partial equilibrium with maternal bone. Both these human sources of measured Sr–90/Ca have been placed during 1954 and 1955 at approximately one-sixth of the value for cow's milk; the resultant adult human bone irradiation value for this period is about 0.0019 r. yr. (0.0005 MPC) from the Sr–90 content. Reported values for adults

[15] A convenient concept, established by relating irradiation of bone to bone cancer, is that a maximum permissible concentration (1 MPC) of strontium 90 is equal to 1 μC Sr–90 per 1,000 grams of calcium. The concentration of calcium in the bones is such that 1 MPC can also be expressed as 1 μC Sr–90 per 7,000 grams of bone. The concentrations of radioactive strontium are usually expressed in units of 0.001 MPC; the equivalence is 0.001 MPC $= 1.4 \times 10^{-7}$ μC Sr–90 g of bone, corresponding to 0.0038 r/year.

[16] MPC=maximum permissible concentration.

[17] Harrison et al. have evidence that elemental strontium-to-calcium ratios, compared in food, blood plasma, and bone, are strikingly different; for man they are:

	g Sr/g Ca	Proportional units
Food	17×10^{-4}	7
Plasma	4×10^{-4}	2
Bone	2.5×10^{-4}	1

This is confirmed by Comar in observations using radiostrontium and radiocalcium simultaneously added to the diet. In Comar's observations for milk, the discrimination achieved against strontium in the deposition ratio of Sr/Ca may be less than that for other food sources, in which strontium and calcium may have different chemical binding.

The problem of a protective discrimination for humans against the uptake of the maximum Sr–90/Ca ratio is presented in the Libby report. At this stage of understanding, this apparent reduction of Sr/Ca in bones of humans compared with soil, plants, or animals seems to reside partly in the large calcium pool of the adult cow's body, which constantly dilutes incoming strontium and calcium so that milk, at present, is always intermediate in Sr/Ca ratio between the cow's bones and the forage. Similarly, the human calcium pool dilutes incoming Sr/Ca (largely from milk products) so that human bones at this time always have a lower Sr–90/Ca ratio than cow's milk or cow or calf bones. The content of children's bones is much higher than in adult or stillbirth material. There is some evidence for atomic discrimination between strontium and calcium, but the problem needs further study to determine how much of Sr–90 uptake by bone is lessened at fallout equilibrium. If only dilution operates, with little or no discrimination, humans will develop a higher Sr–90 level than is now expected.

did not exceed 0.004 r. yr. in the sample studied, except for one individual measured at 0.008 r. yr. This is a very small number in terms of radiation effect.

If, in the fallout to be expected, the discrimination against Sr–90 in its course from soil to plant to human bone is by only a factor of 50 percent instead of a factor of 70 percent to 90 percent, Libby's estimate of the future Sr–90 concentration would have to be increased to 0.075 r. yr. (0.020 MPC), based on the present stratospheric and soil burdens. This level of Sr–90 would represent an additional radiation exposure to the bone, equivalent to the additional cosmic radiation experienced by those who dwell at 10,000 feet in this latitude.

Libby has estimated, from soil calcium levels, that if the entire Sr–90 burden reached the soil and humans came into equilibrium with the top 2 inches of average soil, humans would eventually approach a maximum value of 40 $\mu\mu C$ Sr–90/g Ca, or about 0.15 r. yr. of bone irradiation. Such a value would approximately double bone irradiation over natural radiation.

ESTIMATION FROM HUMAN BONE ASSAYS OF FUTURE HUMAN BONE CONCENTRATIONS OF STRONTIUM 90

The uptake of Sr–90 has been directly measured in human bones as a function of age, and of location and time of collection (Libby, (5) Kulp et al.). The following summary conclusions can be drawn from analysis of this information:

1. Strontium 90 content of the bones in human stillbirths is increasing and, on the average, is estimated from Libby as follows:

United States of America	$\mu\mu C$ Sr-90/g. Ca	Percent increase per year
December:		
1953	0.14	
1954	.30	114
1955	.66	120
1956	[1] (1.3)	(100)

[1] Extrapolated.

2. The bones of stillborn humans have a much lower Sr–90 content than those of year-old children. The Sr–90 content of children's bones, which may be averaged from the Libby report, is given in table IV. This table is representative of the Sr–90 concentration observed in children of early ages at two study intervals, namely September 15, 1954, and August 1, 1955, average collection dates. Newborns (stillbirths) have a much lower Sr–90 concentration, because the uterine source of Sr–90/Ca has some intermediate value between dietary Sr–90/Ca and adult tissue-bone Sr–90/Ca. The value for stillbirths, as of January 1955, is 0.31 $\mu\mu C$ Sr–90/g Ca; at this same time, growing children, age 0 to 5 years, are laying down Sr–90 at 2 $\mu\mu C$ Sr–90/g Ca. Thus, the fetal tissues appear to have available to them only 0.31/2=0.16 as much Sr–90 as the growing child. This is a reasonable fraction, considering the lesser relative amount of milk products consumed by the average mother and the fact that her tissue stores of calcium are largely from the prefallout era. The growing child at each interval of growth (i. e., 0 to 1 year, 1 to 2 years, etc.) dilutes the entering Sr–90/Ca by the existing quantity of Sr–90/Ca already present in the body. However, analysis of the increment increase in Sr–90 content shows that children of all ages are consuming and laying down equivalent concentrations of Sr–90/Ca, and that in January 1955, this concentration was approximately 2 $\mu\mu C$ Sr–90/g Ca.

On this date, three sources of milk showed the following ratios:

Radiostrontium content of milk samples, January 1955

	$\mu\mu C$ Sr-90/g Ca
Foreign cheese (5)	2.0
Chicago milk (5)	1.9
New York milk (7)	1.6

Since growing children have milk as their chief source of Sr–90, it is as expected that the value of milk closely approximates the concentration of Sr–90/Ca being deposited in growing bones. These values imply that, should milk remain as it was in January 1955, all children born close to this date

will eventually have in their bones an average concentration of Sr–90 of 2 $\mu\mu$C Sr–90/g Ca.

However, the milk Sr–90/Ca is increasing, and has been increasing since monitoring of milk was begun in 1953. Eisenbud's report [18] gives the following.

TABLE IV.—*Strontium 90 content of cildren's bones (from Libby report)*

Age	Weight		Sr–90 content		Sr–90 content in newly formed bone [1] (corresponding to January 1955)
	Average at measurement	Δ/yr.	Aug. 1, 1954, to Nov. 1, 1954	June 1, 1955, to Oct. 1, 1955	
	kg	*kg*	*$\mu\mu$C Sr-90/g Ca*	*$\mu\mu$C Sr-90/g Ca*	*$\mu\mu$C Sr-90/g Ca*
Birth	3.3		0.25	0.53	
1 year	7.2	3.9	.54	1.16	2.2
2 years	9.6	2.4	.43	.87	2.1
3 years	11.5	1.9	.39	.68	2.2
4 years	13.4	1.9	.35	.54	1.7
5 years	15.1	1.7	.33	.44	1.3
Average					1.9

[1] See the following:

$$\frac{\Delta\ \mu\mu\text{C Sr}^{90}}{\Delta\ \text{g Ca}}=\left[\frac{\text{Sr}^{90}/\text{Ca}_{(t_2)}\times \text{wt}_{(t_2)}-\text{Sr}^{90}/\text{Ca}_{(t_1)}\times \text{wt}_{(t_1)}}{\Delta\ \text{wt}_{(t_2-t_1)}}\right]\times\frac{12.0}{10.5}$$

(The 12.0/10.5 is the correction factor for 10.5-month time interval Sept. 15, 1954, to Aug. 1, 1955.)

Sr–90/Ca content of milk in the New York area

Date:

	$\mu\mu$C *Sr–90/g Ca*
June 1954	1.2
January 1955	1.6
June 1955	2.0
January 1956	2.7
September 1956	5.0

The minimum estimate of future average human burden of Sr–90, then, is that 5 $\mu\mu$C Sr–90/g Ca will be present in the bone. This corresponds to the latest reported value for milk concentration and to the fact that bone acquisition of Sr–90/Ca in growing children is very similar to milk Sr–90/Ca.

A difficult current problem is the estimation of future Sr–90/Ca in milk. The level of Sr–90/Ca in milk is increasing, and, by linear extrapolation, may be expected to raise the Sr–90 concentration in a year's time (by September 1957) to about 7 $\mu\mu$C Sr–90/g Ca. At this date, accumulated fallout of Sr–90, based upon the quantity estimated at the time of the Libby report, may be about 25 percent [6] to 50 percent [5] of the amount initially dispersed in the atmosphere. Since the Libby report was written, other nuclear detonations have occurred, so that it would be very reasonable to assume that fallout; by some 10 years from now, should have increased milk levels significantly. For lack of better information, we may assume a factor of, say, 3 to 5 times as much as September 1957 (allowing for residual hold-up in the atmosphere and for decay of Sr–90). Thus, milk levels and human bone levels by 1967 may be 20 to 35 $\mu\mu$C Sr–90/g Ca.

An additional factor must be considered, which may require that these future estimates be even higher. Cows, in body content of Sr–90/Ca, may be expected to lag several years behind the plant and soil levels. This is because of the large calcium reservoir in their bones and other tissues, and bcause the start of growth to milk-producing stage preceded current time by 4 or more years; moreover, the food consumed by dairy cows is customarily stored for many months before it is eaten. It is difficult to estimate that point in fallout time that corresponds to current milk values; it seems likely, however, that the Sr–90/Ca content of the bones of pasture-fed calves approximates the Sr–90/Ca level that adult cows

[18] Merril Eisenbud, Global Distribution orf Radioactivity From Nuclear Detonations With Special Reference to Strontium 90, Washington Academy of Sciences, fall symposium, November 15, 1956, Washington, D. C.

would secrete in their milk, were they in more rapid equilibrium with fallout. The following table, derived from values averaged from Libby's and Eisenbud's reports, shows that calf bones are approximately 60 percent higher in Sr-90/Ca content than milk. Thus, future estimations of Sr-90 levels should be at least 60 percent higher than the 20 to 35 $\mu\mu$C Sr-90/g Ca estimate, or, say, 30 to 50 $\mu\mu$C/g, in round numbers.

Strontium 90 content of various materials (in $\mu\mu$C/g Ca)

	Mid-1953	Mid-1955	Mid-1956
Milk	1.1	2.1	3.6
Calf bones	1.4	3.5	5.7
Alfalfa (Wisconsin)	6.7	18.0	
Soil (Wisconsin):			
2 to 6 inches depth		9.0	
0 to 2 inches depth		35.0	

It appears that the Sr-90/Ca of cow's milk is a close index of the concentration of strontium in newly acquired human bone. Current milk levels suggest that children's bones in the next decade will approach an average concentration of approximately 50 $\mu\mu$C Sr-90/g Ca. This is in close agreement with the estimation by Libby of a minimum average concentration of Sr-90 of 10 to 40 $\mu\mu$C Sr-90/g Ca. These estimates do not consider local variance in the United States, nor, with respect to future concentrations, the special problem of high-rainfall or low-calcium areas.

The upper value of approximately 40 $\mu\mu$C Sr-90/g Ca has been set by Libby upon the consideration that this is the projected specific concentration ratio when all the fallout is complete and mixed with the average calcium content of 2 inches to topsoil. There does not seem to be a way of independently confirming the upper average limit of radiostrontium concentration from observation of milk or bone. The biological concentrations are increasing rapidly with respect to time, approximately following the level of total accumulated fallout, and 40 $\mu\mu$C Sr-90/g Ca may not truly be a limit.

Whatever the speculation concerning future levels of Sr-90 in humans, we can be certain that current values (1956) represent a low level. If we translate a small dose such as 0.0038 r./year (0.001 MPC) into numbers predicting an increase in leukemia mortality (an estimate may be based upon tentative data that leukemia tendency may be doubled by 50 r. whole-body exposure[19]), an increment of

$$\frac{0.0038 \text{ r./yr.} \times 50 \text{ years mean life span}}{50 \text{ r./tumor doubling}} = 0.004,$$

or 0.4 percent increase in leukemia, is estimated. Since there are only approximately 8,000 cases of leukemia deaths reported in the population of the United States per year (plus 2,000 cases of bone-tumor deaths, which may be similarly affected by radiation), such a radiation burden is equivalent to an increase of 40 cases per year after 50 years' equilibration with this level of fallout. If radiation fallout and uptake of Sr-90 in human bones were to increase by a factor of 10, one could estimate 400 additional cases of bone tumor and leukemia induced per year after a 50-year period, in comparison with 1 million deaths from all causes and 10,000 expected deaths from leukemia and bone tumors. Both above numbers are small in comparison with overall public health problems.

Although there are some sizable uncertainties regarding Sr-90 burdens during the next 10 to 20 years, it seems from the average human values that Sr-90 may increase and become a public health problem if levels should rise to 50 $\mu\mu$ CSr-90/g Ca (equal to about 0.2 r./yr. to bone). There is time—but not much time—for a reevaluation of many unsatisfactorily estimated aspects of this problem, including the extent to which radiation exposure induces leukemia and bone tumors, and more precise estimation of the strontium levels in humans.

[19] This number may be high, since it is based upon whole-body radiation exposure, while induction of leukemia by Sr-90 exposure is the result of direct irradiation of bone and marrow, the specific tissues involved in the leukemia change.

At the reference level of 1 MPC of Sr–90 burden, which is 4 r./yr. to bone, an estimated increase in bone tumors and leukemia is

$$\frac{4\text{ r./yr.}\times 50\text{ years}}{50\text{ r. per doubling of incidence of tumors}},$$

or an approximately fourfold increase in natural expectancy of these neoplasms with respect to the radiation-related component of their origin. This level may be reached by humans as a result of Sr–90 fallout. At some such value, reason argues against further exposure. The 1–MPC value based on radium exposure is consistent with a prediction of a fourfold increase in natural incidence of tumors. It would be difficult to observe a fourfold increase above natural incidence of bone tumors in animal-colony studies with radium, but not at all difficult in large human populations.

In summarizing their opinion for the British Report Cmd 9780, Mayneord and Mitchell write, "It appears however that each unit quantity of radiostrontium absorbed by bone confers a certain probability of bone-tumor formation, the tumor development time perhaps decreasing and the tumor incidence increasing with the dose. On the whole, the experiments seem in favor of a proportionality between the frequency of tumors produced in a given length of time and the amount of radioactive material in the body even at low-dose levels."

The problem in the experimental animal is that the frequency of bone tumor appearance is so slight that statistically significant increases in the frequency are not to be expected as a result of irradiation. The human problem is similar in that osteogenic sarcoma and leukemia are relatively unlikely occurrences, together causing about 1 percent of adult deaths in the United States, so that a small percentage change in incidence caused by radiation could not be distinguished from random fluctuation, and a relatively large fractional increase in the number of these cancers would not appreciably increase the total death rate.

No gross evidence of osteogenic sarcomas has been observed following administration of P–32 (approximately 100 rep to bones) to polycythemia vera patients. However, these patients do have a high incidence of leukemia. This leukemia tendency is probably attributable to both the radiation exposure and the nature of the basic disease of the blood-forming system in these patients.

Special phases of the Sr–90 problem need additional examination:

(*a*) In several areas of the world, Sr–90 concentration exceeds the average world values by more than a factor of 10(4, 5). This excess poses questions as to the origin of the enhanced concentration. To a reasonable extent, it is explained by Libby as calcium deficiency of soils in such areas. Rainfall variation also leads to variation in fallout. It will be useful to know more about these anomalous effects. Current worldwide sampling is perhaps far from representative of the world as a whole, because special effort was made to seek out low-calcium high-rainfall areas.

(*b*) There may be a factor-of-8 difference between Sr–90/Ca concentrations in soil and in humans, resulting from discrimination in favor of calcium (Libby); this must be further studied.

(*c*) Some factor of uncertainty must be allowed for in the prediction of levels today and in the early future of Sr–90 in humans, considering that the most recent of these measures are based on early 1956. These uncertainties may amount to a factor of somewhat more than 10.

(*d*) Although it is unlikely that all these factors would reach their maximum, nevertheless, the total uncertainty in the estimated human burden of Sr–90 throughout the world could mean an upper limit of 10×8×10×Libby's lower estimate of exposure in the near future, 0.02 r./year, which works out to about 15 r./year or 4 MPC. This possibility indicates that the Sr–90 fallout problem urgently calls for further attention.

CESIUM–137 FALLOUT

The Cs–137 problem is quantitatively similar to that of Sr–90. These two fission products are present in the air and in fallout in approximately equivalent quantities (5), and they have similar decay rates. Whereas strontium is a bone seeker, cesium is found in approximately equal quantities throughout the body, though less in bone than in soft tissues. Its distribution roughly approximates that of potassium. Furthermore, cesium is not retained by the body. Thus, the cesium burden at any given time rapidly reaches equilibrium with the rate of fallout, in the potassium pool in plants and animals.

Marley, in the British report (4), writes (p. 124), "The highest body-activity detected so far in the United States is found to be $4 \times 10^{-3} \mu C$. This activity if maintained would produce a total body irradiation of 0.0006 r per year or about one-thirtieth of the dose due to naturally occurring potassium-40 in the body." Since this time in early 1956, the fallout level and fallout rate of Sr-90 have been increased only slightly, so that we may assume that the Cs-137 level in man, which is more reflective of immediate fallout, may have risen by as much as a factor of 2. It should remain nearly at this level, estimated as a maximum of 0.0012 r/year, for an indefinite period.

Cesium-137 body burden at 0.001 r/year is certainly not to be considered an adult hazard. With a linear relation between effect and dosage, 0.001 r/year over a lifetime would be less than 0.1 r, and irreversible accumulative effects of radiation, such as leukemia, might be increased by less than

$$\frac{0.1 \text{ r.}}{50 \text{ r. leukemia-doubling dose}} = 0.002, \text{ or } 0.2 \text{ percent.}$$

Stated in terms of life span lost or of the total tendency toward disease, 0.1 r Cs-137 dose $\times$—10 days of life span per r amounts to 1 day lost from the life span. A loss of 1 day is very small compared with health-modifying factors that are measured in years instead of days. Thus, in comparison with the smoking problem, the long-term effect of Cs-137 is approximately one-forty thousandth as deleterious. Only this extraordinary method of estimation by extrapolation of effect can convince the human reason that there is any such effect at all; even the best statistical procedures could not detect it through study of the most accurate data on the 160 million people in the United States. A 0.2 percent increase in leukemia (which is approximately $0.002 \times 8{,}000$ cases per year) is just 16 additional cases. This 8,000 expected normal incidence can fluctuate by random interplay of chance factors by plus or minus 1 percent, equal to 80 cases per year; thus, 16 cases of increased incidence cannot be detected.

THE LEVEL OF RADIATION EXPOSURE FROM FALLOUT

The total increase in background radiation on a global basis, as a consequence of radioactive fallout, has been very slight. In the preatomic age, natural sources of radiation produced an average radiation exposure of 0.1 to 0.2 r/year. The variation is due to slight geographic differences, to differing radioactive content of earth and buildings, and to the variation of cosmic radiation with altitude. At 5,000 feet above sea level, cosmic-ray intensity (measured by numbers of ionizations produced in matter) is increased to 1.5 times the sea-level intensity of cosmic rays; at 10,000 feet, the cosmic-ray ionization is 3 times that at sea level.

The increased human-tissue irradiation due to fallout and ingestion of radioisotopes is approximately as follows:

	Soft tissue irradiation (r/year)	Bone irradiation (r/year)
1955-56:		
Cs-137	0.0009	<0.0004
Sr-90	0	[1] 0.002 [2] 0.004
Predicted future values:		
Cs-137	0.0012	<0.0066
Sr-90	0	0.04 to 1.5

[1] Adult.
[2] Young.

Table V lists human radiation exposures from a number of sources.

TABLE V.—*Human radiation exposure (r./year)*

A. EXTERNAL EXPOSURE TO WHOLE BODY

Natural radiation exposure		Exposure from fallout from nuclear detonations	
1. Cosmic radiation at—		1. Fallout to earth, October 1952 to September 1956:	
Sea level	0.038	Salt Lake City	0.050
2,000 feet	.043	All other United States cities and other countries	.003
5,000 feet	.056		
10,000 feet	.112		
15,000 feet	.214		
2. Radiation from the earth:			
England	0.04		
Berkeley hills	.12		
Sweden	.09-0.16		
Average	.10		

B. INTERNAL EXPOSURE

	To gonads and soft tissue	To bone		To gonads and soft tissue	To bone (1955)
Potassium 40	0.020	0.005	Strontium 90	(1)	[2] 0.002
Carbon 14	.001	.001			[3] .004
Hydrogen 3	(1)	(1)	Cesium 137	0.001	(1)
Radium	.002	.120	Iodine 131	(1)	(1)
Average	.023	.126	Average	.001	[2] .002
					[3] .004

C. TOTAL EXPOSURE

Prefallout:			Now:		
At sea level	0.164	0.264	At sea level	0.165	0.269
At 5,000 feet (Denver)	.179	.282	At 5,000 feet (Denver).	.183	.287

D. INTERNAL EXPOSURE UNDER OTHER CONDITIONS

1955:			Predicted for individuals born now (if no additional nuclear detonations):		
Strontium 90:					
Average value, USA	------------	0.0019			
Highest value reported	------------	.0075			
			Minimum average value predicted by Libby—0.004 MPC	------------	0.016
			Maximum average value predicted by Libby for United States—0.010 MPC	------------	.038
Certain low-calcium areas	------------	------------	Certain low-calcium areas	------------	0.16-0.38
If humans at equilibrium should approach the Sr/Ca ratio of plants rather than 10 to 30 percent of plant Sr/Ca ratio	------------	------------	If humans at equilibrium should approach the Sr/Ca ratio of plants rather than 10 to 30 percent of plant plant Sr/Ca ratio	------------	0.10-1.5
Iodine 131:			1956:		
Thyroid	[4] 0.004	------------	Iodine 131	<0.001	------------
			Probably	.0002	------------
			Estimated	< .000001	------------
Thyroid (maximum measured by Van Middlesworth)	[4] 0.004	------------			
Other than thyroid (estimated)	<0.000001	------------			

[1] Too small to be considered in this tabulation.
[2] Adult.
[3] Young.
[4] Possibly the true value is 0.001 or less.

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The problems of radioactive fallout may also be examined in comparision with other ways of acquiring exposures to radiation (English values for radiological exposure are generally much less than in America (4)). (See table VI.)

Thus, it is possible that, from common use of X-ray-generating devices, the average person in the United States has already begun to accumulate an exposure to radiation effect that is sizable compared with the fallout problem. That no gross evidence of disease has become evident during these past few years of increasing radiation exposure does not disprove the existence of slight average effects of radiation. For example, at current estimation of leukemia induction by radiation, about 20 percent of the relatively rare cases of leukemia (0.5 percent of adult fatalities) may be attributable to natural radiation. There is no difficulty in believing that supplementary radiation resulting from our modern activities may have been responsible for the other 80 percent of known cases of leukemia; the average additional artificial radiation exposure per year would only have had to be 0.8 r. to account for this difference. Considering the generous use of unshielded and unfiltered X-ray equipment in dental offices and shoestores alone, and the lack of public and professional appreciation of need to minimize radiation exposure, it is even reasonable to conjecture that the addition of artificially created radiation exposure to natural irradiation may essentially account for leukemia. Faber has analyzed 828 cases of leukemia registered in Denmark in the period 1950–53 with regard to the amount and type of irradiation each patient received for 20 years prior to development of leukemia. The incidence of previous incidental X-ray or radiation exposure for the chronic lymphatic leukemia cases was 18 percent, for myeloid leukemia, 30 percent, and for acute leukemia, 32 percent. It appears that both acute leukemia and myeloid leukemia can be induced by radiation; and the traceable X-radiation induction may account for a sizable percentage of current cases in Denmark. Faber's information does not rule out that lymphatic leukemia may also be induced by radiation. The analysis of leukemia incidence in followup of three groups of individuals who had had varying exposures to X-rays or other radiation strongly suggests that the radiation induction of leukemia is proportional to the radiation exposure, and that for whole-body radiation exposure the number would be entirely consistent with an estimation that 50 r. doubles the chance of development of leukemia.[30]

TABLE VI.—*Common means of exposure to radium*

Source	Exposure	
	Directed to the specific body region	Scattered to the whole body (dose per use)
Routine chest X-ray [1]	0.05 to 2 r./exposure	
Fluoroscopic examination [2]	10 to 20 r./min	1/200 to 1/1000 of local dose.
Cinefluorography [3]	25 r. per examination	
Dental X-ray [1]	10 to 150 r. per whole-mouth series.	0.01 to 1 r.
Shoestore fluoroscopy [1, 2] shoe-fitting unit	50 to 150 r./min. to feet	1 to 10 r./min.
Radium-dial watch [1] μ C/watch	7. r/yr. to the wrist	0.01 r./year.
Radium and X-ray [1, 2, 3] technicians (throughout the world).		p.1 to p.3 r./week, 5 to 15 r./yr.
AEC maximum permissible dose for 20 years' exposure.		15 r./yr. at 0.3 r./week.
Average accumulated exposure of 10 most highly exposed individuals over 5-year period—U. C. Radiation Laboratory.[4]		0.1 r./week, 5 r./yr.

[1] William Nolan.
[2] AEC Report to Congress.
[3] Jones.
[4] University of California Radiation Laboratory records.

RADIOIODINE FALLOUT

Of all the problems that we can currently evaluate, the radioiodine fallout problem is disposed of most readily. Radioiodine is produced in thousands of curies by some of the nuclear detonations, and, in falling to the earth's surface, it contaminates grass and is eaten by foraging animals. In its fallout, it is

[30] Court-Brown and Doll, Summary of Leukemia Induction, British Report (4), pp. 84–89.

greatly diluted and does not at any time become a human problem. The herbivorous animal, however, eats large quantities of grass; and in the cow, for example, essentially all the iodine 131 ingested accumulates in the thyroid gland. Over a few days' time, several hundred pounds of grass may be eaten, and all the iodine contained becomes concentrated in the 15 to 30 grams of thyroid tissue. Following nuclear detonations of the last 2 years, the thyroid concentrations of radioactive iodine in pastured cattle reached as high as 0.001 to 0.003 mμC/g (depending upon the quantity of fallout), and the average radiation exposure, as measured over 3 years, was about 1 r./year to the thyroid tissue. This would be of genuine concern to man at similar human burdens of I–131, because it is now known that thyroid tissue is especially sensitive to radiation induction of tumors. However, cattle fed principally in feed lots have only 1/100 (or less) as much I–131 as range-fed cattle. Further careful measurement of fresh human thyroid material has been routinely made during the last 2 years by techniques that are sensitive and reliable for estimation of I–131 content. Direct measurement shows that human thyroid, at any time of high uptake of I–131 by bovine thyroid, has less than 1/5000 of the bovine I–131 content. It is possible that human thyroids had less than 0.0006 mμC/g during the latter part of 1956, when range cattle had 1 to 2 mμC/g. It is certain that the human thyroid exposure during the 1956 period did not exceed 0.001 r./year, and the probable value is 0.00016 r./year or even less. (Interestingly, one human thyroid showed an activity comparable with bovine thyroid content of I–131; the case, when traced to its source, proved to be from a man who had previously been given a small tracer dose of I–131 in the Donner Laboratory. The observed quantity of I–131 was accounted for by the magnitude of the dose, the estimated excretion, and the radioactive decay.)

Up to this time, radioiodine from worldwide fallout is not a problem of concern to humans; and it is not expected that it will become a problem in the future.

SUMMARY

1. This paper reports a broad examination of the levels of radiation exposure incurred from fallout. The discussion is limited to Sr–90, Cs–137, and I–131, the only radioactive isotopes reported to become associated with human environmen in detectable quantities.

2. The worldwide effect of radiation from fallout is now far less than that of naturally occurring radiation from cosmic rays and from radioactive elements normally contained in earth, buildings, and body tissue. The inescapable minimum of natural radiation exposure, for all people, is about 0.1 r./year. The average person at sea level in the United States is probably receiving about 0.16 r./year.

3. During 1954–55 the Sr–90 concentration in human bones (both in adults and in stillborn infants) produced an average exposure to the bones themselves of 0.002 r./year. (Only the bones—not the soft tissues—are exposed to measurable levels of Sr–90 irradiation.) At current fallout trends, the irradiation of human bone by Sr–90 will increase to 0.016 r./year, perhaps even to 0.038 r./year (Libby). The maximum value projected in this discussion is 0.2 r./year. (These are average predictions for the northern hemisphere and for the major population densities of the earth.)

4. Radioiodine (I–131) activity has been measured in humans during periods of likely fallout exposures. Radiation exposure from fallout I–131 is essentially nil for humans.

5. Any analysis of the fallout of radioactive materials on a worldwide basis shows that it does not even remotely approach the threshold for acute radiation effects, which cannot be recognized below 100 r. in a single exposure. Radiation predicted from future fallout is still far less than natural radiation background. Increases in the internal radiation exposure of 0.1 r./year are not meaningful in comparison with acute radiation damage. Attempted comparisons are responsible for most misunderstanding of the fallout hazard to humans.

6. Life-span changes, cancer or leukemia induction, and cell changes appear to be proportional—as are genetic effects of radiation—to radiation exposure. Although these effects are not measurable in any individual exposed to fallout, they can be estimated, in terms of very small risks. The effects are dwarfed

in comparison with the adverse environmental hygienic factors that most persons regard as commonplace. For example:

Factor:	Life-span loss per person (days)
Smoking one pack cigarettes per day	3,000
Being 25 percent overweight	1,300
Having 25 percent elevated lipoproteins	2,500
Living in United States as a driver of an automobile	470
Working in industry (industrial hazard)	100

7. The evidence indicates that Sr–90 may eventually cause a worldwide increase in leukemia, accounting for about 2 percent of all deaths. Compared with the current accident rate, a 2 percent leukemia increase distributed throughout the entire population would be a life-span loss of about 1.0 year per person in the United States; all accidents account for a 2.3-year life-span loss per person, automobile use for 0.87 year. Thus the Sr–90 induction of leukemia is comparable with some of the mechanical mishaps we risk as a partial cost of the "advantages" of our mechanized and energized age.

8. The sum of evidence is that radiation has a deleterious effect upon man's health, but that the effects are extremely small at such slight radiation exposures as are involved in the worldwide fallout. Nevertheless, since radiation probably does affect man's health and progeny—even though minutely for minute exposures—incurring it should be treated as the equivalent of the spending of money or time, and should be allowed only for necessary gainful advantages.

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Interpretation of the Atomic Bomb Casualty Commission Report of Physical Growth of Hiroshima Children Exposed to the Atomic Bomb

Hardin B. Jones

Hiroshima children 5 to 19 years old (approximately 4,000, divided into 4 categories—males and females, exposed and unexposed) studied 5 years after exposure to the atom bomb are significantly shorter and lighter in weight than unexposed children, according to the report of Earl L. Reynolds to the Atomic Bomb Casualty Commission. Statistically the effect is slight in either sex, since weight is depressed about 2 percent and height is depressed about 1 percent (the standard error is 0.35 percent for groups of this size). Average individual variation in height or weight is about 12 percent (standard deviation) in Hiroshima children; therefore, the effect on height or weight of average radiation in the exposed group is approximately $\frac{\text{1 to 2 percent}}{\text{12 percent}} \times 100$, or about 10 percent of the effects of factors generally contribuating to lower-than-average individual differences in grows.

The effect of atom bomb exposure upon growth is measurably correlated with distance from the hypocenter of the explosion. The actual relationship observed is slight but significant, the correlation coefficient being about +0.1 for relationship of children's size and distance from the bomb. Since it is very likely that true exposure dose is rather poorly related to distance from the bomb at exposure and correction of this defective information should increase the correlation, the true effect of exposure upon children's growth is probably larger.

An improved estimation of quantitative relationship between exposure dose and growth in these children is possible by using groupings according to initial severity of symptoms, on the hypothesis that indeterminate shielding factors protected those who suffered fewer symptoms than would be expected from the theoretical dose. In comparisons by age and sex groupings, the asymptomatic groups were significantly larger in average physique, whereas the groups with definite irradiation exposure symptoms were significantly smaller. Such comparisons lead to a rough direct estimation of correlation between exposure and suppression of growth. Chi-square values show that the growth depression associated with exposure is significant. When chi-square values are converted to correlation coefficients, the result is equivalent to a correlation of approximately —0.4, indicating that the true relationship between irradiation exposure and growth, in doses sufficient to produce acute effects, may be larger than the above estimate; the square of the correlation coefficient indicates that 16 percent of the observed difference in growth is probably due to the effects of irradiation. Roughly estimated, the growth-depressant effect in humans should be equal to:

$$\frac{\text{growth change}}{\text{roentgen exposure}} = \left\{\begin{matrix}\text{correlation coefficient}\\ \text{of growth vs. exposure}\end{matrix}\right\} \times \frac{\text{standard deviation of growth}}{\text{standard deviation of radiation exposure}}$$

as derived from the regression equation. The computed value in this case is:

$-0.4 \times \frac{\text{10 percent}}{\text{80 r.}} = -0.05$ percent per roentgen, all values being rough approximations.

The association observed between size and radiation exposure in Hiroshima is strikingly similar to that observed by Russell and Russell when mice were exposed to varying single doses of X-rays at 11.5 days after conception. Size of mice at birth decreased proportionally to radiation exposure from 100 r. to 400 r. The estimated effect upon embryonic growth per roentgen is 0.25 percent depression of size per r. It is noted that those children in Reynolds' study who were exposed in utero have twice the effect of decreased size seen in children exposed after birth; however, since the Reynolds study considered size 5 years after exposure rather than size differences at birth, as in the Russell study, the relative size difference at birth may have been even greater. Although the number of children irradiated before birth was too small to make the difference statistically significant, it is interesting to note that only a small correction factor would make the Hiroshima overall growth-depression ratio of 0.05 percent per roentgen comparable to the embryonic growth-depression ratio of 0.25 percent per roentgen observed in mice. These studies suggest that radiation effect upon both young mice and humans is the same with regard to suppression of growth.

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Representative HOLIFIELD. Now I suggest that we have a discussion. We have certainly had a heavy diet today of philosophy on genetics and facts presented, and theories. I am sure now we can probably focus on some of the points we think should be focused on at this time. I am going to ask Mr. Hollister to lead off with some questions.

DISCUSSION BY DRS. JAMES F. CROW, BENTLEY GLASS, A. H. STURTEVANT, HERMANN J. MULLER, W. L. RUSSELL, AUSTIN BRUES, AND HARDIN JONES

Mr. HOLLISTER. The first question, to Dr. Glass. Do we know specifically of any mutations in man that are radiation caused?

Dr. GLASS. No, you can't identify the individual mutations as being radiation caused or as having occurred spontaneously. The same kinds of mutations can occur through the action of radiation as may occur spontaneously. So you can't identify the particular mutations. There is aways a probability that even if the exposed person—the parent of the mutant person—received radiation, still the mutation might have occurred anyway. All we can go by is the increase in the frequency of mutations, which is thought to occur with a particular increase in dose.

Mr. HOLLISTER. So the radiation mutations produced in man or in any other organism are indistinguishable by kind.

Dr. GLASS. Exactly.

Mr. HOLLISTER. From any other sort of mutations.

Dr. GLASS. If we looked at the chromosomes we might find that a larger proportion of the radiation induced ones were visible losses, as has been brought out today. But just judging from the ordinary clinical symptoms, no, you could not distinguish them in kind from the spontaneous ones.

Representative HOLIFIELD. Are there any comments, or are you in general agreement? I see by your nodding that you are in general agreement.

Mr. HOLLISTER. I would like to ask, Dr. Muller, if you know of any mechanism seriously proposed that would predict a nonlinear effect for the mutation dose relationship.

Dr. MULLER. At high doses; yes. For what we call chromosome aberrations, but not for what we call gene mutations or point mutations.

Dr. Russell referred to a result of his and we, at our laboratory, obtained a similar one in flies, in which there was a less than linear apparent effect at very high doses, owing, as we judged, to the fact that the cells that had been worse hit were killed off more so that we lost the cases. But I do not see any way of getting a fundamentally nonlinear effect, especially at low doses. If the process takes place in any way like what we think it does, that is.

Representative HOLIFIELD. If any member of the panel wishes to comment, if you will raise your hand, I will recognize you. Dr. Russell.

Dr. RUSSELL. I should like to add to that that I think the possibility does still exist, if there is killing of cells at low doses. That is, down to a level at which cells are killed I think the possibility of departure from linearity might still exist. This level may be lower than we might tend to think from experiments on sperm. The spermatogonia seem to be sensitive.

Dr. MULLER. Might I add one word to that? If that is true, then it would work the other way from the way the people argue who believe in a threshold, because it would mean a relatively greater effect at low doses.

Dr. RUSSELL. Yes. I tried to bring this out, that it would be an increase in the effect, rather than less. But I do not think departure from linearity would be expected at such low levels as 0.1 roentgens, for I don't think there would be enough killing of cells at this dose level to make any difference. This would perhaps apply at the 10-roentgen level or something of this order. Down at the very low doses I would agree 100 percent with Dr. Muller that linearity would be expected.

Representative HOLIFIELD. Dr. Jones.

Dr. JONES. It is certainly true that several ionization events may be necessary for critical change within the cells. The ravages that can take place in the cell with time are so similar to radiation-induced events that even though one ionization occurs and several are needed, perhaps the preceding natural aging events have made the internal climate such that one ionization could produce that change.

In this sense we perhaps have a further reason for accepting the hypothesis of a proportional change down to very, very small doses. Of course, at these small doses we have very small effects.

Dr. RUSSELL. Could I add one more point to this? My point is that at the very low doses you would still expect linearity, but the slope of the line at these low doses might be steeper than would be predicted from our present data at the 300 and 600 r. points.

The question is, Have we measured the full effect at the present time? We think we need to measure the mutation rate at lower doses to be sure.

Representative HOLIFIELD. Thank you.

Mr. HOLLISTER. I would like to ask if it is not true that you as geneticists—when you draw a line between a good and a bad muta-

tion—do this almost entirely on the basis of how the mutation affects reproduction. Who would like to answer that. Dr. Crow?

Dr. CROW. From the standpoint of some theoretical computations that are made in assessing damage in the long-time future, this kind of an assumption is made. But in general, although we realize that most harmful mutations have some harmful effect on survival or reproduction, I think most of us—probably all of us—would be agreed that this is not really the point which as humans we are interested in. We are interested in the pain, disease, misery, all of which are associated with the reduction in survival value.

Mr. HOLLISTER. In other words, it is possible you could get into a situation, for example, where by process of continued mutation and selection, you could achieve a population that would reproduce very prolifically, but would have all sorts of other defects, that is, susceptibility to diseases and shortened life span and so on.

If we apply this criteria of good and bad mutation, you could do this?

Dr. CROW. As a very remote theoretical possibility. I don't think that is a likely possibility at all.

Mr. HOLLISTER. Dr. Crow, another question for you. This is the question of numbers of individuals limiting in the quantitative studies of natural mutative rates either in man or other organisms. Is the size of the population that you have to work with a limiting consideration in the kinds of experiment that you can perform or the kinds of data you can obtain?

Dr. CROW. In the study of human populations?

Mr. HOLLISTER. Either in humans, fruitflies, or mice.

Dr. CROW. I would say the numbers are the principal limiting factor. If one studies humans one cannot make experimental matings and that is serious. The organisms that are preferred for research purposes are those that you can grow cheaply and in large numbers, and that is why the fruitfly has been used.

Is that the point of your question? I am not quite sure.

Mr. HOLLISTER. My understanding is that you had postulated somewhere that there was some size or some number of individuals, perhaps a million or perhaps 10 million, that was a practical maximum size that an experimenter could work with.

Dr. CROW. I think you are confusing me with the U. N. committee. I cannot think of the man's name. Yes, Dr. Appleyard.

Mr. HOLLISTER. That is possible.

Dr. CROW. I think these are statements from Appleyard who has done some computations on the size of population that would have to be studied in order to detect differences of a certain small magnitude.

Mr. HOLLISTER. Is it not true that in some of these studies he has concluded that the populations have to be enormous?

Dr. CROW. That is correct. One can reach those conclusions even before seeing his figures.

Dr. MULLER. Could I interpose something since we mentioned the U. N. committee? I received information from someone in the audience in regard to a question that was raised or that I raised during my talk about the composition of the U. N. Scientific Committee. I stated that Dubinin had been scheduled to appear from Russia. I got information that he did not show up. Also, that he had not been

scheduled to be a delegate, but an adviser or consultant. Also, that at the most recent meeting there were various advisers present from other countries who were geneticists, not delegates, however. The geneticists who were delegates were confined to those I listed. Even though most of the discussion was done by the consultants rather than the delegates, it is an unfortunate situation if geneticists are not actual delegates, because although they were evidently given free rein during the discussions on the effects on posterity, nevertheless, as I indicated before, the effects on the exposed generation itself are also very important, and are very possibly closely related to the genetic subjects. Therefore, the committee should have geneticists to discuss those effects also.

Representative HOLIFIELD. Without objection, the record will be corrected.

Dr. MULLER. Yes, sir; it should be corrected. Our country had two good geneticists as consultants.

Mr. HOLLISTER. Dr. Muller, do you suppose that something analogous to the uncertainty principle in physics could exist in genetics with regard to this threshold question? For example, presumably we have to have an effect, tested in a certain number of cells. Leukemia, bone tumor, these have to involve more than one cell for us to observe. If this is true, does not this of itself indicate a possible threshold when in fact there might not be one. That is, we cannot observe a leukemia in 1 cell, can we, or a bone tumor involving 1 cell? Presumably it involves many cells.

Dr. MULLER. If the individual is unlucky enough by reason of the cell being lucky enough, then the one cell that became leukemic could give the individual leukemia.

Mr. HOLLISTER. But you would not know this until it involved more than one cell?

Dr. MULLER. No. There could be a mutation in one cell if our conception is right.

Mr. HOLLISTER. You would not know this experimentally?

Dr. MULLER. You would know it experimentally if that cell was in position in which it could express the tendency given to it by the mutation to divide or to reproduce or multiply in an uncontrolled manner. After a while it would crowd out the other cells and the person's blood would be full of these white cells, and he would have leukemia.

Mr. HOLLISTER. The thing that you would measure would be the presence of leukemia in the person after some of the multiplication had occurred.

Dr. MULLER. That is right.

Mr. HOLLISTER. So you would not know he had leukemia until it showed up as a result of many cells being pathologic?

Dr. MULLER. I think the question is on the same basis as all other genetic questions. All you get is a more or less random sample. You have to judge by that sample. That is why you need large statistics.

Representative HOLIFIELD. Dr. Glass has something to say on that.

Dr. GLASS. May I speak to that question, too? The more we press back into the knowledge of how the genes produce their effects, the more possible it becomes to detect the nature of those effects in individual cells. Thus even in a tissue culture of human skin epithelial

cells, if a mutation occurred in the one gene that we know of that controls the production of pigment, and changed it to an albino type, you would not have to have a thousand cells or a whole individual to know that mutation had occurred. You could spot it in that one cell.

If we knew enough about the biochemistry of different cells and enzymes, we could easily detect this in single cells.

Senator ANDERSON. Dr. Crow, do you agree with that? It looked as if you did not.

Dr. CROW. Let me get into this act, too. Back to what I think maybe you have in mind. At the present time we can demonstrate a linear effect perhaps down to 25 r. We could do a very large experiment and perhaps demonstrate a linear effect down to 10 r. We could do an enormously large experiment and demonstrate it down to 5 r. One cannot continue indefinitely. If that is what you mean by an uncertainty principle, I think there is something here. One cannot do a large enough experiment to demonstrate linearity down to an arbitrarily low value. Having said that, I would like to say that about this time we start relying on purely physical considerations of the kind that other people such as Dr. Pollard have been mentioning.

Mr. HOLLISTER. How about some of the testimony that Dr. Brues gave, which I am not sure I understood perfectly, and do not have in front of me—and if he is here, he might want to comment himself—to the effect that to cause cancer in body cells more than one cell would have to be affected by a dose of radiation before this effect would occur.

This might imply a threshold, although in fact a threshold might not exist.

Representative HOLIFIELD. Dr. Brues, have you been correctly quoted? Will you come forward? Pull up a chair and defend yourself.

Dr. BRUES. Was the question addressed to me?

Mr. HOLLISTER. I think you can help us first to make sure I paraphrased what I thought you said correctly.

Dr. BRUES. I shall in that case rephrase it to say this: I should not necessarily assume that a somatic mutation would be the basis of cancer a priori. But if it is, it still might be a little more complicated than the genetic situation, where just one cell is involved. I can think of at least two different ways in which that might occur, but I shall not take the time to go into them.

Mr. HOLLISTER. But these complications that you speak of involve the notion that more than one cell would have to be affected by the radiation; is that correct?

Dr. BRUES. That would be correct, yes. This is not proven or disproven, but it is a suggestion which I think is as likely as the other one which has been made, rather categorically.

Mr. RAMEY. Do the complications make for the threshold, then?

Dr. BRUES. I beg pardon?

Mr. RAMEY. Do the complications arising mean that you have to have more dose before you would get some sort of threshold effect?

Dr. BRUES. Yes. I think the suggestion I made rather specifically, and this was based on many things which have been observed in cancer pathology, rightly or wrongly, is the fact that cancer rather tends to arise in a tissue which has been generally disarranged. As for a general disarrangement or disturbance of the blood vessel supply of the tissue, this, I think, is not linear with radiation dose, but

like the erythema produced by irradiation on the skin, the old method of measuring dose, this appears to have some sort of threshold. If that is necessary as well as something else that radiation does, then we will not have a linear response. That is the point I made.

Representative HOLIFIELD. Is there any comment on that? Observing no hands raised, we will go to the next question.

Mr. HOLLISTER. Dr. Crow, do you know if experimentally, a population has ever been destroyed genetically?

Dr. CROW. I cannot think of an example. You mean by accumulating so large a number of mutations that ultimately it was killed off?

Mr. HOLLISTER. Yes.

Dr. CROW. Dr. Russell has reminded me of an example. One of Dr. Bruce Wallace's populations died out presumably as a consequence of very heavy radiation. Is the theoretical point here whether it is possible to induce a large enough number of mutations to kill off posterity without killing off the first generation?

Mr. HOLLISTER. Presumably this experiment proves it is possible.

Representative HOLIFIELD. I think we have time for one more question. We will allow each one of you, regardless of the time element, to comment on this. Are conclusions in the field of genetics being arrived at too far in advance of the data?

Dr. STURTEVANT. It seems to me, sir, that we have to draw some conclusions. We have to do something, because not doing something is equivalent to doing nothing. We therefore have to proceed on the basis of the best information we have. This is a common enough human experience; it happens to all of us every day. We have to reach decisions as to what to do or not to do without all the information we should like to have. I don't think that the situation here is any different from that which is usual. We have to proceed on the basis of the best information we can lay hands on.

Representative COLE. You concur with Dr. Crow when he said it is better to guess wrong than not to guess at all?

Dr. STURTEVANT. I think it is not only better to; it is necessary to. You have to make some kind of guess in order to live at all in this world.

Representative HOLIFIELD. Would there be any other comment on that?

Dr. JONES. I think the same thing applies to the question of life span effects. Here we simply do not know how life span effects operate in the small-dose range. We may be a long time in finding out information that applies directly in the small-dose range. In the absence of that, we have to make some estimate of the effect. If we relate this problem to fallout, we may estimate fallout to be now about 2 percent of natural radiation. Natural radiation is estimated to cause a life span loss of 25 to 50 days, if we live as long as 70 years. So the effect of fallout is only about 1 day or half a day on this basis, if we assume proportionality. This loss is extremely small but may be worth keeping in mind, even though it may be as small as one twenty-five thousandth of man's life span.

Dr. RUSSELL. I should like to make two comments on this. I think the geneticists on the National Academy committee have faced up to this problem, and in some sense should be complimented on this. I am a member of the committee, so I do not want to compliment myself, but the others. Most of them were experimental scientists and

they were very reluctant to come out with figures based on what they would consider, in some respects, inadequate evidence. However, it was necessary to face up to drawing a conclusion.

I don't think anyone should be reprimanded for drawing a conclusion when a conclusion was requested.

Representative HOLIFIELD. I certainly do not want you to think that this committee is reprimanding.

Dr. RUSSELL. No. Other people have.

Representative HOLIFIELD. It is very salutary that you brought this out. We believe it is in line with your scientific integrity to point out danger signs, whether you are sure how great they are or how many.

Dr. RUSSELL. I did not mean to apply my remark to this committee. I believe geneticists have been blamed for making too definite statements based on the evidence, perhaps mostly by medical specialists.

The other point I have is that we perhaps know more about this genetic hazard than we did about many other hazards we have experienced in the past. Many hazards man has been encountering were not known to be dangerous until many humans died from them. For example, many industrial poisons and even radium in the first place. I think the genetic hazard represents a situation where we know in advance a good deal more about it than we have done for some other things, including, I might say, some medical treatments. Some of these have been found to be hazardous only after several people have suffered from them.

Representative HOLIFIELD. Of course, the magnitude of the threat of nuclear radiation from war is the compelling factor in this matter.

Dr. RUSSELL. Yes.

Representative HOLIFIELD. Dr. Crow, I am sure you want to say something.

Dr. CROW. I agree heartily with the two people who have spoken.

Dr. GLASS. I agree, but I would like to add just one very brief comment. We know there is a genetics hazard. We don't know the exact amount of that hazard. We think that it is better to overestimate it than to underestimate it, and play safe, than to underestimate it and reap irreparable damage.

Representative HOLIFIELD. Mr. Ramey has a question, I understand.

Mr. RAMEY. I believe most of you gentlemen sat in on yesterday's discussion of the pathological or somatic effects. As biologists rather than geneticists, do you think that this linear effect that was brought out today for genetics applies in the somatic pathological field?

Representative HOLIFIELD. Who would be so bold as to answer that?

Dr. CROW. I will be so bold as to make an answer, but it will not be very definite. I believe most geneticists are convinced that at least some of the somatic effects of radiation are of a linear nonthreshold sort. I don't think anybody would be so dogmatic as to state that all such effects are or even what the fraction is.

Representative HOLIFIELD. Dr. Muller, what do you think about that? Would you have any opinion on that?

Dr. MULLER. My opinion is, as I said before, that the most important effects, those from which the human race when exposed to radiation suffers by far the most damage, and that is the shortening of

life effect, and probably leukemia and some related things, are in all probability linear without a threshold.

Might I also say with regard to the other question of whether we are going too far beyond the evidence, that it was not, of course, possible in these discussions to present the details of the evidence, and the reasoning involved, but that the estimates that were presented as what we regarded as probable were not in any sense guesses or speculations, but arrived at as a result of an enormous amount of work and calculations. Not only that, but that they were arrived at by more than one method. There was a totally different method used recently by a number of geneticists in arriving at the frequency of mutations in man. It was remarkable that at the end it was in very good agreement with the estimate reached by the first method that we had used.

I would make this qualification only of what Dr. Glass said, that we were not trying in the main to show the maximum effect. I would regard the preferred estimates as minimum estimates.

Senator ANDERSON. My question was not too serious an inquiry. I was just wondering if geneticists had a union, guild or gang, or something that teaches you to hang together? This is not only the most agreeable group of seven scientists, but certainly the most agreed group I have seen. I commend you of the fact that you have been able to hang together as long as you have through a rather long day.

Dr. STURTEVANT. I would like to say that I think it would have been very difficult to get together a group that would have disagreed with most of what has been said here among practicing geneticists.

Dr. MULLER. Might I take the occasion to thank the members of the committee on behalf of all of us for their having put on these hearings on this subject.

Representative COLE. Mr. Chairman, I am not sure that the question can be answered, but at least I am curious enough about it to pose the question.

Are there any other firm conclusions that can be reached based on data and experience with respect to the danger or hazard of radiation other than the one that was just voiced by Dr. Glass, that it does constitute a hazard? I am directing the question at all of them and I prefaced it by saying I was not sure it could be answered, but I was going to ask it anyway.

Dr. CROW. I find it hard to answer, Mr. Cole, because of the difficulty of deciding what we really mean by firm in a case like this. I think the conclusion that any effect of radiation is harmful is about as firm as a scientific conclusion ever is. Of course, the quantitative figures are much less firm.

Representative HOLIFIELD. From the field of physiology?

Dr. JONES. I would echo Dr. Crow's opinion that it looks as though we can definitely say that some effects do occur, and at very small dosages they are undoubtedly small effects. But they seem nevertheless to be effects, and we cannot say with certainty what the relative orders of magnitude of these effects are. I don't think there is any reason to be more concerned than to try to get better information as soon as possible. There is no public hazard at the moment compared to usual concepts of public hazard. We certainly do owe it to ourselves to find out what these effects are.

Representative HOLIFIELD. On behalf of the committee, gentlemen, I wish to express our collective thanks for this participation in this set of hearings. I am sure they will be read by a great many thousands of people with great interest. Your audience will be large. I think these hearings will be the year's best seller.

Tomorrow we will have Dr. William B. Looney as the leadoff witness, followed by Dr. Libby, Dr. Ralph Lapp, and Dr. Walter Selove, in the Senate caucus room at 10 o'clock.

(Thereupon at 5:10 p. m., Tuesday, June 4, 1957, a recess was taken until Wednesday, June 5, 1957, at 10 a. m., in the Senate caucus room.)

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

WEDNESDAY, JUNE 5, 1957

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION OF THE
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The special subcommittee met, pursuant to recess, at 10:05 a. m., in the caucus room, Senate Office Building, Hon. Chet Holifield, chairman of the subcommittee, presiding.

Present: Representatives Holifield, Durham (chairman of the Joint Committee), Price, Cole, Van Zandt; Senators Anderson, Hickenlooper, and Bricker.

Also present: Professional staff members: James T. Ramey, executive director; George E. Brown, Jr., Hal Hollister, staff technical adviser, and Paul C. Tompkins, consultant.

Representative HOLIFIELD. The committee will be in order.

The subcommittee will continue its hearings on the effects of radiation on man, and today we will take a step back toward the pathology section of our witnesses. Our witness this morning is Dr. William B. Looney from the John Collins Warren Laboratories of the Huntington Memorial Hospital of Harvard University, Massachusetts General Hospital, who has been for the past 7 years conducting experiments and partaking in clinical work directly with patients who have had radiation damage. We should have had Dr. Looney on 2 days ago when we had the pathology section, but we could not fit him into the program because of the time limitation. We are mighty happy to have him with us this morning.

I notice you have quite an extensive statement here, and it is your purpose to submit this for the record, I understand, and to discuss the highlights of your statement.

Dr. LOONEY. That is correct, sir.

Representative HOLIFIELD. You may proceed, Doctor.

STATEMENT OF DR. WILLIAM B. LOONEY, MASSACHUSETTS GENERAL HOSPITAL [1]

Dr. LOONEY. Mr. Chairman and members of the committee, first, I should like to express my appreciation for the opportunity to

[1] Date and place of birth: March 18, 1922, South Clinchfield, Va. Education: Bachelor of science, Emory and Henry College, 1944; United States Naval Academy, 1941–44; doctor of medicine, Medical College of Virginia, 1948; internship, Presbyterian Hospital, Chicago, 1948–49; assistant residency, internal medicine, 1949–50; AEC postdoctoral research fellow in medical scence, National Research Council, Argonne National Laboratory, 1950–52. Work history: Assistant in medicine, University of Chicago, 1950–52; Radioisotope Laboratory, United States Naval Hospital, Bethesda, 1952–55; Officer in Charge of Radioisotope Technicians School, National Naval Medical Center, Bethesda, 1953–55; subcommittee on radiobiology, National Research Council, 1953–55; Consultant to Egyptian AEC, Cairo, Egypt, 1954–; specialist, Public Health Service Research, fellow of National Cancer Institute, 1955–; clinical and research fellow, Massachusetts General Hospital, 1955–; visiting fellow in physics, MIT, 1955–; consultant in radiation protection to Surgeon General, Department of the Navy, 1957–. (Submitted by witness.)

present this information to the Joint Committee on Atomic Energy.

As was just previously mentioned, my studies primarily have been on the effect of radiation in man, and the importance of the information to be presented this morning on the effect of radiation in man by radium is related to the fact that both radium and strontium are deposited in the skeleton similar to calcium. We have a considerable amount of information about the effects of radium in man; however, we do not on strontium. Since both strontium and radium act similarly to calcium, having the knowledge of radium, we can make estimates of the effects of strontium on man by our knowledge and factual data of the effects of radium on man.

I should like to confine my testimony primarily to the available knowledge we have on the effects of radium in man, and then at the end of the statement, based on the estimates from man, make some estimates as to the radiation dose from strontium necessary to produce similar changes in man over a 70-year period.

Man is constantly confronted with toxic agents in his environment which, if present in sufficient quantities, may produce temporary or lasting changes. By the accumulation of clinical and experimental information about the quantity of these toxic agents which produce minimal changes, safety measures are established. The present maximum permissible concentrations for radio elements in use today are based primarily on the results of clinical and investigative studies made over the past half century.

The principal sources of material for the study of the effects of internally deposited radioactive materials in man have been individuals who were employed in the luminous-dial industry, and persons who received radium for medical purposes. Radium salts had been given orally and intravenously for hypertension, arthritis, anemia, and other medical disorders from about 1915 to 1930. The painting of watch dials with luminous materials containing radium, mesothorium, and radiothorium started in this country in about 1914.

Adequate safety measures were not taken until about 1925–27, following the deaths from bone tumors, anemia, and crippling bone lesions, or all three, which occurred in some of these workers.

The first report in regard to the maximum permissible concentration, which I shall refer to as MPC, for radium was made by the National Bureau of Standards in 1941.

Senator HICKENLOOPER. I hate to interrupt you, Dr. Looney, unless you are willing to be interrupted in your statement.

Dr. LOONEY. That is perfectly all right.

Senator HICKENLOOPER. I notice the statement about the safety measures taken in 1925–27, following the deaths from bone tumors, anemia, and crippling bone lesions, which occurred in some of these workers. Do you comment later on the fact of whether or not all of the workers who received substantially the same dosage had the same results?

Dr. LOONEY. Yes, sir; this will be the context of the report.

Representative VAN ZANDT. Dr. Looney, when radium was first used, was there the same concern regarding the safety factor as there is today for radiation?

Dr. LOONEY. Well, this is a very fascinating history. The initial use of radium medically was based on the finding that radon, which is the first daughter produced by radium, was found in the waters of

the health springs in Europe, and a few biological experiments were done to show that radon or radium had beneficial effects. It was first believed that radiation had primarily a beneficial effect. As a result of the biological experiments and the finding of radon in the health springs, numerous people were treated. Historically, we now have the spectrum shifting from one side to the other.

Representative VAN ZANDT. One more question.

Dr. LOONEY. Yes.

Representative VAN ZANDT. I would like to ask about X-rays. Are you familiar with the development of X-ray from the time it was first used and was concern shown for the safety factor?

Dr. LOONEY. I think Dr. Taylor, who is in the audience here, would be certainly much more competent to comment on the general history. I think he is far more familiar with the overall details.

I will say, though, that certainly throughout the history of the uses of radiation, there certainly has been concern about its deleterious effect.

Representative VAN ZANDT. Thank you.

Dr. LOONEY. To go back to the report, the first report in regard to the maximum permissible concentration for radium was made by the National Bureau of Standards in 1941. Seven individuals having from 0.02 and 0.5 micrograms of radium in their bodies from 7 to 25 years had no noted changes referable to the deposition of radioactive materials. However, death had occurred in patients having as little as 1.2 micrograms of radium. The maximum permissible level of body burden for the amount of radium was established at that time as one-tenth microgram of radium. This was after the initial rapid elimination of radium from the body.

If radium is taken in, almost all of it is eliminated, but the fraction remaining in the bone is eliminated rather slowly. So the maximum permissible concentration for radium was set in 1941 at one-tenth microgram.

There have been two large studies, one made at the Argonne National Laboratory in Chicago, with which I was associated, primarily as coordinator in the evaluation of the clinical aspects with biophysical effects of the investigation. The other was made at the John Collins Warren Laboratories of the Huntington Memorial Hospital of Harvard University, Massachusetts General Hospital, Boston, and the Massachusetts Institute of Technology. These studies are continuing, and I have in the acknowledgments at the end made note of the people involved in these studies.

I would like to emphasize this was a very large study in which I was primarily interested in the clinical aspects, and I shall incorporate in the report the acknowledgment of the large group of people participating in these two studies.

This report is primarily a summary of these two recent investigations of people at or near the present maximum permissible concentration of radium.

Of the 78 patients that have been examined, the mean age of the luminous dial workers was about 21 years, and the mean age of the patients was about 34 years when they received radium. The average time of retention for both groups has been about 25 years with a

range of about 10 to 35 years. The average of these people in 1951 was about 50 years.

To summarize, we have information on people beginning about 15 to 20 years of age, and the major part of these patients have had radium in their bodies for 25 years. Many of these people are in the older age group, although the majority of the people are 50 or 60 years of age. We would like to have for a more complete picture additional information of people in the older age groups, and also people in the younger age groups. We have in the middle of the life span more information, but at either end the information diminishes.

Representative HOLIFIELD. Is there any information that the younger age group, with the same dose, is affected more deleteriously than the older age group?

Dr. LOONEY. Comparison of the effects of skeletal radiation in the unselected luminous dial workers and patients who received radium medically was made. It is not possible to draw conclusions on the present information in these two groups of patients. However, based on the available clinical information one might consider that the luminous dial workers were less affected after beginning employment at ages 15 to 20 years, than the patients who received radium medically in the 1930's.

Representative HOLIFIELD. Is that on comparative doses?

Dr. LOONEY. On comparative doses, yes, sir. As I say, I think this data is inconclusive. This is just based on the information we have available at present in man.

Senator ANDERSON. Do luminous dial workers go to work at 15?

Dr. LOONEY. Fifteen to twenty years of age; yes. Some of them started at 14 and 15. This was in 1915.

Representative HOLIFIELD. And they absorbed this radium, as I understand, by putting the tip of the brush in their mouths to point up the brush in order to paint the numerals on the watches. Was that the way they received it mostly?

Dr. LOONEY. I think this, plus the fact there was radium in the atmosphere and they inhaled it. There were no known effects at that time. In 1924 a dentist discovered destruction of the mandible of some of these people. This established the fact that the ingestion and inhalation of radium produced deleterious effects in man. This very tragic experience is the foundation upon which some of the maximum permissible concentrations of the most important radioelements in use today are based.

There is one other point I think should be brought out in regard to these patients. Ten of these fifty radium patients, and sixteen of the luminous dial workers were selected because of symptoms. In other words, they were discovered because they were having trouble. It is apparent that, if we had a thousand people and we saw only the people who were having difficulty, we would have a biased or selected group of people from the population of radium patients, and luminous dial workers. About one-third of these people were found because they had symptoms. This would tend for one to overemphasize the effect of ardioactive materials based on the present information. This is something the committee should keep in mind.

Thirteen of these seventy-eight patients having between one-hundredth and four-tenths micrograms of radium had no changes attributable to the effects of radium, with two exceptions. One patient

having 0.15 microgram of radium had minor areas of decreased density demonstrated roentgenographically in his teeth. These are minor changes; however, this patient was discovered because of the changes. These findings are characteristic of the dental changes in people we have seen with more radium in their body. The other patient was an 83-year-old woman who had very severe arthritis, was incapacitated, and had minor changes in her skeleton. Old age, plus the incapacity, might have contributed to the changes.

Sixty-five patients had between 0.5 and 23 micrograms of radium. All of these patients had either skeletal changes or symptoms attributable to radium, or both, which we could reasonably attribute to radium, with 3 exceptions; these people had between five-tenths and 1 microgram of radium. We should keep in mind the one-tenth microgram as the present MPC for radium, then we can mentally refer to the changes.

These patients began to have difficulty with about 10 times the amount of radium greater than the maximum permissible concentration.

Now I would like to go into the mechanism of deposition of radium in the skeleton, the pathological changes seen, and then relate these pathological changes to the amount of radium in the body. Radium is deposited in small areas of high concentration, so that we have an irregularity of deposition. This is an important factor to take into consideration, because in the last analysis we are interested in what happens in certain areas. If radium is concentrated in the area of bone tumor formation, then we are not as interested in the average value of total bone, as we are interested in these areas of concentration.

Analyses of samples of bone from some of these people have been made. In samples taken from different bones and from several parts, the same bones, we have found that these concentrations vary as much as by a factor of 10, and maybe greater. When we talk of average values of one-tenth microgram, we must realize that these people have areas of concentration which may be much greater than that. This has to enter into the consideration of the MPC of radium as well as strontium.

If you will go back to figure 2 at the end of my statement, there is an autoradiograph which shows how the radium is deposited in one of the bones. It is the picture here [indicating], showing the actual radium deposition in the bone. You will see that the dark areas represent areas of radium. (See p. 1176.)

In considering the effects of radium, you must consider the effect in the small areas of concentration in relation to the pathological changes.

Now, the changes that have been shown to develop in these people are areas of abnormal bone formation, which occur usually at the ends of the bone. When an X-ray is taken they show up as areas of increased density. The effect of radium causes abnormal bone to be produced, and it is generally considered that tumors develop in and around these areas of abnormal bone formation.

The other characteristic change is areas of decreased density in the shaft, or the middle of the bones, and these show up in the X-rays as areas of decreased density. When we examine people who have had a sufficient amount of radium, we see these areas of either increased or decreased density which are scattered throughout the skeleton, and

these characteristic changes are one of the most reliable clinical methods we have at present for determining the early effects, or the first detectable effect, of radioactive materials in man.

Chairman DURHAM. What kind of a dose, Doctor, was that received by the bone structure in figure 2 of your statement?

Dr. LOONEY. This patient had 1.3 micrograms of radium, sir. This is about 13 times the present accepted MPC for radium.

In figure 4 you will note that this is a very detailed microscopic picture of radium being deposited in one of the fundamental units of the bone, known as the Haversian system. You will note the alpha tracks from the radium coming from those two Haversian systems, and this relates to the black area you saw on the autoradiograph of the whole bone. This is a microscopic picture showing the radium deposited in the small area. (See p. 1178.)

Representative HOLIFIELD. Do you have that same picture on the screen?

Dr. LOONEY. Yes, sir, I do. This is the picture here [indicating]. I might point out that here [indicating] is the area of the increased density that you see at the heads of the bone here and here [indicating]. The areas of decreased density are in here and here [indicating].

The areas that you will notice here [indicating] are the areas of destruction in the bone, and this is an X-ray of the bone showing the areas of increased density and the areas of decreased density, and these are changes characteristic of radium deposition. It is the best clinical method we have of determining the effects of radium.

Representative HOLIFIELD. Is it your judgment that the strontium 90 would have the same effect?

Dr. LOONEY. It is the best means we have of comparing the effects of strontium 90 to the effect of similar radio elements in man, and we have to go on the best available evidence we have.

Representative HOLIFIELD. Have you taken pictures of bones of mammals that have been subjected to strontium 90 exposure?

Dr. LOONEY. This has been done in some of the atomic energy laboratories. I have not been directly connected with this, and I am sure that some of the people in the audience could comment on that. Strontium and radium and calcium are deposited in the skeleton in similar manner; this is the basis of comparing the radium data with the present level for strontium.

This [indicating] is the picture of the alpha track, or the actual radiation from radium in one of the fundamental units of the bone, and this [indicating] is the underlying bone, showing an area of destruction, and another area showing no change.

Here [indicating] it shows the normal bone, and this is a typical osseous tissue adjacent to the normal bone, which results in these areas of increased density seen roentgenographically. In animals we see this atypical osseous tissue formed following plutonium, strontium, and radium administration, and in man similar changes are produced by radium. We consider that the mechanism is similar in the production of these changes.

Chairman DURHAM. What kind of rays are those, Doctor?

Dr. LOONEY. These are alpha rays. This is actually a picture taken by photographic emulsion over the bone itself. In other words, the track was coming up in the photographic emulsion, and it is like

taking a photograph, and this is the photograph of the alpha track.

Representative HOLIFIELD. No one has used "micrograms" before in the presentation. Will you relate that to a microcurie?

Dr. LOONEY. Yes. The microgram of radium and the microcurie are the same, because the microcurie was established from radium. So if I speak of micrograms or microcuries, as far as radium is concerned it would be the same.

Representative HOLIFIELD. That is what I wanted you to say for clarification of the record.

Dr. LOONEY. Yes.

Representative HOLIFIELD. Are the alpha or gamma rays which might be emitted of the same intensity and range in strontium 90 as in radium?

Dr. LOONEY. No, sir, they are not, because most of the radiation from radium is alpha radiation, and the radiation from strontium is from beta radiation.

Now you have heard the term "relative biological effectiveness" used. The RBE of radium has not been taken into consideration in the determination of the present MPC for strontium 90. Tho RBE [indicating] may be more than unity [indicating], so that this would permit the raising of the strontium level based on the radium data, if we were to determine that the effectiveness of irradiation from radium was greater than strontium in producing biological changes. In other words, it is generally considered that the alpha rays are more effective in producing biological changes per unit of energy dissipated to the material. These may be inherent safety factors that can be incorporated in present considerations of the MPC for strontium.

Representative HOLIFIELD. So if there would be a difference, it would be weighted toward the greater effectiveness of radium?

Dr. LOONEY. Yes, it would be weighted. Any change would be to the raising of the strontium MPC.

I would like briefly to comment about the hematological changes in these patients. The changes have been minor, and the only changes we have found have been minor changes in the size and shape of the red cells. In a few exceptions there have been anemias that have occurred in these people, but the hematological findings have been very minor changes compared to the skeletal changes.

I should like to go on to symptoms, and probably I should define what I mean by symptoms in these discussions. I shall refer to symptoms as the time when the patient becomes subjectively aware of these skeletal changes which we see. In other words, we first see skeletal changes in many of these people who have no symptoms at all. When skeletal changes progress to the point that the normal configuration of the bone is destroyed, symptoms usually occur the patient may have a fracture of the femur, or he may have destruction of the hip, and he limps, then these denote symptoms. The patient is aware these skeletal changes are present.

In going over these patients, there have been 11 patients of the 78 who have had destruction of the hip, the head of this [indicating] hip bone, so that they walked with a limb. All of these 11 patients have had seven-tenths micrograms or more of radium in their bodies.

There have been five patients with fractures of the femur. Some of these fractures have occurred from very minimal trauma. I remember one patient we examined had a fracture of the femur after

her husband came to a sudden stop at a stoplight, and the pressure of her foot on the floorboard of the car caused this fracture of the femur. So that the skeletal system does become more fragile, after a period of time, with these people in this range.

Representative HOLIFIELD. How did that particular patient receive that does of radium?

Dr. LOONEY. This patient was a luminous dial worker, sir, and she worked at Ottawa, Ill.

If you will turn to chart 1, you will note that this is the summary of the X-ray findings of 32 patients who had a complete skeletal survey. These patients were arranged in the order of increasing amounts of radium in the body. The frequency with which these characteristic skeletal changes, which I have shown to you, have occurred, were plotted as a function of the amount of radium in the body. You will note from the left hand vertical bar that there were 15 patients having between five-tenths and 1 microgram of radium in the body. You will notice that about 15 percent of the total bones that could be involved had these characteristic changes. (See p. 1174.)

There were 8 patients between 1.1 and 2 micrograms of radium, and you will notice that the frequency goes up to about 55 percent, and that between 2.1 and 14 micrograms of radium it goes up to about 60 percent.

If you will turn to table 1, you will notice that in the first group of patients, the average age was 49; the average age of the second group was 52; the average time of retention in the first group was 20 years; the average time of the second group was 22 years; the average age of the third group was 61 years; and the average time of retention was 22 years. (See p. 1174.)

We have a group of people who are about the same chronological age, who have radium retained for about 20 years, in which we can show a correlation between the frequency of these characteristic changes and the amount of radium in the body.

I think this is probably one of the most significant clinical observations that has been made—the correlation an objective clinical change with the amount of radium in the body within a specific dose range. I want to emphasize that extrapolation of the result of this clinical data either one way or the other would present many difficulties.

Chairman DURHAM. How did these patients receive this, Doctor?

Dr. LOONEY. With two exceptions, all of the people received this for medical purposes.

Chairman DURHAM. For treatment of other diseases?

Dr. LOONEY. Yes, sir; for the treatment of other diseases.

An important point to bring out here is the fact that people with 10 and 15 micrograms of radium did not have a proportionate increase in skeletal changes. In other words, we did not see people with greater amounts of radium in the body having greater changes. This would be consistent with the hypothesis that the changes that we see are the result of the dynamic interrelationship between the destructive processes and the reparative processes of the body. If we had radium with some other destructive skeletal disease, we might see changes at a much lower level than if the patient had no disease. Radium, plus condition A plus condition B might produce changes at 1 microgram. Radium, plus condition B might produce changes at 5 micrograms, and radium alone might produce changes at 10 micro-

grams. This is consistent with the hypothesis that the body is constantly repairing itself from destructive changes. When the body can no longer repair these changes, then permanent changes occur, in regard to radium, the skeletal destruction can be seen on roentgenographic examination.

Representative HOLIFIELD. As a term of common reference, would you say that any effect upon the bone is usually referred to as a tumor or cancer of the bone?

Dr. LOONEY. No, sir.

Representative HOLIFIELD. Or is there a differentiation between those two?

Dr. LOONEY. Yes, sir; there is quite a differentiation between those two.

Representative HOLIFIELD. Will you explain that for the record?

Dr. LOONEY. Yes, sir. These changes which we have seen—these changes here [indicating]—are minor changes, and are from abnormal bone formation. The areas here [indicating] are from small areas of destruction in the bone, and these can be seen in other conditions but usually not with the distribution and the characteristics seen in the radium patients. In other words, this is not diagnostic of radium, but it is very characteristic, and there are very few other medical conditions which will produce the same picture. I might say other nonmalignant medical conditions.

Chairman DURHAM. Doctor, did any of these patients in this group develop cancer from the normal treatment of other diseases from this radium?

Dr. LOONEY. I want to go into the number of tumors that have developed in these people, sir. I could not say whether these developed tumors from other conditions. All I could say is that a very large number of these people developed bone tumors, and out of proportion to other groups of people.

Of these 78 patients, 15 people have developed tumors.

If you will refer to figure 14, you will see the distribution of tumors that have occurred in these patients. You will notice that they usually occur at the end of the long bones, in the same areas in which you see these areas of abnormal bone formation.

I would like to read from that portion of the statement (p. 1170) entitled "Bone Tumors."

The 15 malignant tumors which developed in the 78 patients recently evaluated were found in individuals containing from 0.5 to 10 micrograms of radium in their bodies. The patient having 0.5 microgram of radium was a luminous-dial worker. It is reasonable to assume this patient ingested mesothorium and radiothorium. The patient with the lowest radium concentration, who had received radium medically, had 0.9 microgram of radium. The patient with the lowest concentration of radium, which was considered not to be contaminated with members of the thorium decay series, and who had developed a bone tumor, had 3.6 micrograms of retained radium.

It is important to emphasize that the luminous-dial workers used not only radium, but mesothorium and radiothorium. We have only measured radium, so we have not taken into consideration the radiation dose from either mesothorium or radiothorium.

The accumulated radiation dose with 3.6 micrograms of radium was estimated to be about 5,000 rads during the 25 years from radium administration until tumor formation.

Some of these people were selected because of symptoms. The number of bone tumors in the patients which were not discovered because of symptoms were located in a review of the records. We find that there is a frequency of about 2 percent in this unselected group as compared to a frequency of 14 percent of the entire group of 78 people. Although this is not conclusive, I do think it should be considered as it suggests this may be a biased group of people with whom we are dealing.

Because of the major interest in the possibility of bone tumor induction by radiation, I have taken two comprehensive articles in which the investigators have summarized the cases in which bone tumors have been produced following external radiation.

Vaughan in 1956 reported 39 cases of sarcoma arising in bone following external radiation have been recorded in the literature. The latent period between receiving radiation and the development of the tumor was 3 to 11 years. The radiation dose was not known in all of these cases; however, in most recorded cases it was estimated to be usually greater than 3,000 roentgens (1,500 to 7,000 roentgen range).

Cruz et al. in 1957 reported an additional series of 11 cases in which the bone tumors developed from 4 to 24 years after external radiation. The total radiation dose given ranged from about 1,000 roentgens to 5,000 roentgens, and was given over a period varying from 1 month to 9 years.

I should like to comment now on the possibility of bone-tumor production from strontuim 90, based on this information of tumor induction following external and internal radiation in man.

The assumption is often made that the incidence of the effect of strontium 90 is proportional to the magnitude of the dose. This assumption has been used to estimate the bone tumors which may be produced from the low concentrations of strontium 90 in the skeleton from fallout. There is no exact evidence either proving or disproving this assumption, but there is some clinical evidence which suggests that this assumption is overcautious.

The 50 bone tumors which have been known to have been produced in man from external radiation, and reported, were summarized in the previous section. The skeleton in the localized area of tumor induction received at least 1,000 roentgens, and usually more than 3,000 roentgens of radiation. If it is assumed that the radiation dose to bone is greater by a factor of 2 than the measured skin dose—in other words, the radiation going into the bone would absorb more radiation than the dose measured at the skin. So we are assuming it may be more than a factor of 2, maybe greater than this, but we are saying this as a reasonable estimate—then the minimum observed carcinogenic dose from external radiation would be about 2,000 rads, with the majority of the tumors being produced by more than 6,000 rads of external radiation.

Representative Cole. I would like to ask Dr. Looney, Mr. Chairman, to explain his statement that there is some clinical evidence which suggests that the assumption is overcautious, the assumption being that the incidence of the effect of strontium 90 is proportional to the dose. Now what do you mean when you say "there is some

clinical evidence which suggests that this assumption is overcautious." In what way?

Dr. LOONEY. I am saying that the minimal carcinogenic dose that we have reported for tumors to be produced in man is in the order of 2,000 rads. Based on the present clinical evidence we have today, we cannot prove or disprove that smaller doses would produce tumors, but I am just presenting to you the present evidence we have in man known to produce tumors. I am just saying that this assumption might be overcautious.

Representative COLE. What do you mean by "overcautious"? That is what I want.

Dr. LOONEY. May I come back to this at the end of these comments? Maybe this will clarify this statement.

What I have attempted to do is to present the order of magnitude of radiation we are dealing with. The following estimates on strontium 90 are based on the assumption that strontium 90 will be present in the body over a life span of 70 years. If we assume it is in equilibrium with bone or there is a constant intake of strontium 90, it would tend to approach a uniformity in bone.

Estimates which we can make from this information about the levels of strontium 90 necessary to produce bone tumors, skeletal roentgenographic changes, and total skeletal radiation from background radioactivity are as follows: Ten microcuries of strontium deposited in the skeleton for 70 years would give an estimated dose of about 2,000 rads. This is the minimum radiation dose recorded which has produced a bone tumor in man. This should give some idea of the magnitude of strontium levels which may produce bone tumors in man. You will notice that 6,000 rads is the estimated amount of radiation known to produce most tumors. The amount of strontium 90 which would deliver 6,000 rads to the skeleton over a life span of 70 years would be in the order of 30 microcuries.

The minimum radiation known to produce tumors in man is in the order of 2,000 rads, which would be 10 microcuries. The present MPC for industrial workers is 1 microcurie, and this would give 200 rads over a 70-year period. (The tenth of a microgram of radium would also give about 200 rads.) The total dose from natural radiation, based on the estimates of Dr. Robert Dudley of Massachusetts Institute of Technology, from all sources of radiation to the skeleton over a life span of 70 years, would be in the order of 10 rads. The sunshine unit, 1 times 10 to the minus 3 microcuries of strontium, would deliver about two-tenths of a rad to the skeleton in 70 years.

I will leave this up on the board so you can refer to it as the order of magnitude, and I will try to keep all of the units in rads so that this will give a basis for comparison.

I should like to go back to my statement in the section titled "Comments on * * * Bone Tumor Formations * * *." (See p. 1171.)

The patient with the smallest total body radium known to induce tumor formation, in which the possibility of contamination of the thorium series is unknown, died from a bone tumor in 1952. The estimated total body radium was 0.9 microgram. The time after administration is unknown, however, it is reasonable to assume that it was about 25 years. Based on the estimates above, the patient would have received a total accumulated does of about 1,800 rads during the 25-year period.

Histopathological changes have been demonstrated by roentgenographic examination of the skeletons of radium patients prior to development of the tumors. It is generally considered that the bone tumors developed in or around areas of atypical osseous tissue formation. Bone tumors have been shown to develop in or around the abnormal bone formation in animals given plutonium, strontium, and other radio elements.

If it could be shown that these histopathological changes are preliminary steps to bone tumor formation, then it could be assumed that as long as the body reparative processes prevented the abnormal bone formation, bone tumors would not develop. It should be emphasized that the associated histopathological changes seen prior to bone tumor formation in the radium patients may be coincidental findings. Proof of a correlation must await a better understanding of tumor induction by radiation.

Now there is one other bit of information in which I must emphasize little reliance can be placed. However, it is the best available evidence we have in man. This information pertains to the latent period of tumor development in relationship to the magnitude of the dose.

In reviewing the latent period for tumor induction in the luminous dial workers reported by Martland in 1931, it was found that the latent period for tumor formation was about 5 to 10 years in 6 patients who developed tumors. Only 3 of these patients had estimates of total body radium reported, and these estimated were 6, 15, and 50 micrograms.

The 8 luminous-dial workers who were examined in the recent Boston-Chicago investigations, and who died from bone tumors, lived for an average of 25 years after beginning employment. The average total body radium was 3.4 micrograms—range of 0.5 to 10 micrograms.

We have very meager data, but there does seem to be an inverse relationship between the latent period and the radiation dose necessary to produce bone tumors. The estimates based on this would be in the same order of magnitude as the estimates we have by the known radiation dose that produces bone tumors in man.

Therefore, the available clinical information we have at present indicates that the radiation dose for bone tumor production in man from both internal and external radiation is in the order of 2,000 rads.

The present MPC for radium, as I mentioned previously, was established in 1941. The fact that we are finding changes now at four-tenths of a microgram, which is four times greater than the MPC, is probably offset by the confidence in the larger group of patients which we have studied for longer periods of time. It is reassuring that it has not been necessary to change the MPC of radium over this long period of time, in view of the large amount of information that has been accumulated since it was established.

I have listed some of the factors which may permit the raising of the present levels for strontium 90. As I have pointed out, estimation of the mesothorium and radiothorium content in luminous-dial workers is being carried out. If it is found that the mesothorium and radiothorium contribute significantly to the dose, this would permit raising the present levels.

The characteristic changes we have seen may occur in the normal population, and further studies may permit us to obtain a better

understanding of how these changes are produced, thereby permitting a raising of the MPC.

As I mentioned in the introduction, most of these people were studied beginning about 15 or 20 years of age, so we have a gap in our knowledge of the younger age groups, which might necessitate lowering the present level. The present MPC is based on our present methods, and knowledge of the clinical changes produced by radioactive materials. It is possible that other subclinical effects may occur which may necessitate lowering the MPC.

In summary, it is considered that the best estimates which can be made in regard to the effects of strontium 90 over a life span of 70 years on the present incomplete information on the effects of radium in man are as follows:

The skeletal content of strontium 90 necessary to produce a bone tumor in a life span of 70 years would be in the order of 10 microcuries of strontium.

The skeletal content of strontium 90 necessary to produce significant changes, such as destruction of the hip would also be in the order of 10 microcuries of strontium over a life span.

The skeletal content of strontium necessary to produce minimal skeletal changes, which were demonstrated roentgenographically, would be in the order of 2 microcuries of strontium.

It should be emphasized again that these estimates of the concentrations of strontium 90 which may produce skeletal damage are the result of estimates based on the available information at present.

Representative HOLIFIELD. Have you finished, Dr. Looney?

Dr. LOONEY. Yes.

Representative HOLIFIELD. Are there any questions of Dr. Looney?

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. Dr. Looney, you have spelled out for us the effect of radium on the skeleton and explained how it produced bone cancer. Has radium in any way, shape, or form affected the blood to the point of developing leukemia or arthritis?

Dr. LOONEY. As I mentioned, the hematological changes in these people have been minor. In one case the patient died from renal insufficiency and there was a question of leukemia, but I would have to look this up.

Representative VAN ZANDT. What about arthritis?

Dr. LOONEY. Arthritis—I suppose the effects of radium might be considered a destructive form of arthritis. These people get destruction of the hip so this is, in a sense, a form of destructive or degenerative arthritis.

Representative VAN ZANDT. Dr. Looney, to your knowledge, has radium produced any other effects to the human body other than bone cancer?

Dr. LOONEY. That I know of, no sir. The major effects are the skeletal changes, and the bone tumors.

Representative HOLIFIELD. Dr. Looney, your presentation has been very valuable, and particularly the well-documented written presentation, which will be included in toto in the record.

I appreciate particularly having the pictures of the bone structures that are represented by your clinical and experimental experience in the record.

Thank you very much.

(The prepared statement together with "A Study of the Dynamics of Strontium and Calcium Metabolism and Radioelement Removal" submitted by Dr. Looney follows:)

THE BASIS FOR THE PRESENT MAXIMUM PERMISSIBLE CONCENTRATION FOR RADIUM AND ITS RELATION TO THE MAXIMUM PERMISSIBLE CONCENTRATION FOR STRONTIUM 90

A statement perpared for the Joint Congressional Committee on Atomic Energy on the subject "The Nature of Radioactive Fallout and Its Effects on Man," June 4, 1957, by William B. Looney, M. D.,[1] the John Collins Warren Laboratories of the Huntington Memorial Hospital of Harvard University, Massachusetts General Hospital, Boston, Mass.

INTRODUCTION

Man is constantly confronted with toxic agents in his environment, which, if present in sufficient quantities, may produce temporary or lasting changes. By the accumulation of clinical and experimental information about the quantity of these toxic agents which produce minimal changes, safety measures are established. The present maximum permissible concentrations for radioelements in use today are based primarily on the results of clinical and investigative studies made over the past half century.

The principal sources of material for the study of the effects of internally deposited radioactive materials in man have been individuals who were employed in the luminous dial industry and persons who received radium for medical purposes. Radium salts had been given orally and intravenously for hypertension, arthritis, anemia, and other medical disorders from about 1915 to 1930. The painting of watch dials with luminous materials containing radium, mesothorium and radiothorium started in this country in about 1914. Adequate safety measures were not taken until about 1925–27, following the deaths from bone tumors, anemia, and crippling bone lesions which occurred in some of these workers (41).

The first report in regard to the maximum permissible concentration (MPC) for radium was made by the National Bureau of Standards in 1941 (9). Seven individuals, having from 0.02 and 0.5 micrograms of radium in their bodies from 7 to 25 years had no noted changes referable to the deposition of radioactive materials. However, death had occurred in patients having as little as 1.2 micrograms of radium. The maximum permissible level of body burden for the amount of radium which remains after early rapid elimination was considered to be 0.1 micrograms.

Two investigations of 50 radium patients and 28 luminous-dial workers were made in Boston and Chicago and recently reported (3, 25). As a result of these two investigations, greater reliance can be placed in the MPC for radium. These investigations have given information about the effects of radium deposited for long periods of time in quantities at or near the present MPC. One of the most important results of the recent investigations is that a relationship between an objective clinical finding (skeletal roentgenographic abnormalities) and the physical estimate of total body radium could be made within a specific dose range.

This report is a condensation of a review article published recently in the Journal of Bone and Joint Surgery (27), which summarizes the results of studies made on the radium patients and the luminous dial workers over the past 40 years.

The clinical course following the deposition of varying amounts of radium has been divided arbitrarily into two categories in this report.

A. Patients with total body radium content at or near the MPC of 0.1 micrograms with either no detectable clinical effects or minor skeletal changes

Thirteen of the seventy-eight patients studied in the Boston-Chicago investigations had between 0.01 and 0.4 micrograms of radium, and with the exception of 2 cases listed below, none had either symptoms or skeletal changes which

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could be attributed to radium. One patient with 0.4 micrograms of radium, age 83 years, was incapacitated with arthritis, therefore it was difficult to differentiate between the skeletal changes which may have been produced by radium from those which may have been produced by the skeletal disorder and disuse atrophy. Roentgenographs of the teeth of the other patient demonstrated areas of resorption in the teeth similar to those seen in other patients with greater amounts of radium. This patient was 32 at the time of examination. He had received radium water as a tonic between the ages of 8 to 10 years. The total body radium was only 0.15 micrograms of radium. It was considered that the early age of administration of radium may account for the dental changes with only a 50 percent larger radium content than the MPC of 0.1 micrograms.

B. Chronic effects of radium

1. Patients studied during the first and second decades following the initial medical and industrial use of radium (1915–35).—These patients were symptom free for about 10 years (31). Following the latent period, various changes relating to the skeleton began to develop. Death usually occurred from tumors of the skeleton, anemias or crippling bone lesions, or all three. Twenty-five of these cases were reported in the two decades which followed. Estimates of the radium content of the body began to be reported on these patients; however, the reliability of these estimates is hard to ascertain. These estimates ranged from about 5 to 100 micrograms of radium.

2. Patients studied during the third and fourth decades following the initial medical and industrial use of radium (1935–55).—Sixty-five of the seventy-eight patients evaluated in the recent Boston-Chicago investigations, having between 0.5 and 23 micrograms of retained radium, had either roentgenographic changes or symptoms, or both, which could reasonably be attributed to radium. The exceptions to this were three patients having betwen 0.5 and 1 microgram of radium having no skeletal abnormalities demonstrated roentgenographically. There was no proportionate increase in either the frequency or severity of the roentgenographic changes and symptoms with increasing amounts of radium in the body. The wide variation in the clinical abnormalities is consistent with the hypothesis that the changes observed are the result of a dynamic relationship between the reparative processes of the body and the destructive processes of the body acting in conjunction with the releterious effects of radium.

The mean age of the 28 luminous dial workers in the Boston-Chicago study was 21 years at the time of employment, and the mean age of the 50 patients at the time of radium administration was 34 years. The average time of retention was about 25 years in both groups, with a range of about 10 to 35 years. The average age of the patients in 1951 was about 50 years (range from 40 to 80 years). Seven of the 50 patients given radium have died; 5 died from tumors, 1 from anemia, and 1 from multiple causes; 11 of the 28 luminous dial workers are dead; 8 died from tumors, 1 from broncho-pneumonia, 1 from a urinary disorder, and another from renal insufficiency and leukemia. The average age at death for the radium patients was about 65 years, and the average age at death for the luminous dial workers was about 45 years. The average time of deposition of radium until death of the 18 patients was about 25 years, and average total body radium was about 5 micrograms.

As far as we have been able to determine, 10 of the 50 radium patients and 16 of the 28 luminous dial workers were discovered because of symptoms. It is apparent that these patients represent a biased sample from the population of radium patients and luminous dial workers. Correction for this biased sampling should be made in any attempt to relate the frequency of bone tumors and bone destruction in these patients with estimates of the frequency of these changes with comparable amounts of strontium 90.

RADIUM ELIMINATION

Radium is considered to be dispersed throughout the soft tissues and the skeleton when it first has been taken into the body (43). Most of the radium is eliminated during the first week (42, 43) (fig. 1). Fecal excretion is the major route of elimination in early and late cases. From 90 to 97 percent of the radium which is eliminated is eliminated in the feces and only 3 to 10 percent is eliminated in the urine (2, 35, 42). It has been reported that the rate of elimination following inhalation or ingestion is more rapid than that following intravenous administration (40, 42). Results from recent studies, however, indicate that these differences in the rate of elimination are not so pronounced as it was previously thought (26, 30, 35). Within a matter of

months, 90 to 99.9 percent of the radium is eliminated (40, 43). The age, the total amount of radium injected, and the estimates of the injected dose in 19 patients at 6 months, 12 months, and 20 years, have been reported (35). The average amount of the injected radium remaining at the times these estimates were made was 4.7 percent, 2.2 percent, and 0.8 percent, respectively.

The amount of the remaining radium eliminated daily becomes less and less (26). The available clinical data on the coefficient of elimination[2] of radium are given in figure 1. The marked change in the coefficient is elimination at 1 week has been considered to be the result of the elimination of the principal portion of radium from the soft tissues and the gastrointestinal tract and the change at 1 year has been considered to be the result of the elimination of the principal portion of radium from the more accessible parts of the skeleton. It is reasonable to conclude that the decreasing rate of elimination of radium is from the more inaccessible parts of the skeleton after 1 year.

About 15 to 35 percent of the absorbed radium is eliminated the first day. The amount of the remaining radium eliminated daily after 1 week is less than 1 percent. After about 10 years, it varies from 0.002 to 0.009 percent of the body content. The coefficient of elimination after about 20 years varies from about 0.002 to 0.016 percent of the body content (35, 43).

THE MANNER OF RADIUM DEPOSITION IN THE BODY

A. Autoradiographic study of radium deposition

Entire bones were sectioned serially and a comprehensive picture of distribution throughout the entire bone was obtained by placing the sections on roentgenographic film (3, 23). The precise manner of the deposition of radium was obtained by detailed autoradiography (1, 3, 23). Histological sections were covered with a photographic emulsion. After the emulsion and staining had been developed, simultaneous study of radium deposition and histopathological change was made.

Radium was found in small areas of high focal concentration irregularly distributed in both compact and cancellous bone. In compact bone only a small percentage of the Haversian systems and interstitial lamellae had appreciable concentrations of radium. In some instances radium was concentrated in 1 or 2 concentric lamellae, in other instances it was deposited around the central canal or periphery of the Haversian system.

The areas of radium concentration in travecular bone were usually 5 to 15 micra in the greatest dimension. However, there was a wide range in size and shape of these areas, and they were found at any depth within the trabecula. In some instances linear concentrations ran parallel to the curvature of the trabecula for 50 to 100 micra. Heavy and rather uniform concentrations of radium were present at the junction of the articular cartilage and the traveculae of the long bones in the gross autoradiographs. In the study by gross autoradiography it was found that frequent small highly concentrated areas resulted in outlining the bone contours (fig. 8). Some cementing lines were clearly outlined by heavy concentrations of radium.

In some sections exposed for long periods of time there was a much less concentrated and a much more uniform distribution of radium. These findings are in agreement with existing theories that radium has more than one principal mode of deposition. The small highly concentrated areas may have been areas in which bone formation was taking place at the time of administration or redistribution. The more uniform and less dense distribution may be the result of inorganicion exchange (23). (See figs. 2, 3, 4, 5.)

HISTOPATHOLOGY

The histopathological changes which are most important diagnostically are: (1) the formation of an atypical osseous tissue in the trabecular spaces of cancellous bone, and (2) well differentiated areas of destruction in compact bone.

Martland divided the changes in the skeleton into three stages, all of which are primarily concerned with the bone marrow (31). In stage III, however, he did state that bone absorption and considerable decalcification did occur along with the replacement of bone marrow and noncellular fibroblastic tissue.

[2] The coefficient of elimination is equal to the amount of radium eliminated per unit time (days) divided by the amount of radium in the body at the time the elimination measurements were made.

Studies of the skeletal changes produced in animals following the deposition of radioactive elements clarify the mechanism of production of the histopathological changes that have been found in human material. Heller and Bloom and Bloom concluded from their studies of such bone-seeking radioactive elements as strontium, yttrium, plutonium, and radium that similar changes were produced by all of these radioactive elements.

Following radium injections in mice the most spectacular change was a dense atypical bone in the metaphysis. This started from the proliferation of spindle cells which formed a dense fibrous tissue. Areas of calcification occurred in this fibrous tissue to form an atypical osseous tissue. It was also found to lesser degree in the shafts of the long bones and in the vertebrae. This was accompanied by the disappearance of osteoblasts, swelling and degeneration of cartilage cells, and death of most of the osteocytes. Repair began along with the destricutive effects of radium. In the lower dose ranges, reversible changes occurred so that in a matter of months normal bone was found on histological examination. Larger amounts of radium producted more severe and lasting changes. The epiphyseal cartilage varied tremendously in width, and the entire metaphysis was abnormal in appearance. The atypical osseous tissue remained in these animals at all intervals up to the termination of the experiment at 5 months.

The femoral shaft contained varying degrees of empty lacunae and dead oseocytes. The bones of the shaft varied from the relatively smooth contour to greater or lesser irregularities on the endosteal surface with projection into the marrow cavity.

Historical specimens have been obtained from 3 luminous-dial workers and 6 patients who had received radium. The radioactive elements had been present in the skelton from 12 to 35 years preceding biopsy and autopsy. Two luminous-dial workers and two patients who had received radium had bone tumors.

The histopathological changes were similar in certain respects to those changes found by Martland, Heller, and Bloom and Bloom (4). Atypical osseous tissue was present in cancellous bone. It was usually found near the articular surface of such bones as the humerus and femur, in addition to being found in the metaphyseal area. The atypical osseous tissue was laid down adjacent to the trabeculae in some areas. In some areas there was hyperplasia of the trabeculae, while in others destruction of the trabeculae was present. The trabecular spaces were usually filled with an acellular fibrous tissue. In general there was an absence of radioactivity in the atypical osseous tissue and accellular fibrous tissue was found in the autoradiographic study. In most cases, there was a prominent absence of osteocytes in the lacunae, and there was little evidence of bone regeneration.

In compact bone, the central canals showed a wide variation in size from normal to the nearly complete destruction of the entire Haversian system. A large number of the central canals were occluded with a dark staining material similar to the atypical osseous tissue in the trabecular spaces. There was usually an absence of cells in the lacunae. Minimal evidence of bone regeneration was observed, and areas of destruction usually were replaced with fibrous connective tissue.

In addition to the microscopic areas of destruction, macroscopic areas of destruction have been observed in compact bone 1 to 2 millimeters in width and 5 to 20 millimeters in length. Radium concentrations were rarely found in or around the macroscopic areas of destruction. Areas of transition in which both radium concentrations and microscopic change appear together have been found rather infrequently.

It has been shown that a correlation exists between the frequency of destructive changes and the amount of radium deposited in the body. It has been postulated that these macroscopic areas of destruction occur as the result of the fusion of adjacent central canals of Haversian systems undergoing destructive changes. It has been further postulated that the radium had been removed from the macroscopic areas of destruction by the time the changes occurred. If direct irradiation was the primary mode of production of the skeletal changes, more areas of transition should have been found in which both radium deposition and skeletal histopathological changes were present. It is evident that considerable difficulties are inherent in any attempt to reconstruct a pathological process which has been going on for 20 to 30 years from specimens taken at the termination of the process.

As a result of these observations, it is considered that radium deposited in the skeleton usually initiates a sequence of events which eventually produces patholog-

ical changes. These changes are probably the end result of many intermediate factors such as trauma, damage to blood supply, hormonal imbalance, decreased bone repair, and increased bone destruction from other causes. Evidence from the histological sections indicates that Haversian systems may undergo periods of resorption followed by periods of bone formation. There was a subnormal appearance of the bone in many instances. Some of the destructive changes may be the result of the inability of bone to maintain normal repair. It is evident that the relationship between radium deposition and skeletal change is complex. It is reasonable to conclude that when the destructive effects of radium and other deleterious intermediate factors become greater than the reparative processes of the skeleton, permanent alternations occurs (see figs. 6, 7, 8, 11, and 12).

HEMATOLOGICAL FINDINGS

In 1924, Castle, Drinker, and Drinker (8) reported the hematological findings of 22 luminous-dial workers. The erythrocyte count was below 4 million in 6 percent of these cases and above 6 million in 19 percent. The white-blood-cell count was below 7,000 in 27 percent of the workers. Abnormal erythrocytes occurred in 36 percent of the series. There was an increase in lymphocytes and monocytes and decrease in the polymorphonuclear neutrophils found on differential examination.

In 1943, the Public Health Service (41) made a study of 196 employees of luminous-dial-painting plants. Anisocytosis was found in 11 percent of these people, and poikilocytosis in 8 percent. The average erythrocyte count in the Public Health Service series was 4,300,000 as compared with an average of 4,500,000 in a series of 31 control patients.

Hematological data were available on four patients who died. Two of these patients died from malignant tumors—patient (R–24) and patient (B–7). Patient (R–44) died from aplastic anemia and patient (L–27) died from secondary infection and debilitation.

The marked pancytopenia that occurred in Martland's cases had not occurred in the patient evaluated in 1951. A short time before death, there were usually about 3 million erythrocytes per cubic millimeter and 10 grams of hemoglobin per 100 cubic centimeters. No hemorrhagic manifestations occurred in these patients. Some of Martland's cases had less than 1 million erythrocytes and less than 1,000 leukocytes per cubic millimeter shortly before death. He considered the anaemia which developed in his patients to be of a regenerative type resembling pernicious anemia, and he described the hematological changes in the following way: "The blood in this case showed a profound anemia, characterized by a large cell anisocytosis, by the presence of megaloblasts and by a marked leukopenia. There was not, however, a hyperbilirubinemia, and the van den Bergh tests were negative. The anemia is, therefore, not a hemolytic anemia. It is not an aplastic anemia, since there is marked embryonal blood formation. The fault lies in a long continued irritation of the hematopoietic system, the hemolytic or reticuloendothelial system being unaffected. There is a stage of stimulation followed later by sudden exhaustion of the erythroblastic and leukoblastic centers with the production of a rapid, fatal anemia with leukopenia, which fails to be influenced by any form of treatment." Red regenerative marrow was present in the femora of some of these patients.

These changes in the hematopoietic system are similar to those seen in the acute radiation syndrome. Most patients who lived less than 6 weeks following external radiation had hypoplastic marrow. Some patients who died at 4 to 5 months after exposure had diffuse myeloid hyperplasia which involved even such long bones as the femur. Occasionally the marrow appeared pink and gelatinous.

The bone marrow of patient (R–24) was studied in 1948, 18 years after radium administration. It was reported to be distinctly overactive. There was moderate erythroblastic activity with an excessive number of cells in mitosis without a shift to the left. The remainder of the cells were normal. The erythrocyte count was 5,300,000 per cubic millimeter and hemoglobin was 12 grams per 100 cubic centimeters at this time. The erythrocyte count was 3 million per cubic millimeter and hemoglobin was 9 grams per 100 cubic centimeters shortly before death in 1951. Small pink areas of hyperplasia was found in the femur and tibia at autopsy. Sternal marrow from patient (R–43) revealed extremely atrophic marrow shortly before her death in 1947. There were only small areas or erythropoiesis and the remainder of the marrow was composed principally of fat cells. No evidence of excess destruction, serious fat atrophy, or fibrosis

was observed. The hemoglobin at this time was 12.6 grams per 100 cubic centimeters and the erythrocyte count was 3,890,000 per cubic millimeter.

Patients who have over one microgram of retained radium are more likely to have anisocytosis, poikilocytosis, and hypochromia of the erythrocytes than those having under one microgram. However, significant hematological changes usually do not occur until late in the course of the disease.

In view of this additional information, it appears that little qualitative difference exists in the hematological response to internal or external radiation. If an individual had received large enough amounts of radioactive substances internally, hematological changes occurred which were similar to the hematological changes following exposure to large amounts of external radiation. In individuals who had received smaller amounts of either internal or external radiation, the hematological response at any given time was varied. The type and number of cells in the circulation at any specific time is the result of the natural survival of the cells and the balance between their radiosensitivity and their ability to recover from injury.

The red regenerative marrow that Martland described was in all probability an abnormal attempt of the hematopoietic system at increased production as a result of damage. Martland's term leukopenia anemia of regenerative type which was used to describe the hematological changes in the luminous-dial workers does not seem to be appropriate. The anemia that develops in these patients and the patients who had received radium would be more suitably placed in the category of primary refractory anemia in which either a hyperplastic or hypoplastic (aplastic anemia) bone marrow may be found. No leukemias have been found.

ROENTGENOGRAPHIC CHANGE

Areas of increased density have been found in cancellous bone on a skeletal roentgenographic examination. This roentgenographic change is the result of a typical osseous tissue in the trabecular spaces; infrequently it is the result of hyperplasia of trabeculae. Well-defined areas of decreased density were found in compact bone. These were the result of the destructive changes in the cortex of the bone.

These changes were divided into three groups, principally on a descriptive basis (24):

Group I: Areas of decreased density which were usually 1 to 2 millimeters in width and 5 to 20 millimeters in length in the long bones which gave a streaked appearance, and "punched-out" areas varying from 2 to 20 millimeters in the greatest dimensions present in the skull.

Group II: Areas of increased density usually associated with areas of decreased density with varying degrees of change in the trabecular pattern. These usually occurred in the femoral head, the humeral head, and the glenoid process, giving a mottled or motheaten appearance. There was an increase in frequency of biconcavities and collapse of the vertebras in patients having larger amounts of retained radium. Areas of increased density were found along the superior and inferior borders as well as small areas of increased density in the vertebral bodies.

Group III: The term "aseptic necrosis" has been used in referring to the changes in normal configuration. The heads of the femora, the bones of the feet, and the mandible were most commonly involved.

Serial roentgenograms of a few of these patients over long periods of time have been obtained. In most of the available serial roentgenographic studies, changes were not found until years after the deposition of the radioactive element. Periods of skeletal change may occur and may become stabilized or may improve. Later skeletal changes occur with increasing frequency.

For example, one patient (R–50) was given radium in 1922 (age 42 or 44). Ten years later skeletal roentgenographic changes characteristic of radium deposition occurred primarily in the mandible. These changes gradually improved. In 1940 (age 60) a significant increase in skeletal involvement occurred. There was a gradual increase in frequency and severity until her death in 1949. Roentgenograms of the skull of another patient (R–24) in 1948 demonstrated 12 areas of decreased density; the number of areas had increased to 22; however, there was minimal increase in the size of the areas. The size of these areas usually did not become greater than dimensions given in the description of the roentgenographic findings. Infrequently, however, they may be as great as 3 to 4 centimeters in diameter. The usual sequence of events was an increase in numbers, as well as an increase in the number of bones involved.

It is evident from these and other serial roentgenograms that these changes do not bcome detectable until many years after the deposition of radioactive elements. The possibility of skeletal histopathological changes being produced and remaining undetected reontgenographically for years must also be considered. There were no roentgenographic changes found on complete skeletal roentgenographic examination of patient (R–13) in 1950, with the exception of an aseptic necrosis of the right femoral head. However, minor histopathological changes characteristic of the deposition of radioactive elements were found in sections of the left fibula of this patient. In 1954, roentgenographic changes characteristic of the deposition of radioactive elements were present in the fibula and other long bones.

It has been shown that skeletal histopathological changes are reversible in animals following the deposition of radioactive elements. The history of another patient (R–50) demonstrates that skeletal changes as seen by roentgenogram are reversible in man following the deposition of radioactive elements. It is reasonable to assume, therefore, that reversible skeletal histopathological changes occur in man, as well as in animals, following the deposition of radioactive elements. When the reparative processes are able to be maintained at a level equal to the destructive processes, no detectable changes occur as seen by roentgenogram. If the ability to maintain skeletal repair is impaired or decreased, more and more permanent alterations probably result from the imbalance between the reparative and destructive processes.

Histopathological changes have been present to such a degree that roentgenographic changes have been found in many patients who are symptom-free. However, significant clinical changes usually do not occur in the absence of skeletal roentgenographic change. The results of skeletal roentgenographic, autoradiographic, and histopathological studies made on serial bone sections indicate that repeated skeletal roentgenographic examinations are the most satisfactory clinical methods for the early detection of skeletal alterations following the deposition of radioactive elements.

Thirty of the patients who were given radium for medical reasons and two luminous-dial workers were selected because each of these patients had had a complete skeletal roentgenographic examination. They were arranged in order of increasing amounts of retained radium from 0.5 to 14 micrograms, and the skeletal changes characteristic of the deposition of radioactive elements were tabulated. These patients were arbitrarily divided into three groups, those patients having between 0.5 and 1 microgram of retained radium, those having between 1.1 and 2 micrograms, and those having between 2.1 and 14 micrograms. Table I gives the age and sex of 32 patients 21 years after the deposition of radioactive materials.

Forty individuals were selected at random while undergoing physical examination at the Argonne National Laboratory for skeletal roentgenographic examination to act as a control group. It would have been desirable to have matched controls; however, the results of these roentgenographic examinations greatly minimized the possibility of the minor changes being present to any significant degree in the general population.

Five luminous-dial workers having between 0.02 and 0.4 micrograms of radium and 4 radium patients having between 0.01 and 0.4 of radium had complete skeletal surveys. One 83-year-old patient (0.4 micrograms of radium) had roentgenographic changes characteristic of radium deposition. The interpretation of these changes was difficult because of the marked arthritic abnormalities. Another patient with 0.15 micrograms of radium had areas of resorption of the teeth.

The five bones (skull, radius, ulna, tibia, and fibula) which were most frequently the sites of decreased density were first tabulated (chart I). The frequency of involvement was expressed as a percentage of the total number of bones that could possibly be involved. No distinction was made between unilateral or bilateral involvement, since the long bones were involved bilaterally in all but about 10 to 20 percent of the cases.

For example, in the 15 patients having between 0.5 and 1 microgram of radium, a total number of 75 bones could be involved; only 6 bones had changes characteristic of radium deposition or about 10 percent of the total number of bones. In group II, about 55 percent of the bones had characteristic changes, and in group III about 65 percent of the bones were involved.

Attempts were made to find some correlation between the severity of bone changes and increasing amounts of radium. The skeletal changes were divided into six grades of severity (27). With increasing amounts of retained radium

in the body, a proportional increase in roentgenographic changes does not occur. In addition, there is a marked individual variation in the amount of retained radium in the body. In some patients who had the greatest amounts of retained radium, only minor changes occurred, while other patients who had only one-fifteenth to one-thirtieth as much had more severe skeletal changes. Chart II graphically demonstrates this clinical observation in regard to the roentgenographic changes. Patients in group III had about 5 times as much radium as those in group II; however, there is only a 10 percent increase in the frequency of involvement. This lack of correlation between group II and group III is further emphasized by the fact that the average age of group III is 60 compared with an average age of 50 in group II. This finding and the finding of a significant increase in roentgenographic changes and symptoms at 1 microgram, even though the luminous-dial workers were 13 years younger, are suggestive that age at the time of deposition is not a major factor in the eventual production of roentgenographic changes. The fact that some of the patients had between 0.1 and 0.5 microgram of radium deposited for 25 to 30 years and had no roentgenographic changes is suggestive that time of retention per se may not be a major factor in the eventual production of roentgenographic changes. It is to be emphasized that these observations, suggesting that the time of deposition and the length of retention are not important, may well be the result of biased sampling of the patients, as well as an inadequate number of patients.

The most important result of these studies is that a definite correlation has been made between an objective clinical finding and the estimated amount of radioactive element retained in the body. For the first time, a semiquantitative relationship between the frequency of the roentgenographic changes and retained radium has been established within a certain dose range. No characteristic roentgenographic changes have been observed in patients having under 0.1 microgram of radium and relatively few changes have been found in patients having between 0.1 to 0.5 microgram of radium. Between 0.5 and 1 microgram of radium changes began to occur with increasing frequency and those patients having over 1 microgram of radium had a considerable increase in the frequency of the roentgenographic changes when compared with the patients having under 1 microgram of radium. (See figs. 9, 10, 11, 13.)

SYMPTOMS

Symptoms which can be reasonably attributed to radioactive element deposition usually result from destructive changes in the skeleton in patients with small amounts of radium. When symptoms do occur as a result of skeletal change, there are usually not severe in comparison with the extent of skeletal damage. In some instances, there is neither a progression of the skeletal lesions nor of the symptoms following the production of the skeletal change. For example, aseptic necrosis of the head of the femur in one patient developed in 1940 (age 30 years). This was followed by limping and discomfort on walking. However, during the period between the onset of symptoms and the examination in 1951, little progression of either limping or discomfort on walking occurred. There was also minimal discomfort following the aseptic necrosis of the head of the radius in another patient (R–23) since the beginning of symptoms in 1947. Another patient had aseptic necrosis of the head of the femur 22 years after the administration of radium at the age of 50 years (25, 27). She had no other symptoms or changes that could be related to the deposition of radioactive elements.

Skeletal changes usually occur in bones subject to weight-bearing or to repeated trauma (31). Aseptic necrosis usually occurs in the heads of the femora and the bones of the feet. Collapse of the veterbrae is occasionally found, while fractures almost always occur in the femoral shaft. One patient (L–27) fractured her left femur while pressing her foot to the floor of an automobile. Patients (L–14) and (R–35) sustained fractures of the shafts of the femur on minimal trauma. Healing of the fractures was delayed, but proper union occurred in all patients. Rarely, patients may have femoral fractures with permanent nonunion. Patient J. J. had collapse of some of the vertebral bodies with associated pain that required the wearing of a Taylor brace for control, while patient (R–44) had more marked vertebral changes with minimal discomfort.

Many of the patients did not have any symptoms which might be attributed to radium intake. Other than an increased risk of skeletal injury or tumor

formation, they may never have any recognized clinical changes as a result of radioactive element deposition.

In attempts to correlate clinical change with the amount of radium in the body, marked variations were found. The history of patient (R-49) illustrates this clinical observation. The only difficulty that he had was a march fracture of the foot which had healed uneventfully. The patient was asymptomatic at the time of examination in 1951 and only minor skeletal roentgenographic changes were present which could reasonably be attributed to the deposition of radioactive elements. Fourteen micrograms were present in his body, 15 to 30 times the amount present in some patients who had severe skeletal changes or tumor formation.

The time between the deposition of the radioactive elements and the onset of the first symptoms was analyzed in order to see whether any correlation could be made between this period and increasing amounts of retained radium. Twenty-one of the 25 luminous-dial workers had symptoms referable to radioactive-element deposition and 25 of the 50 patients who had received radium had symptoms that could reasonably be attributed to radioactive-element deposition.

The luminous-dial workers and patients who had received radium were divided into two groups. The first 10 patients of the luminous-dial workers who had symptoms and had between 0.1 to 1.5 micrograms of retained radium were compared with the 11 luminous-dial workers having between 1.6 and 18 micrograms of retained radium. The average time before symptoms occurred in the first group was 16 years and in the second group was 15 years. The 25 patients who had received radium who had symptoms referable to the deposition of radioactive elements were divided into 2 groups. The first 12 patients had between 0.7 and 4.2 micrograms of retained radium, the other 13 patients had between 5 and 22 micrograms. The average time before symptoms occurred in the first group was 17 years and in the second group was 16 years. There was no increase in the time interval before symptoms occurred in the first group, even though there was from about 10 to 100 times less radium in the body. It should be noted that the period of latency before the onset of symptoms that could reasonably be attributed to the deposition of radioactive elements varied from 1 to 32 years.

Since no relation between increasing amounts of radium and the time before symptoms occurred from deposition of radioactive elements was found, the frequency of involvement of the femur was examined to see if some correlation could be made with the retained radium. Nine patients who had received radium had aseptic necrosis of the head of one or both femora as did luminous-dial workers.

It is well known that aseptic necrosis of the femoral head usually occurs as a result of trauma or following fractures of the neck of the femur. It is also true that aseptic necrosis does occur in which the etiology cannot definitely be established. In these cases, it is considered that circulatory disturbances are the primary cause of the necrosis. The cause of the disturbance is poorly understood; it may be from gradual occlusion or from trauma (37). Unfortunately there is little information concerning the incidence of unexplained aseptic necrosis of the femoral head. Estimates of the occurrence of aseptic necrosis of the femoral head from all causes approximated 1 in 200 in an orthopedic practice.

The 14 percent incidence of aseptic necrosis of the femoral head in these patients seems unusually high in the absence of a history of antecedents trauma or fracture. It is interesting to note that all of the lesions occurred in patients having 0.7 microgram or more of radium in their bodies. The average time from the deposition of the radioactive elements until symptoms referable to the femoral head occurred was 15 years, approximately the same as that for symptoms in all other parts of the skeleton. The time of onset varied from 9 to 22 years.

Almost all of the fractures occurred in the shaft of the femur. Some of the patients fractured both femora, as well as sustaining refracture one or more times. Fractures following such minimal trauma as pressure of the foot on the floor of the car emphasize the fragility of the skeleton that may result from the deposition of radioactive elements.

Fractures of the femur occurred in 4 of the luminous-dial workers and in only 1 patient who had received radium. Again it should be noted that all of the fractures occurred in patients having 0.9 microgram or more of retained radium.

TUMOR FORMATION

The clinical course of patients in whom malignant tumors eventually develop is illustrated in the following case histories.

Patient (L–21) began to have symptoms as a result of skeletal changes 11 years after employment as a luminous-dial worker in 1934 (27). Several skeletal lesions occurred over a period of years following the deposition of the radioactive element. A biopsy specimen was taken from the left femur in 1950, following the clinical diagnosis of chronic osteomyelitis. The histological diagnosis at that time was a low-grade inactive fibrous osteomyelitis associated with recent slow-growing cancerous type of fibrous osteoma. In 1952 pain began to occur and to increase in frequency in the left femur, and the roentgenograms were interpreted as being suggestive of malignant changes in the medial condyle. The extremity was amputated and an osteogenic sarcoma was found throughout the femur on histological examination. The patient died a few months later.

Patient (R–24) had noted the onset of pain in the left foot 18 years (1948) after the administration of radium water (27). Roentgenograms of the foot a few months later revealed an area of decreased density in the left tarsal navicular. The condition became so severe that it necessitated the use of a cane. Roentgenograms at that time revealed marked destructive changes of the tarsal bones. Because of the marked increase in symptoms and the destructive changes in the foot in 1950, a biopsy was done and fibrosarcoma was diagnosed on histological examination of the specimen. The pain became generalized and required more and more analgesics to control; it was described as boring and burning in character. Intermittently, sharp knifelike pains occurred which lasted for a few seconds. The pain was not significantly influenced by motion, position, and temperature. The leg was amputated below the knee in February 1951. Examination of the amputated tibia revealed extension of the tumor. A sarcomatous lesion developed on the end of the stump a few weeks after the operation. The patient died in August 1951, as a result of generalized metastases. Areas of fibrosarcoma were found throughout the skeleton. In some parts of cancerous bone, it was difficult to distinguish between the fibrosarcoma and the fibrous connective tissue found in the trabecular spaces of patients without malignant involvement.

It is evident from the case histories of patients (L–21) and (R–18) that the transition from a benign to a malignant lesion may be similar to that in many skeletal diseases. Both of these patients were first considered to have osteomyelitis and then osteoid osteoma. In patient (R–17) the diagnosis of osteoid osteoma of the finger had been made 1 year prior to the diagnosis of osteogenic sarcoma. Characteristic histopathological changes due to the disposition of radioactive elements were found in reviewing the specimens from which these diagnoses were made.

The history of patient (L–8) emphasizes the need to follow these patients carefully. Patient has led a relatively normal life since 1934 as a result of prompt attention to symptoms occurring in the elbow. Following a biopsy and histological diagnosis of osteogenic sarcoma, the arm was disarticulated at the shoulder.

Figure 14 shows the sites of origin of the tumors that developed in 13 of the 78 patients. Eight of these patients were luminous-dial workers and five were given radium for medical purposes. Table II gives the age at the time of administration, the time from administration until the first symptoms occurred which could reasonably be attributed to the deposition of radioactive elements, the time from the first symptom until death, and the amount of retained radium. Since both groups of patients have approximately the same amount of radium retained in the body for about the same length of time it is not possible to make any differentiation between luminous-dial workers and patients who were given radium in relation to the estimated radium present. The average age at the time of employment in the luminous-dial workers was 17 years as contrasted with 39 years as the average age at the time of the administration of radium.

Most of the patients in whom malignant tumors eventually develop follow a similar course. There is a latent period followed by symptoms which usually developed from changes in weight-bearing bones and bones which are subject to repeated trauma. Later tumor formation occurs at a site which is usually not the site of the initial symptoms. When the malignant tumor develops, there may be a marked acceleration of the disease process and death usually occurs about 1 to 4 years after the symptoms began at the site where the tumor originated.

THE RELATION OF THE CLINICAL CHANGES TO THE AMOUNT OF RADIUM PRESENT IN THE SKELETON

A. Hematological changes

Minor changes occurred with greater frequency in the erythrocytes in patients having more than 1 microgram of radium than those patients having less than 1 microgram. The marked anemias found in the earlier cases were not present in the 78 cases studied recently. Two patients developed anemias a short time before death. Only one leukemia has been reported.

B. Roentgenographic changes

One of the most important results of the recent Boston-Chicago investigations was the ability to correlate the frequency of occurrence of roentgenographic changes characteristic of radium deposition with the physical estimates of total body radium. Thirty-two patients[3] having complete skeletal roentgenographic examinations were arranged in order of increasing amounts of radium and the changes tabulated (review section on roentgenographic changes.)

C. Symptoms

Symptoms[4] which can reasonably be attributed to radium occur as a result of changes in the skeleton. Symptoms which could reasonably be attributed to radium did not occur in any of the 78 patients who had under 0.4 microgram of radium. Nine patients who received radium medically and two luminous-dial workers developed destruction of the femoral head. The average time for symptoms to occur was 15 years (range 9–22 years). All of the patients who had aseptic necrosis of the femoral head had 0.7 or more micrograms of radium in the body. The 5 patients who had fractures of the shaft of the femur had 0.9 microgram or more of radium in the body.

The results of these investigations indicate that the relation between the destructive effects of the radioactive elements remaining in the body for long periods of time and the clinical changes produced, is a dynamic relationship between the destructive and reparative processes of the body. For example, some of the patients had aseptic necrosis of the femoral head with less than 1 microgram of radium, while others, with 10 micrograms or more, had no deleterious effects. It is reasonable to assume that these patients had other disturbances in the femoral head, such as an unsatisfactory blood supply. Another example of this dynamic relationship was demonstrated in the patient who had increased changes during pregnancy which subsided with the delivery of the child. Still another possibility is that certain periods of increased bone destruction or reduced bone formation may occur from disease processes which later revert to normal. One patient had rather severe changes in the mandible which later subsided.

D. Bone tumors

The 15 malignant tumors which developed in the 78 patients recently evaluated were found in individuals containing for 0.5 to 10 micrograms of radium in their bodies.[5] The patient having 0.5 microgram of radium was a luminous-dial worker. (It is reasonable to assume this patient ingested mesothorium and radiothorium.) The patient with the lowest radium concentration, who had received radium medically, had 0.9 microgram of radium. The patient with the lowest concentration of radium, which was considered not to be contaminated with members of the thorium decay series, and who developed a bone tumor, had 3.6 micrograms of retained radium. The accumulated radiation dose with 3.6 micrograms of radium was estimated to be about 5,000 rads during the 25 years from radium administration until tumor formation.[6]

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[3] One patient which was evaluated later with 0.15 microgram of radium had dental changes.

[4] Symptoms is a term used to denote subjective awareness of an abnormality. When skeletal changes progress to such a degree that destruction of the hip or fractures occur—symptoms are noted by the patient.

[5] See fig. 14.

[6] The estimates of continuous radiation dose is based on the assumption that 0.1 microgram of total body radium will deliver a dose of 3 rads per year to the skeleton. It was found in the radium excretion studies made in the Chicago investigations that the retention of radium could be expressed as an approximate power function of time (27). From this information Brues and Tyler derived an expression for the estimation of the cumulative radium dose from the estimation of the instantaneous dose rate at the time of measurement. Based on the equation of Brues and Tyler, the total cumulative dose would be approximately twice the dose estimated at the time of measurement.

Only 2 bone tumors have developed in the 52 of the 78 patients who were not discovered because of symptoms arising from the skeleton. The frequency of bone tumors in this group is 2 percent, in contrast to a frequency of 14 percent for the entire group of 78 patients.

Vaughan (45) (1956) reported 39 cases of sarcoma arising in bone following external radiation have been recorded in the literature. The latent period between receiving radiation and the development of the tumor was 3 to 11 years. The radiation dose was not known in all of these cases; however, in most recorded cases it was estimated to be usually greater than 3,000 roentgen (1,500–7,000 roentgen change).

Cruz et al (10) (1957) reported an additional series of 11 cases in which the sarcoma of bone occurred from 4 to 24 years after external radiation. The total radiation dose given ranged from about 1,000 roentgens to 5,000 roentgens, and was given over a period varying from 1 month to 9 years.

COMMENTS ON THE POSSIBILITY OF BONE TUMOR FORMATION FROM STRONTIUM 90

The assumption is often made that the incidence of the effect of strontium 90 is proportional to the magnitude of the dose. This assumption has been used to estimate the bone tumors which may be produced from the low concentrations of strontium 90 in the skeleton from fallout. There is no exact evidence either proving or disproving this assumption, but there is some clinical evidence which suggests that this assumption is over cautious.

The 50 bone tumors which have been known to have been produced in man, and reported, were summarized in the previous section. The skeleton in the localized area of tumor induction received at least 1,000 roentgens, and usually more than 3,000 roentgens of radiation. If it is assumed that the radiation dose to bone is greater by a factor of 2 than the measured skin dose, then the minimum observed carcinogenic dose from external radiation would be about 2,000 rads, with the majority of the tumors being produced by more than 6,000 rads of external radiation.

The patient with the smallest total body radium known to induce tumor formation, in which the possibility of contamination of the thorium series in unknown, died from a bone tumor in 1952. The estimated total body radium was 0.9 microgram. The time after administration is unknown, however, it is reasonable to assume that it was about 25 years. Based on the estimates above, the patient would have received a total accumulated dose of about 1,800 rads during the 25-year period.

Histopathological changes have been demonstrated by roentgenographic examination of the skeletons of radium patients prior to development of the tumors. It is generally considered that the bone tumors develop in or around areas of atypical osseous tissue formation. Bone tumors have been shown to develop in or around the abnormal bone formation in animals given plutonium (22). If it could be shown that these histopathological changes are preliminary steps to bone tumor formation, then it could be assumed that as long as the body reparative processes prevented the abnormal bone formation, bone tumors would not develop. It should be emphasized that the associated histopathological changes seen prior to bone tumor formation in the radium patients may be coincidental findings. Proof of a correlation must await a better understanding of tumor induction by radiation.

In reviewing the latent period for tumor induction in the luminous-dial workers reported by Martland in 1931 (31), it was found that the latent period for tumor formation was about 5 to 10 years in 6 patients in this study who developed tumors. Only 3 of these patients had estimates of total body radium reported and these estimates were 6, 15 and 50 micrograms. (See table II.)

The eight luminous-dial workers who were examined in the recent Boston-Chicago investigations, and who died from bone tumors, lived for an average of 25 years after beginning employment. The average total body radium was 3.4 micrograms (range of 0.5 to 10 micrograms).

It is considered that little reliance can be placed in estimating the minimum total body content for tumor induction over a life span from this meager data. However, if we do estimate the total body radium which would have a latent period for tumor induction greater than the average life expectancy (80 years)

from the 2 average radiation doses and latent periods given above, it would be 0.3 and 0.4 micrograms of radium respectively.[7]

If we assume that 0.5 micrograms of radium retained for 70 years would be the minimal body burden necessary to cause skeletal sarcoma, this would mean that a cumulative radiation dose of about 2,000 rads would be necessary to induce tumor formation. It is considered that the most reliable estimates of the radiation dose necessary to induce bone tumors over a life span of 70 years, should be equivalent to the total radiation dose necessary to induce carcinogenesis in 5 to 30 years (2,000 rads). Therefore, the estimates of the total radiation dose from strontium 90 necessary to cause bone tumor formation in man during a life span of 70 years will be based on the best available estimates from human data of about 2,000 rads. It may be calculated that 1×10^{-9} curies of strontium 90 will give a radiation dose to the skeleton of 3 millirads per year, or 0.2 rads in 70 years under equilibrium conditions. Based on the preceding assumptions, the total skeletal radiation dose from 1×10^{-9} curies of strontium 90 over a 70-year period would be in the order of one ten-thousandths total skeletal radiation dose known to produce tumors in man, either from external or internal radiation.

COMMENTS ON THE PRESENT MPC OF RADIUM

Results of the Boston-Chicago investigations have shown that clinical changes begin to occur in patients with 0.4 micrograms of radium. The fact that changes are found to occur in man with 4 times the present MPC of 0.1 microgram of radium instead of 12 times the MPC when it was established in 1941, is offset by the greater confidence in the MPC by the studies of a much larger group of patients with radium present in their bodies for longer periods of time.

It is reassuring that it has not yet been necessary to change the MPC of radium of 0.1 micrograms following the large amount of information that has accumulated since it was first established. There are certain considerations, which, if sufficient information becomes available, may permit raising, or may necessitate lowering, the present MPC. These are as follows:

A. Factors which may permit the raising of the MPC for radium

1. The contribution of the thorium series to the dose from radium.—It is well established that mesothorium I (half life 6.7 years) and radiothorium (half life 1.9 years) were used in the luminous-dial paints, and mesothorium has been found in patients given radium medically. The radiation dose from mesothorium in some of these patients has been significant. It has been the major source of the cumulative radiation dose in some of the luminous-dial workers.

The time relation in this question is very important. It is assumed that reversible changes are produced by the presence of mesothorium, then its major clinical effect should have occurred before the changes began to occur in these patients. Roentgenographic changes do not begin to occur until 15 to 30 years after the deposition of the radioactive elements. However, once changes begin to develop in these patients, there is usually a gradual increase in the frequency and severity rather than a decrease. If the changes produced during the period immediately after deposition are irreversible and contribute significantly to the changes that are being seen in these patients 20 to 30 years later, then it may be possible to elevate the MPC. Further work is in progress in an attempt to clarify the relationship of mesothorium, radiothorium, and radium in the clinical changes observed.

2. Controls.—Even though the roentgenographic changes found in these patients are characteristic of radium deposition, further control studies to estimate more reliably the incidence of these changes in the general population should be carried out. The skeletal roentgenographic findings may eventually prove to be the most reliable method for the early detection of clinical changes from the deposition of bone-seeking radioelements. A better definition of the normal limits of skeletal variation is needed, in order that a more precise evaluation of the skeletal changes in the radium patients may be made.

The destruction of the head of the femur is another clinical finding in these patients which emphasized the need for further control studies; 11 of the 78 patients had destruction of the hip. This frequency of aseptic necrosis is

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[7] These estimates are consistent with the findings of animal data of Brues[6], in which the latent period was found to vary inversely with the square root of the dose. The value of 0.3 micrograms of radium is obtained from assuming that the latent period of 10 years is increased by a factor of 8, and the average radium content is 25 micrograms. The value of 0.4 micrograms is obtained from assuming that the 25 years' latent period is increased by a factor of 3, and the average radium content of the patient is 3.4 micrograms.

unusually high in the absence of a history of antecedent trauma. Proper evaluation of this condition in the radium patients must await a better understanding of the cause of aseptic necrosis, as well as more reliable information as to its occurrence in the general population.

B. Factors which may necessitate lowering the MPC of radium

1. Age at the time of administration and duration of retention of radium.—The average age of the 50 patients at the time they received radium medically was 34 years. The average age of the luminous-dial worker at the time of employment was 17 years. Almost all of these patients either received radium, or were exposed to radium from 1920 to 1930. It is evident that the clinical information on the effects of radium deposited in the body for more than 40 years is lacking. Since the findings of the recent investigations suggest that clinical changes are the result of an imbalance between reparative and destructive processes of the body, it may be found that small amounts of radium in persons of the older age groups may produce significant clinical changes. This may result from either diminished reparative abilities, or an increase in skeletal destruction from other debilitating diseases of the older age groups.

There are only a few patients available for study of the long-term effects of radium who received the radium before the age of 15 to 20 years. One of these patients had the smallest amount of total body radium in which dental change characteristic of radium deposition has been observed.

The problem of bone tumor production in the younger age groups is probably the most important question in this regard. The greatest incidence of osteogenic sarcoma is considered to occur in the second and third decades, and almost no clinical information is available on the effects of skeletal radium deposition before this increase in the incidence of bone sarcoma occurs. It is possible that the increased susceptibility to skeletal tumor induction might necessitate a significant lowering of the MPC for children.

2. Undected subclinical effects.—The present philosophy for the establishment of the MPC is based on the correlation of physical estimates of the quantity of the radioelement in the body with detectable clinical changes or abnormalities in clinical tests. As more refined tests are developed to detect abnormalities in body function, it may be discovered that the present MPC is producing changes which are not detected by present methods of clinical evaluation.

Another important consideration is that subclinical changes may cause a reduction in the reserve function of organs. This may go undetected in most instances. However, the combined effects of an intercurrent disease and the reduced function from the effects of radiation may cause more severe effects than either the disease or the radiation separately.

COMMENTS ON THE PRESENT MPC OF STRONTIUM 90 BASED ON THE INFORMATION GAINED FROM THE EFFECTS OF RADIUM IN MAN

The present estimates of the MPC of strontium 90 are based on the assumption that both radium and strontium are distributed in a similar manner in bone. It has been shown that the radium concentration in different parts of the skeletons of the radium patients and luminous-dial workers may vary by a factor of 10 and probably greater. The present MPC for strontium 90 is based on the comparison of the average skeletal does of strontium and radium. Since under equilibrium conditions the strontium 90 distribution would tend to approach uniformity, comparison of the maximum skeletal dose in the localized areas of high radium concentration in the radium patients and the maximum skeletal dose in the strontium 90 patients might permit raising of the present MPC of strontium 90 by as much as a factor of 10.

The best available estimates indicate that biological effectiveness of radium is 1 to 4 times that of strontium 90 (i. e., the radium RBE[8] = 1 to 4) when compared on an equivalent energy basis. This work is based on the results of animal experimentation (15, 16). Comparison of the radiation dose necessary to produce bone tumors in man from internal and external radiation indicates that the RBE of radium may be somewhat nearer to unity. However, the estimation of radiation dose from external radiation might be in error by as much as a factor of 2. If it can be established that the RBE of radium is greater than unity then it might be possible to increase the MPC of strontium 90 by as much as a factor of 4.

[8] Relative biological effectiveness: The ratio of gamma of X-ray dose to the dose that is required to produce the same biological effect by the radiation in question.

It is considered that the best estimates that can be made in regard to the effects of strontium 90 over a life span (70 years) on the present incomplete information of the effects of radium in man are as follows:

1. The skeletal content of strontium 90 necessary to produce a bone tumor in a life span of 70 years would be in the order of 10 microcuries (100 times the present MPC of 0.1 microcurie of strontium 90 for the general population).

2. The skeletal content of strontium 90 necessary to produce significant changes, such as destruction of the hip, would also be in the order of 10 microcuries (100 times the present MPC of strontium 90).

3. The skeletal content of strontium 90 necessary to produce minimal skeletal changes which could be demonstrated roentgenographically, would be in the order of 2 microcuries (20 times the present MPC of strontium 90).

It should be emphasized that these estimates of the concentrations of strontium 90 which may produce skeletal damage are the result of estimates based on the best available information at present.

TABLE I.—*Age and sex distribution of 32 patients having between 0.5 and 14 micrograms of radium present in their bodies on an average of 21 years after deposition*

	Group I (0.5 to 1 microgram)	Group II (1 to 2 micrograms)	Group III (2 to 14 micrograms)	Total
Sex:				
Male	4	2	5	11
Female	11	6	4	21
Total	15	8	9	32
Age:				
30 to 40	1	0	0	1
40 to 50	9	4	2	15
50 to 60	3	3	2	8
60 to 70	0	0	3	3
70 to 80	2	1	2	5
Average age (years)	49	51.9	60.9	
Average time (years) since administration	20.3	22.2	22	

CHART I

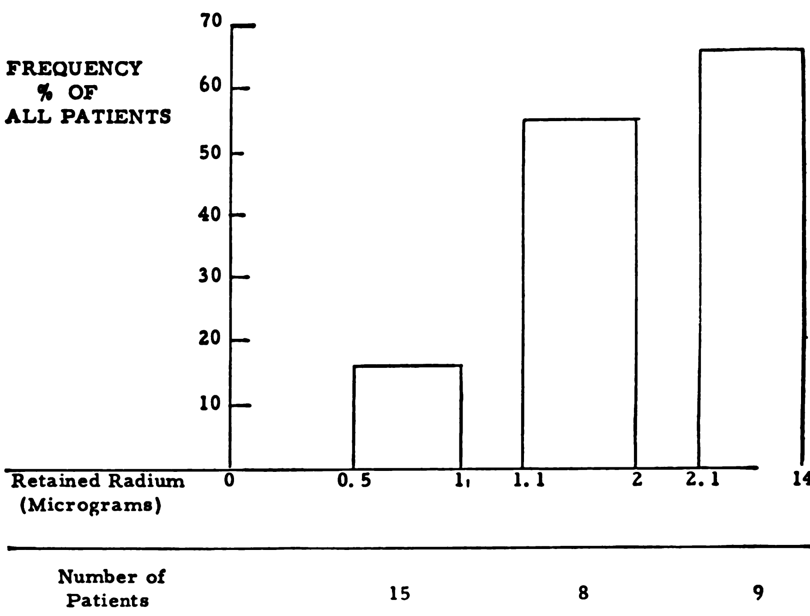

Table II.—*Time interval between deposition of radio-elements and occurrence of symptoms, tumor formation, and death in the 1931 and 1951 investigations*

	Number of patients	Average age at administration (years)	Average time from administration to death (years)	Average retained radium (micrograms)
Luminous-dial patients	3	17	10	23
1931 series				(6–50)
Range, years		16–21		
Luminous-dial patients	8	17	26	3.4
1951 series				(.5–10)
Range, years		15–21		
Radium patients		39	26	3.1
1951 series	5			(.8–8)
Range, years		25–49	1–8	

Figure 1

CHANGE OF ELIMINATION COEFFICIENT WITH TIME

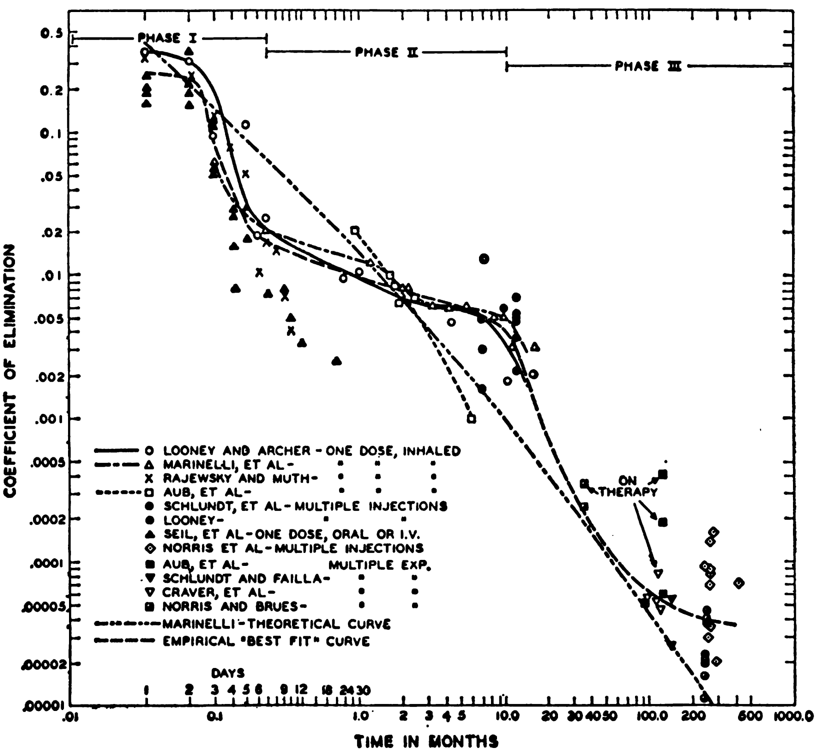

FIGURE 2.—Gross autoradiogram of a longitudinal section of the femur of patient M. K. Note the irregular distribution of radium in the cancellous bone of the upper end of the femur and the small areas of focal concentration in the cortex. The contour of the head is outlined by small concentrations at the junction of the articular cartilage and cancellous bone. The medial portion of the shaft is outlined by the small highly concentrated areas near the periosteum and the endosteum. (Reproduced courtesy of Journal of Bone and Joint Surgery.)

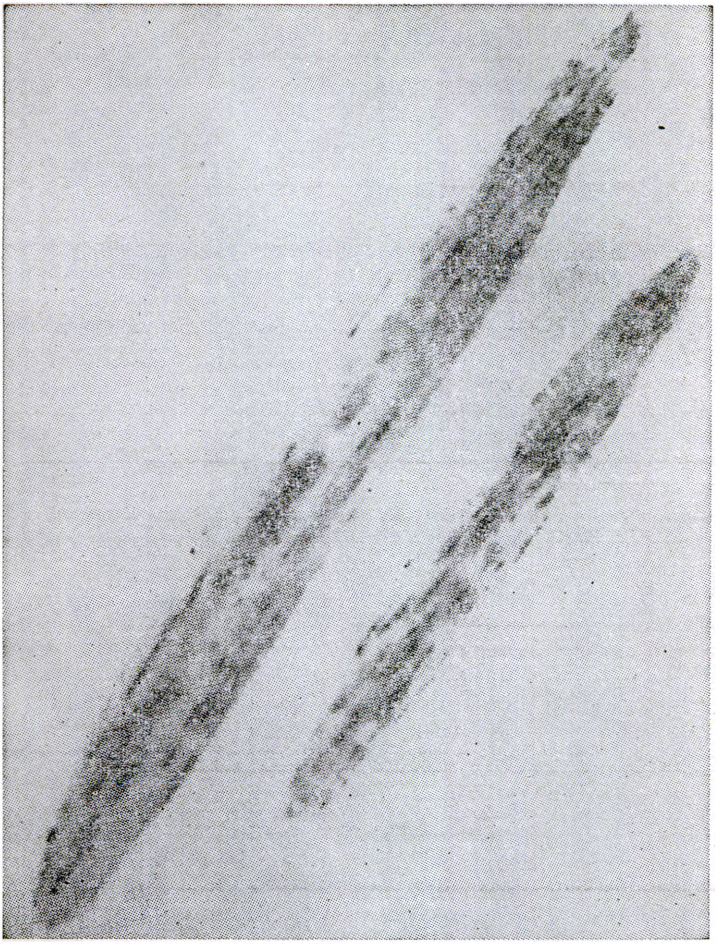

FIGURE 3.—Gross autoradiograms of two longitudinal sections of the cortex of a femur from patient R–24 (5-week exposure). Smaller section was taken 2 millimeters from the periosteum and the larger section was cut just medial to it. Areas of concentration are more frequent in the smaller section and the lower end of the larger section, which are nearer periosteum. The large area of darkening in the center of the larger section is the result, probably, of an endosteal concentration of radium, since this section was on the periphery of the narrow cavity. (Reproduced courtesy of American Medical Association, Archives of Pathology.)

FIGURE 4.—Detailed autoradiogram of a cross section of cortical bone from the humeral shaft of patient R–24 (5-week exposure). Two of the small number of Haversian systems have radium concentrated in this section. The greatest concentration of radium is on two concentric lamellae in the center of the Haversian system. Alpha tracks are less dense in remainder of the two Haversian systems and interstitial lamellae between the systems. Observe that the activity suddenly falls off around these areas of concentration and that the rest of the photomicrograph is free of alpha tracks (X 207). (Reproduced courtesy of American Medical Association, Archives of Pathology.)

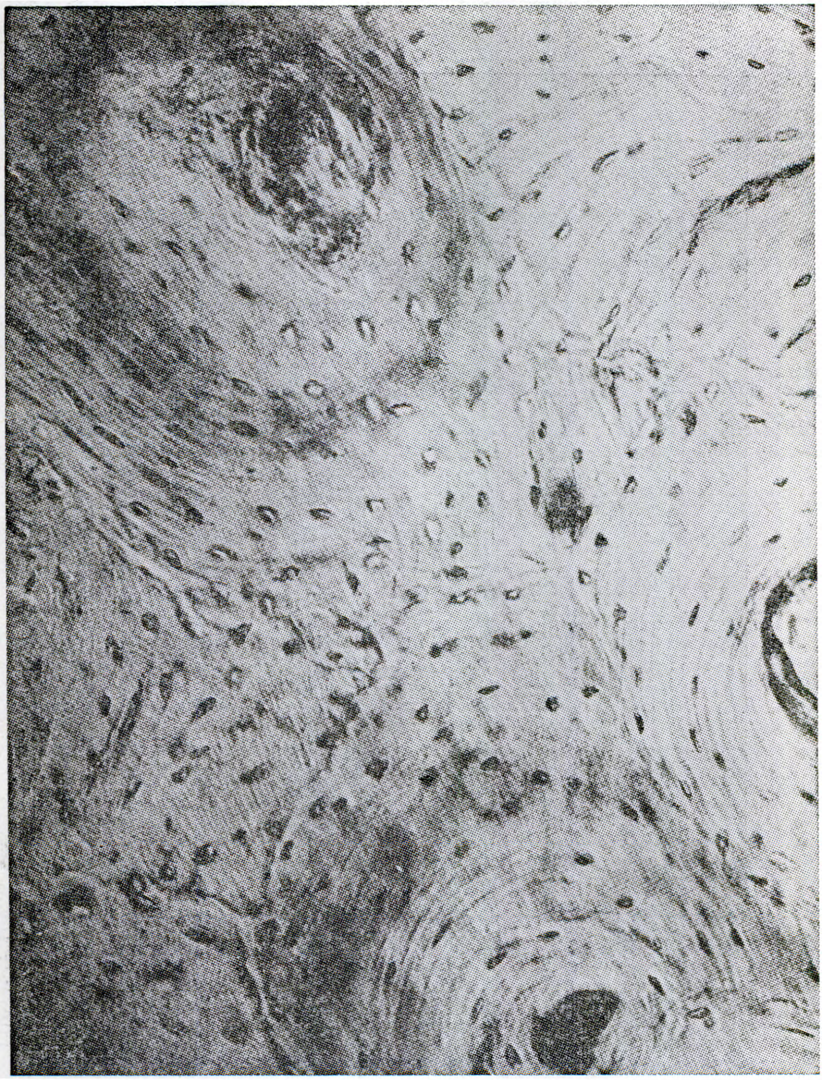

FIGURE 5.—Photomicrograph of bone underlying detailed autoradiogram shown in Figure 4. Note the dark concentric rings outlining lamellae with greater concentration of radium. Central part of Haversian system in upper left is undergoing destructive changes. This, together with figure 4, demonstrates how radium concentration and histopathological changes can be studied at the same time (X 218). (Reproduced courtesy of American Medical Association, Archives of Pathology.)

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FIGURE 6.—Histological section of the head of a right humerus (fig. 2). Note the areas of atypical osseous tissue throughout the head. The largest area of atypical osseous tissue is seen at the top of the photograph near the articular surface. (Reproduced courtesy of Journal of Bone and Joint Surgery.)

FIGURE 7.—Photomicrograph of one of the areas of atypical osseous tissue seen in the histological sections. The atypical osseous tissue is seen at the top of the photograph. The acellular fibrous tissue is seen throughout the trabecular spaces. (Reproduced courtesy of Journal of Bone and Joint Surgery.)

FIGURE 8.—Higher magnification of one of the areas seen in figure 4. Bone is seen in the lower left corner, with darker staining atypical osseous tissue adjacent to it. Fibrous connective tissue is seen in the remainder of the field. (Reproduced courtesy of Journal of Bone and Joint Surgery.)

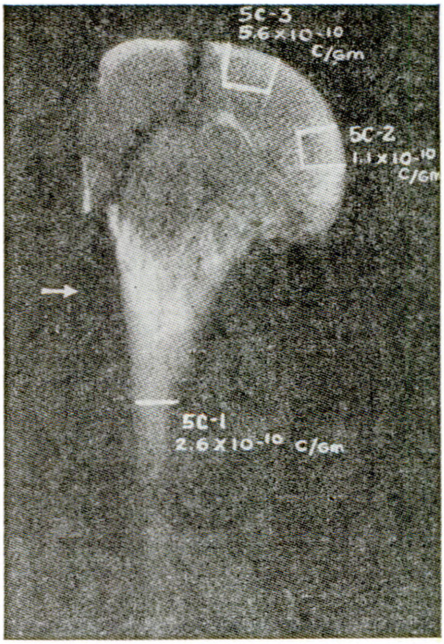

FIGURE 9.—Roentgenogram of a 2-millimeter section taken from the head of the right humerus. (See fig. 6.) Sections of bone, 5C–2 and 5C–3 were removed following the making of gross autoradiographs of this bone section. As will be noted in the figure, 5C–2 was taken from an area which seemed to have a large concentration of radium as shown by gross autoradiography; 5C–3 was taken from an area which seemed to have a small amount of radium as shown by gross autoradiography and a normal roentgenographic pattern. It should be noted that the radium content in RC–3 was five times the content in RC–2. Radium values are expressed as curies per gram of ashed bone. (Reproduced courtesy of the Journal of Bone and Joint Surgery.)

FIGURE 10.—Roentgenogram of the head of the femur of patient (R–34). The mottled appearance is the result of the atypical osseous tissue and hypertrophy of the trabeculae and is characteristic of the changes seen in cancellous bone following the deposition of radioactive elements (Review figs. 6, 7, 8, and 9). (Reproduced courtesy of Journal of Bone and Joint Surgery.)

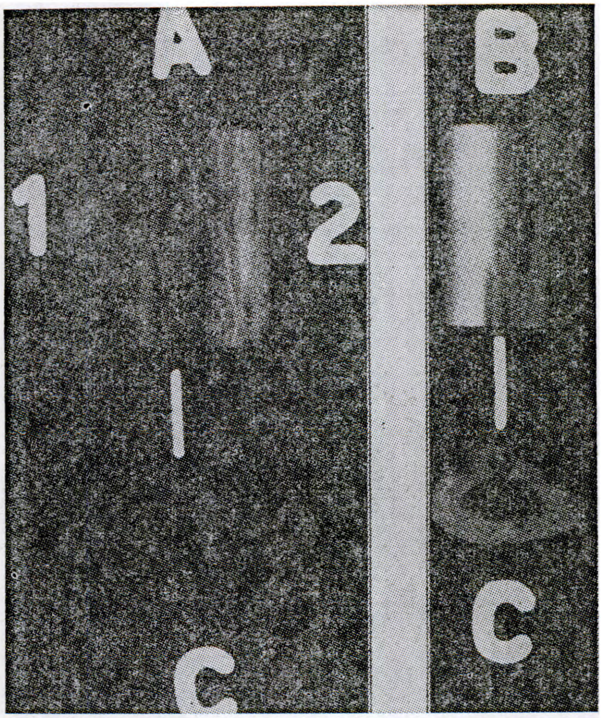

FIGURE 11.—Roentgenogram of a horizontal and vertical section of the fibula of patient (R-24). Note the "streaked" areas that are seen in the long bones as the result of well differentiated areas of destruction in the cortex. The pointers show the areas of destruction in the vertical and horizontal planes (Argonne National Laboratory Report, ANL 4666).

FIGURE 12.—Photograph of the areas of destruction seen in figure 11B. Note that the areas of destruction which give a "streaked" appearance to the long bones are about 3 to 6 times the diameter of the Haversian systems. The central canal in between the two macroscopic areas of destruction is about one-third the diameter of the Haversian system, while in the upper right corner there is a central canal that is enlarged to about two-thirds the diameter of the Haversian system (Argonne National Laboratory Report, ANL 4666).

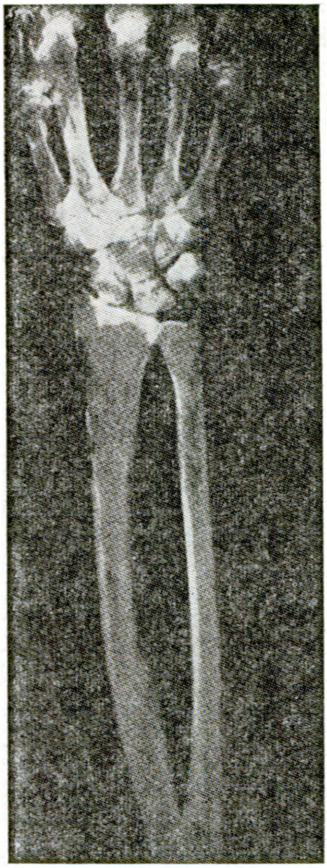

FIGURE 13.—Roentgenogram of the lower arm and hand of patient M. L. (L–27). The small areas of decreased density in the radius and ulna and bones of the hand give a streaked appearance. These small well differentiated areas of decreased density are characteristic of the deposition of radioactive elements. (Reproduced courtesy of American Journal of Roentgenology, Radium Therapy and Nuclear Medicine, 72 : 842, 1954.)

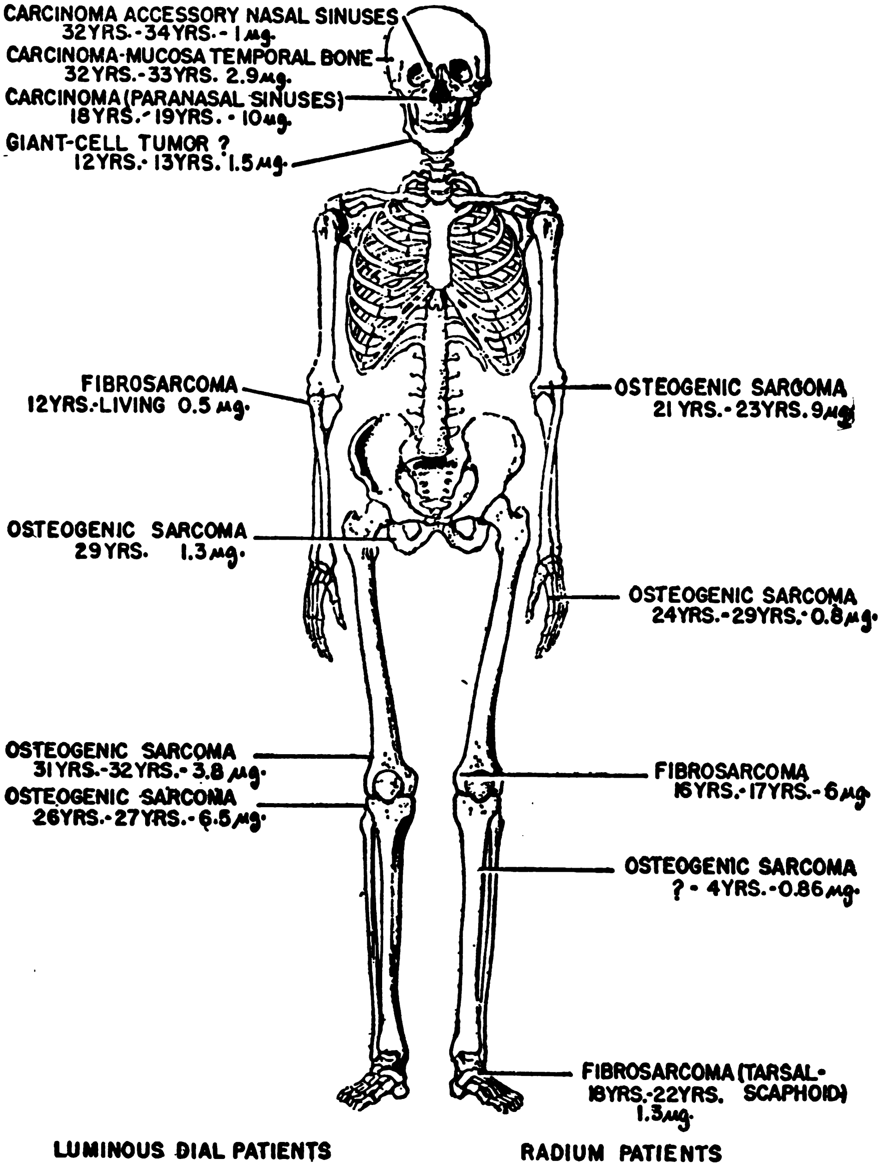

FIGURE 14.—Tumor formation in patients in Boston and Chicago investigations (1951). The five tumors which developed in the 50 patients who had received radium are shown on the right side of the skeleton and the 8 tumors which developed in 8 luminous-dial painters are shown on the left side of the skeleton. Two more tumors have developed in the patients who had received radium medically. These are not included. The type of tumor, the time from deposition to occurrence of symptoms, the time from deposition to death, and the amount of radium are given in each case. Reading from top to bottom, the numbers of the luminous-dial workers are 15, 20, 25, 16, 8, 14, 21, and 23; the numbers of the radium patients are 45, 17, 36, 18, and 24. (Reproduced courtesy of Journal of Bone and Joint Surgery.)

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From 1950 to 1952, the author was a postdoctorate Atomic Energy Commission fellow in the medical sciences of the National Research Council at the Argonne National Laboratory. He was assigned the responsibility for the clinical aspects of the investigation of individuals who had been given radium salts and people who had been employed as luminous-dial workers (1915 to 1930) and the responsibility for the correlation of the clinical aspects with the biophysical aspects of the investigation. This investigation was conducted by the following: Dr. A. M. Brues, Dr. L. D. Marinelli, Dr. W. P. Norris, Dr. R. J. Hasterlik, and Dr. A. H. Stehney, their associates and the author. The histopathological studies were made at the department of orthopedic surgery, University of Chicago, with Dr. C. Howard Hatcher. Detailed reports of the various phases of the investigation will be published.

The author was afforded the privilege of reviewing the clinical data of a similar investigation carried out at the Harvard Medical School and the Massachusetts Institute of Technology by Dr. J. C. Aub, Dr. R. D. Evans, Dr. L. H. Hempelmann, and Dr. M. S. Martland.

The Atomic Energy Commission fellowship, 1950–52, was under Dr. A. M. Brues, Director, Division of Biology and Medicine, Argonne National Laboratory.

Dr. L. D. Marinelli, Dr. W. P. Norris, Dr. A. H. Stehney, and their associates were responsible for the physical and radiochemical aspects of the investigation at the Argonne National Laboratory.

Dr. R. J. Hasterlik and Miss Ellen Shermont gave their assistance in the clinical and hematological aspects of the investigation at the Argonne National Laboratory.

Dr. Joseph Aub, Dr. Robley Evans, Dr. Louis Hempelmann, and Dr. Lawrence Robbins gave their assistance in the review of the clinical data of the investigation carried out at the Harvard Medical School at the Massachusetts Institute of Technology.

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Mr. Atlee Tracy of the Argonne National Laboratory and Mr. J. T. Stringer of the Naval Medical School were responsible for the photographs.

Mr. Melvin Runkel of the Naval Medical School and Miss Frances Fee of the Argonne National Laboratory prepared the illustrations.

A Study of the Dynamics of Strontium and Calcium Metabolism and Radioelement Removal [1]

PRELIMINARY REPORT, MAY 31, 1957

W. B. Looney,[2] C. J. Maletskos,[3] M. J. Helmick,[4] J. Reardon,[5] J. Cohen,[6] W. Guild,[7] and F. I. Visalli [4]

The classic studies of Aub, Evans et al.[1a] in 1938 in radioelement removal demonstrated that the renal clearance for radium was less than 1 percent in 24 hours. This finding suggested the possibility that direct radioelement removal from the blood might prove to be an effective way of eliminating radio-

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[6] Visiting fellow in physics, Massachusetts Institute of Technology.

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[1a] Aub, J. C.; Evans, R. D.; Gallagher, D. M.; and Tibbetts, D. M.: Effects of Treatment on Radium and Calcium Metabolism in the Human Body. Ann. Int. Med. 11: 1443–1463, Feb. 1938.

elements. Both the artificial kidney[2] and ion-exchange resins[3] afford means for radioelement removal, as well as providing an opportunity for extension of the work by Hastings and Huggins[4] on the mobilization of calcium in the circulating body fluids.

Preliminary in vitro experiments were performed to determine some of the parameters necessary to evaluate the feasibility of using either approach.

The first experiments were carried out on a simulated artificial kidney. Stable calcium, calcium 45, and ethylenediamine-tetraacetic acid were readily dialyzable under conditions similar to those in the artificial kidney (calcium being omitted for the purpose of the present experiments).[5]

In later experiments solutions of strontium 85 and calcium 45 were passed directly through ion-exchange columns to determine the effectiveness of a synthetic cation exchange resin in removing radioelements.

A total of 20 dogs has been studied, each dog having been connected to either the artificial kidney or ion-exchange column following the administration of strontium 85, strontium 89, and calcium 45. The ion-exchange column has been used in preference to the artificial kidney because of its more effective removal of calcium, its simplicity and potential adoption for more extensive utilization.

The ion-exchange resin offers a wide range of potential application to biological investigation. Appropriate adjustment of the cation concentrations in the column permits preferential removal of a particular cation of interest. The ability to place electrolytes of a biological system out of equilibrium affords an excellent tool for the study of the dynamics of both the stable and the radioactive electrolytes of the biological system.

The efficiency of radioelement removal as a function of time after administration has been determined by both single- and multiple-isotope methods. Between 30–40 percent of the radio-isotopes injected intravenously 1 hour prior to connecting the dog to the resin column can be removed during a 4- to 6-hour period. At 12 hours after injection, total removal of the radioelements decreased to about 6 to 12 percent, and at 24 hours to about 3 to 6 percent. After 3 days about 2 percent is removed. When the experiment was repeated in 2 dogs 1 week later, less than 1 percent was removed.

Analysis of the removal of the isotopes in three dogs indicated the following: About 80 to 90 percent of the dose is in a hypothetical compartment of bone which has a half time of removal of 8 to 16 hours; the remainder of the dose is in a compartment approximating in size the extracellular space and the half time of removal is 15 to 30 minutes.

To test the influence of serum calcium concentration on isotope removal, calcium 40 was infused intravenously at the same time as the resin perfusion. No significant decrease in radiostrontium removal has been found in 2 dogs in which the serium calcium levels of 6 to 8 milligrams percent were maintained. This result would be consistent with the hypothesis that the principal mechanism of radioelement removal by the ion-exchange column is cation exchange rather than enhanced physiological response from depressed calcium levels.

These studies demonstrate that the ion-exchange column and the artificial kidney are practical means for studying the dynamics of stable and radioactive electrolytes. No major contraindications have been found to prevent its adoption for clinical use.

Representative COLE. Mr. Chairman, if you do not mind, I would like to ask again, Dr. Looney, if you will interpret your expression that the assumption that the incidence of the effect of strontium 90 is proportionate to the magnitude of the dose. You have indicated that experience and studies led you to the conclusion that this is an overcautious—did you say overcautious?

Dr. LOONEY. Yes.

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Representative COLE. Assumption?

Dr. LOONEY. Yes.

Representative COLE. What do you mean by that?

Dr. LOONEY. I mean that the present clinical information that we have would not substantiate the concept of a linear relationship, neither does it disprove it. If I had to weigh and balance these two factors, I would say that most of the information indicates that a certain amount of irradiation is necessary to produce bone tumors in man. However, this is based on the available information. It certainly is to be again emphasized that these tumors may be produced in man at a lower dose, and we have not been able to detect this at present. There are some methods of obtaining more information from clinical studies. I think they will be done.

To say this might be an overcautious assumption is probably not a good term, but I hope I have given you the reason for making the statement.

Representative COLE. Since your clinical evidence indicates that the incidence may be less in proportion to the magnitude of the dose, would that indicate the possibility of a threshold for the effects of strontium 90?

Dr. LOONEY. I would say that the present clinical information in man would not substantiate either conclusion.

Representative COLE. Based on clinical evidence?

Dr. LOONEY. Based on the evidence in man; yes, sir.

Representative COLE. You are not able to determine yes or no with respect to threshold?

Dr. LOONEY. No, sir; I am not.

Representative HOLIFIELD. Dr. Looney, how were you able to determine the amount of exposure these people had, in view of the fact it was years later before you were aware of their illnesses?

Dr. LOONEY. The patients in Chicago were found by reviewing the records of a mental hospital in which the patients were given radium. The files of the United States Radium Corp. were also made available to the Argonne National Laboratory, and we were able to get names and to locate these people by following names.

Representative HOLIFIELD. This did not obtain to those employed as radium painters?

Dr. LOONEY. Yes; it did.

Representative HOLIFIELD. Were you able to measure the dose they received at the time they received it by the residual amount in their bones when it was called to your attention?

Dr. LOONEY. The physical estimates were made by the physicists at Massachusetts Institute of Technology, and at Argonne National Laboratory. That is a physical area, and I would prefer to leave it to the physicists.

Representative HOLIFIELD. I was just interested to find out if we had an accurate estimate of the original dose. I know in the case of the Hiroshima and Nagasaki people that is one of the missing elements in our evaluation of the dose, that we do not know exactly how much they have received.

Dr. LOONEY. Yes. In regard to the patients in the mental hospital, we do have a record of the amount given, and the estimates at 6 and 12 months. Estimates were also made after 20 years. This is the

best available evidence we have in man. Based on this information we can make estimates of the original dose of radium in other people in which we find the radium 20 to 30 years after administration. The physical data on the luminous dial workers is confused by the fact that mesothorium and radiothorium were present in the paint. This presents a very difficult problem in trying to establish reliably the relationship of the clinical changes to the physical estimates of the radiation dose.

Representative HOLIFIELD. Thank you.

Representative VAN ZANDT. Dr. Looney, is it possible to leach out selectively the poison that has gotten into the skeleton of the body?

Dr. LOONEY. You are talking about removal of these radio elements once they are deposited in bone?

Representative VAN ZANDT. Yes.

Dr. LOONEY. We have been working on that in Boston in the past 2 years, trying to remove strontium 85, strontium 89, and radio calcium from bone. We found in the first hour we can remove approximately 30 or 40 percent of the strontium. This efficiency of removal rapidly declined, until after 2 or 3 days, it was less than 1 percent.

Other methods using chemicals to remove bone-seeking radio element have been attempted. There has been some progress in this field, but once the radio element is deposited in the bone, the chances are very remote that we will be able to remove it.

Representative VAN ZANDT. Thank you.

Chairman DURHAM. Doctor, figure 14, where you have the skeleton——(See p. 1188.)

Dr. LOONEY. Yes, sir.

Chairman DURHAM. Was any conclusion made as to why the carcinoma was more prevalent in the head and nose and mouth than other parts of the body?

Dr. LOONEY. You will notice that these people who developed these tumors were luminous dial workers. There has been the hypothesis that because the material was ingested and inhaled, rather than injected intravenously or given orally, that it might be the result of the local effect. But, I might point out that one of the radium patients has also developed a similar type of tumor. I have no readily available conclusion as to why this developed.

Senator HICKENLOOPER. Mr. Chairman.

Representative HOLIFIELD. Senator Hickenlooper.

Senator HICKENLOOPER. Just from a visual examination of the chart, it would indicate that one might assume the tumors occurred in proximity to the point of contact or ingestion of this radium treatment, or the radium material. In other words, with the luminous dial patients, as I understand it, they ingested this as a result of either inhalation or putting the brushes in their mouths to wet them so they could point them up and paint the figures on the dials.

Dr. LOONEY. Yes, sir; that is correct.

Senator HICKENLOOPER. And the incidence seems to be much greater in the nasal and throat area in those. On this chart there is no incidence of a tumor above the elbow in the strictly radium patient as differentiated from the luminous dial patient.

Dr. LOONEY. Yes. However, you will note it is a different type of tumor in the radium patients. You will notice these tumors are carcinomas in the luminous dial workers, and you will notice that the

sacromas of bone have occurred both in the luminous dial workers and radium patients.

Senator HICKENLOOPER. I am speaking purely as a layman who knows nothing whatsoever about this thing. A layman might be led to the conclusion from the chart that radium treatments might have been given in other parts of the body, but in the luminous dial workers the repeated incidence is at the point of ingestion very frequently.

Dr. LOONEY. This is an interesting observation, sir. I know this has created considerable comment as to whether there is a casual relationship among the people associated with these investigations. I am trying to point out both arguments for and against this. It is a different type of tumor that has developed in his area, and most of the tumors have been sarcomas of the bone.

Chairman DURHAM. Did the same type rays produce the three different types of sarcomas and carcinoma? You have three different types. Did the same rays produce all three types?

Dr. LOONEY. I think that the present available evidence is that any radiation has a similar effect biologically. So that it would be a combination of these three effects.

As far as radium is concerned, the majority of the radiation comes from the alpha particles, probably 90 or 95 percent.

Chairman DURHAM. Then the conclusion would be that beta rays or gamma rays would produce all types of carcinomas?

Dr. LOONEY. In sufficient quantities we must be aware of the quantitative aspects of the effect of radiation. I think it was brought out previously that the very sensitivity of physical measurements may lead to overemphasizing the effects of minute amounts of radioactivity.

We have a range of thousands of times between the amount of strontium 90 that is present, and the amount of strontium 90 that we calculate to produce equivalent energy to cause tumors in man from the available information on radium.

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. Dr. Looney, you have been talking about bone tumors. What about the utilization of radiation to attack these bone tumors?

Dr. LOONEY. Well, sir, I am not a radiologist, and this is in the field of radiology. I am aware of this, but I think you would get a much more competent opinion on this from some radiologist who is actively engaged in treatment.

Representative VAN ZANDT. Would the same thing apply to arthritis?

Dr. LOONEY. Yes, sir.

Representative VAN ZANDT. One more question, Dr. Looney. You have had many years of experience now in studying the body that has absorbed radiation from radium. Have you developed any kind of a preventative to this type of radiation?

Dr. LOONEY. No, sir, we have not yet. So far as I know, there has been no effective means of treatment.

Representative VAN ZANDT. Have you developed any type of a program that would prepare the body to resist radiation stemming from radium?

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Dr. LOONEY. I have no knowledge of such.

Representative HOLIFIELD. Thank you very much.

Our next witness will be Dr. W. F. Libby, Commissioner, Atomic Energy Commission, a distinguished chemist, and member of the Commission since 1954.

Dr. Libby, before you start your testimony, the Chair will insert, without objection, 2 letters of notification, 1 under date of May 3 to Mr. Strauss, which gave an outline of our hearings, and the witnesses that had been invited up to that time, and the second letter under date of May 21, which supplemented the first letter. I would like to read the pertinent part of that, in which I said to Mr. Strauss:

> I would like to make clear that we would be happy to have Dr. Libby on hand to provide extra testimony and comment throughout the hearings after the introductory witnesses beginning on Monday afternoon, May 26. We understand that he is preparing written statements for certain portions of the outline. We will be glad to receive these statements for the record, and will be happy to have Dr. Libby comment on them orally as each topic comes up.
>
> We would also appreciate hearing from Dr. Libby on topic 11, in which he could discuss the implication of our present knowledge of the fallout situation as it exists.

(The letters are as follows:)

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
May 3, 1957.

Mr. LEWIS L. STRAUSS,
Chairman, United States Atomic Energy Commission,
Washington, D. C.

DEAR MR. STRAUSS: The Joint Committee on Atomic Energy plans to hold open hearings in Washington, D. C., on the subject "The Nature of Radioactive Fallout and Its Effects on Man," May 27–29 and June 3–7. This letter is to confirm arrangements made informally for representatives of the Commission and AEC laboratories to testify before the committee. It is our understanding that these arrangements have already been discussed at some length on an informal basis by our respective staffs.

We are attaching material covering the scope, approach, plan and outline of the hearings. You will note the planned division of the hearings into two parts: (I) an organized sequential presentation by expert witnesses, and (II) an open presentation by those working in the field or by interested members of the public who have asked for an opportunity to testify before the committee. You will also note that the outline gives guidance, by each topic I–XII, as to whether the presentation is planned as oral, written inserts for the record, or bibliography—or some combination of those. It is intended that witnesses use their own discretion as to the details of the presentation, and it is not necessary that the outline be rigidly followed. Points that the committee is particularly interested in may be developed by questioning of witnesses.

We are requesting the following representatives of the AEC to testify as expert witnesses for various parts of the organized sequential presentation:

Dr. Charles L. Dunham, Director, Division of Biology and Medicine, Topics I, XI, and XII.

Dr. Willard F. Libby, Topics IV, VI–C–4, IX (Sr–90), and XI.

Gen. A. D. Starbird, Director, Division of Military Application, topic V (General Starbird to be preceded by Dr. Bradbury, of LASL, and Dr. Shelton, of AFSWP).

Dr. Gordon M. Dunning, Division of Biology and Medicine, topic VII.

Dr. Merril Eisenbud, New York Operations Office, for topic VI–C–3 and VI–C–4 and topic III.

Dr. R. F. Reitemier, Division of Biology and Medicine, for a joint presentation of topic VIII, parts B, C, D, E, and G, with Dr. Lyle T. Alexander, of the Agriculture Department.

Dr. Forrest Western, Division of Biology and Medicine, for topic VIII, parts A and H, and perhaps to provide continuity for the other topics in

topic VIII. (The portions of VIII dealing with oceanography and marine life are planned for coverage by Dr. Roger Revelle, Scripps Institute.)

In addition, there is one topic, topic III, for which we would be glad to insert into the record any written statements the AEC would care to submit.

From the AEC laboratories, we are planning to invite the following persons to present oral or written testimony:

Dr. Mark Mills, UCRL, Livermore.
Dr. Wright Langham, LASL.
Dr. E. C. Anderson, LASL.
Dr. ——— Marinelli, ANL.
Dr. Austin Brues, ANL.
Dr. E. P. Cronkite, BNL.
Dr. Norris Bradbury, LASL.

These persons are being contacted directly.

It is possible that other persons in AEC or its laboratories will be needed or that changes in the planned presentation will develop. However, it is not expected that there will be major changes.

The Joint Committee would appreciate receiving as soon as possible short biographies of each person giving testimony, covering professional background, present work, home address and phone number, business address and phone number.

The Committee hopes that the forthcoming hearings will lead to a better understanding of a problem that has become the subject of serious concern to the Congress and the people of this country. Such understanding is essential, in the Committee's view, to the development of sound national policies and to the maintenance of good relations with our friends and allies throughout the world.

The cooperation of the AEC in contributing to the success of the hearings will be greatly appreciated.

Sincerely yours,

CHET HOLIFIELD,
Chairman, Special Subcommittee on Radiation.

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
May 21, 1957.

Hon. LEWIS L. STRAUSS,
Chairman, Atomic Energy Commission,
Washington, D. C.

DEAR MR. STRAUSS: We are pleased to receive your May 17 letter offering to cooperate with us as we begin our radiation fallout hearings next Monday morning.

As you know from my May 3 letter, we plan that the first portion of the hearings, taking perhaps the days up to about Thursday, June 6, would be devoted to an organized sequential presentation by expert witnesses of the scientific subject matter related to fallout. This presentation would wind up with two topics concerned with the impact of the present state of affairs scientifically on national policy and on the research programs related to fallout.

The purpose of the presentation is primarily to educate the committee, the Congress, and the public on the scientific aspects of this important subject. The outline of this organized presentation, sent to you with my May 3 letter, was developed after consulting with various scientists to figure out the best way to present the subject matter fairly and impartially.

In line with the above arrangement, scientific witnesses were selected to present the subject matters. The list of witnesses and order of presentation was distributed last week. Dr. Dunham was selected to lead off with an objective presentation of the nature of the overall topic of the hearings: radiation and radioactivity, particularly fallout. He was chosen as a qualified represenative of AEC who happens not to have become publicly involved in controversy on the fallout question. It has been our aim to begin and carry on the hearings in a noncontroversial spirit.

My May 3 letter to you listed Dr. Libby as a desired expert witness for several topics, beginning with topic IV (natural background radioactivity) of the outline. We would be happy to have him come before the committee to present objective testimony on these topics. It has been our informal understanding

that Dr. Libby might prepare written statements for these topics but did not wish to appear personally as a witness except once, for a comprehensive statement. Accordingly, our latest timetable lists Dr. Libby only for topic XI (impact on policy), where he can cover any of the scientific facts and implications that he cares to.

Your cooperation and that of the AEC staff in assisting the committee with preparations for the fallout hearings is greatly appreciated.

Sincerely yours,

CHET HOLIFIELD,
Chairman, Special Subcommittee on Radiation.

Representative HOLLIFIELD. Dr. Libby, we are happy to have you before us, and we are ready for your statement.

STATEMENT OF DR. WILLARD F. LIBBY,[1] COMMISSIONER, ACCOMPANIED BY DR. CHARLES L. DUNHAM, DIRECTOR, DIVISION OF BIOLOGY AND MEDICINE, ATOMIC ENERGY COMMISSION

Dr. LIBBY. Mr. Chairman and members of the committee; it is a privilege to be present today because it gives me an opportunity to discuss with you this very important subject—one in which I have taken great personal interest, having spent a major part of my time in the last 4 years directly on this research—watching the progress, performing experiments, and making calculations. Excluding weapons, I consider Project Sunshine—the study of worldwide radioactive fallout and its effect on man—to be one of the most important projects the Commission has. The project is essential, if the Commission is to fulfill its responsibilities in protecting public health and safety. It is being conducted as a scientific study whose primary purpose is to discover the scientific truth and present the facts—publicly.

Senator ANDERSON. Before you go further, is this your own statement, or does this represent the views of the Atomic Energy Commission?

Dr. LIBBY. This is my own statement, Senator Anderson.

Senator ANDERSON. It has not been cleared?

Dr. LIBBY. I have asked for comment, but it is not an official statement of the Commission; it is my own statement.

Senator ANDERSON. Thank you. I thought that was important.

Dr. LIBBY. Yes.

Representative COLE. On that point, Mr. Chairman; you say you have asked for comments. I assume you meant you submitted your statement to other members of the Commission for their comments?

Dr. LIBBY. Yes.

Representative COLE. And have you received comments from any of the Commission?

Dr. LIBBY. There have been no substantial changes. I am not sure all of the Commissioners commented. I left it to my staff to collect the comments, and I am not just sure.

Representative COLE. Did you submit your statement to all of the Commissioners?

Dr. LIBBY. I believe so, sir; yes.

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[1] Date and place of birth: December 17, 1908, Grand Valley, Colo. Education: Bachelor of science, University of California, 1931; doctor of philosophy (chemistry), University of California, 1933; Work history: Instructor of chemistry, University of California, 1933–38; assistant professor, 1939–43; associate professor, 1943–45; professor instructor nuclear studies, Chicago, 1945–54; member, General Advisory Commission, AEC, 1950–54; member AEC, 1954—. (Submitted by the Atomic Energy Commission.)

Representative COLE. Were there any of the Commissioners who did not respond or indicate a comment?

Dr. LIBBY. I will have to check with my staff on that.

Representative COLE. So far as you know?

Dr. LIBBY. I do not know of any.

Representative COLE. One more question at that point: How long previous to this morning did you submit the statement?

Dr. LIBBY. It was about a week ago, as I recall, Mr. Cole.

Representative COLE. Thank you.

Dr. LIBBY. This is my statement. It is not the Commission's statement, but it is my statement. But I know of no——

Representative VAN ZANDT. During the course of these hearings, this Project Sunshine has come up from time to time.

Dr. LIBBY. Yes.

Representative VAN ZANDT. Dr. Libby, just how did you arrive at naming this Project Sunshine.

Dr. LIBBY. Well, it happened in the summer of 1953 at the RAND Corp. conference at Santa Monica. I have been trying to think for the last several hours just how it happened. I do not remember, and I do not know, Mr. Van Zandt.

We recognized the need for some name, and one of the boys in the meeting invented this name, and we took it. I am sorry I have no better memory.

Senator HICKENLOOPER. Mr. Chairman, may I ask Dr. Libby—as I understand it, sunshine is stimulated by radiation, is it not?

Dr. LIBBY. Yes, sir; in the ultimate, sunshine is derived from radiation.

Senator HICKENLOOPER. And sunshine, as we know it, and as life exists, is completely vital to life?

Dr. LIBBY. Yes, sir.

Senator HICKENLOOPER. And the effect of the sun and radiation is vital to life. I am not trying to say how the term came up, but it seems to me there is quite a close correlation between sunshine and the effects on human life of radiation.

Dr. LIBBY. I am trying to find out how this name was invented. I am sorry I have not been more successful. I have given you the chronology of it.

Representative HOLIFIELD. There is this exception, however, that sunshine is beneficial to the growth of life, and radiation seems to be the other way. Is that not right?

Dr. LIBBY. At least radiation has many deleterious effects, Mr. Holifield. I think it has a few good ones.

Representative HOLIFIELD. As a Californian, I would like to say, the RAND Corp. being out of California, where we have a lot of sunshine, I would say, coming from California, this helps offset the reputation of smog we have out there.

Senator HICKENLOOPER. As long as we are in that field, Mr. Chairman, I might say, inasmuch as sunshine brings, and has over the millenniums, brought substantial amounts of radiation, perhaps it is that the more sunshine, the more danger there is to the skeletal structure of the human body. That might be argued in favor of those areas which are less blessed with sunshine. I do not know.

Representative COLE. Mr. Chairman, on this point of the name Project Sunshine, I fear a feeling may have developed that that name

was deliberately selected to mislead the public with respect to the importance of the subject under discussion.

Since you were connected to a rather direct degree with this project from its inception, can you assert unequivocally that the selection of the name "Sunshine" has no purpose or intent of misleading or minimizing the importance of the study?

Dr. LIBBY. Yes, Mr. Cole, I certainly can. It never had any purpose to mislead or be flippant about the whole matter. The name was selected—and I am afraid perhaps we did not pay too much attention to the name in selecting it. But there was never any intent to mislead or to minimize the importance of the hazards.

Senator ANDERSON. I thought you testified you did not know how it was selected. If you did not know how it was selected, how could you know the circumstances under which it was selected?

Dr. LIBBY. What I testified to, Senator Anderson, was I did not understand how the word "sunshine" rather than any other word was taken.

Senator ANDERSON. Then how could you answer Mr. Cole's question in the affirmative?

Dr. LIBBY. Well, we certainly did not select this word with any intent of misleading anyone about the seriousness of this subject. That is all.

Representative COLE. The witness may very properly testify he does not know how this was done, but he also may testify he does know why it was not done.

Senator ANDERSON. If you can prove a negative, you may go to it.

Dr. LIBBY. Fallout has been a subject of interest ever since the first atomic bomb was exploded. At the Alamagordo test, July 16, 1945, there were scientists present who were interested in fallout and studies were subsequently made of the sparse vegetation of that countryside.

The organization which is today known as the Health and Safety Laboratory came into being ith the establishment of the Commission on January 1, 1947. Scientists from the Health and Safety Laboratory made their first collection of fallout material on February 1, 1951, when radioactive snow was reported in Rochester, N. Y. At this time, assays of mixed fission products were made on snow and water from samples collected from throughout the Northeast. The source of this radioactivity as undoubtedly our very first Nevada tests conducted in the early spring of 1951—Operation Ranger. The first actual fallout collection network was established a few weeks later for Operation Greenhouse, an Eniwetok test series, and the collections were analyzed for mixed fission products; the network operated from April to June 1951, making collections in eight stations in the United States. The following year, 1952, the Weather Bureau and the AEC collected radioactive fallout at 120 Weather Bureau stations in connection with Operation Tumbler-Snapper.

Representative COLE. At that point, Mr. Chairman.

Dr. Libby, do you intend to indicate the present system of collection stations?

Dr. LIBBY. I would be very pleased to. It is not in my statement, though.

Representative COLE. Your statement says that in 1952 you did have these 120 stations. That was 5 years ago.

Dr. LIBBY. Right.

Representative COLE. I think the committee and the public would be interested to know what the present is.

Dr. LIBBY. I believe Mr. Eisenbud, who is in charge of this, did describe this system to you in earlier testimony, Mr. Cole. Did he not, Mr. Chairman?

I would be pleased to recount it.

He has a system now which is essentially worldwide, 2 sorts, I believe 3 sorts, really. He collects the gummed papers, which he described to you. He puts out buckets, or washtubs at various places to get a total collection. Then, in addition, we have a system of collecting soil samples on a worldwide basis. So we have a much more thorough coverage, not only of the United States, but of the whole of the Western World, at least, and through the U. N. Radiation Committee, I can say of the whole world to a certain degree than existed in this early time of 1952.

Representative COLE. Instead of 120 collecting stations located in the continental United States, can you indicate the approximate number of collecting stations throughout the world?

Dr. LIBBY. I would be hard put to give you even an approximate number. I am sure it is larger, but I can ask for that.

Representative COLE. Dr. Eisenbud is in the audience, and since this is a matter of general interest, let us have it in the record.

Dr. LIBBY. I am sorry. It comprises now only 94 stations in the United States. These are the gummed paper stations. We make a distinction between the gummed paper and the washtubs and the soil.

Ninety-four stations of gummed paper in the United States, with 75 now in foreign countries.

What I call the washtubs are located at seven stations in the United States, and in the following countries: Hawaii, Chile, French West Africa. Austria, Union of South Africa, Thailand, South Rhodesia, Pakistan, Kenya, Japan, Colombia, and Brazil. And this network is being extended.

Senator ANDERSON. Since we are considering the Hawaii statehood bill this morning, I think the Hawaiians would object to being put outside of the United States.

Dr. LIBBY. Well, we have had this network of gummed paper operating continuously since 1951 and 1952. The washtub or stainless steel pot system is just now beginning to operate. It was set out last fall, actually. And the soil collection data—well, I will describe those in my statement.

Representative VAN ZANDT. Dr. Libby, does this sampling go on around the clock, or just at a given time?

Dr. LIBBY. No, it goes around the clock, Mr. Van Zandt.

Representative VAN ZANDT. Around the clock?

Dr. LIBBY. Yes.

Representative VAN ZANDT. In other words, your gummed paper is in the open atmosphere around the clock, and picking up samples continuously?

Dr. LIBBY. Yes. The washtubs are, too. That is, you do not put the washtub out just when it starts to rain. You leave it out continuously. And after a rain you bring it in and swish it around to suspend the particulate matter and bring the rain and the sediment in for analysis. But you put the washtub right back out so that you get the total fallout in the area.

We have established rather definitely that the washtubs give good numbers that check with the total soil content of fallout. This has been done in 2 or 3 places quite carefully, and checks resulted.

Representative VAN ZANDT. Thank you.

Dr. LIBBY. Meanwhile, at the Oak Ridge National Laboratory, Dr. Nicholas M. Smith, starting in 1949, had made a theoretical analysis of the long-range aspects of fallout. Smith concluded that over a period of years strontium 90 would be the most hazardous component in fallout and that an accurate knowledge of the distribution of this substance over the world would be essential to any scientific estimate of the potential long-range health hazard due to fallout. In 1952, RAND Corp. was given a contract to make an independent study of fallout and this study culminated in the summer of 1953 in a conference of selected consultants who made an intensive overall review and evaluation of the fallout problem. The conference recommended that the study of mixed fission products, then current, be supplemented by a worldwide assay of the individual fission product, strontium 90, produced by nuclear detonations, and so Project Sunshine was born. Most of the information presented to you so far in these hearings, if we exclude genetics and the toxic effects of ionizing radiation, was developed in the Sunshine project.

Samples for assay have included soil, alfalfa, animals, dairy products, human bone, rain, well and spring water, snow, and many other similar materials from various parts of the United States and from countries all over the world. Some of the scientists were exceptionally eager to get started on the program and the first experiments had been performed before the 1953 conference adjourned. As a result, Sunshine was born with essentially three working laboratories—a group working at the Health and Safety Laboratory in New York City under the able direction of Merril Eisenbud and later Dr. John Harley; a group at Columbia University under the competent direction of Dr. Laurence Kulp of the Lamont Geological Observatory; and a group at the University of Chicago, which was initially under my direction and later under Dr. Edward Martell. A little later, Dr. Lyle Alexander, of the Department of Agriculture, became interested in Sunshine and has personally collected many of the extremely important soil samples which have been analyzed in the program. Subsequently, two commercial laboratories, Nuclear Science & Engineering Corp., in Pittsburgh, and Isotopes, Inc., in New York, were asked to assist in carrying the burden of isotopic analysis. In the last year or two, several other countries, notably the United Kingdom, have taken up the study, and the United Nations has organized, at the suggestion of the United States, the Scientific Committee on the Effects of Atomic Radiation with which all countries collaborate freely. Dr. Shields Warren, outstanding scientist and medical doctor, and the first director of our Division of Biology and Medicine, heads the United States delegation. This international committee has been very beneficial. The information furnished by the cooperating countries has been a substantial and significant addition to ours.

Originally, Project Sunshine was focused on the study of strontium 90 fallout and its effects, but with the passage of time, Sunshine has come to mean all of the Commission's activities directly connected with offsite fallout so that any summary of Sunshine activities must include the important work of Dr. Austin Brues and his collaborators

at the Argonne National Laboratory on the toxicity of strontium 90; of Dr. Cyril Comar at Oak Ridge on the metabolism of radiostrontium in humans and animals; Dr. William Neuman at the University of Rochester on the depoition of strontium 90 in bone; Dr. Kermit Larson, of the University of California at Los Angeles, on fallout materials in plants, and many others. At the present time, there are upwards of 50 contracts with outside laboratories bearing upon Sunshine and, in addition, work at the Commission laboratories.

Representative HOLIFIELD. Dr. Libby, this is a very impressive statement of the different projects which are engaged in in studying the effects of radiation. We asked Dr. Dunham for specific information in regard to numbers of people employed either directly or through contract. If that information is available, I believe this would be a good place to put it in.

Dr. LIBBY. All right, we will see that it is done, Mr. Chairman.

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. Dr. Libby, will you put in the record the 50 contracts, the names of the companies, as well as the amount of money involved, and the number of personnel employed?

Dr. LIBBY. We would be very pleased to do that.

Representative VAN ZANDT. And the nature of their studies?

Dr. LIBBY. Yes, sir.

Senator ANDERSON. Since there has been an interruption, maybe I can go back a bit.

You mentioned the work of the United Nations in organizating the Scientific Committee on the Effects of Atomic Radiation, and Dr. Shields Warren, whom we all recognize as a very fine person, heading the United States delegation. Is there a geneticist on the United States delegation?

Dr. LIBBY. The composition—I will have to ask Dr. Dunham to answer that.

Senator ANDERSON. I think we had some testimony from geneticists there was none.

Dr. DUNHAM. The official representative of the United States is Dr. Shields Warren. He has two alternates, Dr. Austin Brues, and Mr. Merril Eisenbud.

At the last meeting of the United Nations Scientific Committee, which was held in Geneva in April, he took with him as consultants, as did the representatives of the other member countries of this committee, two outstanding geneticists from this country—Dr. George B. Beadle of California Institute of Technology, and Dr. Sterling Emerson of the Division of Biology and Medicine.

Does that answer the question?

Senator ANDERSON. The answer to the question, then, would be, "No, there is no geneticist on the United States delegation." Is that not correct?

Dr. DUNHAM. That is absolutely correct.

Senator ANDERSON. It is a simple answer.

Dr. DUNHAM. There can be only one official delegate.

Representative COLE. How large is the official delegation?

Senator ANDERSON. And two people who accompanied him?

Dr. DUNHAM. Yes.

Senator ANDERSON. Who are these others that go along if they are not delegates?

Dr. DUNHAM. They are so-called alternates who may go along, and particularly act for the—officially act for the representative in his absence.

Representative COLE. Are either of the alternates specialists in any one field of biology?

Dr. DUNHAM. Dr. Austin Brues is a specialist in radiation pathology. He is an outstanding authority on the subject. Your committee asked him to testify as he did the other day.

Mr. Merril Eisenbud is not a biologist. He, however, is, as Dr. Libby has just indicated, the one who has been a prime figure in the development and accumulation of the information we have in this country on fallout. Therefore, it was felt appropriate that he be an alternate, because he lived this problem day in and day out.

Representative HOLIFIELD. Dr. Dunham, could you furnish now, early, for the benefit of the press and the audience, the numbers which we asked you for the other day? I think it would fit in at this point in the record.

There is a great deal of propaganda going around throughout the Nation that we are not concerned about this matter governmentwise, and I think this would help to show that there is concern on the part of the Congress and the Atomic Energy Commission, and that there is considerable effort both in terms of personnel and in money being devoted to a study of the problem.

Dr. DUNHAM. I have here a document I believe the committee already has copies of (p. 1393). I was planning to give it to you for tomorrow. I will read you the first page.

Scientfic man-years for sampling and analysis of radioactive fallout, including fission products, toxicity and transport, 253.

The effects of radiation on humans, mammals, and other organisms, exclusive of genetic studies, 449.

Treatment and methods of ameliorating radiation effects, 60.

Genetic effects of radiation, studies on human genetics, 12.

Experimental studies on the genetic effects of radiation on species other than man, 71.

Biochemical and microbiologic studies of radiation effects, 110.

Environmental studies, 4.

Dosimetry research, development of improved methods of measuring fallout, 47.

A total of 1,006 scientific man-years.

Representative HOLIFIELD. Thank you very much.

Do you have a complete presentation for tomorrow?

Dr. DUNHAM. It is already in the hands of the committee, I believe.

Representative COLE. You confused me by that last expression, "scientific man-years." Get it down to the number of people the Commission has engaged, either direct by contact, in the study of biological aspects of radiation hazards.

Dr. DUNHAM. The figure, if we gave you numbers of people, would be much larger, but it would also be misleading, because many people do not spend full time on this work.

Representative COLE. Give me the total number, and allow me to decide how far I have been misled.

Dr. DUNHAM. I do not have that number, but I will get it for you, Mr. Cole. I will be happy to get it.

Representative COLE. Would it be in the order of three or four thousand?

Dr. DUNHAM. It would be in that range, yes; I am sure.

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. Dr. Dunham, I asked a question the other day, but I think it should be repeated at this point in the record.

Are there any independent studies being made of the radiation hazards, that is, independent of Government, independent of the Atomic Energy Commission?

Dr. DUNHAM. If you are talking about the actual research work and the collection of data, I believe the Atomic Energy Commission is responsible and supporting by far the large majority of that work which is done in this country. If it is a matter of evaluation of the data, as you know, the National Academy of Sciences has a continuing group of committees reviewing this problem. They have been meeting ever since their report a year ago, and will continue to do so, and review the subject.

Representative VAN ZANDT. Dr. Dunham, I imagine that the AEC has access to the studies of the independent groups?

Dr. DUNHAM. That is right. We are kept in touch with each other.

Dr. LIBBY. But these groups are entirely independent, Mr. Van Zandt, in their functioning.

But on the point of geneticists, Senator Anderson, I assure you we have no intention of slighting in any way the subject of genetics. We consider it to be very important.

Senator ANDERSON. I assume you have heard about the testimony of Dr. Glass.

Dr. LIBBY. I have not read it; I am sorry, but I read the morning papers.

Senator ANDERSON. I am sure your agency does not differ from any other. You have them reporting to you what is going on.

Here is a group that met 22 times in the last 3 years while Dr. Glass has been a member of it. He had been hopeful, I guess, sometime that the Atomic Energy Commission would ask him what they thought about the project, but nobody thus far has.

Dr. LIBBY. You see the committee reports to Dr. Dunham, and I would like him to comment upon that.

Dr. DUNHAM. The committee reports through me to the Commission.

Representative COLE. What committee are you talking about?

Dr. DUNHAM. I assume Senator Anderson is talking about the Advisory Committee for Biology and Medicine.

Senator ANDERSON. Exactly, which is the one Dr. Glass testified about.

Dr. DUNHAM. That is correct. I think the record will show that on a number of occasions Commissioner Libby has specifically requested that this subject be on the agenda. It has been discussed at practically every one of the meetings in the last 2 years.

Senator ANDERSON. Were any members of the committee there?

Dr. DUNHAM. Members of——

Senator ANDERSON. Any members of the Advisory Committee there, or in absentia?

Dr. DUNHAM. This is the Advisory Committee meetings, I am talking about. At their meetings.

Senator ANDERSON. You were not trying to imply it had been brought up by Dr. Libby at the meetings of the Atomic Energy Commission?

Dr. DUNHAM. Oh, no. That is something else.

Senator ANDERSON. You allowed the Advisory Committee to talk about what it is supposed to advise about?

Dr. DUNHAM. That is right.

Senator ANDERSON. But do not let it get to the Atomic Energy Commission.

Dr. DUNHAM. I do not——

Senator ANDERSON. You say that Dr. Libby has asked this be put on the agenda. The agenda of what?

Dr. DUNHAM. The Advistory Committee.

Senator ANDERSON. The very purpose for which it is created.

Dr. DUNHAM. That is correct.

Senator ANDERSON. He asks that the subject for which it is created be put on its own agenda?

Dr. DUNHAM. Oh, no. He asks that they specifically consider the status of project Sunshine. He has done that on several occasions. So has Commissioner Murray.

Senator ANDERSON. How many times has the report of the Advisory Committee been an item on the agenda of the Atomic Energy Commission that you recall?

Dr. LIBBY. The Committee makes a report to the Commission every meeting, does it not?

Dr. DUNHAM. Yes. After each meeting they write a letter summarizing their deliberations and any recommendations they might have.

Senator ANDERSON. I guess you misunderstood my question. My question was: How many times has a report of the Advisory Committee been on the agenda of the Atomic Energy Commission?

Dr. LIBBY. I do not believe that the reports, as such, ever are on the agenda, Senator Anderson, but that does not mean we do not give them the most serious consideration.

For example, the General Advisory Committee's report is never, as such, on the agenda. Items which they report on and recommendations which they make will be on the agenda, but the Commission is reported to through the mechanism of a letter to the Chairman of the Commission, which is circulated to all members of the Commission. Just a technicality. It does not go on the agenda. We do pay attention to it. We certainly are most interested in supporting genetic research, and furthering its investigations in every way.

Representative COLE. Dr. Libby, a point of clarification. This Advisory Committee covers all aspects of the biological consequences of radiation, does it not?

Dr. LIBBY. All aspects of biology and medicine.

Representative COLE. Of which genetics is one element?

Dr. LIBBY. Yes.

Representative COLE. And I understand from Dr. Dunham's statement that you, as a Commissioner sitting in with this Advisory Com-

mittee, have from time to time specifically asked the Advisory Committee to give consideration to the genetic effects of radiation?

Dr. LIBBY. I cannot recall——

Senator ANDERSON. He did so testify.

Dr. LIBBY. I cannot recall specifically emphasizing the genetic rather than the somatic hazards, but we have repeatedly made it clear to the Committee that any words of advice or conclusions that they have on any aspect of the effects of radiation, whatever its origin be, be it weapons tests or accidents in peaceful uses—or whatever its origin—we certainly want the information.

Representative VAN ZANDT. Dr. Libby, let me ask you this question: In your opinion, as a commissioner, do you think that the subject of genetics has been adequately covered?

Dr. LIBBY. I am not a geneticist, and I know so little about the subject of genetics, Mr. Van Zandt, that I hate to answer that question. It seems to me that I do not know the answer. I know we do want to encourage good, capable research men to work in this field in every way we can, and I am sure Dr. Dunham can testify more definitely to your question. I would like to ask him to.

Dr. DUNHAM. I think I can best answer it, Mr. Van Zandt, by calling your attention to the fact that the Advisory Committee for Biology and Medicine as always had a geneticist on it. This indicates the preoccupation of the Commission with the genetics problem. The first Committee included Dr. George Beadle, of Cal-Tech. Following his 5-year stint, Dr. Kurt Sturn, of the University of California, took over, and currently it is Dr. Bentley Glass. Furthermore, since 1950, the Division of Biology and Medicine has always had a full-time geneticist of considerable statute on its staff, so as to be certain that this area would not be neglected.

Representative COLE. How large is the Advisory Committee?

Dr. DUNHAM. Seven people, I believe.

Representative COLE. And 1 out of the 7 has always been a geneticist?

Dr. DUNHAM. One has always been a geneticist.

Chairman DURHAM. That is just the Advisory Committee on Biology and Medicine?

Dr. DUNHAM. That is correct.

Chairman DURHAM. That does not include the Advisory Committee on the whole thing?

Dr. LIBBY. The law sets up the General Advisory Committee, and, because of the existence of the Advisory Committee on Biology and Medicine, there has been a tendency, I think, to not appoint biologists to the General Advisory Committee. So we have never had—I do not recall there ever has been a biologist or medical doctor on the General Advisory Committee. But it is for the reason that we have this special Committee for the area of biology and medicine.

Senator HICKENLOOPER. Mr. Chairman, may I verify this with Dr. Libby? As I understood the testimony of yesterday, first, the General Advisory Committee is an arm of the Commission; that is, it reports directly to the Commission?

Dr. LIBBY. Yes.

Senator HICKENLOOPER. And the Advisory Committee on Biology and Medicine is also an arm of the Commission and reports directly to the Commission. Is that right?

Dr. LIBBY. Well, it reports through the Director of Biology and Medicine, though I think the Committee certainly knows we are very glad to have them talk directly to the Commission at any time, and I try to attend the meetings when I can. I think your statement is correct, Senator Hickenlooper; yes. But there is a statutory connection which exists for the General Advisory Committee, which does not exist for the other Committee. You will not find the Advisory Committee on Biology and Medicine specifically mentioned in the Atomic Energy Act of 1954, I believe.

Dr. DUNHAM. May I amplify this?

Representative HOLIFIELD. Let's get to the main part of the testimony, now, and then we can have——

Senator HICKENLOOPER. If there is some amplification at this point, Mr. Chairman, I would like to hear from Dr. Dunham.

Representative HOLIFIELD. Go ahead, Doctor.

Dr. DUNHAM. I merely wanted to state that, when I said earlier the Committee reports through the Director of the Division, this is not actually in fact so. They report directly by letter, the letter that goes between the Chairman of the Committee and the Chairman of the Atomic Energy Commission.

Representative HOLIFIELD. And how does that differ from the General Advisory Committee? Do they report through the General Manager, or do they report by letter, or do they report directly to the Commission?

Dr. LIBBY. The Chairman of the General Advisory Committee reports by letter to the Chairman of the Commission.

Representative HOLIFIELD. Proceed.

Dr. LIBBY. At first it was believed that the work of Project Sunshine was so intimately concerned with weapons that it was necessarily secret, but it presently became apparent that more and more aspects were declassifiable; consequently, a year or so ago, virtually every aspect of the Sunshine project not already unclassified was declassified. It is also true, of course, that many of the problems which we think of today as part of Project Sunshine were not so considered in the beginning and had been in the unclassified areas since their inception. The information obtained has been issued publicly. With one exception, I do not know of any significant information on fallout in the possession of the Commission which is not available to the public.

Representative HOLIFIELD. Is that exception in the classified area?

Dr. LIBBY. Yes. That is in the next sentence. The exception is certain facts which would reveal information concerning intelligence and weapon design and, therefore, cannot be made public; however, your committee has access to these facts in executive session. The portion now classified is only a small segment of the large body of knowledge about worldwide fallout and, although important, is not vital in the understanding of fallout, its effects and its hazards.

There is another point I would like to make at this time about the study of worldwide fallout and that is that it is a scientific study. Thus it has as its objective just one thing, truth, the scientific truth. We have a consistent policy of encouraging all competent scientists, both here and abroad to contribute to the study. Many of these contributions can be comparatively indirect and can be carried out in most research laboratories as an adjunct to the regular work. In this con-

nection, I have frequently suggested particularly to those critics of the Commission's weapons testing program who are competent scientists that any contributions they might make in the way of scientific theories, data, or experiments would be most welcome.

Representative COLE. Mr. Chairman?

Representative HOLIFIELD. Mr. Cole.

Representative COLE. Would the chairman allow me to interrupt Dr. Libby again with reference to the studies of worldwide aspect of fallout?

Reverting to your system of sampling which you have indicated is substantially worldwide, as widely distributed as possible, do those studies support the assertions which have been made by some that the distribution of fallout is not widespread, is not general throughout the world, but rather is concentrated in the Northern Hemisphere?

Dr. LIBBY. Mr. Cole, the interpretation—I may say this: The only data that exists are the data Project Sunshine have collected and published. There are not any other data, except those few that have been collected in the last year or so by the other countries. So the majority of them are ours and whatever difference in interpretation there may be, or difference in statement, must be based on the same data.

Senator ANDERSON. I hope we talk about that, but I was going to wait for that portion of your statement, where you talked about the uniformity of stratospheric fallout.

Dr. LIBBY. Yes, sir; I would be very glad to discuss this question.

Representative COLE. All right.

Representative VAN ZANDT. Mr. Chairman?

Representative HOLIFIELD. Mr. Van Zandt.

Representative VAN ZANDT. Dr. Libby, this morning's Washington Post carried an article by Edwin Diamond, in which he refers to a scientific document entitled "Preliminary Data on the Effects of Atomic Bomb Explosions, and the Concentration of Radioactivity in Lower Atmosphere and Soil."

This is supposed to be a Russian document. Does the Commission have this document, or a copy of it in its possession?

Dr. LIBBY. Well, Mr. Van Zandt, I do not recognize it by the title, and I read every article that comes to the U. N. Radiation Committee from whatever source. That does not mean that I have not read it, and that we do not have it. I do not recognize the title.

I would say, from what Mr. Diamond said in the paper, that I have not read it. Maybe it is real new and we just have not seen it yet.

Representative VAN ZANDT. Thank you.

(A clarifying letter from Dr. Machta follows:)

JUNE 9, 1957.

To: For the record.
From: Chief, Special Projects Section, OMR, United States Weather Bureau.
Subject: U. S. S. R. fallout document.

Several questions by members of the Joint Committee on Atomic Energy at the fallout hearings dealt with a U. S. S. R. document entitled "Preliminary Data on the Effects of Atomic Bomb Explosions on the Concentration of Artificial Radioactivity in the Lower Atmosphere and in the Soil," edited by B. M. Isaev and D. L. Simonenko, Moscow, 1956, which was mentioned in an article by Mr. Diamond of INS in many newspapers last week.

Our office first showed this document to the reporter many weeks ago, as a result of inquiries about U. S. S. R. contributions to the International Geophysical Year in nuclear radiations. Mr. Diamond, however, neglected one fact in his

article, which the undersigned and Mr. R. J. List, of the United States Weather Bureau, gave him and repeated at least a half dozen times. This is the fact that at the recent meeting in Geneva of the United Nations Scientific Committee, the U. S. S. R. has withdrawn the data included in the report. In other words, there is no authentic fallout data from the U. S. S. R., insofar as we are aware. This is contrary to the facts of the newspaper article. My personal opinion is that the data in the report of the U. S. S. R. are wrong and withdrawal is justified.

LESTER MACHTA.

Dr. LIBBY. Our policy is to discover the truth about fallout and to make it public.

The Commission has provided its data to the United Nations Scientific Committee on the Effects of Atomic Radiation, which I mentioned a moment ago. Of course, the scientists employed by our contractors, and who are working in this field are encouraged to publish their findings in scientific journals—a practice which insures the availability of the information to all scientists everywhere. Any data or information which is not suitable for publication in scientific journals, but which has sufficient merit to warrant its distribution, can be and is published through the Technical Information Service Extension at Oak Ridge.

If one takes the summer conference at RAND in 1953 as the real beginning of the worldwide fallout study, then we began by considering all known aspects of the problem, and by planning a careful experimental attack on each of them. A continuous review has been going on ever since, conducted principally by the scientists engaged in the program, but vitally assisted by the study, The Biological Effects of Atomic Radiation, made last year by the National Academy of Sciences, and the continuing advice of the Advisory Committee on Biology and Medicine and the General Advisory Committee. Whenever it appears that some facet has been overlooked or is not receiving enough attention, an immediate effort is made to get the additional work going. This method of continuous criticism and evaluation has enabled the program to be productive and effective.

It is fortunate for our national interest—and for that matter the national interest of other countries as well—that this has been so, because were it not for the efforts of the scientists of Project Sunshine—they are relatively very few in number—not much would be known about worldwide fallout. As it is, the broad aspects of worldwide fallout are understood. In this connection, Dr. Dunham has asked that you put in the record 11 papers and technical speeches with detailed data. (See p. 16.)

As regards the physical rather than the biological facts about fallout, many possibilities still exist for minor controversy over detail, but no scientist who takes the trouble to learn what the facts really are will fail to agree with the overall picture. I know of no scientist who has studied the data who does not agree on the general amount of fallout received, the amount of strontium 90 in the body, the rainfall effect, and other features such as the stratospheric reservoir and the storage there for a time of many years. We still are not certain that 10 years is the best figure, but everyone agrees that it is a matter of years and the uniformity of stratospheric fallout is still under study as Dr. Machta has made clear.

Senator ANDERSON. Now that is the point where I would like to ask you a question, because you say the scientists do agree on the overall pictu. ɔ.

Dr. LIBBY. Yes.

Senator ANDERSON. Dr. Machta testified in here, and it seemed to me he testified that the stratospheric fallout was not uniform. I will just quote from him.

> After 2 years debris in the atmosphere from our Castle tests is still not uniformly distributed in the stratosphere. The upper air program of the Atomic Energy Commission can check this thesis in the near future.
>
> Delayed fallout has not been deposited uniformly over the earth. On the average, there is more delayed fallout in the north temperate latitude, even though the main injection was in the tropics, that is, the Marshall Islands.

Incidentally, this deposition of worldwide fallout was confirmed, I believe, by both Dr. Kulp and Mr. Eisenbud, who showed that the Northern Hemisphere had from 2 to 3 times as much strontium 90 as the Southern Hemisphere.

How do these check with your statement made on the 26th of April on uniformity?

Dr. LIBBY. I think they check very well. But let me explain that apparently paradoxical reply.

Senator ANDERSON. To a layman uniformity and nonuniformity are quite different terms, and you have used the term "uniformity" here again, and he keeps using the term "nonuniformity," Doctor. How do you put those into the same church?

Dr. LIBBY. I would like to use a chart for the rainfall in the Chicago area in the year 1955. I thought by selecting this one narrow thing I could explain it better.

Senator ANDERSON. It may be very helpful. But do you believe that strontium 90 is deposited uniformly over the world?

Dr. LIBBY. No, sir; and I never said that, sir. What I said was different—that the stratospheric components of the fallout might very well be uniform, and for the time being until we know better, we would so assume.

So the argument, really, if there be one—and I do believe it to be minor—is whether the stratospheric components of the fallout is uniform, Senator Anderson.

Senator ANDERSON. Do you think that sort of distinction occurred to the average person who is groping for information in this field?

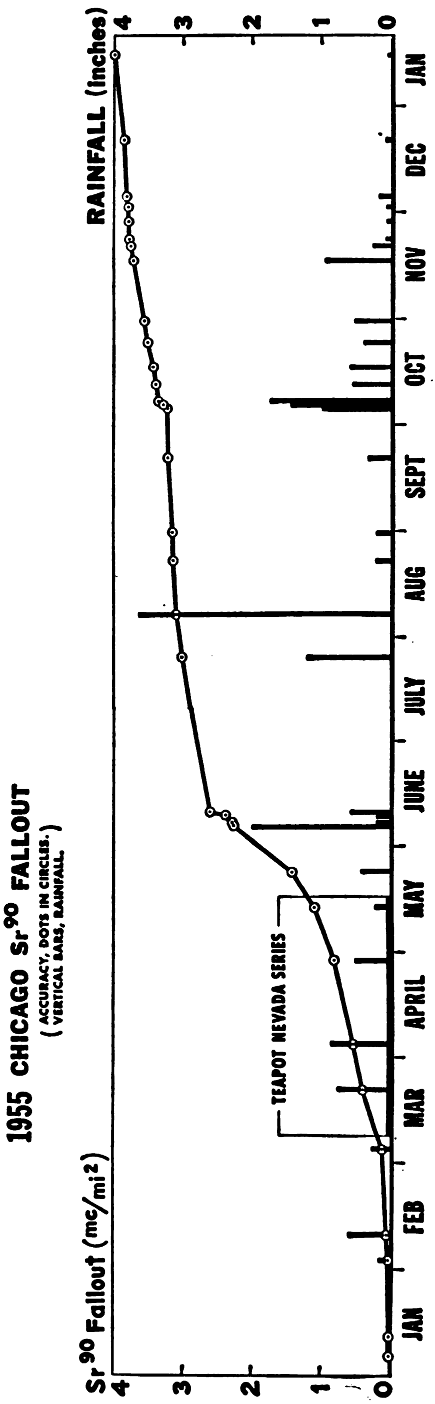
Dr. LIBBY. I am afraid not, and I am sorry to have to agree with you, Senator. I think it is not. We have tried hard to explain the nature of the fallout from what we call the lower atmosphere, the troposphere. This stuff has no time to mix worldwide and comes down in the same general latitude as the bomb was fired.

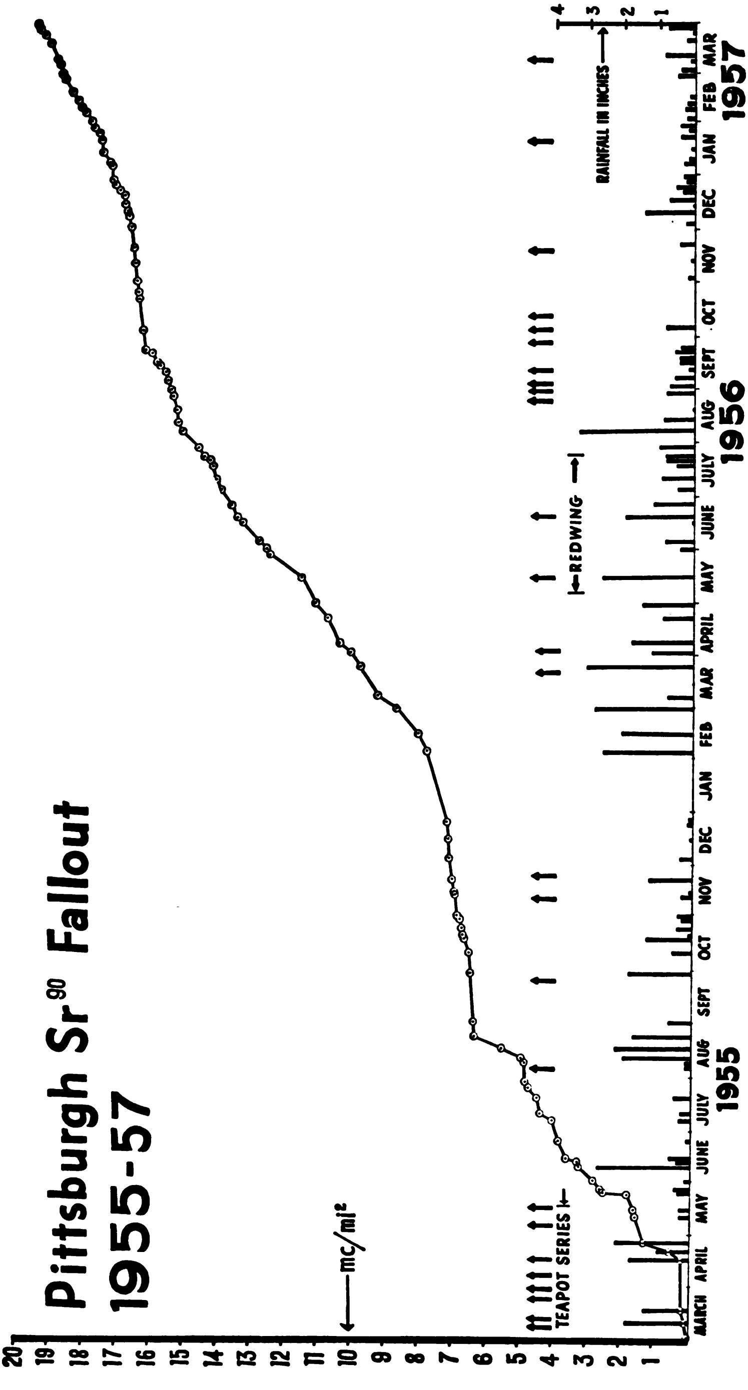
The stuff that goes upstairs into the stratosphere stays so long that it has time to mix pretty much all over the world.

Now, the real point of discussion here, Senator, is this stratospheric material, which comes from large-yield megaton-class bombs—though it could come from small bombs if they were fired in the stratosphere—but the material that gets upstairs into the stratosphere and stays there for a matter of years. And Dr. Machta agrees with this.

Now the question is: How thoroughly does this mix before it comes down in its slow fashion?

This figure I have over here perhaps will help me explain it to you.


1955 CHICAGO Sr90 FALLOUT
(ACCURACY, DOTS IN CIRCLES.)
(VERTICAL BARS, RAINFALL.)
Sr90 Fallout (mc/mi2)
RAINFALL (Inches)
TEAPOT NEVADA SERIES
JAN
FEB
MAR
APRIL
MAY
JUNE
JULY
AUG
SEPT
OCT
NOV
DEC
JAN
0
1
2
3
4


Pittsburgh Sr90 Fallout
1955-57
mc/mi²
TEAPOT SERIES
REDWING
RAINFALL IN INCHES
MARCH APRIL MAY JUNE JULY AUG SEPT OCT NOV DEC JAN FEB MAR APRIL MAY JUNE JULY AUG SEPT OCT NOV DEC JAN FEB MAR
1955
1956
1957
20 19 18 17 16 15 14 13 12 11 10 9 8 7 6 5 4 3 2 1
4 3 2 1

These are washtub data from the city of Chicago for the year 1955, where a washtub was placed on the roof of one of the buildings of the University of Chicago, and left out there continuously. It was only when it rained that we would empty the water and the solids that had accumulated and take those into the laboratory, and analyze them. The size of the dots indicates the error of the analysis.

I think you will notice at a glance that there is one big cliff in that curve of the strontium 90 fallout against time, which begins in January 1955, and extends throughout that year into January 1956.

There is a big cliff, and it is obviously correlated with the time at which we started firing small bombs in the Nevada test site, the so-called Teapot series.

So I say that of all of the fallout which occurred in the year 1955 in this particular area, on this particular roof, on the University of Chicago campus, most of it came from the Nevada series.

Now this is a difference of point of view which is not serious, in the sense that further information will straighten it out, and the difference for the prediction for the future is not measurable.

Dr. Machta and we agree about the occurrence of the stratospheric storage. We are not in disagreement. We have a range of opinion about the length of time it stays there.

The point about this band of stuff which lies in our latitude is that I say it is due to material that was deposited quickly, and I think Dr. Machta, to a certain extent, implies that part of that came from the stratospheric fallout which had not mixed.

Well, we do not know the answer to this.

I know that we have no place in the world where any rainfall at all occurs where we do not find fallout. I have measured snows from the South Pole, and every bit taken from the surface has detectable fallout.

We have had testimony from Dr. Kulp that bones from whatever part of the world, whatever part of the Southern Hemisphere, showed detectable strontium 90. The only places in the world where you do not find it are places where it has never rained.

Senator ANDERSON. I think we had one witness—and I will not try to recall his name—who testified that in the Latin American countries, or the Southern Hemisphere, it was half what it was in the Northern Hemisphere.

Dr. LIBBY. That is true.

Senator ANDERSON. Would that prove or disprove this theory of uniformity?

Dr. LIBBY. I think, Senator, that you and I understand; we must try to make this clear to everyone now.

You see that cliff there is not from the stratosphere, in my opinion. That cliff, that rapid rise in March, April, and May of 1955, was material that came from Nevada which had just come over. It did not go worldwide. That did not get in the Southern Hemisphere.

The flatter portion which you have there, in my opinion, was just about the same in the Southern Hemisphere. Now Dr. Machta says it was less. I do not think we know the answer, Senator.

Senator ANDERSON. I will read what you said on just the previous page. You said, "but no scientist who takes the trouble to learn what the facts really are will fail to agree with the overall picture."

Dr. LIBBY. I think that is right.

Senator ANDERSON. And you just got through saying you believe one thing and Dr. Machta something else.

Dr. LIBBY. No; he believes the same thing.

Senator ANDERSON. And naturally the rest of us get confused.

Representative PRICE. Will the gentleman yield?

Senator ANDERSON. Yes.

Representative PRICE. I think Dr. Machta said his findings—when questioned about your statement of April he said the disagreement came because of the fact that he was making his statement based on later findings.

Dr. LIBBY. I guess that is true, sir. I did not want to raise that point though. I mean it must be true; yes. That is, I am perfectly willing to change our mind if our stratospheric sampling—you see, we have a program now of going upstairs and actually finding out how much is there. This program is going and we will know this answer—and it is not very far away.

Representative HOLIFIELD. I think we are missing the point, if the Senator will yield. The point is not that there is a uniform distribution in the stratosphere. The point is, is the fallout from the stratosphere uniform or is it uneven, and I think Dr. Machta showed where the breaks in the stratosphere occurred and the downwinds came through those breaks that this was conducive to depositing the material unevenly on the earth's surface. Do you agree with that statement?

Dr. LIBBY. Certainly Dr. Machta is a most eminent meteorologist, and I am not a meteoroligist. I think your point is very well taken, Mr. Holifield, and may very well result that there is a band in both hemispheres, you see. That would follow from this analysis you have given us—that, if the mixing is at all uniform worldwide, there could be a band in both hemispheres.

Representative HOLIFIELD. It is also true, is it not, that the terrestial winds, the so-called jet winds, are more concentrated over the temperate zone, and as the material comes into a stream from other zones, why, it would tend to concentrate in the temperate zone; would it not?

Dr. LIBBY. Yes.

Representative HOLIFIELD. Your answer to that was "yes"?

Dr. LIBBY. Yes; I believe so.

Representative HOLIFIELD. On the chart over here, this really has very little, if anything, to do with stratospheric fallout. It is mostly the tropospheric fallout; is it not?

Dr. LIBBY. I think the stuff before and after the cliff is largely stratospheric.

Representative HOLIFIELD. You mean from January to March?

Dr. LIBBY. January to March, and then from——

Representative HOLIFIELD. That would be the stratospheric?

Dr. LIBBY. Yes. After the Teapot, Nev., series did you have a rise in the fallout, but that was tropospheric and not stratospheric. That was kiloton bombs.

Senator ANDERSON. Dr. Libby, in that speech in April you said:

For air-fired megaton weapons, our present indication is that the fallout is almost worldwide; and for reasons of simplicity, and in the absence of better information at the present time, we work on the model that this is a uniform distribution over the entire world of the material that falls from the stratosphere.

If you had to assume something, why not assume what the results show—that it was nonuniform distribution?

Dr. LIBBY. You see, sir, my interpretation of the results would be that they are not conclusive in showing nonuniformity.

Senator ANDERSON. No, but 50 percent of this radioactive material fallout of fission products goes into the stratosphere. Now it is quite significant if you start to assume all of this is going to come down evenly, but if you start to assume it may not come down evenly, there may be a band where deposits are heavy, it might affect people in the Northern Hemisphere, it might really concern them, and you might have to revise, it would seem to me, your estimate of the future fallout pattern in the United States.

I am only trying to say, why do you always say "uniformity," when the experience shows nonuniformity?

Dr. LIBBY. The experience does not show nonuniformity on the stratosphere, Senator, definitely.

Senator ANDERSON. I do not say that. You twisted it to mean something else. I said: "Why do we always have to assume uniformity in the fallout pattern of these materials, when all of the experience shows nonuniformity in fallout?" I am not talking about what is upstairs.

Dr. LIBBY. We certainly should not do that, and we have never done that, Senator; and I think it is important to make that clear.

Senator ANDERSON. You mean this speech does not assume that?

Dr. LIBBY. That speech is correct as far as I know.

Senator ANDERSON (reading):

We work on the model that this is a uniform distribution over the entire world.

Dr. LIBBY. Stratospheric, sir.

Representative COLE. Senator, read the rest of the statement.

Senator ANDERSON. I have read it with care.

Representative COLE. You will see it is not——

Senator ANDERSON (reading):

Further evidence and data on this are rapidly being collected which will undoubtedly settle the stratosphere horizontal mixing question.

That does not give me any more comfort than the first statement. You assume uniformity even here this morning. After Dr. Machta's very enlightening discussion, you start with the uniformity of stratospheric fallout. Why do you use the word "uniformity"? Why don't you use the word "nonuniformity"?

Dr. LIBBY. Because it is a question which is subject to some—like all scientific conclusions, you take the data and you draw conclusions from the data. The fact that banding of fallout occurs around our latitude, Senator, nobody argues about. Nobody argues about that. We do have this heavy banding in our latitude. And in my opinion it is due very largely to young fallout which never has been up there for years. It has been up there for weeks or months, and has not had time to mix. This difference of opinion is going to be settled. I do not believe that it is a major controversy. It is a minor one and will not seriously affect the general conclusions of this hearing.

Senator ANDERSON. It affects tremendously the question of how much fallout is safe, how much testing is safe, because if you assume that the pattern is uniform around the world, when actually it is 2 times or 3 times heavier in a given place, then you have, by this as-

sumption, lowered the possibility of damage from fallout. And the question is: Is that the reason why we say fallout is uniform over the whole world, whereas if we calculated it directly according to the way it is coming down, we might get a different answer?

Dr. LIBBY. You are certainly right, Senator.

Senator ANDERSON. That is my only point.

Representative HOLIFIELD. Dr. Libby, may I clear up one technical point here?

Dr. LIBBY. Yes.

Representative HOLIFIELD. The figure has been used in testimony before this committee of 50 percent of the fission products of a weapons test going into the stratosphere. Let us clear up now, if we can, that we cannot rely upon the 50 percent, and I ask you this question: Does it not depend upon the size of the weapon as to how much goes into the stratosphere? And therefore the percentage would vary from a large weapon, let us say, from as much as 75 percent going into the stratosphere and a smaller weapon that would puncture the stratosphere might deposit 25 percent, and a still smaller one would not go into the stratosphere at all? Is that clear? And if it is not, will you please clarify it?

Dr. LIBBY. It is quite true, Mr. Holifield, all that you have said. The statement about 50 percent is sort of rough. I mean there is nothing magic about that number.

Representative HOLIFIELD. No.

Representative COLE. On that point, if I may, Mr. Chairman; the size of the weapon is not the only factor which determines the amount of material that goes into the stratosphere, is it?

Dr. LIBBY. Oh, no, no.

Representative COLE. What are the other factors?

Dr. LIBBY. There are many factors. For example, if you took a kiloton bomb upstairs and fired it, it would be in the stratosphere.

Representative COLE. Well, the altitude has a great deal to do with it?

Dr. LIBBY. Surely.

Representative COLE. Does the nature of the weapon have anything to do with it?

Dr. LIBBY. The yield certainly determines whether it is going to push up the yield, among other things. That is, if you fire two bombs at the same distance above the earth's surface, the megaton bomb will get into the stratosphere and the kiloton bomb will not. Of course that is a rough general statement, too. I mean a 1-megaton bomb would not all go into the stratosphere, and some of the 100-kiloton bomb would go into the stratosphere.

Representative HOLIFIELD. And it is true, as you said, a thousand-ton bomb, if projected in the stratosphere by missile, would release all radioactive material in the stratosphere.

Dr. LIBBY. It would all be in the stratosphere.

Representative HOLIFIELD. In my evaluation I was, of course, talking about ground explosions.

Dr. LIBBY. Yes, I understood that, Mr. Holifield. But I thought it might be interesting to make this point that even kiloton bombs can give stratospheric fallout in the southern hemisphere if they are fired high enough in the air.

Senator ANDERSON. I was about to ask you how many people live in the stratosphere, but let me get down to this: Since the worldwide average is not calculable if it did fall down in a uniform pattern, would it now appear desirable or necessary to raise the predictions for the Northern Hemisphere, including the United States?

Dr. LIBBY. We certainly, in considering this tolerable limit should consider the fallout pattern, Senator, not a theoretical or simplified one. One should take this into account, those bumps, like that [indicating] which are due to Nevada. We have to add those things in there.

Yes. The answer to your question is, "Yes," one must take these into account. To reach an overall simplified figure, it might be justified to say, "If we fired everything in the stratosphere, this would be the figure." Or to take another one. If we fired everything in the troposphere, this would be the figure. But in the actual determination, you must take the actual figures for the fallout as distributed rather than depending on some theory. This is certainly true.

To proceed with my statement:

These essential points are generally agreed and the questions under debate are really largely political and sociological. For example, we agree that the extra radiation from the test fallout is a small fraction of the natural dosage we receive from our own bodies, our surroundings, the cosmic rays and a very small fraction of X-ray doses taken by many individuals.

Representative COLE. Mr. Chairman.

When you say, "We agree," who are you talking of?

Dr. LIBBY. I am thinking of the scientists who are studying the physical part of the Sunshine project; that is, the fallout, worldwide fallout.

Representative COLE. You are referring to the scientists who are directly engaged in the Sunshine project?

Dr. LIBBY. Yes, sir. Dr. Machta, and me, for example.

Senator ANDERSON. I had not intended to interrupt, but in the previous sentence you say, "the questions under debate are really largely political and sociological."

These 2 or 3 weeks have been set aside for the examination of witnesses who are primarily scientific in their approach. Are we wasting our time on scientific testimony, and not getting the political and sociological?

Dr. LIBBY. Oh, no. I think it is very important. The service that the Congress has done in holding these hearings is tremendous, and I commend you on it. You have gotten the facts out in a way that is just fabulous. It would have taken years to get this information out in the normal course, it seems. And it is so wonderful. No, sir, it has not been a waste of time.

Representative HOLIFIELD. At this point, Dr. Libby, because of the comment you made, you studied our agenda, which was prepared with a great deal of care and with a great deal of scientific advice by our staff members and scientific consultants, and you have been to some of these hearings. Do you consider that these hearings are being held in a fair and impartial manner, and in a beneficial manner to the people?

Dr. LIBBY. I certainly do, Mr. Holifield. You could not have done a better job, in my opinion. It is possible you might have taken up

some other subjects, but I would not have been able to devise a better program or set of hearings than you have conducted. I certainly commend you for it. No criticism at all.

Representative HOLIFIELD. We asked for suggestions, as you know, from the AEC.

Dr. LIBBY. Yes.

Representative HOLIFIELD. On the preparation of the agenda. And of course, we have tried to limit it as much as possible to scientific testimony.

Dr. LIBBY. Yes.

Representative HOLIFIELD. Now we recognize that there are other fields, sociological, political, and moral fields, of interpreting the effects and the reasons for having these tests; but it is not the purpose of the committee to get into that in these hearings. We are trying to collate a great amount of scientific information, so that people can make their decisions on information and statistical data from reputable sources, rather than upon uninformed statements.

Dr. LIBBY. Yes. I certainly commend you for it.

Senator HICKENLOOPER. Mr. Chairman?

Representative HOLIFIELD. Senator Hickenlooper.

Senator HICKENLOOPER. May I ask Dr. Libby about my interpretation of his statement, which, of course, came to my attention when he read it, that—

> These essential points are generally agreed and the questions under debate are really largely political and sociological.

I interpret that to mean not that there were not scientific data involved here at all, but that the public has been debating the sociological and political implications of this, and that is one of the great interests in the minds of the public which might be clarified, or some scientific truth be developed.

Dr. LIBBY. Yes.

Senator HICKENLOOPER. Which would dispel a great many fears and doubts, and all of those things that have been created by hysterical journalism on occasion, and headlines which seek, or which do, in effect, create fears and minimize the actual facts. I took that to be your reference.

Dr. LIBBY. That is the sense of it.

Senator HICKENLOOPER. I do not mean to have you pass on the question of distorted understandings of these things, necessarily, but there is a social and political concern in the country about these things which scientific facts may give many answers to.

Dr. LIBBY. That is right.

Senator HICKENLOOPER. I will frankly say I have seen many news stories which attract the avid interest of the people, and I have had telegrams on them about the terrible effects of these tests, which in my opinion are not borne out by the facts in these hearings, but still they get into the social and political thinking of people as a result of what at least to me is a misunderstanding of the actual facts and data that are brought out in these hearings.

Representative HOLIFIELD. Referring again to your sentence—

> These essential points are generally agreed and the questions under debate are really largely political and sociological.

would you not also modify that statement to include a difference of interpretation of the meaning of certain scientific facts?

Dr. LIBBY. Yes, I certainly would.

Representative HOLIFIELD. As well as political and sociological?

Dr. LIBBY. Yes.

Representative HOLIFIELD. There is an honest difference of opinion among scientists as to what the facts which they are agreed upon mean, how they should be interpreted in relation to the effect upon human beings; is there not?

Dr. LIBBY. There certainly is, particularly in the biological side, and particularly—well, particularly in the effects of fallout on the human body. There is not such a range of disagreement about the amount of fallout, or how much is where or how much has been there. There is not so much disagreement about these.

Representative HOLIFIELD. Particularly in the field of genetics.

Dr. LIBBY. That is my impression, Mr. Holifield.

Representative HOLIFIELD. There seems to be a very great deal of unanimity in the opinion of geneticists that people in the area of science in which they are most interested are more deleteriously affected than in the biological, physiological sense.

Dr. LIBBY. I have been reading the testimony yesterday with great interest. As I say, I am not a geneticist in any way, but it certainly is interesting to hear what the gentleman had to say. My impression agrees with yours.

Representative HOLIFIELD. You may proceed.

Dr. LIBBY. We agree that the radiation given new bone—as in children—from strontium 90 amounts now from 1 to 2 percent of the natural dose and that the increase in dose from a wide variety of ordinary experiences such as living in the mountains, moving from one locality to another where the difference in uranium and thorium content of the ground may be very different, living in a brick instead of a wooden house—all these normal experiences give doses which far exceed those from fallout. This we all agree is so.

Senator ANDERSON. Dr. Libby, right there. Do we also not then worry about what this maximum permissible dose is going to be when some of this fallout descends from the stratosphere?

Dr. LIBBY. No, sir, not at all. But I am showing here the wide range of general agreement on the physical aspects of the fallout problem. Everybody agrees that the fallout dosage is very small compared to both the natural dosage and the variation in the natural dosage.

Senator ANDERSON. Dr. Kulp, in his testimony, indicated that if we proceed at the same rate of testing, in 30 to 50 years we would get up to 25 to 50 percent of the maximum permissible dosage.

Dr. LIBBY. I am talking about the present time, Senator. The present time. You see, when you get into the future, you get into theory.

Senator ANDERSON. It is like erosion in my State. If you go out and look at a field, you will say, "We do not have to worry about runoff at the present time. It is not doing anything but dipping a little bit." And then it dips a little bit more, and finally you have a gully. We are interested in what happens 50 years from now.

Dr. LIBBY. Absolutely, we certainly are.

Senator ANDERSON. That is the reason why we might recognize this. What do we do with the acceleration of tests? If we have new countries coming in with tests, and the tests pick up, we raise this figure of 1 to 2 percent very decidedly, do we not?

Dr. LIBBY. Where is the wide difference of opinion? Why do some say testing should be continued despite the fallout hazard which they regard as tolerable considering the advantages continuance of testing has, and others say essentially the opposite? The differences in opinion about the scientific facts are not the real issue. The differences exists, but they do not explain the wide divergence in final conclusions.

Representative HOLIFIELD. There was a challenge put out, Dr. Libby, in this hearing that the Commission should come forward with a clear-cut statement regarding the scientific reasons for the continuance of testing. The geneticists gave reasons which they thought should be considered in the elimination of testing.

Now we are facing, for instance, in Nevada, a series of tests. Do you intend in your statement, or could you at this time give some of the scientific reasons—I am not talking about moral or philosophic reasons—but some of the scientific reasons why we consider, if we do, that testing should be continued?

You might divide that up into two sections, into the military area and also into the area of increasing our knowledge from a protective standpoint.

Dr. LIBBY. I would like to ask you, Mr. Chairman, for permission to prepare a statement on this point for the record, because I think what I would say off the cuff, so to speak, would be less helpful than something I could say after careful consideration. I would be very pleased to do that.

Representative HOLIFIELD. This is the thing I think we ought to be alert to—an affirmative drive for the acceptance of democratic ideas, instead of always being on the defensive side in relation to Russian propaganda in tests.

I think if you will prepare a careful statement, we will have it appended to your presented statement today, so that we may have this subject covered also (see p. 1373).

Dr. LIBBY. I certainly will, Mr. Chairman.

Representative COLE. Mr. Chairman, on that point.

Representative HOLIFIELD. Mr. Cole.

Representative COLE. I am advised that this morning in the press conference at the White House, the President discussed the question of the need for continuing the weapons testing, the subject of the hazards of radioactive fallout; and I would like to ask at the conclusion of the meeting this morning, a synopsis of that press conference be inserted in the record.

Representative HOLIFIELD. The committee will consider accepting that for the record. Up to now we have received testimony from scientists. I do not consider the President of the United States a scientist. He may have the advice of scientists, but unless the committee directs the chairman to break our rules of presenting evidence from nonscientists, we will have to take specific action on that request in executive session.

Representative COLE. Do I construe the chairman's statement, then, as being an objection to my request that the President's press conference be inserted in the record following Dr. Libby's testimony?

Representative HOLIFIELD. It is not an objection; it is a matter of postponement for the committee to decide upon, as we have already agreed we will have no one but scientists to testify during this set of hearings. And we have turned down many people of outstanding character and belief throughout the Nation because of the fact they were not scientists because we want to hold this to a collection of scientific testimony.

Representative COLE. Mr. Chairman, I am going to at this time ask unanimous consent that the President's statement at the press conference this morning on this subject be inserted in the record at the point following Dr. Libby's testimony this morning.

Senator ANDERSON. I would hope that might be done, because the scientists are going to get a supplementary statement, and it may be well to have the official position of the Government on this. I think this might be all right, Mr. Chairman.

Representative HOLIFIELD, There is a unanimous consent request before the committee. If it is the will of the committee, the chairman will entertain the motion of Mr. Cole.

Those in favor of Mr. Cole's motion signify by saying "Aye."

Those against say "No."

(Representative Holifield was the only one voting no.)

Representative HOLIFIELD. Proceed.

Representative COLE. What was the conclusion?

Representative HOLIFIELD. The conclusion of the vote is a vote of 4 to 1 in favor of inserting it in the record. It will be inserted.

The Chair may ask to have other nonscientific statements put into the record at a later date, and if that request is made, we hope that the committee will also agree to the same principle.

Proceed, Dr. Libby.

Representative COLE. Well, Mr. Chairman, I do not want to belabor this point, but to make certain the extent to which the action we have just taken creates a precedent. It has been my understanding that in order for material to be inserted in the hearings requires a unanimous approval of the committee.

Representative HOLIFIELD. There has been no such action on that point by the committee.

Representative COLE. I know. Therefore, my request was a unanimous consent request. If there is a single objection, my request will not be accepted.

Senator ANDERSON. Let me plead with Mr. Cole not to worry about that. I think when we get to considering it we may come to the conclusion that these scientists are asked for an explanation of America's point of view, it is proper for the President of the United States to outline what the point of view is. I think we can solve it without any difficulty. I think we would like to insert it in the record, and I certainly would favor and not oppose it. I do not believe it is a precedent.

Representative HOLIFIELD. I have no personal feeling about the matter. I am merely trying to conform to the rules which were decided upon in executive session, and my vote is still "no" until the rules are changed.

Proceed, Dr. Libby.

Dr. LIBBY. Testing constitutes a small risk—very small compared to ordinary risks which can be tolerated. It is not contended that there is no risk. But all life, and every minute of our day and night, is

measured in terms of risk—40,000 highway deaths each year in this country, accidents in the home, et cetera. We make our choice: How much risk are we willing to take as payment for our pleasures—swimming at the seashore, for example—our comfort, or our material progress? Here our choice seems much clearer. Are we willing to take this very small and rigidly controlled risk, or would we prefer to run the risk of annihilation which might result if we surrendered the weapons which are so essential to our freedom and our actual survival?

Senator ANDERSON. Right there, Dr. Libby, we have much to talk about, I would think. In your words: "Are we willing to take this very small risk?"

Do you regard this a very small risk?

I am referring to the fact that Dr. Russell, who is the principal geneticist at Oak Ridge National Laboratory, was quoted in a Science Service news story as saying that:

> Offspring from a man exposed to such radiation will have their lives shortened on the average of 20 days for each unit of the radiation their father had received.

We figured that out to about 22 years when we attain this maximum period. Then Dr. Russell revised his figures to mean from 5 to 35 days, and at the lowest it would come to about 11 years.

Would you think the shortening of life in offspring to the amount of 11 years was a very small risk?

Dr. LIBBY. Did I understand you to say that the present fallout would shorten our lives 11 years?

Senator ANDERSON. No, you did not.

Dr. LIBBY. I am sorry, sir.

Senator ANDERSON. I quoted Dr. Russell. I thought it would be better if I stayed with the scientists, and I quoted Dr. Russell, who is the principal geneticist at the Oak Ridge National Laboratory, who should be well known to you.

Dr. LIBBY. Yes, sir.

Senator ANDERSON. I referred to a story put out by Science Service which Dr. Russell explained. I think it has to be taken in view of his explanation. I was very happy we had it, because it was most helpful.

The story said: "Offspring from man exposed to such radiation"—and his radiation was neutron radiation from an atomic bomb. I had better read the first paragraph, Dr. Libby.

Dr. LIBBY. I do not know of this.

Senator ANDERSON. I recognize that, and I am sorry. I will be happy to send you the clipping.

> Neutron radiation from atomic bombs can shorten the life of a man's children, Dr. W. L. Russell, the principal geneticist at the Oak Ridge National Laboratory here has found. Offspring from a man exposed to such radiation will have their lives shortened on the average of 20 days for each unit of radiation their father has received.

I do not know how much radiation he is going to have from an atomic bomb. I do not know how much he is going to have from atomic testing. I am not concerned about that, to try to measure it. But here is a scientist—and I am happy to have your assurance that all scientists are pretty well agreed on these essential facts—who believes it will shorten life. And when we discussed it the other day—I hope I do not misquote Dr. Russell; I do not mean to—he ex-

plained the 20 days, and said perhaps a 5 to 35-day figure might be a much better figure.

But do you regard that shortening of life in succeeding generations as a very small risk?

Senator HICKENLOOPER. Mr. Chairman, would the Senator yield to the Senator for a question before he answers the question?

Senator ANDERSON. I recall the old radio show where the Baron said, "The Baron always asks the questions," but I will yield.

Senator HICKENLOOPER. I understand, but I would like to have the Baron answer the full question.

It was my understanding of this testimony it was qualified by saying that a man who got the maximum dose would suffer, and the testimony seems to be replete with the fact that we are in no danger at the present time of coming anywhere near the maximum dose.

Senator ANDERSON. I will be happy to read it. I read then, and I will read now from the news story which does not talk about maximum doses, and says, "Neutron radiation from atomic bombs."

The question then came, If a man should get a maximum dose of, say, 400 roentgens, it would shorten life 10 or 20 years. But I tried to avoid that by pointing out he said it can shorten the life of a man's children.

If Dr. Libby questions that, then we can have Dr. Russell to defend himself at a later time.

Do you believe that neutron radiation from atomic bombs can shorten the life of a man's children?

Dr. LIBBY. I have nothing but respect for Dr. Russell. I do not know the details of his study. This is outside my field.

I would point out to the Senator, though, that there are no neutrons in fallout radiation. I am not saying this to beg the question. I do not know whether Dr. Russell has studied the effects of fallout radiation but neutrons are emitted only instantaneously when the bomb is fired, so you do not get neutrons from fallout.

Senator ANDERSON. Do I understand Dr. Russell was on the Sunshine Project?

Dr. LIBBY. Surely, but I hesitate to comment on Dr. Russell's statement, since I have not seen it, Senator.

Senator ANDERSON. Very well.

Dr. LIBBY. I will be very pleased to answer your question for the record after reading the statement.

Senator ANDERSON. I will skip the words "very small," and come to the next. "Are we willing to take this very rigidly controlled risk."

Dr. LIBBY. Yes.

Senator ANDERSON. How rigidly is this controlled? Do we have any control over Russia as of now?

Dr. LIBBY. Well, I was speaking more about our rigid controls, which are very rigid.

Senator ANDERSON. But we are not the only people that are depositing fission products in the atmosphere, are we, Doctor? Are we?

Dr. LIBBY. We certainly are not.

Senator ANDERSON. All right. If we are not, do we have any control over what Russia does?

Dr. LIBBY. Obviously no.

Senator ANDERSON. Do we have any control over the British?

Dr. LIBBY. Obviously no.

Senator ANDERSON. And there may be a fifth or a sixth or a seventh country coming along. Do we anticipate we will have any control over them?

Dr. LIBBY. I would not comment about them, because I would not know. But the point is this, Senator: We do have control over ourselves, and the debate is whether we stop testing, is it not?

Senator ANDERSON. No, I do not think so, at all.

Dr. LIBBY. Well, part of it.

Senator ANDERSON. I think the debate is whether or not the world tries to bring this under some sort of control.

Dr. LIBBY. Which we are all for.

Senator ANDERSON. I was particularly attracted, if you do not mind my saying so, to the testimony given by Dr. Langham, in which he suggested a level of 10,000 megatons of fission products put into the atmosphere as a possible goal toward which all countries——

Dr. LIBBY. You do not mean 10,000 megatons.

Senator ANDERSON. Ten megatons. Did I not say 10 megatons of fission products? I am sorry. I meant 10 megatons yield equivalent of fission products put in the atmosphere. We have been putting it into the atmosphere at about the rate of 10 megatons a year, and he thinks that is about the maximum, and certain other scientists agree with him, and some of us are interested in trying to bring this situation worldwide under control.

Now, the language of your statement says: "this very small and rigidly controlled risk." And I question the term "rigidly controlled risk," because I realize it is like the term "clean bomb," it makes good reading. But it is not rigidly controlled, is it?

Dr. LIBBY. I had reference to our controls, sir, and they are rigid.

Senator ANDERSON. Well, I go on—"or would we prefer to run the risk of annihilation which might result if we surrendered the weapons which are so essential to our freedom and our actual survival."

Is that a suggestion that if we had control we would have to disarm?

Dr. LIBBY. It is my opinion, Senator, that this testing is an integral part of armament and cessation of testing is a part of disarmament. I think most people agree with that.

Senator ANDERSON. You say you want testing continued?

Dr. LIBBY. I say, and what I did say, Senator, was that testing was an integral part of armament, and so stopping testing is a disarmament move, and I think most people agree with that.

Senator ANDERSON. And I understand you favor continuing testing?

Dr. LIBBY. I am in favor of disarmament under proper controls, very strongly, sir.

Senator ANDERSON. I again come back to your letter to Dr. Schweitzer, in which you say:

> Of course, a workable, safeguarded system of international disarmament is a paramount objective of the United States Government, and one which we must work for and hope and pray will be achieved.

Dr. LIBBY. Yes.

Senator ANDERSON. If you are going to work for it, and hope and pray it would be achieved, an initial step certainly would be to stop testing, a step to disarmament?

Dr. LIBBY. The cessation of testing would be a disarmament move.

Senator ANDERSON. And you are in favor of disarmament?

Dr. LIBBY. In general, yes; under proper controls.

Senator ANDERSON. Very well.

Senator HICKENLOOPER. Mr. Chairman?

Representative HOLIFIELD. Senator Hickenlooper.

Senator HICKENLOOPER. May I ask, Dr. Libby, I take it that what you are saying is that you do not believe it is in any interest of our own future security if we stop testing alone and permit Russia to go on testing, and other nations in the world to go on testing. Is that correct?

Dr. LIBBY. That is exactly correct, Senator.

Senator HICKENLOOPER. And you are thoroughly and completely in agreement with the disarmament which would include the stopping of testing if reliable controls were had, and reliable assurances by other nations that their testing would also be stopped, and the field of weapon experimentation would be eliminated?

Dr. LIBBY. That is right, sir.

Senator HICKENLOOPER. I just wanted to get that straight here.

Representative HOLIFIELD. Proceed, Dr. Libby.

Dr. LIBBY. The cause for real concern is not the deleterious effect of radiation resulting from weapons tests, but rather what would be the effect of the infinitely greater amount of radiation which would result from the massive use of nuclear weapons in warfare. Here we would be dealing with excessive radiation, not to all the people of the world as has been suggested, but quite probably to large numbers of people residing in areas of substantial contamination.

Senator ANDERSON. Right there, Doctor, did the geneticists not seem to indicate yesterday that radiation spread around the world? What about these areas of substantial contamination, Doctor? This is just narrowed down to a city or township near where it happened.

Dr. LIBBY. I would again have to study their statement, and I am not a geneticist. It would seem to me there is no doubt there would be damage to very large numbers of people. Whether there may be remote corners of the world which would escape is a question I think we do not know the answer to, really.

With regard to the people so overexposed——

Chairman DURHAM. Dr. Libby, yesterday Dr. Glass, I believe, testified that in some 2,000 skeletons strontium 90 showed up in all of those in all parts of the world.

Dr. LIBBY. Yes.

Chairman DURHAM. I mean, you do not doubt the fact that it falls out in some quantity around the world, do you?

Dr. LIBBY. No, sir. The question is what the fatalities would be in terms of an atomic war, nuclear war.

Chairman DURHAM. That is why I asked the question, because you are speaking to numbers here now, of course, in at least an act of war.

Dr. LIBBY. Right.

Chairman DURHAM. In your statement, "quite probably to large numbers of people residing in areas of substantial contamination"—what do you mean by large numbers?

Dr. LIBBY. Well, Mr. Durham, what I wanted to bring out is what we call the cigars, that is, the areas of local fallout, which Mr. Holifield spent so much time working on in connection with civilian defense. We have been in these hearings talking about the other side of the coin, the worldwide fallout. In the local fallout pattern which you get that

looks like a cigar, perhaps 7,000 square miles in area as from the March 1954 detonation, and so on.

We must not forget those things. On the one hand we have the subject of this hearing, which is worldwide fallout, and on the other hand we have the blanketing of the country with local fallout pattern. This is the distinction I had reference to.

There is absolutely no doubt that the people in those areas of heavy local fallout would be seriously damaged unless they took care of themselves, and looked to the problem ahead of them. There is absolutely no doubt.

Now the question of worldwide fallout and how serious it will be from the genetic point Senator Anderson brought out, well, I do not think we know; and I think the geneticists would be the first to agree they are not absolutely certain in the magnitude of effect. We know the effects are not good.

Chairman DURHAM. You know the life of strontium 90, though?

Dr. LIBBY. Yes, sir.

Chairman DURHAM. You know, of course, the first contamination is not the end of it.

Dr. LIBBY. Oh, no. There is no doubt there will be serious effects every place in the world if there were a nuclear war.

Representative HOLIFIELD. Proceed.

Dr. LIBBY. With regard to the people so overexposed there would be serious increases in the pathological effect of excess radiation, such as cancer and leukemia. There would also be the genetic effect which would manifest itself in the children and the children's children of such people.

Let us consider the present situation. We can take our normal experience with natural radiation as strictly limiting possible effects from the fallout—since natural radiation is so much larger. It is a queer fact of the present situation that we know far more about fallout dosage than about natural dosage.

The Sunshine project is only beginning measurements on natural radiation, though, as you have seen, its amassed data and its understanding of fallout makes it one of the most impressive scientific investigations ever made. Somehow, even those who have known for years about the wide variation in natural dosages have been more concerned with the study of test fallout than of building materials, homes, public buildings, schoolrooms, et cetera.

(Remarks prepared by Dr. Willard F. Libby, Commissioner, United States Atomic Energy Commission, follow:)

NATURAL OCCURRENCE OF RADIOACTIVITIES AND RADIATION

The radiation dosages that people receive from natural radioactivities and cosmic rays are of importance, for it is only from these dosages that limits on the deleterious effects of radiations received over long periods of time at low rates as in the case of radioactive fallout can be obtained. Studies of vital statistics for populations exposed to varying natural dosage levels offer hope for determination of human susceptibility to radiation effects both somatic and genetic. A brief summary report on this, published some time ago, is offered for the record. It was prepared with the thought of application to the fallout studies in mind: Dosages from Natural Radioactivity and Cosmic Rays, W. F. Libby, Science, 122, 57–58 (1955). (See p. 1459.)

Other lengthier and more complete studies are listed:

1. P. R. J. Burch and F. W. Spiers, Science 120, 719 (1954).
2. P. R. J. Burch, Proc. Phys. Soc. Lond. A67, 421 (1954).

3. Bengt Hultqvist, Kungl. Svenska Vetenskapsakademiens Handlinger. Fjarde Serien. 6, No. 3 (1956).
4. Radiological Data in Japan, Government of Japan, Yoshio Hiyama. U. S. Scientific Committee on the Effects of Atomic Radiation, A/C.82/G/R.70, March 27, 1957.
5. The Toxicity of Skeletal Irradiation at Naturally Occurring Radiation Levels, Robert A. Dudley, April 1, 1957.

I would call attention particularly to the work of Hultqvist on building materials in Sweden, particularly brick and light weight concrete containing alum shale. Measurements in this country indicate that our bricks may not differ appreciably.

Natural dosage in middle of rooms in Swedish homes (mr/yr) (Hultqvist)

Wood	83± 9
Brick	143±22
Concrete (light weight with alum shale)	215±65

We are beginning a survey on natural radiation dosages which should elucidate this and many other points. In the initial results we find that very appreciable variations from spot to spot occur just as would be expected on the basis of the occurrence of thorium, uranium, and potassium in various minerals. Inside a granite church in New York State the dosage was double that outside in the same vicinity.

The close reader of the reports cited above will note a discrepancy in the magnitude of the cosmic ray dosages reported. This has its ultimate origin in a real discrepancy in the physical measurements as between Professors Millikan and Neher in California and Dr. J. Clay and his school in the Netherlands. I have chosen the Millikan-Neher result which seems to have been confirmed in the Japanese report submitted to the U. N. Committee a few weeks ago, whereas others have selected the Clay measurements which give dosages about 70 percent of those of Millikan-Neher. The altitude variations are the same in all studies within the error of measurement.

We have only recently started measuring brick and arranging for widespread natural dosage studies, and most of what we know has come from studies abroad, part of which were through the United Nations Committee mentioned earlier.

For example, in Sweden, Hultqvist has shown that homes of wood give an average dose in the center of their rooms of 80 to 90 milliroentgens per year, while those of brick give 140, and those of lightweight concrete with alum shale, over 200. These numbers are to be compared with present United States fallout doses—and remember that the United States—I know there is an article in the morning paper that said something different—as far as we know is the highest in the world. The present United States dosage is 1 to 5 milliroentgen per year.

Certain areas of India occupied by many thousands of people have natural dose rates—due to the thorium in the sand—several times our average of about 150 milliroentgens per year. An important article on natural dosage appeared in one of our own technical magazines a few days ago. Written by Prof. H. V. Neher, a colleague of Robert A. Millikan's, it presents important early data gathered prior to any nuclear weapons tests. I would like to refer to the principal graph from Professor Neher's paper. It is upon the stand over there.

(The article including the graph referred to follows:)

[Reprinted from Science, v. 125, May 31, 1957, pp. 1088–1089]

Gamma Rays From Local Radioactive Sources

There is considerable interest at the present time concerning the possible effects of manmade radiations on man himself. Because one source of these radiations is of worldwide extent, the interest has also become worldwide. Although considerable literature now exists on the subject of manmade radioactive contamination, on the one hand, and on the biological effects of radiation, on the other, the actual importance of the first as far as the second is concerned has often been obscure. It is thought desirable at this time to present some independent experimental data that will allow individuals to reach their own conclusions.

As early as 1928, R. A. Millikan became interested in the gamma rays emitted by local radioactive materials in the soil and rock at various localities in order to determine the effect of these radiations on the cosmic-ray measurements in which he was primarily interested. These measurements extended from California into the Rocky Mountain area and on up to Churchill, Manitoba.[1] They probably represent a unique series of measurements, since they were made before manmade contamination became widespread.

Anionization chamber measures directly the quantity of interests as far as the biological effects of gamma rays are concerned, and this is the instrument here employed. One of the instruments Millikan made and calibrated is still in good condition after 26 years and is very convenient to use. A recent redetermination of the absolute value of the calibration[2] agrees with Millikan's value to 0.3 percent. In this survey, Millikan's instrument has been used for some of the measurements, and a more modern ionization chamber[3] for others. The two give essentially the same answer. Both were used unshielded in the measurements reported here.

[1] R. A. Millikan, Phys. Rev. 37, 242 (1931).
[2] A. B. Johnston, thesis, California Institute of Technology (1956).
[3] H. V. Neher, Rev. Sci. Instr. 24, 99 (1953).

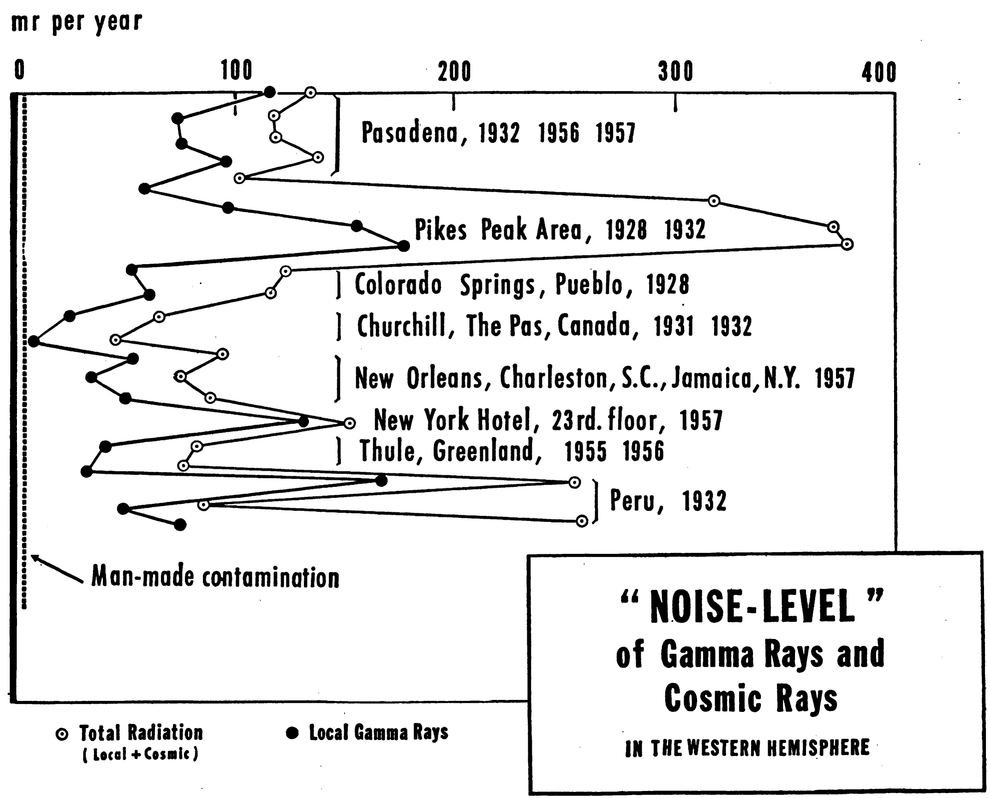


FIGURE 1.—"Noise level" of gamma rays and cosmic rays in the Western Hemisphere, Abscissas roughly increase with increase of distance from Pasadena. The amount of manmade contamination is taken from the National Academy of Sciences report, Biological Effects of Atomic Radiation (see footnote 7). As is stated in that report, "* * * United States residents have, on the average, been receiving from fallout over the past 5 years a dose which, if weapons testing were continued at the same rate, is estimated to produce a total 30-year dose of about 0.1 roentgen" (an average of 3 mr. per year).

In figure 1, most of the values taken during the years have been entered. The ordinates are in milliroentgens (mr.) per year. To convert into ion pairs per cubic centimeter, per second in 1 atmosphere of air, divide the ordinate by 15. The various stations are plotted as absciees with the same increment from one to the other. Roughly, the stations get farther from Pasadena with increase in abscience. The chief reason for plotting in this manner was to bring out the variability of radioactivity from one station and region to another.

Measurements were made of the total radiation at a given station; then the known contribution from cosmic rays[4] was subtracted to get the effect of the gamma rays from local radiation only.

In the Rocky Mountain region, the local radiation is high, presumably because of the granite, which is known to contain something like 4 g. of uranium and 15 g. of thorium per ton.[5] In Peru, the radioactivity of the coastal plain is much the same as that of the Mississippi region near New Orleans. The local radiation at an elevation of 15,000 feet in southern Peru is only slightly higher than that of the soils of the coastal plain. Most of the houses of Arequipa are built of a light rock called tuva which is of volcanic origin. This rock is 3 or 4 times as radioactive as the soil near Lima.

There is considerable variability of local radiation in some cases over small distances. According to Millikan,[6] the gamma rays on the Laurentian Shield near Churchill, Manitoba, give 0.8 ion cm^{-3} sec^{-1} atm^{-1} of air, or 12 mr yr^{-1}, while nearby the intensity on the glacial sand is 35 mr yr^{-1}. It may be of interest that the radioactivity on the icecap near Thule, Greenland, in August 1956 was less than 2 percent of cosmic rays.

A wooden building forms some shielding from local gamma rays. In my own house, the gamma rays on the first floor give 60 mr yr^{-1}, while in the back yard the intensity is 95 mr yr^{-1}. The rather high value of 130 mr yr^{-1} on the 23d floor of a major hotel in New York is presumably owing to the material from which the building is constructed.

The root mean square "noise" level of the total radiation given in figure 1 is about 160 mr yr^{-1}. To find the effect on the population, the local radiation must be weighted according to the population. This has not ben done. Perhaps it is fortunate that most of the population of the country lives where the radiations due to cosmic rays and local radiations are relatively low.

The dashed line near the bottom of figure 1 is taken from the summary reports on the Biological Effects of Atomic Radiation of the National Academy of Sciences.[7] Even though there is some error in the determination of this value, as well as considerable variation of fallout over the country, it is quite evident that manmade contamination is still small compared with the changes in radiation from one part of the country to another.

The data presented here are for gamma rays only, since the walls of the ionization chamber are too thick for beta rays to penetrate, either from naturally occurring or artificially produced radioactive materials.

H. V. NEHER,
Norman Bridge Laboratory of Physics,
California Institute of Technology, Pasadena.

The upper curve is the total dosage in milliroentgens per year. The first four points are various locations in Pasadena, Calif., in the years 1932, 1956, 1957. The lower curve is just that part of the dosage which is due to gamma rays. That is due to the radioactivity in the rocks. The difference between the two curves is the cosmic rays.

And as he goes to various places over the country, and over the world, you will notice that the very widest variation in these natural dosage rates.

In many cases the variation is due to the cosmic rays; in other cases it is due to change in the composition of the rocks.

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[4] I. S. Bowen, R. A. Millikan, H. V. Naker, Phys. Rev. 46, 641 (1934).
[5] H. Paul, Ed., Nuclear Geology (Wiley, New York, 1954).
[6] R. A. Millikan, unpublished results.
[7] Summary Report of the Committee on the Genetic Effects of Atomic Radiation, in Biological Effects of Atomic Radiation (National Academy of Sciences. Washington, D. C., 1956).

This presents the total dosage due to cosmic radiation and radiation from the ground and general surroundings but does not include the 21 milliroentgens per year from our own bodies, so to calculate the total exposure under the various conditions described, we should add this in.

Of course, houses shield from the ground radiation although not appreciably from cosmic rays—so that they are a diminishing effect. Professor Neher remarks that in his own house in Pasadena, the gamma rays on the first floor give 60 milliroentgens per year while in the backyard the intensity is 95 milliroentgens per year—just to give you a feeling for the effect of being inside as compared to outside.

Well, these dosages are all very large as compared to the 1 to 5 milliroentgens per year which we receive here in the United States from fallout, and we must put our experience from fallout in this context.

Now certainly our experience with these much larger normal doses shows us that the effects due to fallout radiation are relatively small.

The fallout facts are pretty well known and agreed. On the other hand, small as the effects are, they most likely do exist and the critical and essential question is, Are they tolerable?

This question, however, calls for a political or sociological—rather than a scientific—judgment, and I think it is important that this distinction be made.

Chairman DURHAM. Mr. Chairman?

Representative HOLIFIELD. Chairman Durham.

Chairman DURHAM. In the previous years' study up until 1945, there was no such thing as strontium 90?

Dr. LIBBY. There certainly was not, Mr. Durham, at all. There was no strontium 90. These data are important because they are before any contamination from weapons at all, you see. This radiation is still there, but it may be a little more difficult to be sure about it.

I should point out on that graph—you see that line that is right down near zero, that is the test fallout line.

Chairman DURHAM. Of course, that was made by a higher authority than man, the natural radiation.

Dr. LIBBY. That is right.

Chairman DURHAM. And since then, of course, we are adding to it by putting strontium 90 into the air streams.

Representative HOLIFIELD. Dr. Libby——

Dr. LIBBY. I want to make sure, Mr. Holifield, this graph is clear to everyone. This top line is the total. If you take a counter and measure these various places, that is what is there. This part [indicating] of it is from the rocks. The difference is the cosmic rays, and this [indicating] is the way it varies.

If you go to Pikes Peak, Colorado Springs, and so on, this is the way it varies.

Here is the test fallout down here, this dotted line. The smallness does not mean that test fallout is not important, that it is not a risk and a hazard, but it does mean that the test fallout is a relatively small hazard.

The first point is, since we have been living on this earth so long, we have some practical experience where commonsense can take hold, and we can say there is some limit just by the fact that we have not noticed these big things in our normal experience—there is some limit

to the bad effects from test fallout, and that is the only point I wanted to make.

Representative HOLIFIELD. Dr. Libby, to make the picture complete, is it not true that there is a very important difference in the bomb fallout which is not contained in either the cosmic rays or in the natural radiation, and that is the factor of strontium 90?

Dr. LIBBY. Yes, sir.

Representative HOLIFIELD. Which gets into the bones of people?

Dr. LIBBY. Yes, sir.

Representative HOLIFIELD. Where cosmic rays and natural radiation do not have that particular residual effect in the bones of people. So we have got another factor and, therefore , your analogy there must take into consideration also this new factor of strontium 90 that we must consider.

Dr. LIBBY. There is a great deal of truth in what you have said, Mr. Chairman. To comment on your statement requires a small technical remark.

Biological——

Representative HOLIFIELD. Why did not you make that statement first, Dr. Libby——

Dr. LIBBY. I wanted to——

Representative HOLIFIELD (continuing). Instead of making a statement which was susceptible again to a benign interpretation?

Dr. LIBBY. I want to tell you that the statement I have just finished making is true.

Representative HOLIFIELD. I know it is. Most of your statements are true, sir. But the point is this: That when once you have made those true statements, they are susceptible of misinterpretation, and if you follow up with a complete truth, it might give a more balanced effect in the layman's mind.

Dr. LIBBY. Let me do that. I think it will only take a couple of minutes. I will try to make it short.

A milliroentgen is an amount of energy absorbed by a unit amount of tissue. Now, depending on the kind of radiation the biological effect varies. The biological effect from 1 milliroentgen will be different for 1 kind of radiation as compared to another. This is called the relative biological effectiveness.

While it is true that we have never had strontium 90 before, we have had radiations which have essentially the same relative biological effectiveness, and have always had them, and those are the cosmic rays.

The cosmic rays, milliroentgen per milliroentgen, are within a small error the same as strontium 90, milliroentgen per milliroentgen. This is not true, Mr. Holifield, for gamma rays from rocks, but for the cosmic rays which make up a good fraction of the total natural exposure, they are just as damaging to the bone milliroentgen per milliroentgen as strontium 90.

Representative HOLIFIELD. Do they reside in the bone and of themselves make emissions, such as strontium 90?

Dr. LIBBY. No, no; but that is not necessary, you see. It is not the presence of strontium 90 in the bone that is damaging. You ordinarily have ordinary strontium in the bone. The bad thing about strontium 90 in your bones is that it emits radiation.

Representative HOLIFIELD. That is right.

Dr. LIBBY. So the cosmic rays in passing through the bone, as they do through all of your body, deposit in the bones energy which is in excess of the energy deposited by the present burden of strontium 90.

What I am telling you, Mr. Chairman, is that you can compare these two kinds of milliroentgens. You can say that if the milliroentgens from cosmic rays are large as compared to those from strontium 90, then the bone cancers and leukemias, and all the other things that may be caused by strontium 90 should follow even more from the cosmic rays. That is what I am saying.

In other words, the gamma rays you cannot compare, but the cosmic rays you can.

So when I went to the chart and said that about test fallout, I was telling you the truth in the sense that a good part of that total radiation is cosmic rays.

Representative COLE. Mr. Chairman?

Representative HOLIFIELD. Mr. Cole.

Representative COLE. I was going to ask Dr. Libby to explain that point on the chart which indicates that the local gamma radiation is almost identical with the radiation resulting from the fallout from the tests.

Dr. LIBBY. It is described in Dr. Neher's article. It is a local condition, but he does describe it.

I am sorry. He does not give a detailed description. I can give you a general answer, Mr. Cole.

Representative COLE. That will be good enough.

Dr. LIBBY. I am sorry but these effects are complicated, the way thorium and uranium occur. You see that low point is due to the fact that the uranium and thorium are missing there. Uranium and thorium occur every place—in all granite rock, for example. That is why you seldom can get away from it, unless you are living on a sand, or a formation which has through the accidents of geology had the uranium and thorium washed out of it. So in order to tell you why that point is low I would have to consider the local geology very carefully.

Representative COLE. Do you know the locality of the point?

Dr. LIBBY. It is on the Laurentian Shield in Canada. But the exact locality is not specified. However, the reference to the article is given, and we could look it up for you.

Representative COLE. Thank you.

Representative HOLIFIELD. Are there further questions, now?

Senator ANDERSON. Doctor, I want to come back to this question of clean weapons which has concerned me quite a bit, and to the article in the U. S. News where they asked you that question.

One question was:

> Dr. Libby, at one time the Commission announced they were developing a clean bomb, and then many scientists argued with you and criticized and even ridiculed the statement of the Commission. How do you account for that?

Your answer was:

> Well, in most instances, they were not privileged to know what it was.

Then the question:

> They did not know what they were talking about, possibly?

You said:

> I did not quite say that, but they were not apparently mamiliar with all of the details of it.

In your speech you referred to a clean weapon. Do you still feel we have a clean weapon?

Dr. LIBBY. Well, sir, I think we all know what the facts are. It is a question of how you describe the situation. We certainly have succeeded in cleaning them up to a great degree.

If you call cleaner "clean," maybe you are telling a small fib. But we certainly have a cleaner weapon, as you know.

Now, the question of the adjective which is applied to that, all I can say is that perhaps we have used the wrong adjective, and perhaps we should say "cleaner."

Senator ANDERSON. It is very important, because headlines all around proclaimed this clean bomb.

You said:

> We have taken the problems of radioactive fallout into consideration—

and this is on fallout, and this is why I bring it up—

> in our weapons development program and have developed and tested clean weapons, that is, weapons in which the amount of radioactive fallout per megaton of explosive is very greatly reduced.

Dr. LIBBY. Yes.

Senator ANDERSON. If I took a little of this coffee I have here, and poured it in that glass, it is dirty (pouring coffee into glass). Then, if I pour a little bit over there (pouring coffee in another glass), that is clean because the amount of dirt, to use your language, is very greatly reduced.

Would that be a fair statement? You cannot get a better example of what happened.

Dr. LIBBY. I think we know what the situation is. We have very greatly reduced the fallout per megaton of yield, and that is imporant.

Senator ANDERSON. I am only trying to say that a lot of our people have been reassured on this question by this so-called clean weapon, and it is still a dirty weapon. We have also learned how to make our weapons even dirtier than they originally were.

I think it is dangerous to use terms like "uniformity" when we mean "nonuniformity," and "clean" when we still mean "dirty."

Dr. LIBBY. I think our responsibility, Senator, is to tell the truth as best we can. I think that we must stay as close to it as we can. We are all clumsy in the use of language, and I am one of the worst. We may have chosen not exactly the right adjective, but I think we have explained it enough now so people know what it means.

Senator ANDERSON. I hope so, because I have seen many headlines talking about clean weapons. And knowing you and respecting you as I do, I know you would be the last one to want to mislead. I just hope no one will think there is no fallout damage when it comes to the use of weapons in atomic warfare, because it will come.

That is all.

Representative HOLIFIELD. Are there any further questions?

Representative COLE. Mr. Chairman, before releasing Dr. Libby, I would like to express my own compliment to him for the very fine presentation he has given to the committee this morning.

So far as time will allow, they have been complete answers, unequivocal so far as possible, sound, and dispassionate, and I think he has rendered a great help to the committee, and his work on the problems of fallout hazards has been most beneficial.

Representative HOLIFIELD. Dr. Libby, we will excuse you at this time, but we hope that you are able to be present for the general conference this afternoon at the conclusion of the testimony.

Dr. LIBBY. Mr. Chairman, the Commission meets at a quarter to three, and I will try my best to get back here at 4 o'clock. I understand it is at 4 you are having your meeting.

Representative HOLIFIELD. We are reconvening at 2 o'clock in this room, and we will continue with our witnesses until they are through, and then we will have the conference. I am not sure just when it will be.

Dr. LIBBY. I will come back as soon as I can, Mr. Chairman.

Representative HOLIFIELD. Thank you, Dr. Libby.

The committee will be in recess until 2 o'clock.

(NOTE.—Several articles and statements by Dr. Libby have been printed as appendix 1, p. 1459. An article entitled "Radioactivity in Man and His Environment," by Dr. F. W. Spiers, appears in appendix 2, p. 1671.)

(A paper entitled "Radiation Dose to Man From Natural Sources," follows:)

RADIATION DOSE TO MAN FROM NATURAL SOURCES

Robert A. Dudley [1] and Robley D. Evans,[2] Department of Physics, Massachusetts Institute of Technology, Cambridge 39, Mass.

I. INTRODUCTION

Man has always been exposed to high-energy radiation from cosmic rays and naturally occurring radioactive substances in his body and in his environment. Twentieth century society has brought about an increase in his exposure to such types of radiation, from such sources as commercial radium preparations, medical X-ray machines, and fallout. This increase has focused attention on the deleterious medical effects of high-energy radiation. Since the effects of this additional radiation on man are very difficult to determine when the effects are small, several methods of predicting them have been attempted. One of these has been to compare the magnitude of the artificial radiation with the magnitude of the natural radiation to which man has always been exposed.

The types of radiation associated with cosmic rays and natural radioisotopes include α rays, β rays, mesons, and γ rays. The first two are important only when their source is deposited within the body; the second two are important only for sources external to the body. These natural radiations are very similar in their biological effects to the β and γ rays of artificial sources, although quantitative differences, considered later, do exist between α rays and the others.

The organs of the body receive different exposures, depending chiefly on the degree to which various radioisotopes concentrate in them through either physical or chemical processes. Thus the skeleton is exposed to radioisotopes chemically similar to calcium. The intestines are exposed to all radioisotopes which enter the stomach, even if the element is insoluble and is not absorbed into the bloodstream. The lungs are exposed to radioactive dust retained from inhaled air. All organs are exposed to cosmic rays and γ rays from the environment.

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[1] Oberlin College, 1943–45; University of Pennsylvania, 1945–46 (B. A., physics); Massachusetts Institute of Technology, 1946–51 (Ph. D., physics); Fulbright student, British Medical Research Council, radiotherapeutic research unit, Hammersmith Hospital, London (1953–55); lieutenant USNR, attached to Division of Biology and Medicine, United States Atomic Energy Commission (1953–55); consultant to Egyptian Atomic Energy Commission, Cairo, Egypt (1955–56); research associate, department of physics, Massachusetts Institute of Technology (1956–). Professional research experience: Use of radioactivity as a tool in various applications, particularly biological research; evaluation of Sr^{90} fallout problem while attached to United States Atomic Energy Commission; study of radiation dose to humans from natural and artificial sources. (Submitted by witness.)

[2] B. S. in physics, 1928, California Institute of Technology, Pasadena, Calif.; M. S. in physics, 1929, California Institute of Technology, Pasadena, Calif.; Ph. D. in physics, 1932, California Institute of Technology, Pasadena, Calif.; National Research Council fellow in physics, 1932–34, University of California, Berkeley, Calif. 1934 to present, physics department, Massachusetts Institute of Technology, Cambridge, Mass.: Assistant professor, 1934–38; associate professor, 1938–45; professor, 1945 to present. (Submitted by witness.)

On account of the many types of radiation and the many processes for deposition of radioisotopes in different organs, the general subject of the natural irradiation of man has many facets. The present study is confined to the natural radiation dose received by the skeleton and the gonads, these being the two organs of greatest interest in the case of fallout.

Many previous investigations can be drawn upon for pertinent data. In particular, reference may be made to the review articles of Libby,[1] Lowder and Solon,[2] Hultqvist,[3] and Spiers.[4] Although past studies by no means close the subject, reliable quantitative estimates of dose from the several important sources may now be given.

II. NATURAL IRRADIATION OF THE SKELETON

A. *Radiation originating within the body*

There are four natural isotopes which with their descendants contribute skeletal doses of comparable importance: potassium 40 (K–40), radium 226 (Ra–226), mesothorium (MsTh, or Ra–228), and radium D (RaD or Pb–210). In addition to these, there are many others (e. g., uranium, thorium, carbon 14) which are of negligible importance by comparison. The four important isotopes will be considered in turn.

K–40 emits β rays of 0.6 Mev. average energy and at a rate equivalent to 8.4×10^{-4} microcuries per gram of element K. Its γ radiation is of negligible importance compared with its β radiation in the case of K deposited in the skeleton. The abundance of K in adult bone is about 0.09 percent on a wet-weight basis.[5] From these data the dose rate to the skeleton may be calculated as about 8 mrad/year, where by definition 1 mrad$=10^{-3}$ rad$=0.1$ erg absorbed per gram of bone. Since the element potassium has a definite physiological role, it is controlled by the homeostatic processes of the body and is therefore expected to be found in the skeletons of all individuals in nearly equal concentration.

TABLE 1

Investigator	Description of samples			Equivalent RA content in bone ash	
	Number	Age	Origin	Mean	Standard deviation
		Years		*Microcuries per gram*	*Microcuries per gram*
Hursh and Gates [1,2]	21	33–85	Mostly New York State	5×10^{-14}	2×10^{-14}
	5	Stillborn	New York State	3.6	.7
Palmer and Queen [3]	50	32–93	United States, mostly Pacific Northwest.	1.6	1.0
Muth et al.[4]	14	27–74	Germany	13	7
Stehney and Lucas [5]	8	15–29	Chicago	1.2	1.2
	8	15–18	Lockport, Ill.	12	5
	30	Adult	Joliet, Ill.	6	(6)

[1] Hursh and Gates, Nucleonics 7, No. 1, 46 (1950).
[2] Hursh, Brit. J. Radiol., Suppl. No. 7, 45 (1957).
[3] Palmer and Queen, HW-31242 (July 6, 1956).
[4] Muth et al., Brit. J. Radiol., Suppl. No. 7, 54 (1957).
[5] Stehney and Lucas, International Conference on the Peaceful Uses of Atomic Energy, A/Conf. 8/1/852, U. S. A. (June 23, 1955).
[6] Variation depending on length of residence in Joliet.

Na–226, the familiar Ra isotope, decays to the noble gas Rn by alpha emission. Rn itself is radioactive, decaying with a 3.8-day half life by alpha emission. Subsequent descendants emit alpha, beta, and gamma rays, of which only the alpha rays are important dose contributors. Po–210, the last of the three Rn descendants which emit alpha rays, is of minor importance in this sequence, as its formation is long delayed by the intervening RaD isotope.

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[1] Libby, Science 122, 57 (1955).
[2] Lowder and Solon, NYO–4710 (July 1956).
[3] Hultqvist, Kungl. Svenska Vetenskapsakademiens Handlingar. Fjarde Serien 6, No. 3 (1956).
[4] Spiers, Brit. J. Radiol. 29, 409 (1956).
[5] Tipton et al., ORNL CF 56–3–60 (March 12, 1956), and personal communication (March 1957).

Ra is found in the skeleton as an unessential trace element by virtue of its chemical similarity to Ca. Its most important source is solid food, although in some areas its abundance in drinking water exceeds its abundance in food. Many measurements of Ra concentration in human bone are available. The important published measurements are summarized in table 1. From these results a mean of about 4×10^{-14} microcuries per gram in chosen (the higher values being given less weight since they represent, in part, a study of a selected high region). Variations about this mean by a factor of 3 to 5 are found, depending on local conditions. The extremes of skeletal Ra concentration, as set by geography and other variables, cannot be accurately specified; however, it is unlikely that large population groups exist at concentrations differing from this mean by more than a factor of 10.

The magnitude of the radiation dose delivered to the skeleton by Ra and its descendants is influenced by the migration of the noble gas Rn out of the bone and its subsequent exhalation from the body. No data are available on percentage Rn retention from natural skeletal burdens of Ra, but at artificially elevated burdens the retention is about 25 to 50 percent. Presumably the natural situation is in this respect very similar. Taking the lower figure, one computes an average energy of about 11 Mev. dissipated within the body for each Ra disintegration. The mean skeletal dose rate is then found to be about 3 mrad/year attributable to Ra–226 (and its descendants produced within the body).

MsTh, a Ra isotope occurring in the radioactive series associated with Th, is itself a beta emitter of 6.7-year half life. However, it gives rise to several descendants which emit alpha as well as beta and gamma rays. While some of these descendants have such half lives or chemical properties that a possibility exists for migration out of the bone, it seems probable from animal experiments that, in fact, little such migration occurs.

MsTh has not yet been sought with sufficient thoroughness to provide good experimental data on its natural concentration in the skeleton. However, since it is an isotope of Ra–226, the dose attributable to it and its descendants may be reasonably accurately estimated from our knowledge of the Ra–226 situation. The activity of MsTh and Ra–226 in soil and water is on the average nearly equal. Therefore the MsTh/Ra ratio activity in newly formed bone will be nearly unity, while in older bone the ratio will be lower as a result of the shorter MsTh half life. Assuming nearly all the MsTh decay products remain in the bone, the energy ultimately delivered to the bone subsequent to each MsTh disintegration is about 30 Mev. Therefore MsTh gives a dose almost 3 times greater than an equal activity of Ra–226. In children the MsTh dose is expected to be about twice the Ra dose while in adults it will fall below the Ra dose. Averaged over the individual's life, MsTh (with its descendants) is expected to produce approximately the same skeletal dose as Ra–226, or about 3 mrad/year. This value is probably subject to approximately the same variations with geography as is Ra.

RaD is a member of the Ra series, decaying by beta emission with a 22-year half life. Its daughter, RaE, is a 5-day half life beta emitter, and its granddaughter, RaF (Po–210), is a 140-day half life alpha emitter. Only rough estimates of the RaDEF dose to normal bone can as yet be given. It is clear that RaD descended from Ra in the bone will accumulate with time, but in a quantity which is unimportant compared with the other members of the Ra series. In addition to the RaD generated from Ra in the bone, however, there is now good reason to expect a greater quantity deposited from food and water. These expectations are based on experimental measurements of stable Pb in the skeleton, from which calculations can be made for RaD. Preliminary unpublished measurements have been made of RaDEF in bones at the University of Rochester.[6] These measurements and calculations indicate that the dose from RaDEF is of the same order of magnitude as the dose from Ra, and it is tentatively assigned an average value of 3 mrad/year. More precise measurements will soon be available from several laboratories.

It is known that in the production of biological damage, the effectiveness of alpha rays, per unit of energy dissipated, is different from the effectiveness of beta rays, gamma rays, and mesons. This relative biological effectiveness (RBE) of alpha rays relative to the other radiations varies with the effect under consideration. In evaluating the toxicity of low dose rates to the skeleton, probably

[6] Black, Personal Communication (May 1957).

the most pertinent effect to consider is the production of bone tumors. Mouse experiments at Argonne National Laboratory on bone tumor production by beta rays (Sr-90) and alpha rays (Ra-226) suggest that the alpha ray RBE is about 4.[7] This number is here adopted, with recognition that at lower dose levels in humans the correct value could differ either way by as much as a factor of 2 to 4. A better estimate will be available from the dog experiments now in progress at several laboratories in the United States.

B. Radiation originating external to the body

Cosmic radiation at the earth's surface is composed mainly of mesons, gamma rays, and energetic electrons produced from the primary cosmic rays high in the atmosphere. The magnitude of the cosmic ray dose increases both with increasing altitude and with increasing geomagnetic latitude. The determination of this magnitude has been the subject of several series of observations. While the results are not all in close agreement, the discrepancy is not serious with respect to the accuracy here required. Taking the dose rate at sea level and high geomagnetic latitudes as given by Burch's review,[8] and the dependence of dose on altitude and latitude as given by A. H. Compton,[9] the skeletal dose rates of table 2 may be calculated. Using other data, slightly higher values are obtained.

TABLE 2

Altitude	Geomagnetic latitude		
	0°	30°	50°
0 feet	23 mrad per year	24 mrad per year	26 mrad per year.
6,600 feet	37 mrad per year	39 mrad per year	46 mrad per year.
14,300 feet	76 mrad per year	83 mrad per year	100 mrad per year.

There are three important sets of naturally occurring γ-emitting isotopes in man's environment: the uranium 238 (U-238) series, the thorium 232 (Th-232) series, and K-40. In the case of a man standing on flat ground, the contribution of skeletal dose from each series may be quite accurately calculated when its concentration in the ground is known. Furthermore, direct measurements have been made of the combined external γ dose and cosmic ray dose at several points.[3,4,10]

The concentration of the radioactive elements in ground is quite variable, tending to be highest in igneous rock (e. g., granite), and lowest in sedimentary rock (e. g., limestone) which is the more common at the earth's surface. Typical geochemical data [11] yield the skeletal dose rates attributable to each radioactive series as shown in table 3. (In this table, a factor of 0.7 has been applied to surface dose rate to give skeletal dose rate, thereby allowing approximately for shielding of one part of the body by another.) Considerable variation is found for the concentration of each series in such rock, but to some extent these variations in individual series cancel out. Some rocks (e. g., dunite) yield less than 1 mrad/year, while others (e. g., U or Th ores) give several thousand mrad/year. An example of the latter situation is found in the monazite sand region of south India, where about 100,000 people live in areas where the skeletal dose rate from Th and U in the ground is several hundred mrad/year.[12] However, these extreme conditions are surprisingly rare, and it may be assumed that only a very small fraction of the world's population is exposed from the ground to dose rates more than a factor of 2 outside the range shown in table 3.

[3] Hultqvist, Kungl. Svenska Vetenskapsakademiens Handlingar. Fjarde Serien 6, No. 3 (1956).
[4] Spiers, Brit. J. Radiol. 29, 409 (1956).
[7] Marinelli, Personal Communication (December 1956).
[8] Burch, Proc. Phys. Soc. London A67, 421 (1954).
[9] Compton, Phys. Rev. 43, 387 (1933).
[10] Evans and Raitt, Phys. Rev. 48, 171 (1935).
[11] Rankama and Sahama, Geochemistry (University of Chicago Press, Chicago, Ill., 1950).
[12] Bharatwal and Vaze, Report to United Nations environmental radiation committee numbered A/AC.82/G/R.33 (October 19. 1956).

TABLE 3.—*Skeletal dose rate in radioactive series*

Rock	U	Th	KK	Total
Igneous	17 mrad per year	22 mrad per year	27 mrad per year	66 mrad per year.
Sandstone	5 mrad per year	12 mrad per year	11 mrad per year	28 mrad per year.
Shale	5 mrad per year	20 mrad per year	28 mrad per year	53 mrad per year.
Limestone	6 mrad per year	21 mrad per year	3 mrad per year	30 mrad per year.

In buildings the dose rate from local gamma emitters will be somewhat altered from its value outdoors. In framehouses a slight reduction is expected, but by only a small factor since thin wood walls do not provide effective shielding. In houses built of brick or stone, shielding will be appreciable. However, the radioactivity in these materials (if of the same composition as the ground) will more than compensate for the shielding, and from purely geometrical considerations the dose rate inside will be approximately double that outside.

C. Summary of natural skeletal irradiation

The several components of skeletal dose rate previously listed are collected together in table 4. The unit now used is the mrem/year, where

$$\text{mrems} = \text{mrads} \times \text{R. B. E.}$$

Dose in mrems is thus equal to dose in mrads, except for α radiation where the R. B. E. is taken as 4. The values in table 4 are rough averages, and considerable variations in individual components are found for different population groups. However, these variations partially cancel, such that only a very small fraction of the world's population is expected to differ from the mean total exposure by as much as a factor of 3.

For comparative purposes, the following artificial dose rates may be considered: A fallout Sr–90 body burden of one one-thousandths the industrial maximum permissible concentration (roughly the present average in newly formed bone[13]) corresponds to about 3 mrem per year. The external gamma radiation from fallout is of the order of 5 mrem/year at present.[13] Medical X-ray dose rates to the skeleton vary from zero in backward areas to roughly the same magnitude as total natural dose rate in medically advanced societies.[14]

TABLE 4

Source of radiation:	*Skeletal dose rate (mrem/year)*
K^{40} (internal)	8
Ra–226 (internal)	12
MsTh (internal)	12
RaD (internal)	12
Cosmic rays (external)	30
Local gamma rays (external)	60
Total	134

III. NATURAL IRRADIATION OF THE GONADS

The only important internally deposited natural radioisotope in the gonad is believed to be K–40. Since it is an essential element having a definite physiological function, potassium in the gonads will be found at closely similar concentrations in all individuals. The mean value is approximately 0.2 percent,[4] giving a dose rate of about 18 mrads (or mrem) per year.

The gonads are exposed to the same external irradiation from cosmic rays and environmental gamma rays as is the skeleton, and the dose rates will be essentially identical.

The total mean natural dose rate to the gonads is therefore about 110 mrem/year, with variations from 1 region to another as associated with variations in cosmic rays and environmental gamma rays. These natural dose rates may be compared with the dose rates from fallout gamma radiation and medical X-rays as given above for the skeleton.

[13] Libby, W. F., speech to American Physical Society, Washington, D. C. (April 26, 1957).
[14] National Academy of Sciences, The Biological Effects of Atomic Radiation, a report to the public, Washington, D. C. (1956).

(Whereupon, at 1:10 p. m., the committee recessed, to reconvene at 2 p. m., of the same day.)

AFTERNOON SESSION

Representative HOLIFIELD. The committee will come to order.

Our first witness this afternoon is Dr. Ralph Lapp, a well-known physicist and author of books on radiation and related subjects. Dr. Lapp, we are pleased to have you before us this afternoon, and you may proceed.

STATEMENT OF DR. RALPH E. LAPP, WASHINGTON, D. C.[1]

Dr. LAPP. Thank you. Mr. Holifield, I received your invitation to testify before this committee while I was in Japan. I cut short my trip in order to attend the hearings. May I say that I appreciate very much this opportunity to appear here. I would like to add that I am very gratified that your investigations to date have thrown so much light on the problem of radioactive fallout.

I regret that the rate of accumulation of data in my files is so great that I have trouble in digesting it. I hope that during the next month I will have the time to analyze properly the amount of data that has been presented and that I hope I may be able to give you a later statement. I have been out of Washington for 3 months, and I have had trouble keeping up with things. Nonetheless, I am delighted to be here and testify before your committee.

Representative HOLIFIELD. We have permitted other witnesses to present additional data to substantiate their statements, and that same permission will be granted to you.

Dr. LAPP. I believe that these hearings will stand as a landmark in the history of our knowledge about this relatively new phenomenon.

Now I would like to add a comment about Dr. Libby. Appearing as I do after Dr. Libby, I would like to comment on his contributions to fallout. Dr. Libby has not only stimulated extensive research in fallout investigations such as Project Sunshine, but he has also taken the initiative in publication of his findings. I feel very strongly that he deserves a great deal of credit for his work on fallout. Were it not for Dr. Libby, we might well be confronted with a considerably smaller body of knowledge about fallout than we have today.

Mr. Chairman, I do not intend to read this entire document because it is too long, and I will skip over certain sections, with your permission, I might just mention that my interest in fallout extends over quite a period of years. I do not claim to be a great prophet with regard to the ultimate effects of radioactive fallout, since I think any examination of my publications about the Bikini shot of 1946 will

[1] Nuclear Science Service, Washington 7, D. C. Physics, Buffalo, N. Y., August 24, 1917. Strong Foundation fellow, Chicago, bachelor of science, 1940, doctor of philosophy (physics), 1946. Instructor, Chicago, 1940–41; research associate metallurgical laboratory, Manhattan project, 1943–44; division director, 1944; assistant to laboratory director, 1945; assistant laboratory director, 1946; Argonne National Laboratory, 1946; science adviser, General Staff Research and Development Division, U. S. War Department, 1946–47; Deputy Executive Director, Atomic Energy Commission, Joint Research and Development Board, 1947–48; Executive Director, 1948–49; head, Nuclear Physics Branch, Office, National Research, Department of the Navy, 1949–52; Director, Nuclear Science Service, 1953–; industrial consultant and lecturer, 1953–; assistant, Chicago, 1940–41. A. A., physical sociologist, cosmic radiation; meson; bursts; showers; mass spectroscopy; radiological safety; nuclear radiation physics. (From American Men of Science, 1955.)

show that I was not terribly impressed with the phenomena of radioactive fallout at that time. I had to learn the hard way.

I might say that my interest in radioactive fallout was really stimulated by the 1954 Bravo test at Bikini. This was the test which resulted in radioactive contamination of the *Lucky Dragon No. 5*, a Japanese tuna trawler.

Based upon results which were made available largely by the Japanese, I began the publication of a series of articles on fallout at the invitation of the editor of the Bulletin of Atomic Scientists, and with your permission I will put these into the record, if you so desire.

Representative HOLIFIELD. They will be received and filed with the committee.

(The series referred to follows:)

November 1954, Civil Defense Faces New Peril
February 1955, Radioactive Fallout
June 1955, Radioactive Fallout III
November 1955, Global Fallout
September 1956, The "Humanitarian" H-Bomb
October 1956, Strontium Limits in Peace and War

Dr. LAPP. I am dividing my testimony into four parts: general remarks, local fallout, remote fallout, and I am so bold as to add some constructive proposals.

Under "general remarks," I would say I believe public confusion about fallout will continue to increase unless scientists can provide a quantitative or semiquantitative evaluation of the various hazards associated with fallout. Precision is probably not possible due to the nature of the hazards, and we may have to be content with numbers which vary by a factor of 2, 3, or even 10. This committee, I believe, has already performed a valuable service in narrowing the range of estimates made by individual witnesses.

Regarding disagreement among the scientists, I think that the public may well conclude that if scientists cannot agree upon hazard, then all is confusion. It would be nice if the scientists could all agree upon a quantitative estimate of the hazard, which could then be given out to the public. I maintain two unusual circumstances have combined to produce the current confusion on radioactive fallout.

First, the urgency of our times has focused attention upon problems for which science did not have textbook answers. Available knowledge was inadequate and research such as Project Sunshine had to be initiated to provide answers.

Second, the ordinary process by which scientists argue out their answers was interdicted by the complexity of the problem and also by secrecy. Scientists outside the Atomic Energy Commission have full-time jobs, as do those inside, and could scarcely be expected to tunnel into the complexities of the problem in a few leisure hours.

With regard to the responsibility of the Atomic Energy Commission, I think that if we consider these factors, the Atomic Energy Commission has responsibility for providing the outside world with the facts about fallout as promptly as they become available.

Representative COLE. Mr. Chairman, I wonder if Dr. Lapp would mind interruption while his statement proceeds?

Dr. LAPP. I would welcome interruption.

Representative COLE. We all agree with your statement that the Commission should make revelation of its information about fallout as promptly as it becomes available. Do you have any feeling that the Commission has failed in that responsibility, except insofar as fallout material may have a direct hearing on weapons information?

Dr. LAPP. Mr. Cole, I believe that your qualification there makes it difficult for me to answer the question. When you say "except insofar as it may have bearing upon weapons information," you, of course, bring into play the difficulty. May I answer it as specifically as I can with regard to the question of radioactive contamination of large areas.

I believe that the Atomic Energy Commission could have made available to the public far earlier than it did its report of February 15, 1955.

This is the report on the effects of high yield nuclear explosions. It took almost a year from the time that explosion occurred until this, I would call it nonquantitative report, appeared. It has taken additional time until the weapons-effects handbook—I believe called The Effects of Nuclear Weapons—will appear, presumably this month.

It is my personal opinion, Mr. Cole, that could have been done more expeditiously.

Representative COLE. But if it had been published earlier than 10 months after the test, would you not agree that any report would have been based on conjecture rather than on facts?

Dr. LAPP. No, Mr. Cole, I think the facts about this particular incident were fairly well known as of May 1954, and I believe if one looks at the record of the investigation of this committee—I am referring to the investigation of March 24, 1955, called The Atomic Energy Commission-Federal Civil Defense Administration Relationship—in this report published by your committee, the Atomic Energy Commission has stated that it had a summary report of the radioactive effects of the Castle series of tests available, I believe, in May of 1954. So based upon that testimony by the Atomic Energy Commission, I would say that they had the facts.

Representative COLE. The Commission, as you know—either the President or the Chairman of the Commission—did shortly after that test give a generalized report on the results of the test.

Dr. LAPP. Yes. It occurred, I believe, in my memory serves me correctly, on March 29 or 30, 1954.

Representative COLE. Some time in March.

Dr. LAPP. Yes. I personally do not feel that report was an adequate or quantitative evaluation of the hazard.

Representative COLE. Of course, it was not. It was not intended to be. But it was a general report to the public as quickly as general facts could be made known.

Dr. LAPP. It was a general report.

Representative COLE. As to the detailed lessons based on facts as they were observed might necessarily involve a period of time. It would seem that a period of 11 months, considering the importance of the subject under study, was not inordinate.

Dr. LAPP. I think we may disagree how long would have been required for this. I might say, Mr. Cole, if I am permitted a moment to look into my own notes here—that so far as I personally am concerned,

based upon data made available from the Japanese, I was able to publish in November 1954 in what I would call semiquantitative form an analysis of the hazard, and again in 1955 I was able to publish in the Bulletin of Atomic Scientists in February—I am sure I have it here, but it just escapes me—let me quote from memory.

Senator ANDERSON. I think I have a full file of the bulletins.

Dr. LAPP. It is the February 1955 issue.

Senator ANDERSON. That is probably the one I do not have.

Dr. LAPP. In this issue I believe I presented a quantitative estimate of the hazard which was more detailed in numbers than was the Atomic Energy Commission's release of February 1955. The situation, I realize, is difficult if one wishes to make a precise statement. But I do not think it was a precise statement that was needed. I believe something which would indicate the general magnitude of the hazard which civil defense faced would have been important.

By the way, I will come to this point in connection with later testimony.

May I continue?

Representative HOLIFIELD. You may proceed.

Dr. LAPP. I believe, then, that scientists, technicians, and officials of the Atomic Energy Commission must present reasoned and careful estimates of the hazards based upon factual knowledge. Reckless or nonsubstantiated statements do a disservice to the Atomic Energy Commission and to the Nation.

Representative VAN ZANDT. Dr. Lapp, you say reckless or nonsubstantiated statements do a disservice to the AEC and to the Nation. You make this statement under the title "The Responsibility of the Atomic Energy Commission." Are you charging that the AEC spokesmen made reckless or nonsubstantiated statements?

Dr. LAPP. I believe that the examples I have given here, particularly when we have a man like Dr. Richard Doan coming to Tokyo only last month stating that the bomb test did not have the slightest possible effect—I would be happy to place the entire quotation in the record—upon humans, that is nonsubstantiated. I believe it constitutes what may be regarded in the light of the great world importance of atomic energy as amounting to a reckless statement.

I have not charged that Dr. Libby made a reckless statement here when he said, "However, as far as immediate or somatic damage to health is concerned, the fallout dosage rate as of January 1 of this year in the United States could be increased 15,000 times without hazard."

Representative VAN ZANDT. Dr. Lapp, do you have all available information in this field?

Dr. LAPP. I have such information as is available to the public.

Representative VAN ZANDT. But you do not have the reservoir of information available that Dr. Libby has, do you?

Dr. LAPP. If I am to believe from Dr. Libby's testimony this morning that it was all unclassified, I would say that I must have.

Senator ANDERSON. It is either classified or you have it?

Dr. LAPP. Yes. I am sorry you are asking me something that is difficult for me because I simply do not know whether or not there exist things which are buried in secrecy.

Representative VAN ZANDT. I am not worried about classified information. I regard Dr. Libby as a member of the Atomic Energy Commission and therefore having at his fingertips all possible informa-

tion with access to AEC files. Therefore, when he makes a statement he must certainly make it based on the most comprehensive information available.

Now, I am asking this question: Do you have comparable information?

Dr. LAPP. No, sir.

Representative VAN ZANDT. By making the critical statement regarding the AEC spokesmen, in effect you compare yourself with Dr. Libby from the standpoint of availability of information. Therefore, it amounts to this, does it not, that it is simply your own opinion?

Dr. LAPP. It is my opinion based upon the facts which Dr. Libby has presented.

Representative VAN ZANDT. That is all, Mr. Chairman.

Dr. LAPP. I might say I can only go on what Dr. Libby has stated plus my knowledge of science and physics.

Representative PRICE. Mr. Chairman, may I ask Dr. Lapp a question?

Representative HOLIFIELD. Yes.

Representative PRICE. Is it merely your personal opinion, or is it your opinion based not only on information made available to you, but on consultation and association with other scientists?

Dr. LAPP. I have consulted with many scientists. I may not say in the specific preparation of this document because I was working until the early hours this morning doing this. I have in the past number of years consulted with many scientists of considerable note. I do not pretend to speak for scientists. But I might say this, Mr. Price; I have been encouraged by scientists.

Representative PRICE. You do not feel that you are alone in these opinions?

Dr. LAPP. I am alone right now, sir.

Representative PRICE. I do not think you have been alone. I think others have testified pretty close to the testimony that you started out to give here. I do not think you are alone.

Dr. LAPP. I would like to make this statement, Mr. Price. I have come here today to try to present such observations as I can to be of help to this committee. I do not want to be put in the position of challenging every statement that Dr. Libby makes.

Representative PRICE. I did not assume that you were challenging everything. The point I wanted to draw out was whether or not these were just strong personal convictions on your part, or whether or not you had arrived at your conclusions not only through the examination of the material furnished to you but through your personal studies and association with other leading scientists not only in this country, but in other parts of the world.

Dr. LAPP. I have recently consulted with scientists in Japan. I might say that one of the useful things that I accomplished in discussing these problems with the Japanese was that I think I explained some of the strontium measurements to the Japanese and tried to reconcile Dr. Libby's data with their own. I think it was useful because many of their figures were quite high as compared with Dr. Libby's. Although I am not yet sure that they believed everything I have said, I have been in correspondence with them since and I have tried to bring about some agreement in this field.

Again, I state that I personally owe a great debt of gratitude to Dr. Libby as I think the country does in providing us with these very valuable data.

Representative PRICE. I think we all agree with you on that. I think you were interrupted before you were permitted to give the examples that you were going to give on statements you referred to as perhaps "reckless" statements.

Dr. LAPP. I think we must interpret this and perhaps qualify it somewhat. That is, we are dealing now with statements made to the public. This is an area in which I have had some experience, namely, in the translation from the world of megacurie and the megaton into the world of ordinary understanding. I think Congressman and Senators know how difficult this translation can be. Dr. Libby's speeches have not always been masterpieces of simplicity for the press to understand. I have in many cases attempted to translate these speeches so that they might be more useful.

Senator ANDERSON. Doctor, now that you have had a chance to refresh your memory as to your fallout data, do you find you reported in the February 1955 issue of the Bulletin of the Atomic Scientists?

Dr. LAPP. Thank you. It was in the February issue of 1955 that I presented a quantitative estimate of the radiation exposure. I believe this curve showing cumulative dosage from fallout agrees with the data which are now presented some 3 years later in The Effects of Nuclear Weapons. I am going to testify about this.

Representative HOLIFIELD. You made that chart without access to classified information?

Dr. LAPP. The basis of making this chart was, if I may be permitted—this is a complex technical matter—to give you a complete explanation.

Representative HOLIFIELD. Make it simple.

Dr. LAPP. I have here the reports of the Japanese scientists. These are called Research in Effects and Influences of Nuclear Bomb Test Explosions. I want to point out that this has been published only recently, but it contains many of the scientific papers of 1954 upon which I based my deductions.

These reports might just be illustrative to show how much other countries are publishing. These reports are the research in the effects and influences of nuclear bomb test explosions in two volumes compiled by the Committee on Compilation of the Report on Research and Effects of Radioactivity, published in Japan for the Society for the Promotion of Science. There are about 1,800 pages of data in these reports.

Representative HOLIFIELD. The chart that you gave in February 1955 has stood up, then, against challenge, has it?

Dr. LAPP. I believe so. I have discussed this with Dr. Eugene Wigner, who is a scientist of outstanding note. We were much concerned about the tail of the curve. This tail of the curve is quite important because it is upon the tail of the curve that the long term persistence of radioactivity will depend. At the time I talked with him, Dr. Wigner had his doubts about how this tail may be extrapolated.

My own feeling is the recent data of Dr. Dunning, contributed to this committee, support this curve.

Representative COLE. Dr. Lapp, you will concede that your report of January 1954—or is it February?

Dr. LAPP. I think it came out in February.

Representative COLE. That was the same month that the Commission's report came out.

Dr. LAPP. Yes, Mr. Cole.

Representative COLE. But your report was based on studies of exposure to the Japanese fishermen.

Dr. LAPP. Yes.

Representative COLE. Whereas, that was only a part of the persons who were exposed to radioactivity. Is that not correct?

Dr. LAPP. That is correct, Mr. Cole.

Representative COLE. There were other people, the Marshallese, who were also exposed.

Dr. LAPP. That is true, but the data which I am concerned with is not data on people, but rather the actual number of curies of radioactivity deposited on the surface of the Lucky Dragon.

I might say, by the way——

Senator ANDERSON. Your report came out in the February issue which reaches the subscribers ahead of the first of the month. The Commission report came out in March and followed by a considerable period of time the publication of the data that you have there, did it not?

Dr. LAPP. I believe there was a difference in time of about 8 days. I prepared this, by the way, in December of 1954. I again received a tremendous amount of help from Dr. Libby's first speech of December 1954. It was very useful to me. I really feel that Dr. Libby has contributed immensely to this whole phenomenon of fallout.

Representative COLE. Mr. Chairman, before Dr. Lapp leaves this question of the responsibility of the Commission to make factual publication of its information, Dr. Lapp has indicated three examples which in his opinion represent a failure to fulfill that responsibility of frank and honest statements. He has characterized them as reckless and nonsubstantiated by the facts.

Dr. LAPP. Not all the statements.

Representative COLE. I notice you made an exception with respect to Dr. Libby's statement that it was not reckless, which leaves the conclusion that his statement nevertheless was unsubstantiated by the facts.

Dr. LAPP. With respect to that, Mr. Cole, I would wonder whether or not in the light of the testimony that has been received by this committee that Dr. Libby would still wish to subscribe to the statement that the fallout dosage rate as of January 1, 1955, in the United States could be increased 15,000 times without hazard.

Representative COLE. I am not seeking to argue with you. I am simply trying to establish clarity in the record.

Another question is this. These three examples which you have enumerated, do they constitute the only instances which have come to your attention where employees or representatives of the Commission have made statements on this subject which you feel were perhaps reckless to some degree, or not substantiated by the facts?

Dr. LAPP. No, Mr. Cole. I would have to go through my files. I believe there are many more.

Representative COLE. But would you agree that these are the most glaring instances of recklessness on the part of such people?

Dr. LAPP. Until I really combed my files and did a thorough job I can't answer the question.

Representative COLE. It would seem from the fact that these impressed themselves on your memory, and that you have cited them would indicate that they were the most extreme instances.

Dr. LAPP. No, I would not believe that. I think I cite them because they were readily at hand when I prepared this testimony.

Representative COLE. On this point, Mr. Chairman, it would seem to me that in fairness to these individuals whose responsibility has been questioned in this regard—Dr. Eisenbud and Dr. Doan—they should be given an opportunity to give whatever explanation they may have by way of defense against the charge of having issued a reckless and unsubstantiated statement.

Representative HOLIFIELD. I will respond to that. They will be given this opportunity if they care to.

Representative VAN ZANDT. Dr. Lapp, I am not doubting your ability as a physicist nor am I challenging your right of opinion, but for the first time during the course of these hearings we have the integrity of a Government agency challenged. That is what it amounts to. It has changed the complete tone of these hearings as far as I am concerned. We are not sitting here as an investigatory committee. We are sitting here for the purpose of trying to find the answer to this radiation problem. I think you would make a great contribution to these hearings if you would delete from your statement that charge you have made against the AEC and certain physicists employed by them.

Senator ANDERSON. I thought we were going to hear their replies to the charge. I think if Dr. Eisenbud will come in and prove as they laid down these criteria of proof the other day, that fallout to date from all tests would have to be multiplied by a million to produce visible deleterious effects, except in areas close to the explosion, that it would be very interesting.

Dr. Libby says he does not know what will happen when this comes out of the stratosphere. If Dr. Eisenbud would give us a short statement proving that it will have to be increased a million times, I think that would be very interesting. It would contradict a great deal of testimony from the Atomic Energy Commission itself.

Representative COLE. Mr. Chairman, I would point out by way of important emphasis that this statement of Dr. Eisenbud was made on March 20, 1955.

Dr. LAPP. Yes.

Representative COLE. So whatever response he may have to make by way of justification or explanation will have to be predicated as of that day, and not as of today.

Dr. LAPP. I would like perhaps to amplify this one bit, that is, that when I talk of the use of the word "reckless," there, I am speaking in terms of how the noneducated public will interpret statements made to them. That was my only purpose. I am not challenging the integrity of those people.

Representative COLE. Dr. Lapp, would you assert that the Commission's spokesmen in the scientific field were the only scientists who have made reckless and unsubstantiated statements on this problem?

Dr. LAPP. I would not. Reckless statements have been made by scientists who are not in the Atomic Energy Commission.

Representative COLE. Would you also say unsubstantiated statements were made by scientists who are not in the Atomic Energy Commission employ?

Dr. LAPP. I would readily agree to that.

Senator HICKENLOOPER. Mr. Chairman, may I ask Dr. Lapp, have you examined the full statements of Dr. Eisenbud and Dr. Libby and Dr. Richard Doan that you referred to here?

Dr. LAPP. Yes, sir.

Senator HICKENLOOPER. So it is based on that examination of the full statement that you quote out of context here?

Dr. LAPP. Yes, sir.

Senator HICKENLOOPER. And draw your conclusions that these add up to what you call reckless statements?

Dr. LAPP. Yes.

Senator HICKENLOOPER. I think perhaps one might argue that there is a degree of recklessness in dogmatic statements even by yourself in drawing these conclusions, not having been closely associated with the investigation of the data involved, and I rather question the advisability of the use of the word "reckless" in this rather difficult and quite ramified and uncertain field.

Senator ANDERSON. Dr. Lapp, would you not agree with me that there is a possibility that Dr. Eisenbud might not have been correctly quoted in the newspapers? I say that because Dr. Eisenbud gave us what I regarded as extremely fine testimony in his appearance before the committee. I thought it was scientifically based and carefully put together, and you have spoken very highly of Dr. Libby. I share your high regard for him, and I would hope that you might express to me how you feel about Dr. Eisenbud. I think he is a very capable and fine man. I hope that would sort of find some response in your system also.

Dr. LAPP. I was much impressed with the statement he made before this committee. The statement he made was in the Sunday News for New York of March 20, 1955. I believe that this illustrates one of the problems, Mr. Anderson, of the scientists and the press. He is really responsible for being careful in issuing statements to the press. I am not in any away attempting to attack the integrity of any people. I merely point out the impact which this will have upon the public.

So far as world interest in fallout is concerned—I will try to read this quickly—the committee may be interested in my observation that fallout has become an acute weapon of propaganda. For example, I found the Japanese scientists are actively studying the radioactivity of their tea, because of the assertion from the Chinese mainland that Japanese tea is radioactive. Apparently fallout does not occur in China. Some people in Japan are so keenly aware of fallout——

Representative HOLIFIELD. I think you should clarify that facetious remark, because in the print your manner of delivery might create the impression that you have said that it does not.

Dr. LAPP. I am sorry. It seems to me that here is an example of how radioactive fallout can be used as a weapon of propaganda in which a country which might stand to gain from its sale of a product accuses another country of having radioactive tea, and forces that other

country to engage in fairly laborious research project to find out how radioactive their tea is.

I visited this laboratory where tea was analyzed and saw the vast quantities of tea tested for radioactivity. I cite it merely as an example of how this can be used.

Representative COLE. What were the results of those tests?

Dr. LAPP. The tests are still underway. I believe the results will be given out within the year. I did not find any unusual radioactivity from the contact I had with Dr. Shiokawa. Some people in Japan are so keenly aware of the fallout that they actually take showers after being out in the rain.

There was a great public outcry against the British Christmas Island tests, but there was no great demonstration against the Soviet tests. It seems to me that this is a great victory for psychological warfare experts when they can induce selective sensitivity to fallout.

Representative COLE. Dr. Lapp, since you were there in Japan at the time of these tests or immediately prior to the tests, can you account for the fact that there was such a striking demonstration against the Christmas Island tests and yet there was no equal or even slightly proportional demonstration against the Russian tests?

Dr. LAPP. I might point out in all fairness that so far as the Japanese Government was concerned, they did protest the Soviet test, too, but as far as the public demonstration was concerned, I know of no such demonstration against the Soviet test. I think, however, one can give a partial explanation for this. This is in the fact that the Japanese people were immensely affected by the radioactive fallout in the fishing areas of the Pacific. After the accident of March 1, 1954, the Japanese Government went to considerable effort and expense to monitor the fish supply, which is a great source of protein for the Japanese. I think therefore they associated with the Christmas Island tests an effect upon their food supply.

Representative COLE. If that is so, how do you account for an equally demonstrative representation against the tests here in Nevada?

Dr. LAPP. I was not aware that they had such an outcry.

Representative COLE. You must have been aware that some Japanese people stormed the gates of the American Embassy in Tokyo in protest against the Nevada tests.

Dr. LAPP. I am sorry. I was traveling about Japan a good deal and perhaps I missed this one, Mr. Cole.

Representative COLE. This only occurred within the past month.

Dr. LAPP. I see. I did not know about this.

Representative COLE. Yes.

Dr. LAPP. I cite my reason as one factor.

Representative COLE. It is understandable that the Japanese people should be unusually sensitive to the hazard of radioactive fallout. That is very understandable. But it is difficult for me to understand why they should distinguish between the hazard of the British or American fallout without apparent protest to the hazard from Russian test fallout. Do you have any explanation for it?

Dr. LAPP. I could not profess to be extremely competent in this regard. I believe, however, there is a Communist Party at work in Japan. I believe that they use radioactive fallout as a political weapon. In fact, I believe that in the case of the survivors of the

Lucky Dragon, that their families were visited by representatives of the Communist Party who promised them money by way of being helpful. To my knowledge they never showed up with the money, but they promised them money.

Representative COLE. Were you not told when you were in Japan, as I was, that the newspaper reports of the Russian tests indicated to the Japanese people that the Japanese scientists had detected some unusual turbulance in the atmosphere? It was characterized as such, with no direct reporting that this was radioactive material. It was simply tagged as an atmospheric disturbance, whereas the press described the British and American tests as radioactive contamination of the air.

Dr. LAPP. I think they are probably allergic to American bomb fallout. This is rather interesting, in view of the fact that the data given to me by the Japanese show that some 70 percent of the total gross activity of the fission debris falling upon Japan is of Soviet origin, 20 percent from the Pacific tests and 10 percent from the Nevada tests.

Representative COLE. That was given to you by Japanese scientists?

Dr. LAPP. Yes.

Representative COLE. Have you ever seen that conclusion published in the Japanese newspapers?

Dr. LAPP. I gave a number of interviews when I was in Japan, and I pointed this out, but I unfortunately do not read Japanese, and do not know whether they reported it. It may have been. I stated the fact that there ~~is~~ greater fallout on Japan from Russian tests than from United States tests or United Kingdom. I am unaware that it was published. It may have been.

Representative COLE. This is a rather remote and roundabout route by which to provide these conclusions for the information of the Japanese people. I can hope that the Japanese reporters who are present here today may report to their respective newspapers published in Japan the conclusion which was given to you while in Japan, a conclusion by Japanese scientists—that their tests of the contamination of the atmosphere over Japan was caused 70 percent from the Russian tests and the balance from American and British tests.

Dr. LAPP. I think, Mr. Cole, I hope that these facts are reported in the Japanese press. I have reason to believe they will be. I think it is entirely reasonable that the fallout upon Japan should be predominantly from Russian tests because of the greater tropospheric fallout which Dr. Libby explained this morning. The greater tropospheric fallout from these tests will occur from the Russian tests. Because of the fact that they are in the air mass trajectory from the Soviet test region, this fallout will occur upon Japan sooner than upon other parts of the world. Because of the freshness of the fallout, there will be a greater radioactivity.

Representative COLE. That being a scientifically provable fact, then, is it not appropriate that the Japanese should be far greater concerned over Russian tests and the hazard of Russian tests than the tests by the United States and United Kingdom?

Dr. LAPP. If I were a Japanese I certainly would be.

Representative HOLIFIELD. It is not strange, as far as I know that the Atomic Energy Commission has never revealed this very important fact, and that the first knowledge I have of it is as of today? If

they have revealed it, I am unaware of it. We are being subjected to propaganda as you have stated by the Communist Party seizing on the part of this technical information which is to their advantage, and using it in Japan. Why is not this a piece of scientific fact that could be used on our part and let the world know that 70 percent of the fallout on Japan comes from Russian origin, if that is a true fact.

Dr. LAPP. Mr. Chairman, I am trying to strain my memory to remember this, but I believe that the National Academy of Sciences report contains data on relative fallout. I am trying to remember if it was on Japan. I think that it does contain data on this, but it is probably buried in the scientific literature.

Representative HOLIFIELD. I have read the report fully, and if it is in there, I have either forgotten it or I could not understand it, the way it was stated.

Dr. LAPP. I do not think it was in the summary or general report, but was in the greater compilation of the appendix, the physical measurements.

Representative HOLIFIELD. But a report of the American Academy of Sciences is not a publicized report of the Atomic Energy Commission.

Dr. LAPP. I might say this. I cannot remember any Atomic Energy Commissioner making a point of this in his speeches. I read most of the speeches quite carefully. I do not remember this being made.

Representative COLE. You do not know whether this information has been made available or known to the Commission, do you?

Dr. LAPP. I could not state positively.

Representative COLE. Of course, if the Commission had wanted to or happened to it could have found the same information which was given to you. I do not question that. But there is no evidence, or is there, that the Commission had this information?

Dr. LAPP. I am sorry, I am not competent to discuss that.

Representative COLE. Mr. Holifield referred to this conclusion of yours, or this information which was given to you as having an element of propaganda. I am sure he did not intend to use that word in the strictest sense, because this information in my opinion is not propaganda. This is a statement of the scientific facts as resulting from the examination of competent Japanese scientists.

Representative HOLIFIELD. I will accept the gentleman's amendment, if he will allow me to substitute the word "publicity" in the place of "propaganda."

Representative COLE. Yes. It should be publicized generally, and widespread, and that is why I am talking about it as much as I am in the hope that the reporters who are present will make certain that it is publicized fully and in all of the newspapers in Japan.

Representative HOLIFIELD. I had assumed that Project Sunshine was all over Japan, too, and we knew about this.

Representative VAN ZANDT. Mr. Chairman, I would like to ask Dr. Lapp another question.

Dr. Lapp, the Japanese scientific documents you have at your elbow, have you gone through them as yet?

Dr. LAPP. I have gone through a great many articles, yes. Some of them did not interest me too much, so I skipped over them. I have gone through a great many.

Representative VAN ZANDT. Dr. Lapp, do you think all the information contained in those Japanese documents is original or has somebody borrowed the information from the American scientific family?

Dr. LAPP. I would say that much of it is original and of course scientists borrow wherever they get the information. That is the nature of science. It is an international community. There are a great many references in each paper to the United States reports, and to other nations as well. I think that the United States reports predominate.

Representative VAN ZANDT. In other words, the volumes contain much United States data?

Dr. LAPP. No, they reference United States data. For example, if we have a radiochemical technique for detecting a particular radio element, then they would use the technique and reference this. I want to point out, however, that the Japanese scientists, despite the fact that they are not many in number as compared with other nations, have quite a history of excellence. I had the great pleasure of talking with Professor Kimura only two Sundays, ago, and he was very kind to show me some of his original work, and I know it was of very high quality.

Representative VAN ZANDT. Dr. Lapp, have you had made available to you a copy of a Russian scientific document entitled, "Preliminary Data on the Effects of Atomic Bomb Explosions on the Concentration of Artificial Radioactivity in the Lower Atmosphere and Soil"?

Dr. LAPP. I have not. (See p. 1209.)

Representative HOLIFIELD. It was just called to my attention by the chairman that one of our outstanding scientists in California is an American Japanese.

Dr. LAPP. The Japanese scientists are particularly competent in the field of theoretical physics. One of the greatest theoretical physicists in the world is Professor Yukawa, famous for his discovery of the meson.

It is inherent in the very nature of the biological research into the effects of radioactivity upon humans that a high degree of accuracy is not attainable, especially on human experience basis. As Dr. Langham, of the Los Alamos Laboratory, has testified, human experience with retention of radium 226 is the basis for setting upon a maximum permissible concentration for radiostrontium. Yet our actual experience is confined to a small sample of acutely exposed individuals and a small sample of less acutely exposed people.

Actually, our concern should focus not upon acute effects in man, which are highly unlikely from peacetime bomb testing, but rather with the chronic, debilitating, long-term effects from irradiation of humans. We must be conscious of the need to appraise long-delayed effects, say 50 years after entry of radio elements in the body. Here our knowledge is quite limited.

SECTION F. RADIATION LIMITS FOR A GLOBAL POPULATION

I would like to stress the fact that consideration of safe limits for irradiation of the world's population is essentially a new problem. Prior to the awareness of global fallout, the International Commission on Radiological Protection made its recommendations for those who would be exposed to radiation in pursuit of their occupation.

Such groups initially were numbered in the hundreds and then in the thousands as atomic energy came of age. Individuals within such groups were healthy adults exposed to known and restricted hazards; they were subject to administrative controls and medical supervision.

In setting up limits for a total population, we must take into account the varying radiosensitivity of individuals, the complete spectrum of age, the persistence of the hazards, the lack of medical control, the varying degrees of health of people, and the variety of their diet. Yet it was not until last year that the Atomic Energy Commission introduced the difference between an occupational MPC and a global MPC into its releases on fallout.

May I explain that briefly? Dr. Libby, in his speeches referred always to the MPC. I am not accusing Dr. Libby of deliberately trying to mislead anybody, but, from the standpoint of the ordinary layman reading these things, there was no distinction between a maximum permissible concentration for occupational workers and for the world population. I think this is one of the things that is necessary when you are putting information out to the public—that you must distinguish between these different units.

Representative COLE. Why is that, Doctor? What is the difference whether the individual absorbs or is exposed to the maximum MPC in an atomic plant or as an employee or whether he is exposed to it outside. Why do you feel that a distinction should be made?

Dr. LAPP. I think the distinction should be made on the basis, first of all, of the difference in radiosensitivity of the individuals. You are dealing with the total population now when you are dealing with the global risk. You are dealing with people who have no medical supervision. You are dealing with people who are of different ages. I believe that the International Commission on Radiological Protection recommends a factor of 10 and others believe it should be more—a factor of safety—when dealing with the total population than when you are dealing with the small population, the occupationally exposed population. These are the recommendations of the international body on the subject.

In view of the nature of our knowledge and the totality of the sample with which we are dealing, I would urge a big factor of safety in setting limits to bomb testing. It would be tragic to find someday that we had erred in setting the limits.

Perhaps I might explain, in response to Mr. Cole's question, the probabilities involved here. Supposing that the probability of damage were only one in a thousand and you only had 10 people working in the laboratory; this would be a small risk for 10 people. But if you had one in a thousand and apply the same statistics to a total population of 2 billion people, obviously, you have a very different situation. It is part of the philosophy that goes into establishing such figures.

Representative COLE. When you say, in your opinion, that we should set a big factor of safety, don't you mean that we should set a very low factor of safety in order to be on the safe side?

Dr. LAPP. Perhaps my language is not clear there.

Representative COLE. You mean the same thing.

Dr. LAPP. Yes.

Representative COLE. We should be ultracautious in fixing a factor of safety.

Dr. LAPP. Yes.

The Soviet nuclear tests: I felt that the committee might be interested in learning some miscellaneous data I picked up in Japan. I learned that the Japanese scientists collected sufficiently active samples from the Russian tests to perform radiochemistry upon the bomb debris. I was informed that five Soviet tests produced a fallout on Japan from which scientists measured and identified the presence of radioisotope uranium 237. The Soviet explosions were characterized by such fallout that they were judged to be in the megaton range. These estimates are subject to considerable uncertainty, but one authority told me that he estimated that at least 2 bomb yields were in the range of 10 megatons.

Two Soviet nuclear tests were observed to originate in the Arctic region, whereas the remaining tests took place in a region estimated to be the Ozero Balkash, which is southeast of the new coal area of Karaganda. The air-mass trajectories from central Siberia frequently sweep across the islands of Japan, especially Hokkaido. They also produce tropospheric fallout over the United States, as well. Here in Washington you could swipe a Kleenex over a car top and cause a Geiger counter to respond. Perhaps that is a qualitative statement, because the normal counting rate of the counter is 20 counts a minute, and the counter may go up several times over that. But it was readily detectable by even such a simple analysis as this.

Representative VAN ZANDT. Dr. Lapp, that last statement you make; is that a fact or is it just hearsay?

Dr. LAPP. I was told this by one of my scientific colleagues, since I was not here at the time. I have no reason to doubt it.

Representative VAN ZANDT. Then we are getting it secondhand.

Dr. LAPP. Secondhand, but I can give you the source, if you wish. I have seen the measurements done myself. The presence of uranium 237 in the Soviet fallouts proves that the Soviets have achieved a compound fission-fusion or so-called multiple-stage weapon. According to my information, this was first accomplished in September 1954.

Representative COLE. First accomplished where?

Dr. LAPP. In the Soviet Union. The next statement has already been made.

I would like now to talk about the problem of local fallout. I am not going through all of section A, because what I am doing there is trying to explain a term which I coined some time ago in order to eliminate confusion in popular translation with the megacurie. The term I used is the "eternity roentgen" per square mile. It turns out that this particular compound unit is extremely easy to use in estimating the roentgen exposure of people in a bomb area. I will not go through all of this.

Could I have the chart of Dr. Shafer put up? (See p. 119.)

Last week Dr. Shafer testified before this committee about an attack of 2,500 megatons of bomb yield upon the United States, and specified that they were surface burst dirty weapons. By dirty it is meant that the ratio of fission to fusion is high. I assumed that 2,000 tons of fission products deposited locally. This I calculate as 12 billion roentgens per square mile.

Representative COLE. You have characterized these as dirty weapons, Dr. Lapp. How would you evaluate the content or force of a weapon which might be called a clean weapon? As you know, there

has been a considerable discussion of the maining of the words dirty versus clean, clean versus cleaner. You have used the words "a dirty weapon." Are there degrees of dirtiness?

Dr. LAPP. Yes. I would estimate the degree of dirtiness as the ratio of the fission to the fusion release in the bomb.

Representative COLE. So that in your opinion it is possible to fabricate a weapon which is clean.

Dr. LAPP. Relatively clean. To answer the question precisely, the question would be whether or not you could fabricate a weapon in which there were no fission products.

Representative COLE. You could not do that.

Dr. LAPP. This I do not believe is possible. So you can fabricate a cleaner weapon.

Representative COLE. Would you conclude that it is possible to fabricate a weapon which is so clean that dirtiness is not of great importance?

Dr. LAPP. I do not have the facts on which to answer that question.

Representative HOLIFIELD. No one else has come before us who had the facts to answer that question. Dr. Graves answered the exactly opposite. He said there is no such thing as a clean weapon. There are varying degrees of dirtiness.

Dr. LAPP. If I may muddy the water a little more, I would say that one must also include here in this argument about clean and dirty bombs the operational aspects of the weapon. You have first the problem of how much dirt is actually produced by the bomb, and then you have second the problem of how much dirt comes down. If you test the bomb at high altitude and set it off at high altitude, then you minimize the local fallout.

So we have two problems here. To get an index of dirtiness of an actual weapon tested you have to apply some formula here for the eternal dirtiness, and then the operational dirtiness. So it is a complex thing.

Senator ANDERSON. Doctor, if you had developed a type of bomb that would not explode high in the atmosphere but equipped it to explode close to the ground, would you add or subtract from its dirtiness? Would you not add to its dirtiness, so-called, to get down where it picked up particles of soil?

Dr. LAPP. When you pick up, I would call it the ballast, the soil debris, it tends to maximize the local fallout and make the weapon dirtier.

Senator ANDERSON. Therefore, if we were to prove whether our interest was clean or dirty weapons, we would need testimony from the military as to whether they had or had not developed weapons that would explode closer to the ground.

Dr. LAPP. I believe that the operational aspects, namely, the altitude of detonation would be very important. The testimony that Dr. Shafer gave is illustrated on this chart in which the varying degrees of contamination are illustrated by different colors. I am not going to use the exact figures. I merely wanted to illustrate the type of continental contamination that you get into if you have an attack with 2,500 megatons of dirty bomb.

I would take an example and then discuss the implications of dirtiness in a strategic attack upon a country. If you assume that 50 percent of this dirtiness falls out locally, you have then 1,000 megatons

of fission products concentrated upon Northeastern United States, the region which I use, and much of the region would then be subject to a fallout of 10,000 eternity roentgens.

May I define that? The eternity roentgen is a unit of the exposure from one hour, considering that the time of fallout to deternity, it is divided up on the following time schedule. May I jump to section C, which is the persistence of fallout. May I bypass the comments about this and jump directly to the data.

If you have 10,000 eternity roentgens, this amounts to a rate of 2,000 roentgens per hour at one hour. Here is the time table of the delivery of the roentgens to a person exposed in the open on a flat area. From the first hour, assuming the fallout occurred, then, to the end of the first day, there would be 4,700 roentgens of exposure. That would be about 1C times the lethal dose for an individual. So he had better be somewhere besides the top of the earth.

From the end of the first day to the end of the first week, there would be a dose of 1,730 roentgens, or about 3 times the lethal dose. From the end of the first week to the end of the first month there would be an additional dose of 920 roentgens which would probably also be lethal. From the end of the first month to the end of the first year, there would be slightly over 1,000 roentgens. From the end of the first year to 50 years would be an additional 840 roentgens, but I make the note that weather and terrain would make a significant difference in cutting down that dose.

This point, I think, is worthy of stressing, because of its great implication for civil defense and for analyzing what the ultimate consequences of an attack upon a country are. You have the problem here of confronting 920 roentgens from the first week to the end of the first month after the attack, and even after that is over, you have the problem of the 11-month dose of slightly more than 1,000 roentgens. After the first year, there would be a smaller dose of some 840 roentgens that would be the theoretical maximum.

Having listened to Dr. Crow's testimony yesterday, I proceeded to calculate late last night or early this morning—I forget which—just what this would mean because Dr. Pollard and the others pointed out the consequences of an attack upon the United States, and Dr. Libby this morning emphasized the great consequence of a nuclear attack itself.

Representative HOLIFIELD. I am glad you are going into this, because the chairman has received any number of telegrams—in fact, 1 was delivered at 6 o'clock this morning when my doorbell was rung by a messenger, and they got me out of bed to give it to me—condemning the committee for not going into the effects of a war and possible multimegatons. Apparently the people writing in are not aware of all the testimony, and are not aware of the fact that the extrapolation from this information can be applied to multimegaton exposure of the population. So I am glad you are bringing that point out.

Dr. LAPP. I just had the opportunity of discussing this at lunch with Dr. Crow. I have some adjustments to make with it, but nonetheless in view of the nature which he will agree is fairly approximate data, I will let these figures stand.

What I did here was the following: I assumed that by some means we were extremely fortunate in having the attack. I mean we were

fortunate after the attack in being able to hide our people from acute dosage during the first month.

I went on to calculate how much dosage these people would get if we took a generalized smear out of the radioactive fallout over the continental United States. Here is where the eternity roentgen square mile is an extremely useful concept, because I can simply make the calculations very quickly.

What I did was to arrive at through this mathematics adjusting for weathering at an average dose of 400 roentgens for the average exposure to every American who survived an attack of 2,500 megatons, with some 2,000 megatons of fission products released.

Senator ANDERSON. Doctor, it is hard to translate these things back and forth. The other day when Dr. Russell was testifying, we got into the question of the fact that if a person was exposed to so many units, it might shorten the lives of children by a certain number of days and so forth. Are these roentgens what you have called eternity roentgens the same thing he was talking about?

Dr. LAPP. I have adjusted. I used the eternity roentgen just for the simplicity of calculation. I have gone back to the pure roentgen.

Senator ANDERSON. 400 roentgens would be a very substantial dose for not only the person exposed, but would have very, very substantial effects upon children that generation and continuing, as he pointed out, for several generations to come. Geneticists did not stop when the individual was exposed. They went right along through the several generations.

Do I understand that this is a sufficiently large dose so that it would shorten the lives of those children depending upon whether you used the upper or lower limits of Dr. Russell's table from 10, 15, or 20 years or something of that general nature, if the father and mother got 400 roentgens.

Dr. LAPP. I am not familiar with Dr. Russell's data. I am sorry.

Senator ANDERSON. Are these the same roentgens he was thinking about?

Dr. LAPP. They are the same roentgens. The roentgen is the roentgen and if he was talking about the roentgen, this is the same roentgen. If he was talking about the neutron unit, this would be different.

Representative COLE. Was he talking about the neutron unit?

Dr. LAPP. As I judge from what I heard in this hearing, I thought he was testifying about the neutron unit. Is Dr. Russell here?

Representative PRICE. He definitely stated he was talking about the neutron.

Dr. LAPP. You all saw Dr. Crow's table, I believe, showing the expected genetic effect upon the first generation and upon the total succeeding generations. What I did was to calculate how this would scale up if you had an aftermath of a nuclear war under this very optimistic condition that the people who survived got no radiation for the first month, but then were exposed to a cumulative dose of 400 roentgens over the period of a generation—a reproduction generation. The first generation, according to this table, would have 1,600,000 physical and mental defects, a total of 16 million for all total generations. There would be stillbirths and childhood deaths of 4 million, a total of 120 million for all generations. Embryonic and neonatal deaths, 8 million, and 140 million total, and a much larger but unknown number of intangible defects. (See p. 1021.)

I think I would have to change this on the basis of what Dr. Crow told me, instead of 2 out of every 10 children in the United States in the first generation would be genetically defective, it would have to be 1 out of every 10. The sum total of all deferred deaths from the attack would be 272 million. I believe that should be scaled down because of what he told me to somewhat less than that. I believe about 150 million. I will have to check with Dr. Crow on this.

This, then, is the kind of genetic consequence from an attack which, according to Dr. Shafer, would have produced, I believe, of the order of 80 million deaths.

This kind of a calculation is to me a rather awesome one. When I was thinking it over and talking with Dr. Crow at lunch, I was thinking, supposing this happened and you tried to imagine what you could do about it in advance. Of course, one of the things you could do is to arrange for shelter of people, but shelter of people for the time periods we have in mind is going to be increasingly difficult even if we have the funds for it. I hope I can be forgiven for injecting in the testimony at this time a thought which I had. It is the nature of nuclear warfare which provokes this. There perhaps might be a national objective to have a stockpile of human sperm—the male sperm—which would be stockpiled at strategic locations in the United States for providing at least on the masculine side a pure line of nonirradiated sperm. I realize that this may seem like a bizarre suggestion. I understand according to biologists that you can keep human sperm viable for considerable periods of time. If you did that, then I believe you would cut your genetic consequences more than in half, because I understand that the female is less sensitive to radiation than is the male in terms of the sperm versus the ovum. This means you could cut in half or less than half—you could probably cut down between a half and a third—the consequences to future generations. It may even be, and here we would have to do a great deal of research, if you could continue the integrity and viability of the sperm through more than one generation, you then could continue nonirradiated nonmutated sperm through more than one generation.

I realize here I am getting a little fanciful. I am merely injecting this into our discussion, the kind of things you come up against when you consider the awesome consequences of nuclear warfare.

Representative COLE. Dr. Lapp, you have, of course, posed a most intriguing and bizarre as well as fanciful suggestion, but it occurs to me that is it not likely that if such an event occurred in which such a large proportion of our population were affected to the point where it would be advisable or helpful if we could have a reservoir of sperm, would not that concentration also affect other animal and plant life to a degree in which even though we were able to reproduce the human race, we nevertheless could not survive because of insufficient food, water and such?

Dr. LAPP. Mr. Cole, I believe that so far as the crops reproducing themselves are concerned, this would not be the fundamental problem, because I believe the mutation rates are quite different in crops and some of the other animals. I would like the specific question of the relative biological effect genetically to be addressed to one of the geneticists.

Representative HOLIFIELD. Of course, the suggestion you have made is an unusual suggestion, but we are dealing now with a world in

which the possibility of releasing these quantities of megatons of fission are either here or will be here very soon. It just accents the gravity of nuclear warfare and this is, of course, one of the things which mankind has to deal with for survival of the human race. If we are going to have this kind of warfare, these are the problems that are presented.

Dr. LAPP. Mr. Holifield, I personally believe that projections of the probable consequences of a nuclear warfare are in themselves the greatest deterrent to war. But this has to be absorbed on both sides of the Iron Curtain. From what Dr. Muller said yesterday about the state of genetics in the Soviet Union, I think it might be quite advisable to make sure that no one in the Soviet Union is in doubt as to the consequences of a nuclear war.

I am at a loss to say how to do this, but it might be accomplished through a good conference on genetics to which the Russians were invited.

Representative COLE. Dr. Lapp, you have indicated that since your luncheon visit with Dr. Crow you have revised your conclusions from your original script which estimated an effect on the first generation of 2 out of every 10—since your luncheon visit with Dr. Crow you have revised that downward to 1 out of 10, which is a very striking revision, a difference of a hundred percent; if that can be the consequence of a luncheon visit with Dr. Crow, might it not be conceivable that if you spent a dinner evening with Dr. Russell and other scientists, you might further revise your figures one way or another, or if you spent a week with them there might be even a greater revision?

Dr. LAPP. I am not sure that the degree of revision would be proportional to the time of contact with these individuals.

Senator ANDERSON. You would find out that at one time the Atomic Energy Commission had a figure of 50 which in a short time they brought down to 2. Maybe they should go to dinner also.

Dr. LAPP. I apologize to the committee for introducing this figure, but I did not understand from Dr. Crow's testimony yesterday that these two figures he gave were not mutually exclusive.

Representative HOLIFIELD. Mutually what?

Dr. LAPP. The point was that in the column of data he presented, he had two figures, one of which actually enveloped the other. I had thought they were separate. I was in error.

Representative COLE. I was impressed by the apparent fact that a casual luncheon conversation could result in such a striking revision of your conclusions.

Dr. LAPP. The same factor of two would have been produced from a single sentence he gave me when I showed him the results. This is entirely in the nature of how scientists iron out these differences. They talk with one another.

Representative HOLIFIELD. I might say that we have had many statistics given to us that range all the way from a factor of 2 to a factor of 10 or 15. So this variability in your figure is not unusual to other testimony we have had.

Dr. LAPP. I believe in presenting the data Dr. Crow mentioned that it probably was not exact by a factor of three. I suspect it could be even more. I am merely using this as an indication. I would claim no precision.

Representative COLE. But his admission that his calculations might be in error by a factor of 3 was based upon long periods of sober study and concentrated thought, and that even after such long period of study, he came to certain conclusions which he admits might nevertheless be in error by a factor of 3.

You have indicated that your statement today was composed in the wee hours of last night and the early hours of this morning, as well as the luncheon visit. Therefore, might it not be reasonable to conclude that your estimates might be in error by a factor of as great as 15 or 20?

Dr. LAPP. I think the physical calculation I have made I would be willing to stand on. I think it is correct within the method that I have estimated the figures are accurate. The uncertainty comes in when I apply it to the genetic data. Actually, I think, Mr. Cole, I have underestimated the situation deliberately. I have taken lower values, not upper ones.

One of the most important things that I wanted to discuss at least in my opinion before this committee, and I hope I can jump to it is the problem of the present test rate. I have indicated in the last page of this testimony my rough estimate—and I emphasize that it is a rough estimate upon all the data available to me of the rate of testing, the injection of fission products into the stratosphere over the past period of time since the first bomb was exploded in 1945.

This is a semilogarithmic plot. That is, the scale on the left in logarithmic. Starting at the bottom, the lowest value given in one megaton, and it runs up to 10 and 100 megatons of fission products injected into the stratosphere. To be perhaps redundant, may I explain that this is the number of fission products associated with the explosion of 100 million tons of TNT equivalent fission energy in a bomb. The reason why nothing appears up in 1951, up to the small value which I indicated as two-tenths of a megaton, is that prior to that time we were in the fission domain of weapons—the pure fission domain of weapons—in which the weapons had their energy released by a chain reaction in fairly expensive uranium 235 or plutonium 239, material which, as a rough estimate, we can say cost $10,000 a pound. The price is somewhat less now. Up until that time we were dealing with relatively small weapons and because of the fact that these weapons did not have great explosive power as compared with the megaton class weapons, their fission debris was restricted to the lower atmosphere or to the troposphere.

This meant that so far as the global aspects of radioactive contamination were concerned, and considering the method of transportation, the total amount of fission debris deposited was small and negligible. It was only when we entered into the era of the megaton in weaponry that we started to get into a situation where the injection of radioactive fission products into the stratosphere because of consequence and could be measured remotely all over the world.

If we look at that, we will find in the injection corresponding to the test in 1952—by the way, this includes the United States and U. S. S. R. I have not included any contribution from the British—and in 1953, relatively little testing so the curve goes down. In 1954 was when we had the Castle series of tests with a total estimated contributed fission yield of 30 megatons into the stratosphere.

The next year was cut back to a total of 3 megatons and then the next year, 1956, it went up. If you will permit me to go to another curve, I can give you the estimate. It is to about 14 megatons. I am not in a position to make an estimate of the 1957 contributions since I am not aware of what the British have contributed with the Christmas Island test, and am only partially informed of what the Russians have contributed with their spring series of tests. But I believe that this chart does illustrate some of the problems you have in discussing the present rate of testing.

One way—and this is the way Dr. Langham suggested—is that you simply average the testing over the past 5 years, which would mean we take these 5 bar graphs and add them up and divide by 5.

I believe that when this is done that this curve will come out to somewhat more than Dr. Langham estimated. I am not sure that in just a minute I can give you the answers to that.

Senator ANDERSON. Very close to it.

Dr. LAPP. It is close to it.

Representative HOLIFIELD. Of course, this excludes the Russian test.

Dr. LAPP. This includes the Russian test. I have been conservative with regard to the Russian test, probably overly conservative, because of the problem of estimating just what fission yield they had in the weapons, how much was injected into the atmosphere, and how much dumped out stratospherically.

Senator ANDERSON. Dr. Langham said it came to about 50 megatons in the past 5 years. If you add your figures, it comes out to about 50 and a fifth of that is 10 megatons per year.

Dr. LAPP. It is roughly 50, sir. I do not have the complete detail here as to how I arrived at all these figures. The reason for the Castle series of test figures being there is due to Dr. Libby. He presented the data which allows me to derive this value for the Castle series of tests. That is the principal contributor, and thus explains why my estimate should be so close to Dr. Langham's.

There is one thing which has puzzled me, that is, that if these data are in fact correct, then the Russians have not really tested a series comparable to our Castle series of tests.

Senator ANDERSON. It could be that we had one fairly sizable shot that has not yet been approximated by anybody else. That is a possible explanation.

Dr. LAPP. That is possible. I can only draw an inference.

Senator ANDERSON. Would you not be satisfied to take the total figure that Dr. Langham used in view of the fact that he is so closely associated with Los Alamos Laboratory which for many years of this period was doing the major part of the testing and has since been joined by Livermore—but the two laboratories work very closely together and their scientists are certainly knowledgeable of everything that has taken place in the tests thus far—they have the translations of what the Russian tests have been insofar as we are able to detect them, and could you not agree with him that 50 megatons would be about the total for 5 years and 10 for each year?

Dr. LAPP. I believe the agreement is very close, and I would certainly go along with Dr. Langham. The range of estimates for the test limit for the injection of radioactive material, assuming equilibrium, runs in the range of from 2 to 10 megatons, according to the discussion before this committee at the round table of Dr. Neuman and Dr.

Langham, Dr. Eisenbud, Dr. Kulp and one other person whom I have forgotten.

Because of the way I have made estimates before, I have used a value of 3 megatons per year, with a factor of 3 either way. In other words, it might be as low as 1 megaton per year or it might be as high as 9 megatons per year. One can see from this that our own test rate has exceeded this value. If we take three, it is exceeded about twice. If we take 10, it has exceeded once plus the Russian contribution, which would be a global passing limit of 2 times in the past decade.

This is assuming equilibrium. You are going to have to have more tests before you will load up the stratosphere. So from the standpoint of being reckless, the United States has not yet exceeded the limit.

I would like to make that very clear. I do not think my written statement adequately brings that out.

I realize I am taking a great deal of the committee's time, and I would like to go through some of my testimony relatively quickly. Perhaps the question of the future nuclear tests should be discussed briefly. I think in dealing with future commitments of radioactive debris to the earth's atmosphere we must deal with many unknowns. Had we attempted an estimate 5 years ago prior to Castle-like weapons we would have arrived at most misleading and optimistic projections. The end of weapon development is not in sight, and no one can say that unexpected developments may not occur. For example, may not smaller nations be stimulated by British success with thermonuclear type weapons and place maximum emphasis upon such development?

Additionally, can we be sure that a nation would restrain itself and not test a 100 megaton dirty weapon if military requirements and nuclear technology indicated that such a weapon was desirable in its own security interests.

Will not the requirements of adapting maximum megatonnage to a small warhead put emphasis on further development of dirty weapons?

I cannot answer these questions at this time, but I know that single weapons tests of very high fission yields can add a strontium burden to the atmosphere far beyond the limits we have been discussing here.

The United States has contributed the largest fraction of the radio strontium to the stratosphere, and I think it is distinctly encouraging that the fullest discussion of the strontium fallout should occur in this country. I am not aware of any large body of published information on this subject of Soviet origin. It is known, however, that the Soviets are engaged in strontium studies.

In concluding this section, I would again like to stress Dr. Libby's contributions to this subject. They are of very great value and I feel sure that we would be in a much poorer position today to evaluate the strontium problem were it not for Dr. Libby's personal interest in this field of investigation and the research which he has pushed so vigorously.

On the strontium problem itself, I would like to state briefly strontium 90 determinations in man must be expanded to assess the increase in strontium 90 burden which will occur in future time. Careful determinations of natural strontium in humans deserve increased

attention. We know that more strontium 90 will accumulate in humans as a result of bombs tested in the past and as a result of current tests. I believe that Dr. Selove is going to give a further discussion of this. The determinations as to how much of the radio element may be tolerated safely is a matter for the biologists to discuss.

This committee has heard a fairly wide range of opinion from its expert witnesses on the probable biological effects of strontium 90 in man. But it seems to me that even in this area some agreement was reached, especially when Dr. Shields Warren stated on June 3:

> I would be reluctant to see the average strontium 90 content of bones, particularly in children, go much above 10 times the present level.

Dr. Libby's speeches show that the strontium 90 fallout will continue, and the strontium 90 level in human bones will increase.

I believe that unless restraints are imposed upon commitments of fission products to the atmosphere, it is only a matter of time before the strontium 90 level of Dr. Warren will be reached.

I would not be able to extrapolate that curve very well into the future to determine this.

I would like to jump to constructive proposals—at least I believe they are constructive—if I may just insert one comment here.

A colleague, Dr. Jack Shubert, who is presently at the Laboratory for Inorganic Chemistry in Zurich, Switzerland, from the Argonne National Laboratory, and he has been in Britain recently and discussed data with the British on the question of the relative sensitivity, I would like to quote from a letter I received last night from Dr. Shubert:

> It used to be thought that at least 1,000 roentgens of absorbed radiation would induce cancer. Within the past few years, it has been found that as little as 200 roentgens delivered to children would induce cancer in later life. Now it has been found (this is by the British) that as little as 3 to 5 roentgens received by the unborn child in its last 2 months before birth has been responsible for cancer of all types appearing a few years later.

I believe that this statement from Dr. Shubert which represents the final conclusions of the data of Dr. Alice Stuart in England, is significant in that it does show that the incidence of cancer malignancy in children correlates with the X-ray of women prior to term. This is diagnostic use of X-rays which in the case of X-rays may involve of the order of a few roentgens, an amount which was thought harmless.

I believe also that this would have significant bearing upon the question of the threshold.

In order to allow time for my friend, Dr. Selove, may I jump quickly to my proposals and read them for you.

A. ATOMIC ENERGY COMMISSION INFORMATION POLICY

I suggest that this committee or its parent committee may wish to review the information policy of the Atomic Energy Commission, and I might add the Defense Department, with regard to nuclear weapon effects, with a view toward revising this policy so that information may be made available more promptly and completely. I am thinking particularly of the relations of the Atomic Energy Commission with the press. I believe that the national interest de-

mands a better relation, a freer flow of communication between the Atomic Energy Commission and the press.

My second proposal, I suggest that the Joint Committee on Atomic Energy might wish to recommend or to sponsor the preparation of an analysis of the probable biological effect of nuclear warfare. It would be useful to investigate probable lashback effects from various levels of nuclear bombardment. What I am thinking of under this category of lashback is the fallout which would occur upon the country which uses the nuclear weapon itself. That would be the remote tropospheric fallout.

The third point, data useful to civil defense. I believe that the committee's investigations have produced information of critical value to the civil defense planning. It might be useful to have a summary report of these data transmitted to the FCDA. I say that because I personally have not seen many representatives of the FCDA at these hearings.

Research in long-range estimation of nuclear explosives. It is known that considerable effort has focused on long-range detection of nuclear detonations. Attention should be given to the declassification of such data as would bear upon evaluation of the radio strontium problem. In particular I have in mind helping us to estimate how much the Russians are contributing, if this can be done, without jeopardizing sensitive data. Other data would be most useful in discussion of the feasibility of policing an agreed upon test limit, if one could get a multilateral agreement.

Annual fallout report. In view of the great public concern over fallout hazards, I would urge that the Atomic Energy Commission be required to issue an annual report on the degree of fallout and its uptake in biological systems. Perhaps the Atomic Energy Commission might wish to have that report prepared by a university task force.

Finally, I would urge that the Congress continue its investigations of radiation hazards, extending them into the broader area of peacetime uses of radiation. I believe that the ever-increasing uses of radiation must be subject to legislative controls. Radiation protection in the United States needs, in my opinion, uniform legal status.

Representative HOLIFIELD. Thank you very much, Dr. Lapp. Unless there are questions, we will hurry on to our next witness, Dr. Selove, in order that we might finish with him in time for the conference.

Representative COLE. Mr. Chairman, I should like to ask a few questions of Dr. Lapp, in order only to clear up what appears to be a conflict or some discrepancy in statements with respect to his experience and connection with the atomic energy program.

Dr. Lapp, have you seen the biography of yourself which has been prepared by the Joint Committee?

Dr. LAPP. I am sorry, I did not.

Representative COLE. I wish you would look at it if you would to see if that is a correct representation of your activities in the field of atomic energy.

Representative HOLIFIELD. I understand that the staff took that from the American Men of Science compilation.

Dr. LAPP. I believe it is correct from cursory examination.

Representative COLE. I would call your attention particularly to that part of the biography which states that you were Deputy Execu-

tive Director of the Atomic Energy Commission Joint Research and Development Board during 1947–48, and Executive Director during 1948–49. The information which has been given to me from reliable sources is the fact that you have never been an employee of the Atomic Energy Commission.

Dr. LAPP. That is correct.

Representative COLE. That is why I am giving you an opportunity to clarify this apparent discrepancy.

Dr. LAPP. May I read what it says here? It says, "Deputy Executive Director, Atomic Energy" and then there is an unusual abbreviation "Cmm Atomic Energy Committee of the Joint Research and Development Board," which was part of the Defense Department. It was Vannevar Bush's show. I am happy you brought this up. Frequently (I give lectures) I am introduced in a way which is embarrassing to me because it is incorrect. I have been introduced as everybody from the Chairman of the Atomic Energy Commission on down the line. This statement actually is correct, if you understand that is committee and not commission.

Representative COLE. How can you possibly get committee out of "Cmm"?

Dr. LAPP. I am not responsible for this. I am sorry.

Representative COLE. Then at any rate it is a fact that you have never been an employee of the Atomic Energy Commission or a consultant to the Commission.

Dr. LAPP. That is correct, Mr. Cole, I have never represented myself as such.

Representative COLE. I am not saying that you did. I am simply trying to give you an opportunity to clarify the facts of your experience in this field.

Dr. LAPP. I am glad to have this opportunity to state on this record that is correct.

Representative COLE. I further call your attention to the item "Head, Nuclear Physics Branch, Office of Naval Research, Department of Navy, 1949–52."

Dr. LAPP. That is incorrect.

Representative COLE. That is incorrect?

Dr. LAPP. That is incorrect.

Representative COLE. What is the fact?

Dr. LAPP. It was Acting Head, Nuclear Physics Branch, Office of Naval Research, Department of the Navy, 1949.

Representative COLE. 1949?

Dr. LAPP. I am sorry. Again I am not responsible for that.

Representative COLE. It is the fact that in 1949 you resigned from that post with the Office of Naval Research.

Dr. LAPP. That is correct.

Representative COLE. I do not press you on it, but would you care to give your reasons for your resignations?

Dr. LAPP. I stated on the record that I resigned of my own volition at that time.

Representative COLE. I am sure you did of your own volition. Were there peculiar circumstances surrounding the resignation?

Dr. LAPP. I had taken the job with the Atomic Energy Commission—pardon me, you have me mixed up—at the invitation of Dr. Widdel as a temporary appointment while he slid out from the posi-

tion of Head of the Nuclear Physics at that time. I took it for a short time and then resigned, I think, in June of 1949, Mr. Cole.

Representative COLE. Then there were no unusual circumstances that prompted your resignation?

Dr. LAPP. I think I would have taken this job only for a short time. I was lecturing or beginning to lecture. I was finding it difficult to lecture and also be employed by the Defense Department. But the job was temporary.

Representative COLE. That is all I have, Mr. Chairman.

Dr. LAPP. May I read the rest of this to make sure there are no other corrections? I believe it is correct as it stands now, as corrected.

Representative VAN ZANDT. Mr. Chairman, there still is in the record this statement attributed to Dr. Lapp, "reckless or nonsubstantiated statements do a disservice to the AEC and to the Nation." This statement was directed at Dr. Eisenbud, Dr. Libby and Dr. Doan. I would like to ask the Chair if we are going to give these three distinguished Americans an opportunity to answer the accusation?

Representative HOLIFIELD. Yes, sir. Would the committee like to have them come forward at this time or would you like to go forward with the next witness and have that take place in the conference?

Representative COLE. Mr. Chairman, if we are through with Dr. Lapp, I would strongly urge that we invite Dr. Eisenbud, who is in the audience, to come immediately to respond to Dr. Lapp's charge.

Senator HICKENLOOPER. Mr. Chairman, I would suggest that we finish with Dr. Lapp first. I have some questions I would like to ask.

Representative HOLIFIELD. Senator Hickenlooper has some questions, Dr. Lapp.

Senator HICKENLOOPER. Dr. Lapp, have you ever done what might be called extensive works in genetics under your own responsibility in connection with the effects of radioactivity on human cells?

Dr. LAPP. No, sir.

Senator HICKENLOOPER. Have you ever done any research in biology and medicine in connection with the effect of radioactivity on the genes or other parts of the human body?

Dr. LAPP. I have never done any research on genetic effects.

Senator HICKENLOOPER. Have you ever conducted any research yourself on the fallout or its intensity in this country? By that I mean any laboratory research of any extent.

Dr. LAPP. I have made some simple measurements myself, but my data here and my testimony is based upon the data of the Atomic Energy Commission.

Senator HICKENLOOPER. That was the next suggestion that I wanted to make. I have the impression here—I do not know whether I am correct or not—that you are appearing here more in the nature of a reporter or a correlator with some considerable educational background, I admit, of the scientific data as you personally interpret it, which has been compiled by a number of eminent scientific people who have actually done the work.

Dr. LAPP. I believe the invitation that was extended to me by the committee more or less put me on these lines of testimony.

Senator HICKENLOOPER. Yes. The only purpose of this suggestion is that you are not here, while you are a scientist in your own right on your education, giving firsthand evidence based upon data with which you have been acutely or intimately connected in connection with its

development. So you are reporting what you have read or what you have been told by others with varying degrees of accuracy and drawing your own conclusions.

Dr. LAPP. Mr. Hickenlooper, I think it is a fair statement to say that I have done considerable work based upon information made available by the Atomic Energy Commission and scientists in general. I have done active work in gathering data from people, especially as every scientist will do, when available.

Senator HICKENLOOPER. I know that. I make the point that there are several very eminent newspaper reporters in the scientific field who have also done a tremendous amount of work of gathering data and yet they have done no research of their own. They are reporters and in a very proper field they are reporting what they find. I merely have the impression that you are here today in the nature of a reporter or a compiler of information which you are interpreting as you see it. As differentiated, I might add, from the testimony of most of the rest or all of the rest, perhaps, of the witnesses we have had here who have been intimately working in this field and present the results in the main on their own experience, and the results of their own work.

Dr. LAPP. I believe, Mr. Hickenlooper, this is within the framework of the outline which requested me to testify.

Representative HOLIFIELD. I am very sorry. In order to correct the record, the Chair will have to state that Dr. Libby is a chemist, and the information he has given us in every field of science has been obtained from his association with other scientists and reading of other materials. So in order to keep the record straight, let us have the facts spread there that it is not only Dr. Lapp who had studied the other scientists' work and reported on them, but also Dr. Libby, one of the AEC Commissioners.

Dr. LAPP. May I say in general response, not specifically to Mr. Hickenlooper, that I admit to being critical of the Atomic Energy Commission. My criticisms of the Atomic Energy Commission, I always felt, have been directed toward trying to bring the facts out into the open, and the free play of public discussion. As Dr. Libby testified this morning, some of the problems involved here transcend the area of science. I have felt that this is important. I have tried to help in the somewhat new but I think important problem of education in science.

Senator HICKENLOOPER. Then where do you obtain your facts? Do you have access to restricted data?

Dr. LAPP. I have no access to restricted data, Mr. Hickenlooper. The facts that I have presented here are based upon information which is available freely in the scientific domain.

Senator HICKENLOOPER. Not only in the scientific domain, but from the Atomic Energy Commission also. Is that not true?

Dr. LAPP. Such data as the Atomic Energy Commission published. We were assured this morning by Dr. Libby that all of the data except for a very small fraction dealing with a long-range detection on Project Sunshine had been put in the public domain.

Senator HICKENLOOPER. What information do you have that the Atomic Energy Commission is not making available, if you have no access to restricted data, and if your information comes from data freely available from various sources including the Commission? Does not that indicate that data is being made available?

Dr. LAPP. If I may respond to the question, Senator Hickenlooper, my complaints in the past about the slowness with which the Atomic Energy Commission emitted data, which I thought were vital to civil defense, were, for example, that the data were not available. They had to be derived by me from other sources, other than the Atomic Energy Commission. As for example, the scientific data from Japan. Thus I think the fact that later on the Atomic Energy Commission actually confirmed through its pronouncements that these results were correct testified——

Senator HICKENLOOPER. Have you had an opportunity to go through the data in minute detail from Japan, examine the records and all the data that they have developed, or are you relying entirely upon the verbal statements of certain scientists made to you in the course of your visits with them?

Dr. LAPP. No. I have been in communication with Japanese scientists for some time. I have received letters and photostats and copies of their scientific papers, in some cases prior to publication.

Senator HICKENLOOPER. Thank you very much.

Representative COLE. Mr. Chairman, I had intended to interrogate Dr. Lapp just a little bit about his visit to Japan earlier when he referred to his visit in his statement. While I hate to keep him on the stand unduly, I am curious to have his observations on two points.

One is, Dr. Lapp, that you were registered as one of the scientists who would attend the conference.

Dr. LAPP. Yes.

Representative COLE. And yet you did not attend the conference.

Dr. LAPP. No; I did attend, Mr. Cole.

Representative COLE. It was reported to me, and the reason I raise the question is because that same question was raised by a number of persons who were there, as to the reason why you had not attended the conference.

Dr. LAPP. I would be glad to explain this.

Representative COLE. That is one point. Then the other, since you remained in Japan after the conference was concluded, I am interested to have your observations with respect to the accomplishments of the conference.

Dr. LAPP. On the first point, I arrived in Japan on, I believe, May 1, considerably in advance of the conference. It was my firm intention to attend every session of the conference, because I am very much interested in the work of the Atomic Industrial Conference. However, when I got to Japan, I found myself in a pretty mad race because I was digging up some data on the *Lucky Dragon* story, interviewing some of the fishermen who were in this unlucky boat; and I wanted to visit Hiroshima and visit some scientists in the various places. I found that when I had to deal with just the problem of hours spent in talking with the fishermen that by the time I got back to Japan the conference had already started. This was unavoidable, but I had to meet people at certain places.

Then the amount of commitments I had with the press and visiting the *Lucky Dragon* itself precluded my going to more than one session.

Representative COLE. Which session was that? It does not matter.

Dr. LAPP. I can give it to you. I did attend that. I wanted very much to attend the luncheon and dinner sessions. I had all the tickets, and I just could not use them. For example, the one night I wanted

to do that, I think I was invited to a banquet by one of the prominent newsmen. So I, unfortunately, did not have much time.

As for the second point, just what was my reaction, I did not have too much opportunity to discuss with the Japanese scientists their reaction to the conference, but I gathered from conversation with a few of them that they were greatly impressed with the conference. It was very well managed. It was well attended. I think they were very much impressed with the general conference.

Representative COLE. Then your conclusion is that it was a very worthwhile conference.

Dr. LAPP. I would say so.

Representative COLE. That is my own conclusion.

Dr. LAPP. Yes.

Representative COLE. I can also verify your statement that it was an extremely well-organized conference involving a considerable number of people. There were some meetings that were attended by as many as 4,000, 5,000 and 6,000 Japanese. It was an unusually well-organized conference, probably the first international conference of industrialists and scientists that has ever been held.

Dr. LAPP. I would in no way like to detract from the value of this conference. My only regret is that I was able to attend so few sessions. I cut short my visit in order to come back here. I would have liked to stay in Japan much longer.

Representative COLE. My only basis for concluding that you had not attended the sessions of the conference is that I inquired—because I looked forward to seeing you there—inquired at the registration desk at the end of the conference, and they said you had not, so far as they knew, attended the conference, and the papers in the box—there was a slot for each of the delegates—were still there.

Dr. LAPP. I have the papers in my office, Mr. Cole. Again I regret, and no one does more than I, that I could not attend these conferences. I was working. The press in Japan can at times be very aggressive. Perhaps you found that out yourself.

Representative COLE. Yes.

Dr. LAPP. I found myself going to a 15-minute interview and ending up with a 2-hour luncheon.

Representative COLE. Now, with respect to your sources of information, Dr. Lapp, you have indicated that your observations back through the years are based on reports and studies of unclassified data that have come to your attention. Are they not, also based on discussions with other newspapermen and analysts in this field, such as yourself?

Dr. LAPP. I am proud to say that I communicate with a great many members of the press.

Representative HOLIFIELD. Mr. Eisenbud, you are now given an opportunity to reply to the comment of your colleague in science, Dr. Lapp.

Senator ANDERSON. Before you do so, Mr. Eisenbud, may we sort of follow along in this same pattern of qualification? I look at your qualifications in here, and I see "EE, New York University." Does that mean "electrical engineer"?

Mr. EISENBUD. Yes.

Senator ANDERSON. What subsequent degrees have you acquired?

Mr. EISENBUD. I have no subsequent degrees.

Senator ANDERSON. Just the degree as electrical engineer?

Mr. EISENBUD. Many years ago; yes, sir.

Senator ANDERSON. I see you spent more time working for the Liberty Mutual Insurance Co. as a hygienist than anything you have done in your life and more than all the rest of your experience put together; is that right? Eleven years with them, and only 10 years since then.

Mr. EISENBUD. That is about right.

Senator ANDERSON. I am not trying to be critical, because I tried to speak appreciatively of what you had done a few moments ago. As a hygienist, did you ever have occasion to get into the question of deposit of strontium 90 in the atmosphere for the Liberty Mutual Insurance Co.?

Mr. EISENBUD. This was previous to 1947. Up until that time very little strontium 90 had been formed in this world. The radiaition problems of those days were X-ray and radium. We were only beginning to get interested in the kinds of things which we are talking about today.

Senator ANDERSON. The field that you were in had to do with the detailed discussion of the occurrence of strontium 90 and cesium 137 in the atmosphere, biosphere, and its uptake and behavior in man. What was there in your electrical engineering course that dealt with that?

Mr. EISENBUD. Very little, sir. But a great deal in some 21 years of professional experience, during which time I have attained the rank of adjunct professor of industrial medicine at New York University Medical School.

Senator ANDERSON. What was there in this work with the Liberty Mutual Insurance Co. as a hygienist that started you off lecturing on medicine?

Mr. EISENBUD. I got interested, about 22 years ago, in a legitimate subject for a young electrical engineer, namely, the electrical charges on dust, and went from there to the general physical properties of dust, and then into the physiology of dust and dust diseases, and spent a great deal of time from 1936 until 1947 studying the general behavior of dust, not only in the atmosphere, but in the lung and in the body.

Senator ANDERSON. Your paper was headed, "A Measurement of Strontium 90 in Geophysical and Biological Material." Are you a geophysicist?

Mr. EISENBUD. Sir, I do not know, really, how to answer that. I think anybody that has some interest or qualification in geography and geology and some in physics could at least write on the subject of geophysics, but I am not a geophysicist.

Senator ANDERSON. Are you a biological worker.

Mr. EISENBUD. I work in the biological field. I, myself, am not a biologist.

Senator ANDERSON. Have you conducted experiments in this measurement of strontium 90?

Mr. EISENBUD. I have directed the experiments, and have conducted some myself.

Senator ANDERSON. Have you conducted them? Have you done any experimental work yourself?

Mr. EISENBUD. The experimental work was performed under my immediate supervision. I have done some of the experimental work myself.

Senator ANDERSON. Most of it was done under your supervision?

Mr. EISENBUD. This has been a large program. This is much too large for one man. It was done by my immediate staff.

Senator ANDERSON. Then you are here as a reporter of what someone else has done.

(A letter from Merrill Eisenbud, setting forth a full record of his qualifications, follows:)

UNITED STATES ATOMIC ENERGY COMMISSION,
NEW YORK OPERATIONS OFFICE,
New York, N. Y., July 12, 1957.

Mr. HAL HOLLISTER,
Staff Member, Joint Committee on Atomic Energy,
Congress of the United States, Washington, D. C.

DEAR HAL: You will undoubtedly recall that during the proceedings of June 5, the question of my professional qualifications was raised by Senator Anderson. If this portion of the testimony is to be included in the published proceedings, it would be desirable, for the sake of completeness, that a full record of my qualifications be included as well. The attached curriculum vitae is somewhat more complete than the record to which Senator Anderson referred and which I believe was the Men of Science abstract.

I am also attaching a list of my publications. I do not wish that this list be included in the proceedings but I simply submit it as a matter of record for the files of the subcommittee.

I continue to hear favorable reports about the hearings. We all look forward to the final proceedings.

With best regards.

Sincerely,

MERRIL EISENBUD, *Manager.*

BIOGRAPHICAL DATA, MERRIL EISENBUD, JUNE 10, 1957

Date of birth: March 18, 1915, New York City

Education: New York University, College of Engineering (1932-36) B. S. in E. E. 1936

Positions held:

- Industrial hygienist, Liberty Mutual Insurance Co., 1936-47
- Chief, Industrial Hygiene Branch, Health and Safety Laboratory, United States Atomic Energy Commission, 1947-49
- Director, Health and Safety Laboratory, United States Atomic Energy Commission, 1949 to present
- Manager, New York Operations Office, United States Atomic Energy Commission, 1954 to present
- Senior scientific advisor, Preparatory Commission of the International Atomic Energy Agency, 1957

Lecturer, Columbia, School of Public Health, 1945-50

Adjunct associate professor, department of sanitary engineering, New York University, 1945-50

Associate professor, industrial medicine, New York University, department of industrial medicine, 1950-55

Adjunct professor, industrial medicine, Postgraduate School of Medicine, New York University, 1955-

Committees:

- National Research Council:
  - Toxicology Committee, 1952-
  - Committee on Atmospheric and Industrial Hygiene, 1952-
- American Standards Association:
  - Subcommittee on Radium, Dust, and Radon Gas, Z37, 1949-
  - Sectional Committee on the Use of X-Rays, Z-54, 1951-
- National Safety Council: Executive committee, Chemical Section, 1951-
- Radiological Advisory Committee: Office of Civil Defense, City of New York, 1950-
- Technical advisor, United States delegation, U. N. Conference on the Peaceful Uses of Atomic Energy, Geneva, 1955
- Alternate United States representative, U. N., Scientific Committee on the Effects of Atomic Radiation, 1956-.

Member, National Academy of Sciences Committee on the Meteorological Aspects of the Effecvts of Atomic Radiation 1956–.
Aspects of the Effects of Atomic Radiation, 1956–.
Scientific adviser, United States delegation, Conference on the Statute of the International Atomic Energy Agency, 1956–.
Member, World Health Organization, Expert Advisory Panel on Radiation, 1957–.

Memberships:
American Industrial Hygiene Association (board of directors, 1955–58), American Public Health Association, New York Academy of Science, Radiation Research Society, American Association for the Advancement of Science.

LIST OF PUBLISHED WORK OF MERRIL EISENBUD

Global Distribution of Strontium-90 from Nuclear Detonations, Scientific Monthly (May 1957), pp. 237–244.
Monitoring Network for Measuring Radioactive Fallout, J. Am. Water Works Association, Vol. 48, No. 6 (June 1956).
Radioactive Fallout Through September 1955, Science, Vol. 124, No. 3215, p. 251 (August 10, 1956) (with J. H. Harley).
Industrial Hygiene of Uranium Processing, A. M. A. Arch. Ind. Health, Vol. 14, pp. 12–22 (July 1956) (with J. A. Quigley).
Atmospheric Contamination, Chapter 11, Radiation Protection published by Thomas & Co. (in press).
Radioactive Fallout in the United States, Science, Vol. 121, No. 3150, pp. 677–680 (May 13, 1955) (with John H. Harley).
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Mr. EISENBUD. Thank you.

I am deeply gratified, Mr. Chairman, at the opportunity to correct the record with respect to the item which Dr. Lapp has included on page 2, paragraph C, in which he quotes a New York newspaper sentence and illustrates what, I think, needs not be illustrated; namely, the danger of taking something out of context. The date of that quotation is March 20, 1955. This was a very jittery period. This was immediately following the announcement within the AEC of the results of the March 1, 1954 detonation in the Pacific. It also coincided with the beginning of the Teapot series of detonations in Nevada, which, if my recollection is correct, began a week or two prior to this announcement, or perhaps shortly thereafter.

In any case, the fact that the first post-Castle detonations were about to take place in Nevada was very much in the minds of many of our citizens.

This reporter came to me to find out whether, in my opinion, the kind of accident which occurred in Bikini in 1954 could happen in this country as a result of the test being contemplated or already underway in Nevada. We were not talking about the long-range hazards of fallout. We were not talking about strontium 90. We were talking about the kinds of acute effects which one had, unfortunately, seen in both the Marshallese and Japanese fishermen in 1954.

My comment, which may or may not have been quoted accurately—I really don't know—had to do, primarily, with the relationship of acute effects to the kinds of radiation levels that are expected from the Nevada test, which are of the order of 1 milliroentgen or thereabouts, at least, in the United States.

Senator ANDERSON. Mr. Eisenbud, this statement was made on March 20, 1955. I understand you do not question the accuracy of the quotation.

Mr. EISENBUD. I do not question the accuracy of the quotation nor will I certify it nor will I certify the accuracy of Dr. Lapp's quotation of a quotation.

Senator ANDERSON. If it should prove he is accurate you are willing to stand by it.

Mr. EISENBUD. I am willing to accept this version of it.

Senator ANDERSON. The testimony introduced by Dr. Lapp and the chart introduced showed that we had the Castle series in 1954 in which we had put in the atmosphere more fission products perhaps in that year than we have put in all the rest of the tests together. Therefore we were at our highest peak when you made your comment. The figure was several times above the figure which Dr. Langham has said is a safe figure year by year.

It is your testimony that we could do a million times that and do no damage.

Mr. EISENBUD. No, sir. This was not the question which was asked of me. I was asked specifically whether there was any possibility in the eastern United States of an accident which would produce the

kinds of illness among the people that were seen in the Marshallese and Japanese fishermen.

This would require around 100 roentgen. The radiation doses which had been observed were something less than a milliroentgen. This is a ratio of about a million to one. When you put it into context it is perfectly accurate.

Senator ANDERSON. That is what I am trying to get to. We had just finished the Castle series. You would agree with that; would you not?

Mr. EISENBUD. That was a year before.

Senator ANDERSON. We had finished them in 1954 and this statement was made March 20, 1955. We had finished the Castle series, had we not?

Mr. EISENBUD. Yes, sir.

Senator ANDERSON. You said the total fallout to date from all tests would have to be multiplied by a million to produce visible deleterious effects except in areas close to the explosion itself. It is your testimony that having put that year into the stratosphere, or whatever fallout pattern there was, something in the neighborhood of 30 megatons of fissionable products, that we would have to put a million times that for it to have any ill effect except in the immediate vicinity of the test. Is that correct?

Mr. EISENBUD. That is incorrect.

Senator ANDERSON. You think that is a somewhat reckless statement?

Mr. EISENBUD. This is not what I said.

Senator ANDERSON. That is what I am trying to get to. The paper misquoted you.

Mr. EISENBUD. No, sir. I have not seen that quotation in 2 years. This sentence is out of context.

Senator ANDERSON. I am reading it to you. If it should prove to be an exact quotation, is it a reckless statement?

Mr. EISENBUD. Out of context; no.

Senator ANDERSON. In or out of context, is it a reckless statement to say that the Castle test which included an extremely large shot which turned loose in one series of tests as much fission products as probably all the rest of the tests by all the rest of the countries? If you can place reliance on the data gathered at Los Alamos, do I understand in context or out of context, you could have a million times that and have no visible deleterious effects except in the immediate area? Do you wonder that looking at that statement Dr. Lapp thought it might be a slight degree of recklessness?

Mr. EISENBUD. Sir, I would like Dr. Lapp to comment on what I have——

Senator ANDERSON. The point is that they wanted to give you an opportunity, and this is your day.

Mr. EISENBUD. Yes, sir. I think Dr. Lapp will probably understand what I am talking about. Let me say this, sir: I think it is a great misfortune——

Senator ANDERSON. Dr. Lapp was treated critically because he had used this language. They said in all fairness you would have an opportunity to reply. This is your hour for fairness. Why don't you go ahead and reply?

Representative COLE. I think he would if you let him.

Mr. EISENBUD. I think it is a great misfortune, sir, that to add to public confusion we find it necessary to discuss in the same session the long-range hazard and the close in hazard. For most of these sessions we have been talking about the long-range hazard from strontium 90 and from the gamma radiation.

In this interview I was talking about the close-range hazard, the kind of hazard that may develop within a few hours after detonation and the kind of hazard which produced illness in the Marshallese and Japanese in 1954.

Senator ANDERSON. Then from the close-range hazard—we will just confine it to that—since we put some 30 megatons of fission products in the atmosphere in the Castle test just passed, you believe that we could safely put a million times that in a single year in a test without doing any damage?

Mr. EISENBUD. Certainly not, sir.

Senator ANDERSON. What were you talking about? If it is not from the close range or long range, what other range is there?

Mr. EISENBUD. I was talking about the immediate gamma radiation from the fallout which occurs in the eastern United States within a matter of a day or so after a detonation in Nevada. This is not in the statement because the statement has been taken out of context.

Senator ANDERSON. Could that have been apparent if Dr. Lapp had read the whole interview?

Mr. EISENBUD. I can't vouch for the validity of the interview, sir. I do not recall this.

Dr. LAPP. I would like to make this statement in all fairness to Dr. Eisenbud, and I certainly do not mean to attack his integrity. The reason for quoting these and the use of the word "reckless" was to demonstrate the need for being quite exact when dealing with such a touchy subject as radioactivity. I am trying to interpret this recklessness not in terms of Dr. Eisenbud's personal recklessness but in terms of how it may appear to people who read these things and who do read single sentences.

Senator ANDERSON. I quite agree with you. I tried to say earlier that I think Mr. Eisenbud is a very fine public servant. I went up to him and told him that I appreciated very much the testimony he gave the other day. I did not regard your statement about him as a vicious attack upon him. But if it comes down to whether it was reckless, there are people who read that statement alone who would think it had just a slight tinge of recklessness in it, since the Castle test had just been finished.

Dr. LAPP. May I ask a question of Dr. Eisenbud? Is this permitted?

Representative COLE. Don't look at me.

Representative HOLIFIELD. I think——

Dr. LAPP. I would like to ask one single question.

Representative HOLIFIELD. You might ask the Chair a question.

Dr. LAPP. I would like to ask the Chair a question, as to what the roentgen dosage on the fallout on Troy, N. Y., was.

Representative HOLIFIELD. The Chair has been told, but the Chair has temporarily forgotten now. The Chair will ask Dr. Eisenbud if he recalls.

Mr. EISENBUD. Yes, sir. It has been variously estimated.

Representative HOLIFIELD. Is it an unclassified amount?

Mr. EISENBUD. It is unclassified. It was published 3 years ago. The upper limit of estimate is something under 100 milliroentgen. It loses about 1 milliroentgen. I would personally estimate it at about 10 milliroentgen.

Representative HOLIFIELD. The Chair thanks the gentleman for that information.

Representative COLE. Could I inquire, in order for the information to be helpful, if these samplings that occurred were related to what tests? When were the samplings taken which you say indicated an upper limit of 100 milliroentgen?

Mr. EISENBUD. I believe it was the third or fourth test of the series held in Nevada in the spring of 1953. There was a rain out over the Troy-Albany area which coincided with the passage of a cloud from Nevada. So that a very large percentage of this cloud was washed down.

Senator ANDERSON (presiding). Are there additional questions or statements?

Representative VAN ZANDT. Dr. Eisenbud, I understand your position is that the statement that Dr. Lapp attributes as being a reckless one was taken out of context.

Mr. EISENBUD. Yes, sir.

Dr. LAPP. Is it proper for me to respond? I have done a little arithmetic. Let us take 10 milliroentgens, as Dr. Eisenbud estimates, and we multiply 10 milliroentgens. That would be .01 roentgens by 10 to the sixth, which will give us 10 to the fourth, which is 10,000 roentgens.

Senator ANDERSON. 10,000 roentgens would kill everybody in sight.

Mr. EISENBUD. Yes.

Senator ANDERSON. So that would mean there would not be any immediate danger if you kill everybody in sight?

Representative PRICE. Mr. Chairman, one of the points that we do not want to overlook is that Dr. Lapp is trying to point out the responsibility of the Commission to release information as promptly as possible so that these types of statements would not be made. Is that not one of the reasons for you citing this statement?

Dr. LAPP. I really feel if we had better relations here between the press and the Atomic Energy Commission we could in a minute avoid much of this difficulty.

Representative PRICE. That is the reason I understood that you gave this example.

Dr. LAPP. Yes.

Senator ANDERSON. May I say, Mr. Eisenbud, that I am truly sorry that Dr. Lapp's quotation has caused any embarrassment. I want to repeat what I said before. I certainly regard you as a fine public servant doing a good job. I am very happy that you are here make your contribtuion today.

Mr. EISENBUD. Thank you, sir.

(The full statement of Dr. Ralph E. Lapp follows:)

STATEMENT OF RALPH E. LAPP ON RADIOACTIVE FALLOUT

EXPLANATORY NOTE

Mr. Holifield, I received your invitation to testify before this committee while I was in Japan. I cut short my trip in order to attend the hearings. May I

say that I appreciate very much this opportunity to appear here. I would like to add that I am very gratified that your investigations to date have thrown so much light on the problem of radioactive fallout. I believe that these hearings will stand as a landmark in the history of our knowledge about this relatively new phenomenon.

COMMENT ON DR. LIBBY

Appearing as I do after Dr. Libby, I would like to comment on his contributions to fallout. Dr. Libby has not only stimulated extensive research in fallout investigations such as Project Sunshine, but he has also taken the initiative in publication of his findings. I feel very strongly that he deserves a great deal of credit for his work on fallout. Were it not for Dr. Libby we might well be confronted with a considerably smaller body of knowledge about fallout than we have today.

MY INTEREST IN FALLOUT

I have had an active interest in atomic bomb phenomenology ever since I witnessed the Bikini Baker test in the summer of 1946. However, my interest in radioactive fallout was really stimulated by the 1954 Bravo test at Bikini. This was the test which resulted in radioactive contamination of the *Lucky Dragon No. 5*, a Japanese tuna trawler.

My initial interest in fallout centered upon civil defense. In this connection, I published a series of articles on fallout in the Bulletin of Atomic Scientists as follows:

November 1954: Civil Defense Faces New Peril
February 1955: Radioactive Fallout
June 1955: Radioactive Fallout III
November 1955: Global Fallout
September 1956: The "Humanitarian" H-Bomb
October 1956: Strontium Limits in Peace and War.

NATURE OF MY TESTIMONY

I am dividing my testimony into four parts:

I: General Remarks
II: Local Fallout
III: Remote Fallout
IV: Constructive Proposals

Because of the number and complexity of the topics covered, I am presenting my remarks in terse or fragmentary form. This will permit the committee to bypass topics of less importance and concentrate upon those of more concern.

PART I. GENERAL REMARKS

A. Necessity for numbers

Public confusion about fallout will continue to increase unless scientists can provide a quantitative or semiquantitive evaluation of the various hazards associated with fallout. Precision is probably not possible due to the nature of the hazards and we may have to be content with numbers which vary by a factor of 2, 3, or even 10. Th is committee has already performed a valuable service in narrowing the range of estimates made by individual witnesses.

B. Disagreement among scientists

The public is apt to conclude that if scientists cannot agree upon the hazard, then all is confusion. It would be nice if the scientists could all agree upon a quantitative estimate of the hazard, which could then be given to the public. Two unusual circumstances have combined to produce the current confusion on fallout.

First, the urgency of our times has focused attention upon problems for which science did not have textbook answers. Available knowledge was inadequate and research had to be initiated to provide answers.

Second, the ordinary process by which scientists argue out their answers was interdicted by the complexity of the problem and secrecy. Scientists outside the Atomic Energy Commission have full-time jobs and could scarcely be expected to tunnel into the complexities of the problem in a few leisure hours.

C. Responsibility of the Atomic Energy Commission

Considering these factors, I think that the AEC has the responsibility for providing the outside world with the facts about fallout as promptly as these become available. Scientists, technicians, and officials of the AEC must present only reasoned and careful estimates of the hazards based upon factual knowledge. Reckless or nonsubstantiated statements do a disservice to the AEC and to the Nation.

Example: Dr. Eisenbud is quoted in an article titled "Man Who Measures A-Fallout Belittles Danger" (Sunday News, New York, March 20, 1955) as follows: "The total fallout to date from all tests would have to be multiplied by a million to produce visible, deleterious effects except in areas close to the explosion, itself."

Example: Dr. Libby in a speech dated June 3, 1955, stated: "However, as far as immediate or somatic damage to the health is concerned, the fallout dosage rate as of January 1 of this year in the United States could be increased 15,000 times without hazard."

Example: Dr. Richard Doan while in Tokyo on May 13, 1957 stated that the bomb tests would not have "the slightest possible effect" on humans.

I do not label Dr. Libby's statement as reckless but interpose it to illustrate the spectrum of opinion being given to the public.

D. World interest in fallout

I am informed by a cable from Tokyo that the deliberations of this committee hearing are being "splashed across page 1" of the Japanese newspapers. This comes as no surprise to me for my trip through Japan alerted me to the overwhelming interest manifested there in atomic radiation.

The committee might be interested in my observation that fallout has become an acute weapon for propaganda. For example, I found that the Japanese scientists are actively studying the radioactivity of their tea because of the assertion from the Chinese mainland that Japanese tea is radioactive. Some people in Japan are so keenly aware of fallout that they take showers after being out in a rain. The great public outcry against the British Christmas Island tests, but there was no great demonstration against Soviet tests. It is a great victory for psychological warfare experts when they can induce selective sensitivity to fallout.

America puts itself in a bad light when it fails to present its case clearly to the world. Even casual analysis of the news reporting in this country will show that AEC pronouncements on fallout are not received with full credibility. This situation is obviously not in the full interests of national security.

E. The nature of biological data

It is inherent in the very nature of the biological research into the effects of radiation upon humans that a high degree of accuracy is not attainable, especially on a human experience basis. As Dr. Langham of the Los Alamos Laboratory has testified human experience with retention of radium 226 is the basis for setting upon a maximum permissible concentration (MPC) for radiostrontium (Sr–90). Yet our actual experience is confined to a small sample of acutely exposed individuals and a small sample of less acutely exposed people.

Actually, our concern should focus not upon acute effects in man which are highly unlikely from peacetime bomb testing, but rather with the chronic, debilitating long-term effects from irradiation of humans. We must be conscious of the need to appraise long-delayed effects, say, 50 years after entry of radioelements into the body. Here our knowledge is quite limited.

F. Radiation limits for a global population

I would like to stress the fact that consideration of safe limits for irradiation of the world's population is essentially a new problem. Prior to the awareness of global fallout, the International Commission on Radiological Protection made its recommendations for those who would be exposed to radiation in pursuit of their occupation. Such groups initially were numbered in the hundreds and then in the thousands as atomic energy came of age. Individuals within such groups were healthy adults exposed to known and restricted hazards; they were subject to administrative controls and medical supervision.

In setting up limits for a total population, we must take into account the varying radiosensitivity of individuals, the complete spectrum of age, the persistence of the hazards, the lack of medical control, the varying degrees of health of people and the variety of their diet. Yet it was not until last year that the

Atomic Energy Commission introduced the difference between an occupational MPC and a global MPC into its releases on fallout.

In view of the nature of our knowledge and the totality of the sample with which we are dealing, I would urge a big factor of safety in setting limits to bomb testing. It would be tragic to find some day that we had erred in setting the limits.

G. Soviet nuclear tests

On my recent trip to Japan, I learned that Japanese scientists collected sufficiently active samples from Russian tests to perform radiochemistry upon the bomb debris. I am informed that five Soviet tests produced a fallout on Japan from which scientists measured and identified the presence of uranium 237 (U–237). Soviet explosions characterized by such fallout were judged to be in the megaton range. These estimates are subject to considerable uncertainty but one authority told me that he estimated at least two bomb yields in the range of 10 megatons.

Two Soviet nuclear tests were observed to originate in the arctic region whereas the remaining tests took place in a region estimated to be Ozero Balkash (Lake Balkhash) which is southeast of the new coal area of Karaganda. The air mass trajectories from central Siberia frequently sweep across the islands of Japan, especially Hokkaido. They also produce tropospheric fallout over the United States as well. Here in Washington you could swipe a Kleenex over a car top and cause a Geiger counter to respond readily.

The presence of U–237 in the Soviet fallouts proves that the Soviets have achieved a compound fission-fusion or multiple-stage weapon. According to my information, this was first accomplished in September 1954.

I would like to add that I am informed by Japanese sources that Soviet tests produce 70 percent of the fallout observed on Japan. Pacific tests account for 20 percent and the Nevada shots add 10 percent.

PART II. LOCAL FALLOUT

Definition: By local fallout, I mean that which comes to earth within several hundred miles of the explosion site and is deposited within the first day or so. The following points are discussed with relation to the direct effects of external radiation. My interest centers upon the problem of civil defense in dealing with the radiation hazard in a contaminated area.

A. Areas of contamination

It is evident from the analyses such as Dr. Schafer presented to this committee that a nuclear attack upon the United States would involve an overlapping or smeared out pattern of bomb fallouts, especially over Northeastern United States. In making assessment of the radioactive power of bomb fallout, it is useful to introduce a new unit "the eternity roentgen square mile." This is a measure of the irradiating power of bomb fallout. By "eternity roentgen" I mean the total roentgens accumulated in dosage from 1 hour to eternity. This unit is then multiplied by square miles over which fallout occurs.

Example: To estimate the eternity roentgen square mile contamination from a 15 megaton explosion we proceed as follows. Assume that the ratio of fission to fusion energy release is 2:1. Then 10 megatons of fission energy will be involved. Assume a 50 percent local fallout. This yields 5 megatons of fission products in the fallout area. Simple calculation shows that 1 megaton of fission products could contaminate (if uniformly deposited at 1 hour) 1,000 square miles so that the 1 hour to infinity dose in open air would be 6,000 roentgens. Thus 5 mt. of fission products could raise this dosage to 30,000 r. Or if the area of fallout were greater, say, 5,000 square miles, the dosage would be 6,000 r.

An attack such as Dr. Schafer assumed involved 2,500 mt. of bomb yield and he specified "dirty weapons" surface burst. By "dirty" it is meant that the ratio of fission to fusion is fairly high. If we assume 2,000 mt. of fp (fission products) locally deposited this amounts to a total of 12 billion roentgen square miles of potential contamination. Obviously, this is a maximum since much fallout will occur after 1 hour; this will dissipate harmlessly in the air until it is deposited on the ground. Nontheless, the figure especially for surface burst bombs where local contamination will be maximized gives an indication of the magnitude of the fallout hazard. If any considerable fraction of this total figure is concentrated on a relatively small area, such as Northeastern United States industrial heartland, the corresponding radiation intensities will be severe.

Example: If a 1,000 mt. of fp concentrated upon Northeastern United States much of the region would be subject to a fallout of about 10,000 eternity roentgens. This would correspond to a fallout intensity of 2,000 r./hr. at 1 hr. I shall discuss the significance of such fallout in section C.

B. Clean and dirty bombs

The above discussion should make it obvious that the fallout from dirty weapons is of immense importance because of the area contaminable with a medium-weight attack. However, it may be useful to compare the damage areas of the two types of weapons.

A clean or relatively clean air burst bomb would have to depend upon blast and heat for its destructive effects. Consider, for example, the areas hit by the blast of a 20 mt. bomb.

Blast overpressure (pounds per square inch)	Distance in miles	Area in square miles
100	1.5	7
10	7.5	175
3	15,0	1,600

For purposes of comparison, one might select purely military targets such as air fields, missile sites and "hardened" targets which would require up to 100 pounds per square inch blast overpressure for destruction. Under such cases the aiming accuracy in delivery would have to be very great if you wished to "hit". A miss by as little as 2 miles with a clean bomb could be considered a complete miss. If one is concerned with population bombing and the criterion is the destruction of a framehouse, the 3 pounds per square inch blast would be appropriate. A greater aiming error would be allowable but one would not want to miss by more than 10 miles.

To complete the comparison, it is necessary to assess the persistence of the radioactive effect of fallout to discover whether such contamination would be effective in denying land to normal or to even emergency use.

I make this point, not to assert that there would be no military uses for clean bombs, but to emphasize that from the standpoint of civil defense, it might be very misleading to assume that an enemy would forego the use of dirty bombs.

C. Persistence of fallout

Witnesses before this committee have testified as to the rapid decay of fission products in fallout. It is true that a fresh mixture of bomb-produced fission products exhibits rapid decay. Half of the radioactivity, as measured from a time base at 1 hour, disappears in 32 hours. The AEC states in its report The Effects of High-Yield Nuclear Explosions (February 1955) "The main radioactivity of fallout decreases very rapidly with time—for the most part, within the first hours after the explosion."

Section 10.1 of the "Effects of Nuclear Weapons" (June 1957) states: "The radiation intensity decreases rapidly with time and except for areas of very high initial contamination, it ceases to be a serious hazard within a few weeks."

These statements, it seems to me, give the impression that civil defense has nothing to worry about after a few days, or a few weeks. I believe that the discussion in section A (part II) coupled with Dr. Schafer's estimates of the fallout intensities show that large areas of the United States could be contaminated to the extent of 2,000 roentgens per hour at 1 hour. The following schedule of roentgen dosages results:

From 1 hour [1] through end of 1st day	4,700 r.
From end of 1st day to end of 1st week	1,730 r.
From end of 1st week to end of 1st month	920 r.
From end of 1st month to end of 1st year	1,060 r.
From end of 1st year to 50 years [2]	840 r.

[1] The dosage of 4,700 r. depends upon fallout at 1 hour after detonation. Fallout at later times (i. e., farther downwind) would significantly reduce this first day dose.
[2] Terrain and weathering would play a significant role in reducing this dose.

If we look only at the decay rate, the decay seems rapid. Starting at 1 hour after the explosion, the rate would be 2,000 r./hr. At 7 hrs. it would decrease to 200 r./hr., at 1 day to 45 r./hr. At the end of 2 days it would be 20 r./hr. At

1 week it would be 4.2 r./hr. or 100 roentgens per day. At the end of 1 month it would be 17 r./day; this would decrease to 4 r./day at 100 days, and to 0.8 r./day at 1 year.

D. Significance of local fallout

I believe that the combination of the vast areas contaminable with high-yield thermonuclear weapons with the long term persistence of the fission products poses a problem for civil defense of great magnitude as different from that of the A-bomb as that was from the TNT bomb.

E. Genetic consequences of a 2,500 mt. attack upon United States

Assume, as in section II–A, that the United States is hit by a 2,500 megaton attack, in which some 2,000 mt. of fission products are deposited on the ground. As we have seen this can be expressed as a contamination equivalent to 12 billion roentgen square miles, where the roentgen as used here is the eternity roentgen. If we make the simplifying assumption that this contamination is spread uniformly over the continental United States, this will produce an eternity exposure of 4,000 roentgens (land area is 3 million square miles). In an actual situation this would be unevenly distributed but the probability is that exposures would be greatest nearest inhabited areas, so this calculation probably underestimates the effect.

Let us assume that people go into hiding and receive no signified radiation exposure in the first month after the attack. I grant that this is highly unlikely so the calculation is again underestimated. From 1 month to 30 years the exposure would average 20 percent of 4,000 roentgens or 800 r. Divide this in half to take account of weathering, so that we get 400 r. as the average exposure to every American who survives. We may now apply Dr. Crow's data of June 4 to this figure of 400 r. Instead of 2 billion children in the next generation we consider one-twentieth this figure. This means that we multiply Dr. Crow's values by 400 and divide by 0.1 (Dr. Crow's assumed 30-year exposure) times 20. Thus we multiply all his figures by a factor of 200. This yields:

Effect	1st generation	Total
a. Physical and mental defects	1,600,000	16,000,000
b. Stillbirths and childhood deaths	4,000,000	120,000,000
c. Embryonic and neonatal deaths	8,000,000	140,000,000
d. Intangible defects	(1)	----------

[1] A larger but unknown number.

In the first generation about 2 out of every 10 children would be genetically defective. The sum total of all deferred deaths from the attack would be 272 million or several times the number killed by the direct attack. In addition almost everyone would shoulder an increased genetic burden.

PART III. REMOTE FALLOUT

NOTE.—I believe that previous testimony and the roundtable discussions on the production, injection, transport, and fallout of radioactive debris from bomb explosions provide a more solid base for evaluating the hazard. It is obvious that in the area of the uptake of fission products there still remain some unknowns which future research and global survey will resolve. It is doubtful if the uncertainties inherent in estimating the biological effects of radiation will be resolved as readily. I wish to comment specifically upon several topics which I feel deserve amplification and emphasis.

A. The "present test rate"

This term has been used frequently before this committee as well as in the public domain during the past few years. Rarely has it been defined in a quantitative manner. To interpret the meaning of the present rate of testing one has to specify a precise number of megatons of fission products injected into the stratosphere.

I do not profess to have any inside information upon which to base an estimate of the present rate of testing. However, it is instructive to consider how the test rate has progressed since 1945. I am drawing upon data openly available and I apologize for the roughness of the data. Nonetheless, the progression of the annual increments to the curve may be worth considering.

The curve I shall plot covers the period from 1945 to 1957 with a projection to the future. Extrapolation of the curve will be considered. First, we plot the total fission yield of all bombs tested in each year. Up until 1952, and more generally until 1954, this total fission yield will correspond roughly to the total bomb yield. After that time corrections have been estimated for the fission fraction of the explosive yield. Second, we then estimate the fraction of the fission debris which is injected into the stratospheric reservoir and is globally dispersed. This fraction will depend upon bomb yield and firing conditions.

I claim no accuracy for the estimate and present the curve for qualitative illustration of the trend in the bomb test rate.

As long as weapons tests were confined to pure fission weapons of relatively low yield the global fallout of strontium 90 would be negligible. With higher yield fission weapons, more of the fission products began to be injected into the stratosphere and retained there for global distribution. However, because of the economy limits (cost of fissionable material) the global hazard was still quite small.

On November 1, 1952, the United States entered a new domain of weapons testing. The Soviet Union followed suit within a year. These tests were then followed by the Castle series of tests in the Pacific in the spring of 1954 when high-yield contaminating bombs were tested at Bikini. These tests added to the stratospheric reservoir the majority of the radiostrontium still present there. Dr. Langham of the Los Alamos Laboratory testified before this committee that one might assume an average rate of 10 megatons of fission products per year. This total of 50 Mt. for the past 5 years would check with the data that I have estimated.

In order to define the present test rate one has to wait until the end of the year, add up the Soviet, United States and United Kingdom contributions to the stratosphere and thus reach a reasonable figure. In the absence of international exchange of data, this is done through remote instrumentation.

B. A limit to bomb tests

Dr. Neuman in testifying before this committee arrived at a "safe" test rate of 2.2 megatons of stratospheric strontium 90 per year; i. e., the amount associated with the annual injection of strontium 90 produced in an explosion such that 2.2 megatons of fission energy inject their fission products into the stratosphere. Some time ago, I estimated on a similar line of reasoning that this "safe" limit would correspond to 3 megatons (plus or minus a factor of 3 times this value); i. e., as high as 9 or as low as 1 megaton per year. I believe this is in general agreement with the 2 to 10 megatons estimated by your roundtable discussion of May 29.

Superimposing these values upon the chart for the yearly test rates, it is seen that this "safe" limit was exceeded in 1954, 1956, and probably will be exceeded this year as well.

I believe the concept of a safe testing rate is of very great importance from a global health viewpoint. But the limit seems so low that it would appear that setting up a quota for each nation's annual testing would de doomed to the same fate as the attempt to control battleship construction in the 1920's.

But I believe that an internationally constituted monitoring system could keep systematic check on the level of fission products which fall out as a result of bomb tests. The publication of these measurements would have a profound effect on world opinion.

It would be of interest to learn from the Atomic Energy Commission the value for the safe annual limit which it assumed in its deliberations during the past 3 years.

C. A limit to war

There is obviously a great difference between the risks which a nation and its people take in time of war. During wartime the "safe" limit for the military application of thermonuclear weapons would be, at least in my opinion, 50 times greater than the peacetime safe limit. Whatever the value, the concept of a limit to the use of nuclear weapons in war is quite new. I have in mind here the inevitable lashback which an aggressor would suffer from the fallout of his own bombs. Dr. Libby, in his speech of January 19, 1956, estimated that between 330,000 and 440,000 megatons of bombs would have to be exploded before "the likelihood of untoward effects would be appreciable."

In view of the consequences to humanity, I wonder whether the Atomic Energy Commission or the Department of Defense has ever prepared a full study of the biological consequences of a nuclear war.

D. Evaluation of risk

Dr. Eisenbud's remark that he is not troubled by the hazard of giving milk containing traces of Sr-90 to his children illustrates, I believe, an extreme form in which a radiation risk may be assayed. The probability of injurious effect to a sample of three people is very small; from a personal viewpoint it is probably negligible. But if one applies the same probability of injury to 3 billion people, then even a very minute effect becomes significant to those whose lives are affected. Suppose we deal with a probability of one in a million. For a global population this would involve about 3,000 people. Such an effect would, from a personal viewpoint, constitute a very small risk. The same line of argument applies to Dr. Shields Warren's reference to the personal danger from the radiation emitted by the microgram of radium in his luminous wristwatch dial. The fact that he observes no visual change in the skin directly beneath the wristwatch proves nothing—certainly, it does not constitute a test of a radiation threshold.

If I may comment on the testimony of other expert witnesses who testified on Monday, June 3, I find Dr. H. L. Friedell's philosophy with regard to bomb tests rather whimsical. Apparently, Dr. Friedell believes we do not have enough data to evaluate the risk and we should proceed on a path of blissful optimism.

Testimony introduced on behalf of Dr. Jacob Furth by Dr. Shields Warren contains a recommendation that "the burden of decision rests not with biomedical investigations, but with military experts." In a matter so rooted in nuclear science and so veiled in conflicting opinions, I am reluctant to entrust the burden of the decision to military experts.

Dr. E. P. Cronkite cites three reports as authoritative in evaluation of strontium risks: (*a*) the United Nations report, (*b*) the British Medical Council report of June 1956, and (*c*) the National Academy of Sciences report of June 1956.

(*a*) So far as I know the United Nations report on strontium has not been concluded. I am informed it will not be published this year.

(*b*) With regard to the British report, I would call attention to the last sentence of that report. "Nevertheless, if the concentration in human bones showed signs of rising greatly beyond one-hundredth of that corresponding to the maximum permissible occupational level it would indicate the need for immediate consideration of the problem."

(*c*) With regard to the National Academy study, may I refer to page 60 of the general report, section 3: "However, if the testing programs of the several countries producing theromonuclear weapons were to intensify, strotospheric storage time may become a critical item in terms of the hazard to mankind."

Since the National Academy report was issued the United States, the U. S. S. R., and the United Kingdom have all tested theromonuclear weapons. I submit that the testing programs are intensifying.

E. Future nuclear tests

In assessing the future commitments of radioactive debris to the earth's atmosphere, we must deal with many unknowns. Had we attempted an estimate 5 years ago, prior to Castle-type weapons, we would have arrived at most misleading and optimistic projections. The end of weapon development is not in sight and no one can say that unexpected developments may not occur.

For example, may not smaller nations be stimulated by British success with thermonuclear type weapons and place maximum emphasis upon such development?

Additionally, can we be sure that a nation would restrain itself and not test a 100 megaton dirty weapon if military requirements and nuclear technology indicated that such a weapon was desirable?

Will the requirements of adapting maximum megatonnage to a small warhead put emphasis on further development of dirty type weapons?

We cannot answer these questions at this time, but we do know that a single weapons test of very high fission yield can add a strontium burden to the atmosphere far beyond the limits we have been discussing.

The United States has contributed the largest fraction of radiostrontium to the stratosphere and I think that it is most encouraging that the fullest discussion of the strontium fallout should occur in this country. I am not aware of any large body of published information on this subject of Soviet origin. It is known, however, that the Soviets are engaged in strontium studies.

In concluding this section, I would like again to stress Dr. Libby's contributions to this subject. They are of very great value and I feel sure that we would be in a much poorer position today to evaluate the strontium problem were it

not for Dr. Libby's personal interest in this field of investigation and the research which he has promoted so vigorously.

F. The strontium problem

It is clear from testimony given to this committee that data on the fallout of radiostrontium are becoming more firm as research results come in. I think that scientists can agree on the pattern of strontium fallout around the world. We are in a poorer position in our knowledge of strontium uptake in the biosphere. I am disturbed by fluctuations which have occurred in AEC statements on discrimination factors in the uptake of strontium into the food chain. Discrimination factors are high in the milk link of the food chain, but are much lower in foods consumed directly by humans. A discrimination factor of 7 is estimated by Dr. N. S. MacDonald of UCLA and Dr. W. Neuman estimates an average discrimination factor of 8.

Strontium 90 determinations in man must be expanded to assess the increase in strontium 90 burden with time. Careful determinations of natural strontium in humans deserve increased attention. We know that more strontium 90 will accumulate in humans as a result of bombs tested in the past and as a result of current tests. The determination as to how much of this radioelement may be tolerated safely is a matter for the biologists to discuss. This committee has heard a fairly wide range of opinion from its expert witnesses on the probable biological effects of Sr-90 levels in man. But it seems to me that even in this area some agreement was reached, especially when Dr. Shields Warren stated on June 3:

"I would be reluctant to see the average strontium 90 content of bones, particularly in children, go much above 10 times the present level."

Dr. Libby's speeches show that Sr-90 fallout will continue and the strontium 90 level in human bones will increase.

Unless restraints are imposed upon commitments of fission products to the atmosphere, it is only a matter of time before the strontium 90 level of Dr. Warren is reached.

PART IV. CONSTRUCTIVE PROPOSALS

A. Atomic Energy Commission information policy

I suggest that this committee or its parent committee may wish to review the information policy of the AEC with regard to nuclear weapon effects, with a view toward revising this policy so that information may be made available more promptly and completely. I believe that the national interest demands a much better relation between the press and the Atomic Energy Commission.

B. Report on the probable biological consequences of a nuclear war

I suggest that the Joint Committee on Atomic Energy might wish to recommend or sponsor the preparation of an analysis of the probable biological effect of nuclear warfare. It would be useful to investigate probable lashback effects from various levels of nuclear bombardment.

C. Data useful to civil defense

I believe that the committee's investigations have produced information of critical value to civil defense planning. It might be useful to have a summary report of these data transmitted to the Federal Civil Defense Administration. I have not seen many representatives of the Federal Civil Defense Administration at these hearings.

D. Research in long-range estimation of nuclear explosives

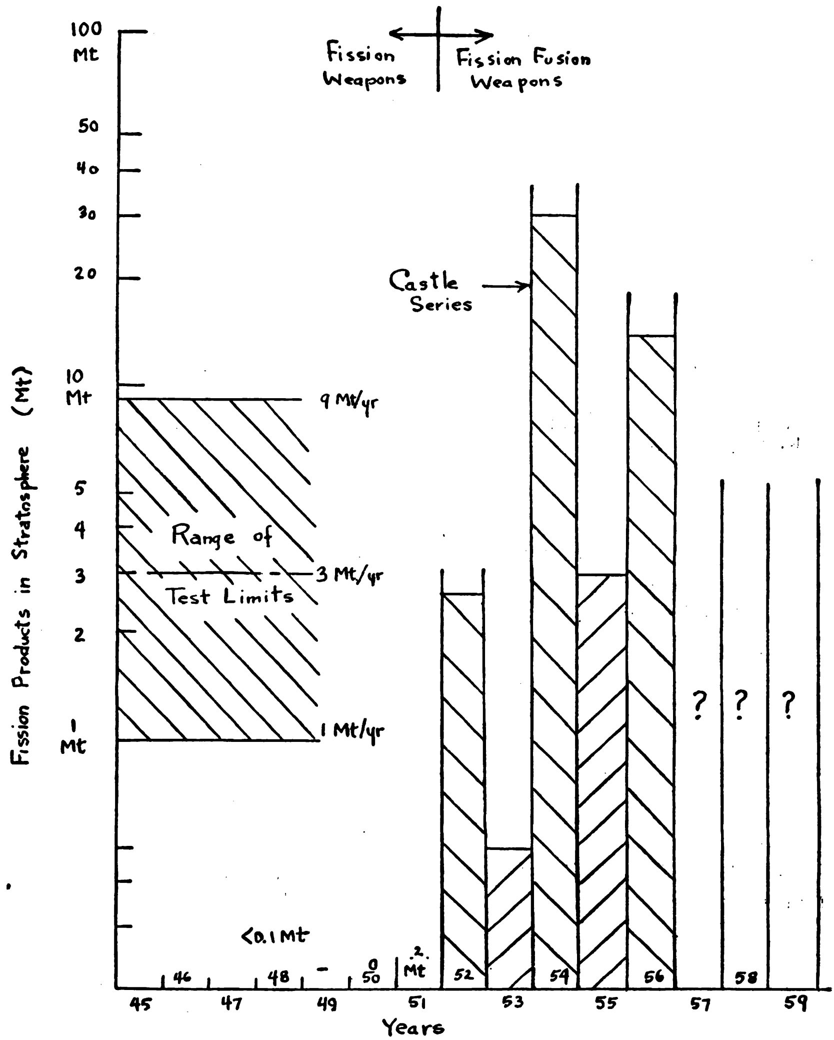
It is known that considerable effort has focused on long-range detection of nuclear detonations. Attention should be given to the declassification of such data as would bear upon evaluation of the radiostrontium problem. Other data would be most useful in discussion of the feasibility of policing an agreed upon test limit.

E. An annual fallout report

In view of the great public concern over fallout hazards, I would urge that the Atomic Energy Commission be required to issue an annual report on the degree of fallout and its uptake in biological systems. Perhaps such a report should be prepared by a university task force.

F. National radiation control

I would urge that the Congress continue its investigations of radiation hazards, extending them into the broader area of peacetime uses of radiation. I believe that the ever-increasing uses of radiation must be subject to legislative controls. Radiation protection in the United States needs uniform legal status.



Rough Estimate of Annual Additions to Stratosphere From Nuclear Weapons Tests of U.S.A And U.S.S.R [in Megatons of Fission Products]

Senator ANDERSON. Dr. Selove, we are happy to have you here today, proceed please.

STATEMENT OF DR. WALTER SELOVE, DEPARTMENT OF PHYSICS, UNIVERSITY OF PENNSYLVANIA [1]

Dr. SELOVE. I would like to thank the Joint Committee for inviting me to present testimony here. I would like to put before you insofar as time is available the results of some independent studies of the radiation hazards problem made primarily by the radiation hazards committee of the Federation of American Scientists.

The federation, as I believe you probably know, consists of scientists who are concerned with the interrelation between science and public affairs, and at the request of a number of members of the federation, a few of the members took on themselves the task of trying to gather such information as was available on the extent of the radiation hazards.

I have been a member of the radiation hazards committee for a year and a half or so and was recently elected chairman of that committee.

Representative COLE. What is the radiation hazards committee to which you refer, Dr. Selove?

Dr. SELOVE. What is it?

Representative COLE. Yes.

Dr. SELOVE. In the second paragraph of the prepared statement here you will see that at present the radiation hazards committee consists of several nuclear physicists, a biophysicist, a biochemist, a chemist, and a cancer research worker.

In the studies which this group has made of the radiation hazards problem we have also consulted with geneticists, and we have consulted with members of the Atomic Energy Commission, who I should say have been most cordial and helpful.

Representative COLE. What I would like to know, Dr. Selove, who is the sponsoring organization—of what organization is this radiation hazards committee of which you are a member?

Dr. SELOVE. This is a radiation hazards committee concerned with possible hazards from radiation sources of all kinds. It is one of the committees of the Federation of American Scientists which is a volunteer organization of scientists—of some of those scientists who are concerned with the impact and interrelations between science and public affairs.

One of the principal objectives of this statement is to emphasize that, although there is no important disagreement among scientists on the magnitude of the average dose of radiation due to fallout, conclusions which may appear to be in conflict with each other can be drawn with equal accuracy from the available facts.

I would like to quote from a report just prepared by the FAS radiation hazards committee which will illustrate this.

[1] Date and place of birth: 1921, Chicago, Ill. Education: Bachelor of science degree, physics, University of Chicago, 1942; doctor of philosophy degree, physics, University of Chicago, 1949. Work history: Assistant instructor, University of Chicago, 1942–43; staff member, M. I. T. Radiation Laboratory (radar), 1943–45; National Research Council predoctoral fellow, 1946–47; physicist, Argonne National Laboratory, 1947–49; instructor, assistant professor, Harvard University, 1950–56; on leave, University of California radiation laboratory, Livermore, 1953–54; member, editorial board, Review of Scientific Instruments, 1955–; National Science Foundation senior postdoctoral fellow, 1956–57; associate professor of physics, University of Pennsylvania, 1956–. (Submitted by witness.)

The committee study of the available scientific facts has led to two conclusions:

First: The added radiation hazard from continued nuclear weapons testing at the present rate is no greater than that from other radiation normally encountered. The radiation from testing will approach a level equivalent to the natural background radiation to which the human race has always been exposed. Similar radiation doses are obtained from annual chest and dental X-rays.

Second: This small added radiation, from whatever source, will cause many deaths.

The committee believes that both conclusions are scientifically correct, and in no way contradict each other.

Unfortunately, those who believe that we should continue testing in order to maintain a lead in nuclear weapons often emphasize the first conclusion and ignore the second. Similarly, those who believe that a test ban is desirable, since it may lessen international tension, often emphasize the second and ignore the first. The committee believes that both statements must be taken together since either alone is misleading.

Rather than try to cover fallout problems exhaustively I would like to restrict my remarks to just a few aspects of the problem, primarily concerned with the global and long-lasting effect of fallout and not taking up the question of close-in fallout effects.

One of the most important aspects of the problem I would like to emphasize is the uncertainties that remain as to just what the effects will be and how large the effects will be. That is, how many individuals will be affected.

There simply has not been sufficient time to learn what all the effects of fallout radiation may be. It is likely to be several decades before we know with much certainty. At present, however, it appears likely that the two principal effects of fallout will be the genetic effects from general gamma radiation and the production of leukemia and bone cancer by strontium 90. With regard to these effects, that is, the somatic effects, there is considerable disagreement as to whether small doses such as are involved in widespread fallout from tests will produce any effect at all. A considerable body of scientific opinion holds that it is very likely that even small doses will produce proportional effects and believes that there is some support for this conclusion both from experimental data and from theory.

Other scientists, as you have heard, are of the contrary opinion and believe there is probably a threshold for somatic effects.

All are agreed, however, that the true behavior cannot be determined from presently available data. I believe it is fair to say that on the basis of the data the pessimistic interpretation seems at least as reasonable as the optimistic interpretation.

In this connection, it should perhaps be emphasized that even if one takes the pessimistic interpretation; namely, that there is no threshold——

Senator ANDERSON. Are you reading from your prepared text?

Dr. SELOVE. No. I have some other statements here.

In connection with this question of pessimistic versus optimistic interpretation, I think it is worth emphasizing that even if one takes the pessimistic approach and assumes that production of leukemia and bone cancer are proportional to even small doses, it is still true that the probability that any given individual would be affected by the levels of radiation received from fallout from tests so far, is very small indeed.

For example, I have made a rough calculation for the type of effect to be expected from the radiation from a wristwatch. I come out with

the following number, regarding which I can go into more detail for you if you wish. The dose of radiation to the wrist from a wristwatch, according to the theory that even small doses may produce bone cancer, would have a certain probability of producing bone cancer, primarily a probability of producing it in the wrist, since the radiation from a wristwatch is concentrated in that area. I have calculated the effects of the dose from a typical wristwatch according to measurements which have been made of radiation from wristwatches. Suppose one had a million individuals not wearing wristwatches, on the one hand and a million individuals wearing wristwatches on the other. Now one would expect that among those not wearing wristwatches some 1,000 out of a million would eventually die, when they die, with bone cancer. This is a number of 1 in about 1,000 of all deaths normally occurring which is due to bone cancer.

Representative COLE. By that you mean that the cause of the death would have been the bone cancer?

Dr. SELOVE. That is correct. Those are statistics for the United States. Two thousand people in this country die every year from bone cancer. Then of the million who do not wear wristwatches, 1,000 would die of bone cancer.

Among those who wear wristwatches I calculate that 1,003 would die of bone cancer.

There would be a certain small effect. It is obviously such a small increase over the normally existing bone cancer and so small an effect on an absolute basis that obviously one individual has absolutely no worry as far as he himself is concerned as to the magnitude of this effect.

As has been emphasized before you by many witnesses, when one takes a very small percentage increase in some normally occurring effect and applies it to the very large population of the world then a considerable number of individuals may still be affected. It might be worthwhile giving a second example for the case which Mr. Holifield raised the other day, as to whether a mother should be concerned about letting her children drink milk which has the amount of strontium 90 in it that has been found to exist at present.

I calculate also on the basis of further numbers that I may present if you wish, and if time is available, that if you have again a number of children or adults drinking milk which has no strontium 90 in it and another group drinking milk which has the amount of strontium 90 currently present, then among those who drink milk with no strontium 90, 1 in 150 of them when they die will die of leukemia. This is a number based on statistics for the United States. It may be surprising to you that every year in the United States 10,000 people die of leukemia among a million and a half deaths. This is 1 in 150.

I calculate that if one drinks milk which contains the present amount of strontium 90 and if one uses the assumption that the effects of strontium 90 even in small doses are that proportionate amount of bone cancer and leukemia will be produced, then the probability that an individual will develop leukemia will be increased from 1 in 150 to 149.

Obviously from the standpoint of an individual, this is an extremely small number. This represents about a 1 percent increase in the probability that one would develop leukemia. The 1 percent increase translated into numbers which apply to a large part of the world popula-

tion, for example a country of the size of Japan means the following: If again we take for Japan—I do not have statistics available for this but I think we can use probably reasonably accurate numbers approximately the same as those in the United States—a number of deaths of leukemia per year which is probably 5,000 or 10,000 and if we have a 1 percent increase in that effect—and a 1 percent increase is the amount that one calculates from the kind of data presented to you by the expert witnesses in these fields—it means that in Japan as a result of the tests so far one may have each year at the present time 10,100 deaths from leukemia instead of the 10,000 which would normally occur.

Again this 100 additional deaths in one country—one should perhaps multiply this by some larger factor if one wants to consider the world as a whole—is a very small percentage compared to the million or so people who die in Japan every year, and even compared to the 10,000 or so who die of leukemia every year in Japan.

It is one purpose of this statement to gather together these numbers which have been presented to you by the various witnesses here and to show that depending on which aspect of the data one emphasizes one gets a result which has quite a different appearance, from the result which appears when the data is presented in another way.

To summarize this point, then, it has been estimated that some 10 percent of normal leukemia cases are due to natural background radiation, and a similar figure may also hold for bone cancer. The fallout radiation from past tests represents only a small percentage increase over natural radiation and consequently the effects of fallout radiation represent only a small percentage of increase over normal effects.

For example, even without fallout some 10 million people would develop leukemia or bone cancer over the next 30 years. This is an estimate based on the United States statistics. Because this number is so large, even the small percentage increase in radiation resulting from tests still can affect a considerable number of people.

Senator BRICKER. What percentage of those bone cancers are caused by natural background radiation?

Dr. SELOVE. There is not sufficient data to draw a conclusion. The conclusion has been reached by Dr. Lewis who testified before you that in the case of leukemia some 10 percent of the leukemia cases are very likely due to natural background radiation. Leukemia is a cancerlike disease of the blood. If the theory as to how cancer develops held by many geneticists, namely, that it is a mutationlike process, is correct, for one cancerlike disease leukemia, probably the same type of effect would hold for bone cancer.

Senator BRICKER. Is that generally accepted?

Dr. SELOVE. I am sure it is not widely accepted. These matters are just coming to be thought about.

Senator BRICKER. There may be other causes of bone cancer.

Dr. SELOVE. Even according to Dr. Lewis' testimony only 10 percent of leukemia would be due to radiation.

Senator BRICKER. Your statement is based on the premise that they are all caused from radiation?

Dr. SELOVE. No. I should say in this connection that the statistics for the United States are that each year some 10,000 individuals die of leukemia, and 2,000 from bone cancer. If we assume that approxi-

mately the same percentage of bone cancers are due to radiation as one would assume from Dr. Lewis' work is the case for leukemia, then we would have a very small number of bone cancer cases due to radiation as compared to the number of leukemia cases due to radiation.

If one lumps together leukemia and bone cancer it will not make much difference how accurate our estimate is of how much bone cancer is caused by natural radiation.

I would like to note that these figures I have presented are consistent with the recent estimate made by the radiation hazards committee of the British Atomic Scientists Association. I do not have the professional affiliation of the various members of that radiation hazards committee but I recognize one name on the list, that of Dr. Alexander Haddow, who is the director of a cancer research institute in Britain. That committee made an estimate, subject to the same assumption we have been discussing, that even small radiation doses produce proportional amounts of bone cancer. They estimated that some 50,000 cases of bone cancer might be expected to develop as a result of nuclear tests already carried out. This is essentially the same type of number I presented.

You have heard estimates of larger numbers of individuals affected. This number I believe represents a sort of median value. These numbers, it should be emphasized are extremely crude and approximate. There is simply not enough data to obtain very accurate numbers. The true effects might be easily 10 times larger than the numbers I am presenting. They might also be easily 10 times smaller. It is the very uncertainty that needs the strongest consideration.

A great deal of apparent disagreement on the dangers of fallout has been due to a difference in emphasis. The AEC has emphasized that the radiation from strontium 90 from tests so far will represent only a few percent increase from the natural background radiation, on the average. The AEC has further emphasized that this average increase in radiation due to strontium 90 is small compared to the additional radiation exposure many people receive simply as a result of living with a higher background radiation level than average or X-rays. Relative to other sources of radiation it is perfectly true that fallout radiation contributes at the present level of testing only a small additional increment. On the other hand it can be stated that even a small percentage increase over the natural background radiation is likely to harm a considerable number of individuals.

The likelihood that even a small percentage increase over background radiation will cause many deaths applies to radiation from other sources as well as to fallout radiation. It should be remarked that the very rapidly increasing awareness of the effects of radiation is already leading to greatly improved X-ray techniques.

Next is the effect of fluctuations. I think this affects the impact of the fallout problem on the people of the world.

The fact that fallout is not distributed with absolute uniformity among all the people of the world affects the evaluation of the fallout hazard in two important ways. First, as has been emphasized by many people, and particularly by Doctor William Neuman, if a "maximum permissible" level of fallout material, say of strontium 90, is agreed upon, then the world average level should not be allowed to go beyond a fraction of it, or else an appreciable part of the world pop-

ulation would be subjected to more than the "maximum permissible" level.

Second, the detectability of fallout effects, which to a considerable extent determines the psychological impact of the effects, will be increased as a result of variations in the amount of fallout, in the diet, and in the nature of the soil. Some 50,000 individuals may develop leukemia and bone cancer from strontium 90 due to tests so far. If these 50,000 were fairly uniformly spread over the world, they would probably be undetectable among the 3 million or so "normal" cases of bone cancer and the 10 million or so "normal" cases of leukemia which would be present even if there had been no fallout. But in actual fact some world areas will suffer more heavily than others, due to diet and soil nature, and due to the nonuniform distribution of fallout. Although the world average increase in leukemia or bone cancer, for 50,000 total cases due to fallout, would be only 1 in about 60 normally occurring cases of bone cancer, or 1 in about 500 normally occurring cases of leukemia, the relative increase in some areas might be as high as about 1 case for every normally occurring case of bone cancer. Even such a 100 percent effect, from tests so far, might be hard to detect, because the normal incidence of bone cancer is so low, and the number of people subjected to extreme levels is probably very limited.

Is there a danger from test fallout?

The two principal effects of fallout appear to be the genetic effects from general gamma radiation, and the production of leukemia and bone cancer by strontium 90. Scientists are agreed that genetic effects are produced even by small amounts of radiation. With regard to somatic effects, such as cancer, a considerable body of scientific opinion holds, with some support from experimental data and with some basis in theory, that these, also, can be produced even by small doses of radiation. On the assumption that this is true, it can be estimated that the number of individuals likely to be directly harmed in the coming generation by strontium 90 will probably be greater than the number showing genetic effects in that generation—although, for a given amount of fallout, the genetic effects will persist for many generations and will eventually cause direct injury to a total number of persons comparable to that affected by strontium 90. In terms of the immediate impact, therefore, we can concentrate our attention on strontium 90.

In regard to the genetic effect of fallout, Warren Weaver, vice president of the Rockefeller Foundation, and Chairman of the National Academy of Sciences Committee on the Genetic Effects of Atomic Radiation, estimated before a Senate subcommittee on January 16 that radioactive fallout from nuclear weapons testing to date will account for some 6,000 of the 30 million "handicapped" babies to be born in the coming generation. In regard to the effects of strontium 90, estimates quoted previously indicate that some 50,000 individuals over the next 30 years may develop leukemia or bone cancer as a result of tests to date.

How much should one be concerned about fallout effects of this magnitude? This is not a question which can be answered on scientific grounds. Even if one accepts the interpretation of the data presented above as a basis for estimating the number of individuals who will show genetic or somatic effects as a result of the 50 megatons—mil-

lions of tons of TNT equivalent—fission yield of tests so far, there are a number of other factors that must be carefully weighed before final judgment can be reached on the significance of the hazard. Probably no absolute evaluation will be possible, but instead, the evaluation will have to be made relative to the problem of securing peace in the world and in relation to the moral problems involved.

A discussion of some of these questions would take us out of the area at which this statement is directed, but it does seem desirable to emphasize that three of the pertinent factors which must be considered are:

(1) The uncertainties as to the number of individuals affected. The number may be smaller than 50,000, or larger. And the remaining uncertainties as to what all the effects of fallout radiation will be;

(2) The size of the number, 50,000, relative to the numbers of people harmed by other reducible effects; and

(3) The fact that the fallout effects are global, involving citizens primarily of countries other than the testing countries.

The global effects of fallout—as opposed to the more local effects—come almost exclusively from explosions of large nuclear weapons; a single one of these can produce as much worldwide fallout as a thousand bombs of the size used at Hiroshima. Those who consider the further development of large nuclear weapons necessary for military security view the probable effects of fallout as a small addition to other hazards of day-to-day living. Those who consider that the further development of large nuclear weapons moves the world away from peace rather than toward it of course do not feel that the military desirability of such weapons justifies the probable fallout effects of tests. Finally, there is a third group who, while not certain whether the further development of large weapons is likely to be useful, feel that in view of the uncertainties as to the nature and magnitude of fallout effects, and in view of the international political impact that testing of large nuclear weapons produces in the involuntarily exposed majority of the world, the nuclear testing powers would be well advised to exercise strong restraint with regard to tests producing further significant amounts of fallout.

As new nations enter the nuclear testing program, it can be expected that they will be interested in testing bomb types which produce a great deal of fallout. There are two dominant reasons for this:

First, about the most economical way possible to increase the yield of a large bomb is to use an outer shell of natural uranium. This leads to an inexpensive large energy release, but also to a large release of fission products—the worst kind of fallout.

Second, a large amount of fallout increases the devastating power of a nuclear bomb. The addition of a shell of natural uranium to a large thermonuclear bomb can increase the devastating fallout to a very much greater degree, for example, than the addition of cobalt to make a cobalt bomb, and moreover can at the same time increase the energy release by a large amount, which a cobalt shell will not do.

Although the United States appears to have turned its efforts to designing nuclear weapons which produce less fallout the U. S. S. R. does not appear to have followed suit as yet. While the United States Pacific tests of last spring did not add very appreciably to the stron-

tium 90 in this country, the first four Russian test explosions of last fall resulted in an increase of about one-third in the strontium 90 concentration in United States soil, within a period of a few weeks. Since that time, the U. S. S. R. has set off at least eight additional nuclear explosions—as of May 15. No information of any precision has as yet been released on the size of these explosions and on the amount of radioactive fallout resulting from them.

The amount of testing which can be tolerated.

I would like to ask Senator Anderson what sort of time scale you would like to proceed on. I can cut this short.

Senator ANDERSON. I have been trying to find out whether we should go ahead with the seminar we had scheduled. If it is agreeable for the other members and participants I would like to suggest that we do not go ahead with it. After all, 6 hours of hearing in 1 day is quite a little bit. If we are not going ahead with it, then I would say if you could conclude in the next 5 or 10 minutes, then there might be questions and we can conclude this afternoon.

Representative COLE. Mr. Chairman, I can only say I exhausted my absorptive powers an hour or so ago.

Senator ANDERSON. I would like to say to the participants that it is very difficult to those of us who are not scientists to try to keep up with papers that are presented and not do what Congressman Cole has just suggested. He has been a faithful attender of these hearings and trying his best to absorb it. It is unjust to him and to the participants.

If there is no vigorous and violent exception I will announce now that the seminar will not be held at the conclusion of this, but we hope to start off with it in a preliminary fashion tomorrow. Maybe it will give us a chance to therefore develop it.

I would ask the witness if he does not mind to try to conclude in 10 minutes.

Is there any objection? If not, you may proceed.

As you know, I am very much interested in the Federation of American Scientists and devoted to their program.

Dr. SELOVE. I would like to introduce in the record at the end of this testimony a copy of the report which the Radiation Hazards Committee has prepared, and if there are any questions we can discuss it further tomorrow.

I think it is useful to put the fallout problem in perspective through a look at some of the numbers of individuals affiected by fallout problems as compared to those affected by other related effects and some of these numbers are written out in this report.

(The report above referred to together with two statements by Dr. Selove follow:)

WORLDWIDE FALLOUT FROM H-BOMB TESTING—ALARMING OR NEGLIGIBLE?

Written by the Radiation Hazards Committee of the Federation of American Scientists

For almost a year highly qualified scientists have been making apparently conflicting statements on the hazards of fallout from continued nuclear weapons testing. Because of the existence of such conflicting statements, the radiation hazards committee has felt it desirable to make an independent and objective study of this important issue.

After 6 months of study and discussion the committee has reached complete agreement on the scientific facts of worldwide fallout and its hazard, and on the scientific conclusions that can be drawn from these facts.

The members are also agreed that arguments for or against the banning of nuclear weapons tests must be based primarily on moral grounds and on considerations of international affairs, and not purely on a scientific evaluation of the radiation hazards.

The committee study of the available scientific facts has led to two conclusions:

First: The added radiation hazard from continued nuclear weapons testing, at the present rate, is no greater than that from other radiations normally encountered. The radiation from testing will approach a level equivalent to the natural background radiation to which the human race has always been exposed. Similar radiation doses are obtained from annual chest and dental X-rays.

Second: This small added radiation, from whatever source, will cause many deaths.

The committee believes that both conclusions are scientifically correct, and in no way contradict each other.

Unfortunately, those who believe that we should continue testing in order to maintain a lead in nuclear weapons often emphasize the first conclusion and ignore the second. Similarly those who believe that a test ban is desirable, since it may lessen international tension, often emphasize the second and ignore the first. The committee believes that both statements must be taken together, since either alone is misleading.

How is it that both statements are correct? Why are they not contradictory? Only by an examination of the facts can these questions be answered.

1. OUR NORMAL RADIATION BACKGROUND

One must first recognize that the human race has always been subjected to a continuing radiation dose from natural, unavoidable causes such as cosmic radiation from outer space and natural radioactivity in earth and rocks. For comparative purposes we have taken as standard the normal, unavoidable, radiation experienced by a person living in a frame house in Germantown, Pa.

The figures below show that a person living in a stone house in Denver experiences a background radiation 35 percent higher than the Germantown resident. Other similar variations occur, owing to differences in the radium content of the drinking water, the wearing of a luminous dial wristwatch, etc. Many of us experience even larger doses from annual dental or chest X-rays.

How will the radiation dose from continued H-bomb testing compare with these "normal" radiations?

	Added radiation dose above the natural background dose received by a Germantown resident living in a frame house (percent)
"Natural" causes:	
Living in a brick or stone house	20
Living at 5,000 feet altitude (Denver, Colo.)	15
Total additional background for a person living in a stone house in Denver	35
Other causes:	
Wearing a luminous dial wristwatch	20
Having an annual chest X-ray	From 10 to 200

2. ADDED RADIATION FROM NUCLEAR WEAPONS TESTING

The radiation hazards from weapons testing that are the most serious are first the hazard to individuals due to the accumulation of radioactive strontium in the bone, and second the genetic hazard to future generations due to increased numbers of harmful mutations produced by the general rise in the external radiation background.

Considering first the bone radiation from strontium, it has been verified that radioactive strontium from weapons already tested is now accumulating on the ground. Since strontium is chemically similar to calcium, it is being taken up by plants and through our food chain is passing into our bodies and is being stored in our bones. Here it emits radiations, causing an increase in our bone dose over that from natural causes.

	Percent
Present (1955) average annual dose to human bone from strontium from past tests	0.2
Average annual strontium bone dose in 1970 if no more weapons are exployed	5
Average annual strontium bone dose by the year 2000 if tests continue	35

If weapons testing continues at the present rate, our children and grandchildren will on the average receive a bone dose of radiation 35 percent again as much as that received from natural causes.

Turning next to the radiation that may be genetically harmful to future generations, it is mainly the increase in external radiations that must be considered rather than internal radiations such as bone strontium. If nuclear weapons testing continues at the present rate the genetic dose will rise by 2 percent of the normal background dose. On the average we receive a 60 percent genetic dose from annual medical and dental diagnostic X-rays.

What will be the effect of fallout radiation doses as small as a bone dose 35 percent of natural background, or a genetic dose 2 percent of natural background?

Cause:	*Added radiation dose—Above the natural background dose received by a Germantown resident living in a frame house (percent)*
Average added genetic dose if tests continue at the present rate	2
Present average added genetic dose per year per person in the United States from X-rays	60

8. THE EFFECTS OF RADIATIONS

(a) The strontium hazard

Radiations in large doses are definitely known to produce leukemia and bone cancer, and at high dose levels the incidence is proportional to the dose. But what is the effect of small doses? For leukemia, evidence has been obtained that indicates that the incidence is directly proportional to the radiation received, and that about one-tenth of all present cases are due to normal background radiation. It is reasonable to assume that the same is also true for bone cancer.

While these interpretations are not yet rigidly proved, they are reasonable conclusions from the data and we can use them as the basis for estimating the extent of radiation effects.

There are 12,000 deaths annually in the United States from leukemia and bone cancer. If one-tenth of these cases are due to the natural radiation background, then an added radiation dose 35 percent of natural background, whether from continued H-bomb testing or from annual chest X-rays for all people, would be estimated to produce 400 additional deaths from bone cancer and leukemia in the United States per year. Over one generation of 30 years this figure would total 12,000 deaths. Many more deaths will be produced outside the United States by continued testing.

Deaths per year per 5 million people in the United States. (Approximately the population of greater Philadelphia)

Deaths per year from all causes	[1] 50, 000
Deaths per year from motor accidents	1, 100
Deaths per year from bone cancer and leukemia	380
Estimated present deaths per year from bone cancer and leukemia caused by natural background radiation	38
Estimated added deaths per year from bone cancer and leukemia for those living in stone houses at 5,000 feet altitude	13
Estimated aded deaths per year from bone cancer and leukemia from bone cancer and leukemia by 2000 A. D. due to continued testing	13
Estimated added bone cancer and leukemia deaths per year in 1970 from weapons already exploded	[2] 2

[1] Per 5 million people.
[2] Per 5 million people.

The above figures are for the United States only. They are probably also correst for the rest of the western world, but may be incorrect for those parts of the world where the death rate from other causes is much higher than in the western world.

(b) *The genetic hazard*

Mutations, or changes in the hereditary material passed on from parent to offspring, are known to be produced by radiation as well as by chemical and other causes, many unknown.

Geneticists are agreed that any increase in the mutation rate is bad, and they are also agreed that any added radiation dose will increase the mutation rate; the larger the dose the greater the number of mutations produced.

By how much does the mutuation rate in man increase, for an added dose of 2 percent of the natural background (from continued testing)? Or for an added dose of 60 percent of the natural background (the average X-ray dose per person under 30 in the United States)?

The absolute answers to these questions are not known. We can say, though, that in the United States the increasing use of X-rays and fluoroscopy for medical and dental diagnosis is a very much greater genetic hazard than is the increased radition from nuclear-weapons testing.

4. NUCLEAR WAR

Both those who favor continued H-bomb testing and those who favor a test ban are agreed that a major nuclear war would be a world catastrophe from which civilization might not rise again. They only disagree on the best method of averting such a war. Apart from the death, injury and destruction caused at the time, the radiation background would be raised many times above that from continued testing, and would result in further hundreds of millions of deaths in the years following the war.

CONCLUSIONS

While the scientific evidence discussed above is admittedly not complete, it is complete enough to allow reasonable conclusions to be made.

First, the radiation hazard from continued nuclear-weapons testing at the present rate is no greater than that from other radiations normally encountered.

Second, even this small added radiation will cause many deaths.

Third, even more lives are endangered by other radiation hazards such as from X-ray examinations. In the case of X-ray examinations the advantages are usually important enough to outweigh the disadvantages. The medical profession is alert to this problem and is taking active measures to reduce the exposure to a minimum.

Will continued testing lead to a hazard that is alarming?

If one believes that even one death from fallout is too many, and that no nation should subject other peoples to the effects of fallout without their consent, then the answer to the question is "Yes."

If, on the other hand, one believes that the security of the country depends on continued testing, then the answer to the question is, "No; it is a small price to pay for security."

Each person, in trying to answer these questions for himself, must make personal judgments based on the moral problems involved and on the problem of maintaining peace.

THE RADIATION HAZARDS COMMITTEE OF THE FEDERATION OF AMERICAN SCIENTISTS

Dr. Walter Selove, associate professor of physics, University of Pennsylvania, Philadelphia (chairman).
Dr. Richard L. Burling, lecturer in physics, University of Pennsylvania, Philadelphia.
Dr. Stanley C. Glauser, biophysical chemist, postdoctoral research fellow, University of Pennsylvania, Philadelphia.
Dr. Donald L. Glusker, research chemist, Rohm & Haas Co., Philadelphia.
Dr. Norman Goldberg, assistant professor of physics, University of Pennsylvania, Philadelphia.
Dr. Philip Grant, research associate, Institute for Cancer Research, Fox Chase, Philadelphia.
Dr. Rosalie C. Hoyt, biophysicist, associate professor of physics, Bryn Mawr College, Bryn Mawr, Pa.
Dr. John R. Pruett, nuclear physicist, associate professor of physics, Bryn Mawr College, Bryn Mawr, Pa.

SUMMARY OF REMARKS ON RADIOACTIVE FALLOUT PRESENTED BY WALTER SELOVE TO THE JOINT COMMITTEE ON ATOMIC ENERGY AT ITS OPEN HEARINGS

Although there is no important disagreement among scientists on the magnitude of the average dose of radiation due to fallout, conclusions apparently in conflict with each other can be drawn with equal accuracy from the available facts. As stated in a report just prepared by the radiation hazards committee of the Federation of American Scientists, the available scientific facts lead to two conclusions:

"First, the added radiation hazard from continued nuclear weapons testing at the present rate is no greater than that from other radiation normally encountered. The radiation from testing will approach a level equivalent to the natural background radiation to which the human race has always been exposed. Similar radiation doses are obtained from annual chest and dental X-rays.

"Second, this small, added radiation, from whatever source, will cause many deaths.

"The committee believes that both conclusions are scientifically correct, and in no way contradict each other.

"Unfortunately, those who believe that we should continue testing in order to maintain a lead in nuclear weapons often emphasize the first conclusion and ignore the second. Similarly, those who believe that a test ban is desirable, since it may lessen international tension, often emphasize the second and ignore the first. The committee believes that both statements must be taken together, since either alone is misleading."

There has not been sufficient time to learn what all the effects of fallout radiation may be. It is likely to be 10 or 20 years or more before we know with much certainty. At present, however, it appears likely that the two principal effects of fallout will be the genetic effects from general gamma radiation, and the production of leukemia and bone cancer by strontium 90.

Rough estimates can be made of the number of individuals likely to show these effects. The estimates are that, from tests through 1956, some 6,000 individuals throughout the world will show serious genetic effects in the next 30 years, and probably also some 50,000 will develop leukemia or bone cancer. (The genetic effects of a given amount of radiation will persist for many generations, and the total number of individuals ultimately affected genetically, from a given amount of fallout, will be comparable to the total number affected by strontium 90.) These estimates can only be made approximately—the correct numbers may be several times larger or several times smaller.

The fallout radiation from past tests represents only a small percentage increase over natural radiation (cosmic rays, natural radioactivity), and consequently the effects represent only a small percentage increase over "normal" effects. For example, even without fallout some 10 to 20 million people would develop leukemia over the next 30 years. Because this number is so large, even the small percentage increase in radiation, resulting from tests, still can affect such a considerable number of individuals as 50,000.

Should fallout effects of this magnitude be called "large," or "small"? This is not a question which can be answered on scientific grounds. A personal judgment is necessarily involved, based on the problem of securing peace in the world and on moral questions. Some of the factors which must be considered are (1) the uncertainties as to the nature and magnitude of fallout effects, (2) the size of the number 50,000 relative to the numbers of people harmed by other reducible effects, and (3) the fact that fallout effects are globel, involving peoples primarily outside of the testing countries.

As new nations enter the nuclear testing program, it can be expected that they will be interested in testing bomb types which produce a great deal of fallout. There are two dominant reasons for this: First, about the most economical way possible to increase the yield of a large bomb is to use an outer shell of natural uranium. This leads to an inexpensive large energy release, but also to a large release of fission products—the worst kind of fallout. Second, a large amount of fallout increases the devastating power of a nuclear bomb. The addition of a shell of natural uranium to a large thermonuclear bomb can increase the devastating fallout to a very much greater degree, for example, then the addition of cobalt to make a "cobalt bomb," and moreover can at the same time increase the energy release by a large amount, which a cobalt shell will not do.

Although the United States appears to have turned its efforts to designing nuclear weapons which produce less fallout, the U. S. S. R. does not appear to have followed suit as yet. While the United States Pacific tests of last spring

did not add very appreciably to the strontium 90 in this country, the first four Russian test explosions of last fall resulted in an increase of about one-third in the strontium 90 concentration in United States soil, within a period of a few weeks.

Official evaluations of the strontium 90 hazard have generally been given in terms of a "maximum permissible concentration" (MPC). The use of such a term is unfortunate, since it implies that the immediate and long-term effects of small amounts of strontium 90 in human bones are known with precision. They are not. Furthermore, it has been noted by many specialists that a "maximum permissible" dose is not necessarily a safe dose. It is possible to estimate from available data that, if large numbers of people are given a "permissible" dose, or one-tenth microcurie of strontium 90, then it is likely that about one in a thousand will develop bone cancer in his lifetime.

The uncertainty in the specification of a "permissible" level is indicated by the fact that the AEC has recently felt it advisable to revise downwards the strontium 90 level considered "permissible," to one-tenth of the value formerly used—namely, to one-tenth microcurie for the "standard" man. This has brought the AEC "permissible" level into agreement with that recommended for "large populations" by the International Commission on Radiological Protection. No recommendation has been given for the case that the entire world population is exposed; for this case, there is good reason to take the "acceptable" value as being still smaller.

The setting of a "tolerance" level for strontium 90 is arbitrary. On the assumption that even small amounts of radiation will produce proportionate amounts of leukemia and bone cancer, it can be estimated that a worldwide average strontium 90 level equal to the (new) "permissible" level would be likely to produce several million cases of leukemia and bone cancer. (A worldwide average level equal to the old "permissible" level would correspondingly be likely to produce several tens of millions of cases of leukemia and bone cancer.) On the basis of these estimates, one would hardly be willing to permit the world average strontium 90 level to come at all near the "permissible" level.

Although the setting of a "tolerance" level is arbitrary, the strontium 90 hazard from further testing can be measured against the yardstick of the amount of strontium 90 already released by tests to date. By such a yardstick, it could reasonably be argued that an increase of, say, 1 part in 1,000 in the amount of strontium 90 already distributed globally would hardly be considered catastrophic. An increase of that amount would be produced by some 50 kilotons (thousand tons of TNT equivalent) of fission yield—2 or 3 of the small bombs used to destroy Hiroshima.

The explosion of megaton-range weapons may or may not produce large amounts of fallout, depending on the bomb design—in particular, depending on whether a large part of the energy release comes from fission. It would take only a few bombs of a type releasing some 10 megatons of fission energy to double the total global fallout now existing. It is clear that so far as radiation hazard is concerned, for bombs of the types which have been tested so far, attention can be confined to the large bombs. As to the question of how much we should be concerned over a doubling, say, of the fallout produced so far, that is a matter into which many complex factors enter, as discussed previously, and no answer can be given on scientific grounds alone.

Although there is much room for disagreement on evoluation of the available data as to the results of fallout from tests, there is no disagreement as to the utter catastrophe that would result from a full-scale nuclear war. Hundreds of millions of people would be killed outright by blast, fire, and radiation, hundreds of millions more would die from the later effects of radiation injury, further comparable numbers in succeeding generations would suffer from genetic effects resulting from the radiation, and large parts of the devastated countries would be not only destroyed but made uninhabitable for extended periods.

The AEC is to be highly commended for the detailed measurements it has made on fallout, and for the steady release of the results of these measurements. However, the AEC has the dual responsibility of conducting a weapons development program and of evaluating the fallout hazard. It can readily be seen that decisions felt to be necessary in one area might conflict with and unduly influence decisions in the other. Because of these conflicting responsibilities, it may legitimately be questioned whether both functions belong in the same agency.

The suggestion has been made that an independent group of qualified scientists be appointed to take over the study and evaluation of fallout hazard and other radiation problems as well. In 1955, the National Academy of Sciences appointed a group to conduct such a study. The report submitted by that group was prepared over a year ago. Since that time, many new data have become available and many highly qualified specialists have expressed criticisms of the AEC's treatment of fallout problems. It is very desirable that appraisal of these problems be brought up to date. It can be expected that a group chosen to include representation of differing points of view can arrive at agreement on an interpretation of the data which can serve as an authoritative basis for policy decisions regarding future weapons testing.

We are at the present time forced into the difficult position of having to make decisions on a problem that we do not fully understand. The data on hand, however, do tell us this much: The fallout radiation from past tests will constitute over the next few decades a small percentage increase over background radiation. It is estimated that even this small percentage increase will cause genetic changes affecting some tens of thousands of individuals over a number of generations, and will probably also produce leukemia or bone cancer in a comparable number of individuals over the next few decades. The fallout effects will increase with the amount of fission yield in future tests. Each small explosion will contribute only a small addition to the fallout radiation already produced; each large explosion can contribute an appreciable addition.

It should be emphasized that the effects of nuclear tests are thousands of times smaller than the effects which would result from a nuclear war. While there is no disagreement that nuclear war would be a disaster which we cannot afford, there is disagreement on the best means of avoiding such a war, and some of that disagreement seems to have spilled over into the area of evaluation of the fallout hazard.

An objective scientific evaluation of the radiation hazard from fallout can be, and should be made independent of any policy or military considerations. The extensive information being compiled at these hearings will be of considerable help in arriving at such an evaluation.

Testimony on Radioactive Fallout

By Prof. Walter Selove, chairman, radiation hazards committee, Federation of American Scientists before the Joint Congressional Committee on Atomic Energy

I am Walter Selove, associate professor of physics at the University of Pennsylvania. I have been a member of the radiation hazards committee of the Federation of American Scientists (FAS), and was recently elected chairman of that committee. The prepared statement which follows is presented with the approval of the national executive committee of the FAS.

INTRODUCTION

A principal object of this statement is to emphasize that, although there is no important disagreement among scientists on the magnitude of the average dose of radiation due to fallout, conclusions which may appear to be in conflict with each other can be drawn with equal accuracy from the available facts. I should like in a moment to quote from a report just prepared by the FAS radiation hazards committee.

The FAS radiation hazards committee consists of several nuclear physicists, a biophysicist, a biochemist, a chemist, and a cancer research worker. This committee has felt it desirable to make an independent study of the available facts on fallout, and has tried to understand the basis of the apparent disagreement in the various statements on fallout hazards from qualified scientists. I now quote from the report just prepared:

"The committee study of the available scientific facts has led to two conclusions:

"First: The added radiation hazard from continued nuclear weapons testing at the present rate is no greater than that from other radiation normally encountered. The radiation from testing will approach a level equivalent to the natural background radiation to which the human race has always been exposed. Similar radiation doses are obtained from annual chest and dental X-rays.

"Second: This small added radiation, from whatever source, will cause many deaths.

"The committee believes that both conclusions are scientifically correct, and in no way contradict each other.

"Unfortunately, those who believe that we should continue testing in order to maintain a lead in nuclear weapons often emphasize the first conclusion and ignore the second. Similarly, those who believe that a test ban is desirable, since it may lessen international tension, often emphasize the second and ignore the first. The committee believes that both statements must be taken together, since either alone is misleading."

I shall shortly discuss these apparently conflicting conclusions in more detail; I wish here, however, to quote further from the final part of this report:

"While the scientific evidence * * * is admittedly not complete, it is complete enough to allow reasonable conclusions to be made:

"First: The radiation hazard from continued nuclear weapons testing at the present rate is no greater than that from other radiation normally encountered.

"Second: Even this small added radiation will cause many deaths.

"Third: Even more lives are endangered by other radiation hazards, such as from X-ray examinations.

"In the case of X-ray examinations, the advantages are usually important enough to outweigh the disadvantages. The medical profession is alert to this problem and is taking active measures to reduce the exposure to a minimum.

"Will continued testing lead to a hazard that is alarming?

"If one believes, as does Dr. Albert Schweitzer, that continued weapons development will not contribute to security, and that no nation should subject other peoples involuntarily to the effects of fallout, then obviously even one death from fallout is too many and the answer to the question is 'Yes.'

"If, on the other hand, one believes, as does AEC Commissioner Libby, that the security of the country depends on continued testing, then the answer to the question is 'No, the hazard is a small price to pay for security.'

"Each person, in trying to answer these questions for himself, must make personal judgments based on the moral problems involved and on the problem of securing peace."

The members of the committee are agreed that:

"* * * arguments for or against banning nuclear weapons tests must be based primarily on moral grounds and on considerations of international affairs, and not purely on a scientific evaluation of the rediation hazards."

I should like to confine this written statement to a discussion of the scientific questions involved, and will not present in it any evaluation as to whether the fallout hazard from tests is alarming or negligible. I omit presenting any such evaluation in this statement because it cannot be made on scientific grounds alone, and I wish to try to follow the Joint Committee's request to distinguish clearly between fact and opinion.

Rather than trying to cover fallout problems exhaustively, I wish to focus these remarks on a few aspects of the matter, primarily concerned with the global and long-lasting part of fallout.

UNCERTAINTIES IN NATURE AND MAGNITUDE OF EFFECTS

Strontium 90 is generally agreed to be the most worrisome component of fallout at the present time. The energy resulting from the radioactive decay of strontium 90 is only about one-two-hundredths of all the radioactive energy in fallout. Nevertheless, strontium 90 is one of the most important of the fallout components because of its long persistence and its concentration into a relatively small part of the human body—the skeleton. In regard to worldwide effect, it does not appear at present that any other fallout component presents a hazard comparable to that of strontium 90, but I do not believe it has been demonstrated beyond reasonable doubt that no other fallout component is concentrated, perhaps in some particular organ, perhaps through the food chain, to a level likely to present an appreciable hazard.

It must be said that no one knows with certainty what effects will be produced in humans by small amounts of strontium 90 carried for long periods of time. There is no direct experience with strontium 90 in humans under these conditions. We are therefore limited to extrapolations from animal studies and from effects of other radiation sources on humans. On consideration of the physical and chemical effects of strontium 90 radioactivity, there seems good reason to believe that no new type of effect will turn up, but again I believe this cannot be said with absolute certainty.

STRONTIUM 90 DOSE LEVELS AND INTERPRETATIONS IN TERMS OF NUMBER OF INDIVIDUALS AFFECTED

With the support of the AEC, data have been collected from any parts of the globe which permit a fairly accurate statement of the average dose of radiation from strontium 90. There is no important disagreement about this data per se. Here it should be stressed first of all that, in some areas, because of differences in fallout patterns, differences in the nature of the soil and differences in dietary habit, the dose of radiation from strontium 90 may be considerably higher than the average. However, accepting the average figure as a basis for discussion, what are the expected effects? Here, there is some disagreement, involving differing scientific opinion.

The principal effect of strontium 90 will probably be the production of bone cancer and leukemia. Although we cannot know the full effects of strontium 90 in humans until 20 years or more have passed, estimates of the number of new cases induced can be made, as already mentioned, by reference to animal experiments and to certain data on humans who have been exposed to radiation of other sorts.

That radiation can induce cancer is known. Animal experiments show that the number of induced tumors is directly related to the total amount of radiation given. It is true that these data are obtained with high doses of radiation and it is not certain that the results can be extrapolated to low doses of radiation. Such an extrapolation is, on the other hand, a reasonable one and results reported in a recent paper by E. B. Lewis (Science, May 17, 1957) strongly support the validity of such an extrapolation in the case of radiation-induced leukemia in man. Dr. Lewis' work suggests, in fact, that 5 to 10 percent of all present cases of leukemia are due to normal "background" radiation reaching the bones—cosmic rays, and natural radioactivity from our surroundings and from internal sources. If this is true for the cancerlike disease, leukemia, it is reasonable to assume that, for bone cancer as for leukemia, a fraction of present cases is due to normal background radiation. The total number of deaths due to bone cancer, in the United States, is only one-fifth of the number due to leukemia, so if we lump bone cancer and leukemia effects together, estimates of the production of these two diseases by radiation will not be far in error, even if we do not know with any accuracy what fraction of bone-cancer deaths is normally due to background radiation. We therefore assume that about 10 percent of the normally occurring cases of bone cancer as well as of leukemia are due to background radiation. (See p. 962.)

If natural background dose to the bone is responsible for 5 to 10 percent of normal leukemia and bone cancer, then even a small percentage increase over background would harm many individuals. The average bone dose of radiation from strontium 90 derived from tests already conducted will rise to 5 to 10 percent of natural background. The incidence of leukemia and bone cancer would consequently rise one-quarter to 1 percent. Since some 10 million individuals in the next generation would normally die of leukemia or bone cancer (estimate based on statistics for the United States), this one-quarter to 1 percent increase represents 25,000 to 100,000 individuals. Thus, although normally only about 1 in 150 or so of all deaths (statistics for United States) would be due to leukemia or bone cancer, an increase as small as one-quarter to 1 percent in this rate still represents many individuals.

It may be noted that these figures are consistent with a recent estimate made by the Radiation Hazards Committee of the British Atomic Scientists Association. That committee estimated that, subject to the assumption that even small radiation doses produce proportionate amounts of bone cancer, some 50,000 cases of bone cancer might be expected to develop as a result of nuclear tests already carried out.

A great deal of apparent disagreement on the dangers of fallout has probably been due simply to a difference in emphasis. The AEC has emphasized that the radiation from strontium 90 from tests so far will represent only a few percent increase over natural background radiation, on the average, and the AEC has further emphasized that this average increase in radiation due to strontium 90 is small compared to the additional radiation exposure many people receive as a result of living with a higher background radiation level than average, or as a result of medical X-rays. Relative to other sources of radiation, then, it is perfectly true that fallout radiation contributes, at the present level of testing, only a small additional increment. On the other hand, it can be stated that even a small percentage increase over natural background radiation is likely to harm a considerable number of individuals, as discussed above.

The likelihood that even a small percentage increase over background radiation will cause many deaths applies to radiation from other sources as well as to fallout radiation. It should be remarked that the very rapidly increasing awareness of the effects of radiation is already leading to greatly improved X-ray techniques.

THE EFFECT OF FLUCTUATIONS

The fact that fallout is not distributed with absolute uniformity among all the people of the world affects the evaluation of the fallout hazard in two important ways. First, as has been emphasized by many people, and particularly by Dr. William Neuman, if a "maximum permissible" level of fallout material, say of strontium 90, is agreed upon, then the world-average level should not be allowed to go beyond a fraction of it, or else an appreciable part of the world population would be subjected to more than the "maximum permissible" level.

Second, the detectability of fallout effects, which to a considerable extent determines the psychological impact of the effects, will be increased as a result of variations in the amount of fallout, in the diet, and in the nature of the soil. Some 50,000 individuals may develop leukemia and bone cancer from strontium 90 due to tests so far. If these 50,000 were fairly uniformly spread over the world, they would probably be undetectable among the 3 million or so "normal" cases of bone cancer and the 10 million or so "normal" cases of leukemia which would be present even if there had been no fallout. But in actual fact some world areas will suffer more heavily than others, due to diet and soil nature, and due to the nonuniform distribution of fallout. Although the world-average increase in leukemia or bone cancer, for 50,000 total cases due to fallout, would be only 1 in about 60 normally occurring cases of bone cancer, or 1 in about 500 normally occurring cases of leukemia, the relative increase in some areas might be as high as about 1 case for every normally occurring case of bone cancer. Even such a 100 percent effect, from tests so far, might be hard to detect, because the normal incidence of bone cancer is so low, and the number of people subjected to extreme levels is probably very limited.

IS THERE A DANGER FROM TEST FALLOUT?

The two principal effects of fallout appear to be the genetic effects from general gamma radiation, and the production of leukemia and bone cancer by strontium 90. Scientists are agreed that genetic effects are produced even by small amounts of radiation. With regard to somatic effects, such as cancer, a considerable body of scientific opinion holds, with some support from experimental data and with some basis in theory, that these, also, can be produced even by small doses of radiation. On the assumption that this is true, it can be estimated that the number of individuals likely to be directly harmed in the coming generation by strontium 90 will probably be greater than the number showing genetic effects in that generation—although, for a given amount of fallout, the genetic effects will persist for many generations and will eventually cause direct injury to a total number of persons comparable to that affected by strontium 90. In terms of the immediate impact, therefore. we can concentrate our attention on strontium 90. In regord to the genetic effect of fallout, Warren Weaver, vice president of the Rockefeller Foundation and Chairman of the National Academy of Sciences Committee on the Genetic Effects of Atomic Radiation, estimated before a Senate subcommittee on January 16 that radioactive fallout from nuclear weapons testing to date will account for some 6,000 of the 30 million "handicapped" babies to be born in the coming generation. In regard to the effects of strontium 90, estimates quoted previously indicate that some 50,000 individuals over the next 30 years may develop leukemia or bone cancer as a result of tests to date.

How much should one be concerned about fallout effects of this magnitude? This is not a question which can be answered on scientific grounds. Even if one accepts the interpretation of the data presented above as a basis for estimating the number of individuals who will show genetic or somatic effects as a result of the 50 megatons (millions of tons of TNT equivalent) fission yield of tests so far, there are a number of other factors that must be carefully weighed before final judgment can be reached on the significance of the hazard. Probably no absolute evaluation will be possible, but instead, the evaluation will have to be made relative to the problem of securing peace in the world and in relation to the moral problems involved. A discussion of some of these questions would take us out of the area at which this statement is directed.

but it does seem desirable to emphasize that three of the pertinent factors which must be considered are: (1) The uncertainties as to the number of individuals affected (the number may be smaller than 50,000, or larger), and the remaining uncertainties as to what all the effects of fallout radiation will be; (2) the size of the number 50,000 relative to the numbers of people harmed by other reducible effects; and (3) the fact that the fallout effects are global, involving citizens primarily of countries other than the testing countries.

The global effects of fallout (as opposed to the more local effects) come almost exclusively from explosions of large nuclear weapons; a single one of these can produce as much worldwide fallout as a thousand bombs of the size used at Hiroshima. Those who consider the further development of large nuclear weapons necessary for military security view the probable effects of fallout as a small addition to other hazards of day-to-day living. Those who consider that the further development of large nuclear weapons moves the world away from peace rather than toward it of course do not feel that the military desirability of such weapons justifies the probable fallout effects of tests. Finally, there is a third group who, while not certain whether the further development of large weapons is likely to be useful, feel that in view of the uncertainties as to the nature and magnitude of fallout effects, and in view of the international political impact that testing of large nuclear weapons produces in the involuntarily exposed majority of the world, the nuclear testing powers would be well advised to exercise strong restraint with regard to tests producing further significant amounts of fallout.

As new nations enter the nuclear testing program, it can be expected that they will be interested in testing bomb types which produce a great deal of fallout. There are two dominant reasons for this: First, about the most economical way possible to increase the yield of a large bomb is to use an outer shell of natural uranium. This leads to an inexpensive large energy release, but also to a large release of fission products—the worst kind of fallout. Second, a large amount of fallout increases the devastating power of a nuclear bomb. The addition of a shell of natural uranium to a large thermonuclear bomb can increase the devastating fallout to a very much greater degree, for example, than the addition of cobalt to make a "cobalt bomb," and moreover can at the same time increase the energy release by a large amount, which a cobalt shell will not do.

Although the United States appears to have turned its efforts to desiging nuclear weapons which produce less fallout, the U. S. S. R. does not appear to have followed suit as yet. While the United States Pacific tests of last spring did not add very appreciably to the strontium 90 in this country, the first 4 Russian test explosions of last fall resulted in an increase of about one-third in the strontium 90 concentration in United States soil, within a period of a few weeks. Since that time, the U. S. S. R. has set off at least 8 additional nuclear explosions (as of May 15). No information of any precision has as yet been released on the size of these explosions and on the amount of radiactive fallout resulting from them.

THE AMOUNT OF TESTING WHICH CAN BE "TOLERATED"

It is not practical to speculate here on what new kind of fallout radiation might be produced by new types of nuclear weapons. Consequently the "tolerable" level of testing will be discussed in terms of the kinds of weapone which have already been exploded. As discussed previously, attention will be concentrated on the effects of strontium 90.

Official evaluations of the strontium 90 hazard have generally been given in terms of a "maximum permissible concentration" (MPC). The use of such a term is unfortunate, since it implies that the immediate and long-term effects of small amounts of strontium 90 in human bones are known with precision. They are not. Furthermore, it has been noted by many specialists that a "maximum permissible" dose is not necessarily a safe dose. It is possible to estimate form available data that, if a large number of people are given a "permissible" dose, of one-tenth microcurie of strontium 90, then it is likely that about one in a thousand will develop bone cancer in his lifetime. (This estimate follows directly from the results of the British Atomic Scientists Association study of strontium 90 referred to above.)

The uncertainty in the specification of a "permissible" level is indicated by the fact that the AEC has recently felt it advisable to revise downwards the strontium 90 level considered "permissible," to one-tenth of the value formerly used—namely, to one-tenth microcurie for the "standard" man. This has

brought the AEC "permissible" level into agreement with that recommended for "large populations" by the International Commission on Radiological Protection. No recommendation has been given for the case that the entire world population is exposed; for this case, there is good reason to take the "acceptable" value as being still smaller.

Strontium 90 decays relatively slowly (half life of 28 years), and so the effects of amounts produced within a span of only a few years are cumulative. The amount of strontium 90 distributed over the world in fallout is proportional to total fission yield for a nuclear explosion. How many megatons (millions of tons of TNT equivalent) of fission yield can we "tolerate?" No simple answer can be given to this question.

As discussed previously, the setting of a "tolerance" level is arbitrary. It can reasonably be expected that for leukemia and bone cancer due to strontium 90, just as for genetic effects, even small amounts of radiation will produce proportional effects. According to the estimates given previously, a worldwide average strontium 90 level equal to the (new) "permissible" level would be likely to produce several million cases of leukemia and bone cancer. (A worldwide average level equal to the old "permissible" level would correspondingly be likely to produce several tens of millions of cases of leukemia and bone cancer.) On the basis of these estimates, one would hardly be willing to permit the world-average strontium 90 level to come at all near the "permissible" level.

This points up the urgency of obtaining a more accurate evaluation of the level of strontium 90 which can produce injurious effects. It can be expected that it will be some time before such information can be obtained, because the full effects of small doses will probably appear only after 10 or 20 years, or more.

This discussion also points up the fact that use of the term "maximum permissible level" does not give an accurate impression either of the uncertainties involved or of the fact that, if the world-average strontium 90 level were to become equal to the (new) "permissible" level, then the damage likely to be produced is measured in millions of cases of bone cancer and leukemia, and is hardly likely to be "permissible." It would be better to express the magnitude of fallout radiation in arbitrary units—say, in "strontium units"—or else relative to the average magnitude of background radiation.

Let us return to the question of how many megatons of fission yield we can tolerate. It is frequently useful to use the magnitude of an existing hazard as a yardstick for evaluating a contemplated hazard. In this case, we may use as a yardstick the amount of strontium 90 already released by tests to date. By such a yardstick, it could reasonably be argued that an increase of, say, 1 part in 1,000 in the amount of strontium 90 already distributed globally would hardly be considered catastrophic. An increase of that amount would be produced by some 50 kilotons (thousand tons of TNT equivalent) of fission yield—2 or 3 of the small bombs used to destroy Hiroshima.

The explosion of megaton-range weapons may or may not produce large amounts of fallout, depending on the bomb design—in particular, depending on whether a large part of the energy release comes from fission. It would take only a few bombs of a type releasing some 10 megatons of fission energy to double the total global fallout now existing. It is clear that so far as radiation hazard is concerned, for bombs of the types which have been tested so far, attention can be confined to the large bombs. As to the question of how much we should be concerned over a doubling, say, of the fallout produced so far, that is a matter into which many complex factors enter, as discussed previously, and no answer can be given on scientific grounds alone.

FALLOUT IN WAR

Although there is much room for disagreement on evalution of the available data as to the results of fallout from tests, there is no disagreement as to the utter catastrophe that would result from a full-scale nuclear war. Hundreds of millions of people would be killed outright by blast, fire, and radiation, hundreds of millions more would die from the later effects of radiation injury, further comparable numbers in succeeding generations would suffer from genetic effects resulting from the radiation, and large parts of the devastated countries would be not only destroyed but made uninhabitable for extended periods.

REAPPRAISAL OF FALLOUT PROBLEMS

The AEC is to be highly commended for the detailed measurements it has made on fallout, and for the steady release of the results of these measurements.

However, the AEC has the dual responsibility of conducting a weapons development program and of evaluating the fallout hazard. It can readily be seen that decisions felt to be necessary in one area might conflict with and unduly influence decisions in the other. Because of these conflicting responsibilities, it may legitimately be questioned whether both functions belong in the same agency.

The suggestion has been made that an independent group of qualified scientists be appointed to take over the study and evaluation of fallout hazard and other radiation problems as well. In 1955, the National Academy of Sciences appointed a group to conduct such a study. The report submitted by that group was prepared over a year ago. Since that time, many new data have become available and many highly qualified specialists have expressed criticisms of the AEC's treatment of fallout problems. It is very desirable that appraisal of these problems be brought up to date. It can be expected that a group chosen to include representation of differing points of view can arrive at agreement on an interpretation of the data which can serve as an authoritative basis for policy decisions regarding future weapons testing.

CONCLUDING REMARKS

We are at the present time forced into the difficult position of having to make decisions on a problem that we do not fully understand. The data on hand, however, do tell us this much: The fallout radiation from past tests will constitute over the next few decades a small percentage increase over background radiation. It is estimated that even this small percentage increase will cause genetic changes affecting some tens of thousands of individuals over a number of generations, and will probably also produce leukemia or bone cancer in a comparable number of individuals over the next few decades. The fallout effects will increase with the amount of fission yield in future tests. Each small explosion will contribute only a small addition to the fallout radiation already produced; each large explosion can contribute an appreciable addition.

It should be emphasized that the effects of nuclear tests are thousands of times smaller than the effects which would result from a nuclear war. While there is no disagreement that nuclear war would be a disaster which we cannot afford, there is disagreement on the best means of avoiding such a war, and some of that disagreement seems to have spilled over into the area of evaluation of the fallout hazard.

An objective scientific evaluation of the radiation hazard from fallout can be, and should be, made independently of any military or policy considerations. The extensive information being compiled at these hearings will be of considerable help in arriving at such an evaluation.

Dr. SELOVE. On the question of how much testing can be tolerated, you have had testimony here as to a possible permissible testing level of a number of megatons per year. I would like to emphasize that in arriving at a permissible level the question of whether or not there is a threshold is a crucially important one. For those effects for which there is no threshold, for which effects are produced by even small amounts of radiation, then certainly without question from anyone effects are produced by even the smallest amount of testing and if one wants to continue a testing program it can only be on the basis of balancing the presumed advantages from testing against the certain disadvantages in terms of individuals affected.

With regard to the specification of a permissible level, I think it should be said that the use of the term "maximum permissible concentration" is rather unfortunate, especially with respect to strontium 90, since this term implies that the immediate and long-term effects of small amounts of strontium 90 in human bones are known with precision. They are not known with precision.

Furthermore, it has been noted by many specialists that a "maximum permissible" dose is not necessarily a safe dose. It is possible to estimate from available data that if a large number of people are given a permissible dose such as is now used for the population and

if the effects of strontium 90 are proportional to the dose, then it is likely that about one in a thousand will develop bone cancer in his lifetime.

Senator ANDERSON. Does that mean one additional person?

Dr. SELOVE. One in a thousand of all individuals. One in a thousand normally die of bone cancer. I am saying it can be estimated from the numbers which have been given to this committee, that if the entire world population were to receive what is now called a maximum permissible dose of strontium 90 for the population, that is 10 times less than the level prescribed for occupational exposure, then it can be estimated that one in a thousand of all the people in the world would develop bone cancer. That means that several million cases of bone cancer and leukemia would be developed. On this basis one would hardly be willing to let the world level of strontium 90 to come to the permissible level.

Senator ANDERSON. I am sorry I did not get the question clearly. You said a certain number would develop bone cancer independent of the fallout?

Dr. SELOVE. Yes.

Senator ANDERSON. Did you say an additional number would develop it?

Dr. SELOVE. Yes.

Senator ANDERSON. One out of a thousand additionally would develop it.

Dr. SELOVE. That is correct. In other words, even without fallout, during the next 30 years when some half of the present world population is dying, some 3 million cases of bone cancer would normally develop. Some 10 million cases of leukemia would normally develop. If the world were to receive a maximum permissible dose of strontium 90 it can be estimated that these normal effects might be almost doubled.

On that basis we would hardly be willing to have a world average level equal to the permissible level. This rests on the crucial question of whether the effects of small doses of strontium 90 are visible or whether there is threshold. This point you have heard much discussion on. I think you have seen by now that there is not at present sufficient data to know whether there is or is not a threshold behavior.

Senator ANDERSON. Would I be safe in concluding that most of the geneticists believe that there was not a threshold?

Dr. SELOVE. I think that would be a fair interpretation.

Representative COLE. No threshold as far as genetic effects are concerned?

Dr. SELOVE. There is complete agreement on that question. Comparable numbers of individuals will be affected genetically to those affected by strontium. It is true the genetic effects are spread over a large number of generations; so in the coming generation, for a given amount of fallout, the number of individuals who show genetic effects may be 10 times smaller than the number who will show the effects of strontium 90, if they show them at all.

Therefore, as far as the immediate impact of the fallout is concerned I would say we can confine our attention to strontium 90.

The AEC has made very fully——

Representative COLE. On that last statement, Doctor, does that mean that in your opinion we should be considerably more concerned about the effect of strontium 90 than be concerned with the genetic effect of fallout?

Dr. SELOVE. No. I think according to the estimates one can make from the present limited data, about comparable number of individuals will be affected genetically and by strontium 90.

However, during the next 30 years one can estimate that some 6,000 babies will show serious genetic effects from fallout so far, whereas some 50,000 individuals may develop leukemia or bone cancer from fallout so far.

Representative COLE. Of what population?

Dr. SELOVE. The world population.

Chairman DURHAM. That is based on the amount of strontium at the present time in the stratosphere.

Dr. SELOVE. From tests so far. Most of the strontium 90 produced is now on the ground.

The AEC has made very fully available its measurements on fallout, and it should be commended for this. However, the AEC has the dual responsibility of conducting a weapons-development program and of evaluating the fallout hazard. This is a very difficult position for anyone to be placed in. I would not want the job. It can readily be seen that decisions felt to be necessary in one area might conflict and unduly influence decisions in the other. Because of these conflicting responsibilities it may legitimately be questioned whether both functions belong in the same agency.

The National Academy of Sciences has made an independent study of these problems with a group appointed in 1955. The report prepared by that group was prepared about a year and a half ago. Since that time, many new data have become available and many highly qualified specialists have expressed criticism of the AEC's treatment of fallout problems. I think it is very desirable that an appraisal of these problems be brought up to date. It can be expected that a group chosen to include representation of different points of view can at least arrive at agreement on what can be positively concluded from the available data.

I would like to summarize that we are at the present time forced into the position of having to make decisions on a problem that we do not fully understand. That data on hand, however, do tell us this much: The fallout radiation from past tests will constitute over the next few decades a small percentage increase over background radiation. It is estimated that even this small percentage increase will cause genetic changes affecting some tens of thousands of individuals over a number of generations, and will probably also produce leukemia or bone cancer in a comparable number of individuals over the next few decades. The fallout effects will increase with the amount of fission yield in future tests. Each small explosion will contribute only a small addition to the fallout radiation aready produced. Each large explosion can contribute an appreciable addition.

It should be emphasized that the effects of nuclear tests are thousands of times smaller than the effects which would result from a nuclear war. While there is no disagreement that nuclear war would be a disaster which we cannot afford, there is disagreement on the best

means of avoiding such a war, and some of that disagreement seems to have spilled over into the area of evaluation of the fallout hazard.

An objective scientific evaluation of the radiation hazard from fallout can be, and should be, made independently of any military or policy considerations. The extensive information being compiled at these hearings will be of considerable help in arriving at such an evaluation.

Senator ANDERSON. Thank you, Doctor. I do want to assure you that this committee has been trying to make some sort of objective scientific evaluation of it.

Dr. SELOVE. I want to commend it for its effectiveness in bringing out such a vast amount of data in a way which will be very understandable to very many people in such a short time.

Senator ANDERSON. We appreciate very much the contribution which the scientists have made and are making to this, you included. I am very sorry that you had to wait to the end of a long day for your paper.

Dr. SELOVE. I am sorry you had to sit through it.

Senator ANDERSON. We are happy to do it.

The committee will meet tomorrow in the House caucus room at 10 o'clock.

(Thereupon, at 5 p. m., Wednesday, June 5, 1957, the committee recessed, to reconvene at 10 a. m., Thursday, June 6, 1957, in the House caucus room.)

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

THURSDAY, JUNE 6, 1957

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION
OF THE JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The special subcommittee met, pursuant to recess, at 10:05 a. m., in the caucus room, Old House Office Building, Hon. Chet Holifield, chairman of the subcommittee, presiding.

Present: Representatives Holifield, Durham (chairman of the Joint Committee), Price, Cole, Van Zandt, Jenkins; Senators Anderson and Bricker.

Also present: Professional staff members: James T. Ramey, executive director; George E. Brown, Jr., Hal Hollister, staff technical adviser, and Paul C. Tompkins, consultant.

Representative HOLIFIELD. The subcommittee will be in order.

We have planned to start the meeting this morning with a discussion on the present state of affairs, and we have invited Dr. Libby, Dr. Selove, Dr. Neuman, Dr. Machta, Dr. Crow, Dr. Brues, and Mr. Eisenbud to take part in this seminar. I understand Dr. Shields Warren is here, and we will be delighted to have you come up.

Will you gentlemen come forward to the table?

Mr. Hollister will start off the seminar with a statement or question.

DISCUSSION BY DRS. WILLARD F. LIBBY, WALTER SELOVE, WILLIAM F. NEUMAN, LESTER MACHTA, JAMES F. CROW, AUSTIN BRUES, MERRIL EISENBUD, SHIELDS WARREN, CHARLES L. DUNHAM, BENTLEY GLASS, AND WRIGHT LANGHAM

Mr. HOLLISTER. Mr. Chairman, I would like to suggest, because of the long and tiring day we had yesterday, and because Dr. Selove has made special arrangements to be with us this morning for about an hour, that he be given about 10 or 15 minutes to summarize very quickly the points that he was making yesterday afternoon.

Representative HOLIFIELD. Will you do that, Dr. Selove, in order that we may regain the points in your testimony. Also I understand you have to leave at 11 o'clock.

Dr. SELOVE. Yes.

Representative HOLIFIELD. So we will let you start off.

Dr. SELOVE. Thank you.

This discussion yesterday and this morning is intended to provide a summary discussion of the interrelationships and implications on pol-

icy of fallout matters. I would just like to simply itemize the principal points that I would like to make at this time.

First, I think it has been brought out clearly before this committee that there is some effect from any amount of radiation. There is complete agreement among geneticists and others who have studied the problem that genetic effects are produced even by very small amounts of radiation. Of course, with extremely small amounts, there are correspondingly small effects.

But the major point is there is no safe level for genetic effects in the sense of the existence of a level below which no effects would occur. There is no such level.

As to the somatic effects, and perhaps the ones that have received the most attention are the production of leukemia and bone cancer by strontium 90, there is not sufficient scientific evidence at present to determine whether or not there is a safe level for such effects.

I believe it is fair to say, as I said yesterday, that on the basis of the available data, the interpretation that effects are produced even by small amounts of strontium 90 seems to be at least as reasonable as the interpretation that they are not.

The second major point I would like to make is that I believe the term "permissible level" is an unfortunate one to use in discussing fallout effects. These effects, to be sure, may be very small on a percentage basis. At the same time they may involve many individuals. I think "permissible level" is a term which is generally understood to mean a level not such that there is no risk at all, but a level which is taken to be acceptable by those subjected to the hazard in return for some presumed benefit.

Now this is true, for example, in the case of X-ray exposure where there is certainly some genetic effect.

Representative HOLIFIELD. May I interrupt you just a moment? I understand Dr. Glass is in the audience. Dr. Glass, we have one empty chair up here. Would you join us?

Dr. GLASS. Yes.

Representative HOLIFIELD. Continue, Dr. Selove.

Dr. SELOVE. To continue, then, in the case of X-rays, for example, the permissible level has been established——

Senator ANDERSON. Just a second. I would hope if we are going to fill this extra chair, we would add another one and ask Dr. Dunham to come up.

Representative HOLIFIELD. Dr. Dunham, we can find a chair for you, too.

Of course, getting this many here is not to make it mandatory that each one of you take 20 minutes. It is only if you have something of real importance to add that you feel like you should add. Otherwise, it might become unmanageable. But we want to show you the courtesy of an opportunity in case you do wish to speak.

Go ahead, Dr. Selove. Excuse me.

Dr. SELOVE. I would like to reemphasize that a permissible level should be understood to be a level which the people who are subjected to it, or their representatives, have decided is an acceptable level in view of the advantages associated with whatever hazards we are concerned with.

In the case of radioactive fallout, although the effects represent a very small percentage of similar effects occurring normally, in view

of the worldwide concern over this effect, it is not correct to speak of permissible level meaning no risk. It should be understood just what is meant by permissible level.

Senator ANDERSON. In other words, you think there ought to be a balance. You weigh the dangers?

Dr. SELOVE. Against the advantages. Ordinarily, in the case of X-rays, for example, a doctor and his patient decide together—actually, of course, the doctor decides— whether the advantages to be obtained from an X-ray are worth the disadvantages, which are relatively small from an individual X-ray as in the case of radioactive fallout. I do not mean to draw any conclusions here. I just wanted to point out that the peoples of the world are subjected to the radioactive fallout produced by tests from all powers, and they may not be quite so convinced that they are receiving advantages to compensate for the disadvantages. In that sense, the term "permissible," I think, is a misnomer.

The third point I would like to make concerns the size of the effects. This is a very complex question. One simply cannot describe it with a single sentence, or even a pair of sentences. One has to take many aspects of the problem to obtain a balanced view of what the size of the effects is.

I would be glad later to make some further comments on this, but at the moment let me make some very short ones.

Effects from fallout from tests so far are small compared to the effects of other radiation sources to which people always have been exposed, and are currently exposed. The effects are so small they may be undetectable. However, the numbers still constitute many individuals affected even if we just concern ourselves with genetic effects. If we concern ourselves with the possibility of somatic effects in addition, this adds some number of individuals.

The effects are global. They are roughly calculable. They can be calculated by scientists of other countries as well as by scientists of our own.

The next point I would like to make concerns the uncertainties, first, in the magnitude of the effects.

The numbers presented to this committee represent, in general, best estimates as to how many individuals will be affected. No one is in a position to say how accurate these estimates are. I would think it is believed that the estimates that are given you are accurate within 10 times. The true numbers may be 10 times larger, and may be 10 times smaller.

There are uncertainties also in the nature of the effects produced by radiation, by fallout radiation in particular. I do not mean to hint here that there are going to be some serious effects turning up that we do not suspect as yet. I just want to repeat what many witnesses have said—that because many of these things are new to the human race, it is likely to be a generation, until people have carried these radioactive materials in them all their lives, before we really know what the effects are.

It has to be pointed out also that we have good reason to believe we know what the principal effects will be.

Next I want to point out that, so far as radioactive fallout is concerned, one should make a clear distinction between the effects produced by large bombs and the effects produced by small bombs. A

large bomb can produce on the order of a thousand times as much fallout as a small bomb. And if one goes into the question of test bans, then one should make a clear distinction between the effects of small weapons and the effects of large weapons. It is actually, of course, the total amount of fission products released that is important.

Next I want to reemphasize what has been brought out here at these hearings, and a point I tried particularly to make here yesterday, that it can be strongly expected that new nations which enter the testing program will be very interested in testing bomb types which produce a great deal of fallout. There are strong reasons why they will want to do so.

We have seen in the tests of Russia last fall that in just a few explosions, the strontium 90 in this country was increased by one-third, in the soil of this country. This does not represent an increase on a worldwide basis, because of the question of uniformity of distribution and storage in the stratosphere, but the level of the soil in this country was increased by that amount by just a few explosions.

Representative HOLIFIELD. Would you hazard a guess at this time—I do not like to say "guess." Would you hazard an opinion that, in view of the fact the Soviets have tested large bombs on their own territory, there may be an appreciably greater extent of strontium deposit from the local fallout on their own territory?

Dr. SELOVE. In Russia?

Representative HOLIFIELD. In ratio to the stratospheric fallout which has occurred here and raised our level one-third.

Dr. SELOVE. I think probably many of us saw in the Washington Post yesterday morning a Russian report indicating that strontium 90 levels, at least in some areas of Russia, were twice as high as the highest levels in this country.

Senator ANDERSON. Which might explain the sudden Russian interest in stopping nuclear tests and preventing nuclear warfare.

Dr. SELOVE. The decision concerning balance of factors in deciding whether or not to try to obtain an agreement for a test ban—these decisions, of course, involve many other factors other than radiation hazards. I am trying to restrict myself here to that one.

I would next like to reemphasize, as many witnesses here have done, that the effect of a nuclear war would be enormously greater than the effects of fallout from tests.

Again, this remark on this matter: In balancing various factors involved in decisions about test bans, one really has to include such things as the question of whether continued weapons testing will actually help us to avoid a nuclear war. No one disagrees that a nuclear war would be just utter devastation. We have to avoid it. There is considerable disagreement as to the best way to avoid it, but there is no reason for that disagreement to extend into the area of evaluation of radioactive fallout hazards. I think we can and should make that an objective evaluation.

Next, as to the evaluation of fallout hazards, this is anything which has been carried out by the Atomic Energy Commission, by the National Academy of Sciences in an independent study last year, by the British Medical Council, and by various other less formally organized groups.

I think with regard to the Atomic Energy Commission, as I stated yesterday, there is likely to be considerable conflict in the dual re-

sponsibility which the Commission has for evaluating radiation hazards and for concerning itself with the weapons development program of this country. It is a very unfortunate and burdensome problem. I do not think any of us would want to be saddled with it.

I think it can legitimately be questioned whether these two functions—the evaluation of radiation hazards, and concern with the weapons development program—belong in the same agency.

As for the question of independent studies of fallout problems, I would like to make my next point the fact that the National Academy of Sciences report, which is still referred to up to the present time as the latest authoritative report in this country, is seriously out of date. That report was prepared about a year and a half ago. It was issued about a year ago.

The National Academy has, of course, been continuing to study these problems. I think further reports from them at this time would be very much in order nad very welcome. I am sure they are working on them. I think it might be of interest to hear how soon one can expect such further reports to appear.

Finally, I would like to repeat a conclusion of the radiation hazards committee of the Federation of American Scientists, a report from which committee I am inserting in the record. It contains, I think, several numbers of interest.

It is a principal conclusion of that committee, representing some eight people who have studied the radiation hazards problem from fallout quite a long time as such committees go, I think—it is a principal conclusion of theirs that a decision on testing nuclear weapons cannot be based purely on the scientific problems involved, cannot be based purely on radiation hazards, but must involve primarily moral questions, and questions of the effect on international relations and international affairs of our decision on a test ban.

I would like to summarize, then, by saying that the view I have tried to present of fallout problems I think is neither an alarming one nor a calming one. The problem simply does not reduce to such simple terms as those. It is a very complex problem. I do not think a balanced study of it delivers so spectacular a conclusion as that the problem is either of alarming proportions or of negligible proportions. It deserves a lot of serious study. You gentlemen have given it much of that here, and I believe it will be extremely useful.

Representative HOLIFIELD. Thank you very much, Doctor.

Dr. Libby, do you have any comment?

Dr. LIBBY. I find myself in agreement with much of what Dr. Selove said. We certainly must continue to take this problem very seriously, and study it.

In reading the testimony before the committee, I am impressed with the disparity in our knowledge of the biological effects as compared to our knowledge of the physical facts about fallout, which leads me to suggest that we ought to try harder to get some answers to the questions of the genetics, the effects of low radiation doses taken over long periods of time in causing cancer and leukemia, from experimental data.

But we certainly ought to continue the program.

I certainly agree with Dr. Selove this is not a negligible factor. We must take it seriously.

My testimony yesterday shows that I agree with him that nonscientific considerations are involved in the final decision.

Chairman DURHAM. Dr. Libby, I think you are in general agreement that the biological studies should be increased. Does the Commission have facilities available at the present time—and when I speak of facilities, I mean brains and everything else connected—to step this up in the biological field?

Dr. LIBBY. I believe so, but I would like to ask Dr. Dunham to answer that question.

Dr. DUNHAM. Mr. Chairman, we do have facilities. We do have some manpower. This can be stepped up. It cannot be stepped up suddenly by a matter of several orders of magnitude either within the Commission or without the Commission, because there still is, as you hinted in the way you put the question, a limited number of available competent scientists to work in this field.

This is a very real problem, because, if the work is not done competently, it will cause more confusion.

But there definitely could be a step-up in the program over the next few years. There is no question about it.

Chairman DURHAM. Dr. Dunham, since you are in charge of this primarily, do you have funds available, or will you have with what you requested this year from the Congress?

Dr. DUNHAM. We have, as the Congress knows, in our 1958 budget an increase in the request. As you know, these budgets are prepared nearly a year and a half in advance of the coming before the Congress for appropriations.

The cost of scientific living, I mean the cost of doing experiments, is constantly going up. So I would be less than candid if I said that what seemed like a reasonable budget in May of 1956, looks so reasonable today.

Chairman DURHAM. Doctor, since you made that statement, I wish you would send up to the committee your recommendation on it as early as you can. It will not be long before Congress adjourns here, and I think this committee feels like, of course, you should have whatever funds are necessary to carry on the investigation in the biological field.

Senator ANDERSON. May I just ask there: Dr. Selove suggested that the report of the National Academy of Sciences is out of date, and he wanted to know when the next report might be ready. Is there anyone here who can give us any indication of when the new data will be studied and a report made on it, since he says the old report is seriously out of date?

Dr. WARREN. If I might comment on that, Mr. Chairman.

Senator ANDERSON (presiding). Very well.

Dr. WARREN. There is a continuing study made by the various groups assigned by the National Academy of Sciences to all this program. At the present time there are no available facts which would warrant a change to a significant degree in the reports for the pathological effects, the somatic effects. I think Dr. Glass would probably agree with me that there is no very large increment of data in the genetics field.

Senator ANDERSON. The testimony of the geneticists would not indicate that, certainly, would it?

Dr. WARREN. I would rather have Dr. Glass comment on that particular point.

Dr. GLASS. I will later.

Dr. WARREN. Both of the committees have been asked by the president of National Academy of Sciences to meet periodically to keep in close touch with new data as it becomes available, and to issue reports as often as it seems wise to them.

I would say that at the present time, so far as the report on the pathological effects are concerned, that there are no points that as yet are sufficiently well established to warrant a significant change in that report as it stands.

Senator ANDERSON. Now, Dr. Selove, if you think the report is out of date, and Dr. Warren says there are now new facts that have been discovered to change it in any degree, could you give us briefly the basis for your statement?

I rather got the impression there were some new facts, and I thought these scientists had been presenting them. I judge from Dr. Warren's statement, as far as the group, they are going to stand pat on what they put out a year and a half ago.

What do you think about it?

Dr. SELOVE. Dr. Warren is, of course, the chairman of the pathology section of the National Academy of Sciences' study, and is certainly in a position to speak for that group. Some 8 months ago, I communicated with the president of the National Academy, Dr. Bronk, asking whether, in view of the renewed attention that some of the fallout effects were receiving, particularly in view of the attention to strontium 90, whether the National Academy group had any expectation of producing a new report at any time in the near future. And he replied that the matter was under consideration; that a certain number of members of the study group had asked for a renewed study of this problem; and he expressed agreement with the idea that the problem needed more attention, it being understood, of course, that the radiation committees, the subcommittees of the National Academy, have been organized on a continuing basis.

I think there may be some statements in the pathology committee's report which perhaps, in view of the testimony that has been presented to this committee here, might not represent a sort of average view or balanced view of the various notions that specialists in the field now have.

I recall at the moment, not having the report here with me, only one. I think there is a discussion, in the report of the pathology committee, of an unequivocally safe level. I think, in view of the testimony that has been presented here, it is clear that the scientific evidence presently available is not sufficient to say whether or not there is a threshhold level, a level which would be absolutely safe. If the interpretation presented by many specialists in the field that even small doses of strontium 90 can produce bone cancer and leukemia is correct, then I think this point, which I tried to make earlier, that there would be no safe dose at all, is a point which might well be expressed so as to revise that particular statement.

Senator ANDERSON. I know you have to catch a plane, and a member of the staff, Mr. Hollister, has some questions to direct to you. I guess we had better proceed with those.

Representative COLE. If I may come back in where Mr. Durham was discussing with Dr. Dunham the possibility of stepping up this inquiry in the field of biological aspects.

The impression has been created, I fear, that in this field there has been a party line, what might be called a party-line concept of disclosure of the consequences of radiation, in that the Commission has laid out a policy, and other people in the laboratories have tried to find answers which would support that policy.

On the other hand, to disprove that the witnesses, many of them, indicated a large degree of independence in their research, that there is no control by the Commission with respect to their areas of inquiry or conclusions.

Now my question of Dr. Dunham is: In the event that, Dr. Dunham, you do find there is opportunity for stepping up this inquiry, do you see any need or opportunity for enlarging that aspect of independent research beyond what it is now?

Dr. DUNHAM. I do not feel that there is a need to enlarge the independent aspects, because essentially there is a total independence, as Dr. Brues will, I am sure, testify later today when he discusses the research program.

Representative COLE. You mean the present program is essentially completely independent?

Dr. DUNHAM. That is correct, as far as directing or limiting in any way what the scientists, either in the Atomic Energy Commission national laboratories, the major projects, are doing, or university scientists which we support through contracts with our universities.

Senator ANDERSON. Mr. Hollister.

Mr. HOLLISTER. Dr. Selove, I wonder if you would be willing to state for the record, either speaking for yourself or for the Federation, what you feel are the essential differences and similarities in the conclusions of the National Academy report and the British Medical Council report, as far as such questions as genetic effects, MPC, and so on, are concerned?

Dr. SELOVE. As far as what?

Mr. HOLLISTER. As far as such questions as genetic effects, MPC, and so on, are concerned.

Dr. SELOVE. I have to speak for myself, although I believe what I will say reflects the views of the Radiation Hazards Committee. Nothing I say reflects the views of the Federation of American Scientists. This is, of course, a technical matter, and the federation has no policy on such a matter.

I believe the British Medical Council report, which is a report by an organization in Britain separate from the organization which has to do with weapons-testing decisions, just as the National Academy here is, of course, separate from the AEC—I think that report, in my opinion, tends to be perhaps slightly more conservative than the National Academy report.

I recall, for example, that in the British Medical Council report it was suggested that, if the general level of strontium 90 in the population should show signs of increasing greatly beyond one one-hundredth of the occupational permissible concentration, that it would indicate the necessity for a strong reconsideration of the problem.

I think that was a somewhat lower level at which the British Medical Council expressed concern than the level suggested in the National Academy report.

Senator ANDERSON. How much would that be in sunshine units, Doctor?

Dr. SELOVE. That would be 10 sunshine units. The level in the world from tests so far is expected to rise to 2 to 3 sunshine units on the average.

Representative COLE. Mr. Chairman, I wonder if you would mind just experimenting for a moment to see if we cannot do better with these microphones cut off, and everybody speaking up a little louder.

(Discussion off the record.)

Dr. SELOVE. The British Medical Council report, I believe, took a slightly more conservative tone than the report of the National Academy of Sciences. The report of the National Academy group was also conservative, let me hasten to say.

Representative COLE. Speak up so that the audience hears.

Dr. SELOVE. I am not sure I can. I am trying to speak to the committee. I am not sure I can speak to the committee and have the audience hear it at the same time.

I believe in the British Medical Council report another point of difference which occurs to me, with the conclusions that one would draw from reading the National Academy report—another point of difference was that with regard to the possibility that the effects of even small doses of strontium 90 would be felt in terms of the production of a certain amount of bone cancer and leukemia, this possibility was brought out somewhat. I do not recall whether that possibility was discussed in the National Academy report. Perhaps Dr. Warren can answer.

Dr. WARREN. I think probably Dr. Brues can answer this better than I, because his particular group of scientists were the ones who had special knowledge with regard to strontium and the internal emitters.

Dr. BRUES. I am sorry I do not have a copy of the report with me at the present time. I know that the possibility was very seriously considered. I know that it is in some way mentioned in the report, but I am sorry that I cannot say exactly how.

I think that the report looked at the problem from the standpoint of the fact that some radiation is present, that therefore if——

Senator ANDERSON. Is this it [indicating document]?

Dr. BRUES. I am sorry. I am afraid this is not the one which contains the subcommittee report. Perhaps the committee would like to entertain another question while I consult the report.

Dr. NEUMAN. I wonder if it would be appropriate, since we are going to find out what was intended to be meant in the report, to consider what a very interested reader got out of the report, and have that question while we are waiting?

Senator ANDERSON. We are just trying to get through Mr. Hollister's last question before Dr. Selove will have to leave for his plane.

Go ahead.

Dr. NEUMAN. I came away with a little less drastic view than Dr. Selove. I think that the British report expressed this figure of 10 sunshine units essentially as a worry dose—recognizing that, if the strontium levels in the population rose, that we should seriously recon-

sider the matter of MPCs. This is only a suggestion. The British report does not really differ from the National Academy's position.

In reading the NAS report, I do not know, of course, what was intended, but I came away with the feeling that there was a division of opinion in the committee, and that this report indicated some people's feeling that the threshold dose did exist, (the MPC then might be really an MPC) while others did not accept the concept of threshold.

I think there were areas of disagreement, and that a compromise was reached. I hope that, before we leave, this group might consider the matter of the MPC. This finally (the MPC) is the operational number that you use in translating these discussions into real action.

Senator ANDERSON. Thank you, Dr. Neuman.

Dr. SELOVE. I would like to toss out again at that point that I believe, on the basis of available data, the interpretation that small doses will produce effects of strontium 90 is at least as reasonable as the interpretation that they will not, even though no one is in a position to say certainly. However, if one takes the conservative view and assumes that small doses may produce effects until proven otherwise, one is then in a position to estimate roughly how many individuals will be affected by a given amount of strontium 90. And the dose which has been specified as permissible for the population or for a large part of the population, namely a hundred sunshine units, such a dose can be estimated to produce in the world several million cases of leukemia and bone cancer, if that becomes the average world level.

I think it would be generally agreed that, if the occupational dose, which is ten times that level, if the entire world population were subjected to an occupational permissible level, there would be several tens of millions of cases of leukemia and bone cancer, on the assumption that small doses do produce effects.

The difference between the occupational level, and the population level is a matter about which a question was put yesterday, and I think it should be pointed out, in the answer to that question, that the principal reason which the International Commission on Radiological Protection has for specifying a smaller level as a permissible level for the population—the principal reason is the uncertainty that exists as to just what the effects will be.

The International Commission on Radiological Protection has tried to arrive at a level which, on the basis of the limited data available, it thinks will not injure a large percentage of people exposed to that level.

If one considers exposing the entire world population to that level, then, in view of the uncertainties, one simply has to recommend a lower level. That is the basis of the difference.

I am just pointing out that, on the basis of estimates that can be made from presently available data, even the lower population permissible level, if the entire world is subjected to it, could be expected to produce several million cases of leukemia and bone cancer. So we would hardly want to approach that level until we know more surely whether or not small doses do produce effects.

Senator ANDERSON. Do you have another question, Mr. Hollister?

Mr. HOLLISTER. I would like to ask Dr. Selove, do you have any other comments concerning the numerical magnitude of possible effects?

Dr. SELOVE. I would like to make one further brief remark, which I think may be of help to put these things in perspective.

We have talked here about numbers of individuals, such as 50,000 individuals in the world who may develop leukemia and bone cancer from tests so far.

To reduce these numbers to terms where one can more readily feel the impact they have on an individual, we might talk about the situation in, say, the city of Washington, or take Washington and Baltimore together, which I would say probably have a total population of perhaps 3 million.

In Washington and Baltimore together at the present time there are approximately 200 deaths per year from leukemia from normal causes. On the basis of the estimates that have been discussed here, one arrives at the conclusion that, for the next several decades, the results of fallout from tests so far will be that there will be, instead of 200 or so cases of leukemia a year, 202. So there will be that increase, from tests so far, in deaths from leukemia per year in a population of 3 million.

When this is translated to a country such as Japan, with a population of a hundred million, one finds that, instead of the normal 10,000 or so deaths from leukemia in Japan, there might be expected to be about 10,100; and translated into effects for the world as a whole, as compared to this 100 extra deaths in Japan per year, and 2 extra deaths in Washington and Baltimore per year, one comes to some 1,500 extra deaths in the world per year. And over 30 years that adds up to 50,000.

So one should look at all these numbers to try to get some perspective.

Fifty thousand. Is it many individuals?

In terms of impact on a parent in Washington or Baltimore, it means that if a child dies of leukemia, there may be one chance in a hundred that that particular death was due to fallout from tests so far.

Senator ANDERSON. Now, Doctor.

Dr. BRUES. I think perhaps, sir, if I read what was said in the report it may also clear up some other points.

Senator ANDERSON. You are talking about independence, and this is independent. You are perfectly free to do what you feel like. Go right ahead.

Dr. BRUES. The report said:

> Permissible dosage to large populations: This is a matter on which no complete agreement was reached by the subcommittee. First responses to this question ranged all the way from the permissible industrial level down to no radiation at all.

These were all people who have given a great deal of thought to the problem.

> The uncertainty existing here stems from our ignorance as to whether there is a true threshold for such late effects as malignant tumors, and as to the degree of variation in response of equally exposed individuals.
>
> It is agreed that the only rational approach must take into account the natural radiation background to which the population is exposed.

Then this is tabulated.

> It is noteworthy that considerable differences exist from place to place, due mainly to differences in gamma radiation from the environment and in part to

variation to radium content of individuals. Since these existing variations have not given rise to any changes in incidences of tumors or other pathologic states sufficient to attract attention, it was felt that an amount of internal radiation sufficient to double the large population background could certainly be considered safe.

Unfortunately, the word "safe" is in there, and I have less and less respect for the word "safe" as this discussion goes on.

Senator ANDERSON. Thank you very much.

Dr. Neuman, did you have a comment?

Dr. NEUMAN. No.

Senator ANDERSON. Does someone else have a comment?

Dr. Glass, you are a geneticist.

Dr. GLASS. I would like to comment on two matters that have been brought up already: One in respect to whether the genetics portion of the National Academy Committee's report is out of date, and whether an additional supplement is being planned.

We were so much impressed with the relative magnitude of the exposure of the population to X-rays from medical and dental diagnostic and therapeutic practice, in contrast to the relatively minor amount due to fallout on the basis of the current estimates, that most of our attention over the course of the past year has been devoted through our consultants to a more accurate estimation of the medical and dental exposure of the population to radiation; and the report on this study has been given a preliminary release already. I do not know whether it is in the hands of the committee or not, but it should receive general release soon.

As far as the conclusions in our report which relate to the effect of fallout are concerned, I might read that:

Since any additional radiation is genetically undesirable, the fallout dose is genetically undesirable.

Second, the fallout dose to date and its continuing value, if it is assumed that the weapons testing program will not be substantially increased, is a small one, as compared with the background, or as compared with the average exposure in the United States through medical X-rays. From the point of view of this committee, there are two summary remarks that should be made.

Senator ANDERSON. Any dose is undesirable, you say? Read that again, will you please?

Dr. GLASS (reading):

Since any additional radiation is genetically undesirable, the fallout dose is genetically undesirable.

Now, I believe that the testimony of the geneticists at these hearings is simply an amplification of those two statements, and that our position has not been changed at all. It is in accordance with the views that Dr. Libby has expressed. It is also in accord with the views that the geneticists expressed. It depends on how you look at these things.

Senator ANDERSON. Dr. Selove, I know you are in a hurry. Do you have a comment?

Dr. SELOVE. I am in complete agreement with what Dr. Glass has said. I think the genetics part of the report would probably not require serious modification.

I would like to repeat, however, that a very important factor in discussing levels in terms of a permissible level is the question of who decides what is permissible for whom. If it is a matter of a physician giving an X-ray to a patient, the physician decides whether the X-ray disadvantages are compensated for by the advantages.

If one of the several nuclear-testing powers in the world is subjecting the entire world to fallout, even in terms of very small percentage effects—and it is widely agreed, as Dr. Libby has said, that the scientific data indicate that the effects are only a small percentage increase over normal effects, even in such terms, the term "permissible level" implies that it is acceptable by the people subjected to it.

I think that is the important point on which further clarification is needed in the National Academy report.

I will have to leave. Thank you.

Senator ANDERSON. Dr. Machta, you had some calculations on uniformity and nonuniformity you were working on. Are those ready now?

Dr. MACHTA. Yes; if you would like to hear about them.

Mr. Hollister, do you have something else you thought should come first?

Mr. HOLLISTER. I had planned to go next into the strontium 90 question, but it is not necessary.

Senator ANDERSON. Go ahead, and we will come to this. Go ahead with the strontium 90 question.

Mr. HOLLISTER. I would like to do this with the understanding that, if time allows, we will come back to the question of MPC, and its setting later.

Senator ANDERSON. We probably will have time to get back.

Dr. GLASS. Mr. Chairman?

Senator ANDERSON. Dr. Glass.

Dr. GLASS. May I make one remark before we get too far away from the comments that Dr. Libby and Dr. Dunham made earlier?

I would like to testify personally to the complete freedom of investigators who receive support from the Atomic Energy Commission as to the direction of their own work, and also as to the choice of the problems on which they work. This is so important that, in the stepping up of the program, it actually becomes a problem.

The Atomic Energy Commission has leaned over backward to such an extent that it has never even suggested, except in the most informal and free way, to any person what kind of problems we really need answers to.

There is a need, as these hearings have brought out, for the answers to specific questions, which I hope we can find within the framework of the independence given investigators by the Atomic Energy Commission to encourage them to work on particular things.

Chairman DURHAM. Dr. Glass, of course, the reason the question arose was the fact that Dr. Libby said it should be stepped up, in his opinion, as I recall his statement. I think I am in complete agreement that there has been perfect freedom in this field of investigation. There was no implication of my thinking at all that it has been suppressed at any level.

Representative VAN ZANDT. Mr. Chairman, could we have Dr. Dunham fill that gap out?

Dr. LIBBY. Mr. Chairman, would it be in order to ask the people at the table here whether they agree with me about the need for additional emphasis on the biological effects of radiation?

Senator ANDERSON. I think it would be very much in line to do that. Why not just start around the table.

Dr. Libby said why do we not ask the panel whether they agree with him for the need of additional emphasis on the biological aspects.

Dr. Machta, do you want to comment?

Dr. MACHTA. I do not believe I am in a position to comment.

Senator ANDERSON. Dr. Neuman?

Dr. NEUMAN. Because you are not a biologist?

I feel a biologist is not qualified because he would obviously say "Yes."

Senator ANDERSON. We were talking about complete freedom, so we are going to allow a man to disqualify himself as a juror if he wishes to do so. If you wish to, Dr. Machta, we would be happy to have your comments.

Dr. MACHTA. I have no comment.

Senator ANDERSON. All right.

Dr. NEUMAN. I think it is very important, but how to go about it is also very important. I think we should not be swept away by the urgent need for information to just dump money into a program. Very careful consideration should be given to other aspects, such as future procurement of scientists, maybe not just in the field of biology, but also in many allied fields on which we are dependent. I guess that is all, except to urge careful study be made on the way you would proceed.

I would also say, in my own personal opinion, the Division of Biology and Medicine has certainly had a very balanced program, and certainly my own testimony attests to the freedom of scientists within the Division.

Senator ANDERSON. Would you not think, however, that an announcement by the Atomic Energy Commission that it felt this was a field in which there should be a speeding up might result in many other universities, such as Rochester University has done in this field, coming in and saying, "We would like to have a chance to do something, and here is what we would like to explore"?

Dr. NEUMAN. I think a policy statement would be very important; yes.

Senator ANDERSON. I think that is what I was trying to say.

If Dr. Libby made such an announcement across the country there might be many schools which have not taken part that might at least start to survey to see if there are any contributions they might make.

Dr. NEUMAN. I think from just reading the fallout information, a lot of people have not yet done that.

Senator BRICKER. Dr. Libby, are there many of these students that participate in the research programs in the universities—I know you have many that do—are there many of them who continue in the field or related fields to the atomic-energy program?

Dr. LIBBY. I think so. Our interests are so broad, Senator Bricker, that the answer to your question is undoubtedly "Yes." If they move from one discipline to another, the chances are we are interested in the other one, and they do not often move.

Senator BRICKER. They do not get very far away from the field?

Dr. LIBBY. No.

Senator BRICKER. Thank you.

Senator ANDERSON. Dr. Brues.

Dr. BRUES. I think Dr. Dunham and Dr. Libby are both well aware of the fact I have agreed with this point of view for some time, and

much of my concern has been how to implement, in an acceptable way, the progression of knowledge. Sometimes I think there are people who feel that the biological work is a small tail on the rather large animal.

It is true that you cannot expand biological work as you can a reactor program, just by drawing some designs and setting about it. But I fully agree.

Senator ANDERSON. Dr. Dunham.

Dr. DUNHAM. I do not know that I have anything to add to what these people have said. I think the suggestion of the AEC coming out with a fairly strong policy statement as to the needs in certain areas for scientists to work is a good one.

We have tried, as Dr. Glass indicated, through the staff of the division, by direct contact in talking with scientists as we travel around visiting our own projects, and attending meetings, to sort of seduce people into working on things we have felt were of primary importance.

As I will say later, I think we know, and particularly as a result of these hearings, have a better perspective on where the emphasis ought to be during the next few years.

Senator ANDERSON. Thank you.

Dr. Crow?

Dr. CROW. I agree. I have nothing further to say.

Senator ANDERSON. Dr. Warren?

Dr. WARREN. There are two added points that I would like to bring out.

First, I will say that I am in agreement with the other speakers in this field. However, I think one must guard against swinging a program to concentrate on any one phase of activity to a great extent. We have many other problems than the fallout problem with which to concern ourselves in the biological effects of radiation.

I feel that, while it is highly desirable to expand the research program in relation to fallout, there are many other aspects of the program which may be just as essential as fallout now is within a few years' time.

Senator BRICKER. For instance?

Dr. WARREN. Well, let me take the fallout thing itself. When our program was first started in the Atomic Energy Commission we examined very carefully the work that the Manhattan District had been carrying on, and the Manhattan District had very wisely become interested in the problem of strontium 90 quite early in its existence. We already had a backlog of information with regard to strontium 90.

There have been close to, I would say, 14 years of work already in this field, which has brought us up to the state of knowledge that we have at the present time, and makes a large program with regard to radioactive fallout a perfectly sound and reasonable thing at the present time.

Now there are problems in regard to the effect of alpha radiation, the effect of neutrons. There is the problem of how to protect individuals against radiation effects. There are a whole range of things which must not be overlooked in favor of one aspect of the program only.

I am very heartily in favor of an orderly expansion of the program with regard to fallout, but I would hate to see it at the expense of the present program, which I regard as a well-rounded and effective program.

Senator ANDERSON. Dr. Libby, you raised the question so you do not have to comment. Do you care to comment again now, or go on around?

Dr. LIBBY. No comment.

Senator ANDERSON. Mr. Eisenbud?

Mr. EISENBUD. As a nonbiologist who needs answers from biologists, I would heartily agree that some of these answers have got to be obtained before we can proceed much farther. On the other hand, I am impressed by Dr. Warren's remarks concerning the need for a balanced point of view.

I do not think it has been brought out in these hearings, Mr. Chairman, that one of the reasons why so many of these unanswered questions exist is because we know so much rather than so little. No substance to which human beings are exposed, either in the air or in the food that they eat has been investigated so thoroughly as radiation and radioactive materials. The total investment of the United States Government and private industry in the whole field of air pollution research has been something like $5 million in the last 5 years. Dr. Dunham can tell us how much has been spent in this field, but certainly it is in the order of $200 million or great in the last 5 years. It is simply characteristic of good research that answers beget questions, and for every answer that we find, we raise 1 or 2 or 3 new questions.

I feel intuitively that if we had full information about organic chemical heavy metals, and other things to which we are exposed in our environment, we should be asking the biologists many of the same kind of questions we are asking them today in regard to radiation effects.

Senator ANDERSON. Thank you, Doctor, for a very fine statement.

Senator BRICKER. I have not been able to follow the conclusions that some of you have reached. I have asked the question a time or two as to how much of the leukemia and how much of the bone cancer at the present time, regardless of imposed radiation, is due to the background radiation, and how much is due to other metabolic and biological factors, or facts. I have not had an answer to that question yet.

How in the name of sense are you able, from an indefinite base of that kind, to extrapolate, like one doctor did a moment ago, and say there would be 102 cases of leukemia in Baltimore and Washington, rather than a hundred cases, because of the imposed additional radiation from strontium 90? There is a gap in there someplace I have not been able to fill in in my own thinking. If somebody would explain it to me I would appreciate it.

Dr. WARREN. I do not think I can explain it entirely satisfactorily, Mr. Senator. But I would like to stress that when you establish a hypothesis, you can draw conclusions from that hypothesis, but, as you point out, those conclusions have no more validity than the hypothesis itself.

Senator BRICKER. No more validity than the hypothesis itself?

Dr. WARREN. And I think you will recall that I said on Monday I am not at all satisfied that strontium 90 will cause any additional cases of leukemia. I would think there is a possibility of an increased amount of bone cancer, but I am very skeptical as to leukemia.

This again, you might say, is only a hypothesis.

Senator ANDERSON. We did have, did we not, the statement from Dr. Selove yesterday, in which he quoted Dr. Lewis as saying that 5 to 10 percent of all present cases of leukemia are due to natural background radiation?

Dr. WARREN. I know of no way in which that can be established or proved, Mr. Anderson.

Senator ANDERSON. I do not know either. I just simply say that we have scientists who think they know.

Dr. WARREN. Yes. That is, I think that is a fair and reasonable assumption, but I do not think we are warranted in accepting it as an established fact.

Senator BRICKER. It is nothing more than an educated guess.

Senator ANDERSON. When you say, also, that one microcurie or one-tenth of a microcurie is a safe background, that is also an educated guess, is it not?

Dr. WARREN. No. I feel—well, yes, it is an educated guess.

Senator ANDERSON. Yes. That is right.

Dr. WARREN. But I think it has a little more foundation.

Senator ANDERSON. There is not a particle of difference between the two educated guesses.

Dr. WARREN. I think there is. If I could say one more word here. We know there is nothing unique about the radiation from strontium 90. We also know that there are much greater variations in radiations which have not produced clearly measurable effects. There may be some shadowy effects. We cannot say there are not any, but we can say there are none that are significant and measurable.

I would have no hesitancy in moving my family, myself, to your own State, sir, which happens to have a higher background, I believe, than Washington, and the strontium radiation is only a small part of this difference.

Senator ANDERSON. Now we are back on something we can agree on. I will be happy to say that I am willing to face a slight degree of leukemia and bone cancer for the privilege of living in my State.

Senator BRICKER. May I ask one more question?

Senator ANDERSON. Senator Bricker.

Senator BRICKER. On the assumption, Dr. Libby, or anyone else, that we had a hundred million dollars to spend in biological research this coming year, from what kind of an expenditure would you receive the greatest benefit, an expenditure in this field, or an expenditure in the field I think Dr. Eisenbud mentioned a minute ago, in the field of pollution of the atmosphere, the effect of automobile exhausts, smog in Los Angeles, and many other things that are present in contamination of foods, waters, and the like?

Have we neglected the one for the other? And should we give more attention to the general biological human effects of other deleterious things?

Dr. DUNHAM. My answer to that, Mr. Senator—anybody else can add their comments—we have not neglected one for the other. We have neglected the general effects you are talking about of all chemical

elements and things that are in the environment on man. I think that one should not, by that statement, then say that we should stop all work on radiation effects when we are just on the brink of learning a lot of important things that will help resolve some of the radiation problems. But there is no question but what the other activity dealing with all of these other substances about which we really know relatively little should be stepped up.

Senator BRICKER. You know there is one result of this meeting and this hearing—I think it has a good result generally—people are getting scared.

I said here a while ago that as a result of cigarette smoking and radiation, I am surprised we have got so many healthy people around. In other words, the other field is, to my mind, the more important in numbers, and we have overemphasized the radiation effects insofar as we know what they might be at the present time. I think we ought to turn some attention to the general and not neglect this, mind you, but we ought to give more attention to the general field of biology.

Dr. DUNHAM. I think there is no question about it, sir.

Senator ANDERSON. We got as far around as Dr. Glass.

Senator BRICKER. I am sorry.

Senator ANDERSON. I appreciate, Senator Bricker, your coming in here.

Dr. GLASS. I have already expressed myself on this question sufficiently in my testimony, I think, so I will just pass it on.

Senator ANDERSON. Dr. Langham?

Dr. LANGHAM. I had decided more or less what I was going to say until Mr. Bricker asked his question.

I do not think there is any doubt but that radiation has been the most emphasized environmental hazard that man has ever been subjected to. Why? Because it has a radioactive tag on it, which makes it easy to discover, and it is also new. Therefore, it has received considerable interest. So I would heartily agree with Mr. Bricker.

On the other hand, I would like to point out one thing which applies to radiation specifically, and perhaps to man's industrial environment in general, and that is that probably for the first time in the history of man he has advanced technologically to the point where he is beginning to become the weak link in what he can accomplish in the future.

I mean by that—let's take the human heart. It can wear out any mechanical pump of the same size that can be built by man.

If we want to, we can say that a man can ride a horse until it drops, or he can fly airplanes until they fall out of the sky with mechanical failure.

Now, however, we are reaching the point where man can build machines and gadgets in which he is the weak link in their operation. In fact, I might say that man has now reached the stage where he is the weak link in his ability to wage war, because he is reaching the point where in so doing he might even annihilate a large portion of his own population. This is even true of the man who wins the war.

So I would say, yes, by all means we should emphasize biological and medical or, let me say, human factors investigations. I do not mean we should do this without adequate planning, but I think we

should emphasize more the human factors in all aspects of our technological advancement.

I think for the first time radiation has called this to our attention.

Senator ANDERSON. Thank you very much, Dr. Langham.

We went around the circle. Now we shall start again by asking Dr. Machta if he wants now to present his information.

Chairman DURHAM. Mr. Chairman, I would assume from Dr. Langham's statement, then, that he believes it is a good thing for the human race that we do have radiation. Is that correct?

Dr. LANGHAM. Not necessarily.

Chairman DURHAM. Because of the fact that it has caused us to study all of these related things, such as biology and medicine and everything else. Now we have been studying the cancer cell here for generations. Of course, it is nothing new. We have failed to reach any decisions as to the cause of cancer prior to strontium 90 or prior to any fallout; had we not?

Dr. LANGHAM. Oh, yes, very definitely.

I would not say it is necessarily good that we have radiation, but I should say, also, that I do not think it necessarily means we have to stop further technological advancements because radiation is a factor in that advancement.

Chairman DURHAM. We have to have it.

Senator ANDERSON. I am very happy to see so many heads nodding in agreement. We are happy to find something on which these scientists agree.

Thank you, Dr. Langham, for producting that motion.

Chairman DURHAM. I raised the question because of the fact we have lived under sunshine here for thousands or millions of years, and we still have a future, I think. With the radiation that we are trying to determine—of course, that is the purpose of all of these hearings—can we continue to live with this thing, with the manmade process we have come to today? We have accepted the others for years because there was very little we could do about it.

Is that not essentially correct, or is it?

Dr. LANGHAM. You are very definitely correct. We have lived with many things in the past that are of our own doing.

For example, the smog from automobile exhaust. And I think we will continue to do it.

I am certainly in favor of saying we have faced many things that we have had to face because they were a part of our environment, and we could do nothing about them. We have also faced many things which are of our own creation in the interest of technological advancement. I say, yes, let's continue to face them, and by all means let's not think that radiation is the only one, and let's not think that radiation is the insurmountable one.

Chairman DURHAM. That is right. I agree with you.

Senator ANDERSON. Mr. Van Zandt.

Representative VAN ZANDT. Dr. Warren, did I understand you to say a moment ago that the data we have at the present time represents a spread of 40 years?

Dr. WARREN. It actually represents a still wider spread than that. We began to get some understanding of the effects of radiation in the late 1890's, very soon after the discovery of the X-rays, and the isolation of radium, and our knowledge of the field advanced very

slowly only recognizing the more extreme cases in the early days.

Then, with the concerted attack on the problems of atomic energy in the Manhattan project, there was added a great deal of additional information, which is being kept up and added to all over the world at the present time.

I also mentioned that there is one population in India on the Monazite sands which has lived there for many generations. We do not yet know the medical and biological effects of this, but these will be studied by the Indian Government, and the data made available to the world.

Senator BRICKER. Is there any indication that those people who did live in the Monazite hills have had a shorter life span?

Dr. WARREN. I do not believe that the vital statistics are sound enough——

Senator BRICKER. They are not sound enough yet to determine?

Dr. WARREN. To draw conclusions at the present time.

Senator BRICKER. Either on that, or leukemia or cancer?

Dr. WARREN. No. I think we will have to have pretty careful studies for a fairly long period of time to answer this.

Senator BRICKER. Are there not also some places in South America where the same conditions exist?

Dr. WARREN. Yes, there are. There are Monazite areas in Brazil. But here the inhabitants are chiefly Indian tribes who do not stay in any one area very long. And while studies are being carried out by the Brazilian Government, there will not be as large a population available for study.

Senator BRICKER. There is no evidence, is there, that there is any resistance in the human body built up to this radiation like there is to bacteria?

Dr. WARREN. No, there is no evidence there is any resistance. In fact, there seems to be an indication that, if there has been a significant amount of radiation given at one time, that individual will actually stand somewhat less radiation.

Senator BRICKER. Is that because of the accumulation or because of the weakening of the cells?

Dr. WARREN. That is probably because some of his cells have been injured or killed.

Senator BRICKER. Weakening, yes.

Senator ANDERSON. Now, Dr. Machta.

Dr. MACHTA. Do you want at this time to go into detailed numbers, sir? This is what has been suggested that I do.

Mr. RAMEY. In some detail. Not necessarily in absolute detail. I think the idea was that you made certain statements in your testimony last week, and from that information on the possible nonuniformity of deposits that you could make some calculations on future predictions of what might be deposited.

Dr. MACHTA. Thank you.

There are essentially three reasons why the fallout over the globe should be nonuniform. Two of these have never been in question. These are that tropospheric fallout from tests occurs primarily in the band of latitude in which the tests take place.

(Discussion off the record.)

Senator ANDERSON. Proceed.

Dr. MACHTA. I mentioned that there are essentially three reasons I believe why there might be nonuniform fallout over the globe. Two of these have never been in question and have been well accepted.

First, the tropospheric fallout occurs primarily in the band of latitude in which the test takes place.

Second, rainfall being the primary mechanism by which particles are removed from the atmosphere, will give rise to greater fallout in rainy areas than nonrainy areas.

The new element which has been added, and on which there may be some question, is the point I wish to talk about. This is the fact that fallout may be coming from the stratosphere preferentially in the temperature latitudes of Northern Hemisphere.

My contention is that I am not sure to what extent this occurs, and in view of the possibility, we ought to take into account a very conservative as well as, let us say, a very optimistic picture, in the hope that the truth will lie between the two.

What I would like to offer now are the numbers that one gets out of two pictures; one which essentially gives rise to complete uniformity from now on, and one which gives rise to extreme nonuniformity.

The latter is supposed to represent an absolute upper bound.

In order to compute my numbers, I have to make an assumption of the amount of debris still in the stratosphere, and from the previous testimony, up to mid-1956 there are still about 24 megatons equivalent of fission products left in the stratosphere. When it comes down, it will decay, and consequently there will be a smaller amount deposited on th ground. However, one might say that the tests which have been held since mid-1956 may balance the decay.

I can talk about two conditions: One, there are no more tests, and we want to find the fallout in terms of what has been put in the stratosphere already; and, second, the tests continue at whatever rate you wish to specify.

The curve which I drew several days ago describing the nonuniformity of fallout from all sources—and one need not ascribe it to either tropospheric or stratospheric fallout—shows about 10 millicuries per square mile as the average peak in the north temperate latitude as of 1956. If you have 24 megatons—this is equivalent to 12 millicuries per square mile when spread uniformly over the globe—by adding 10 and 12 one gets 22 millicuries per square mile in the north temperate latitude for the case of uniform future fallout.

Now, superimposed on——

Mr. RAMEY. How much is that in subshine units? Is it the same?

Dr. MACHTA. No; it is not the same.

Dr. LIBBY. You get the answer for the average soil pretty well by multiplying by two.

Dr. MACHTA. But these sunshine units are not in the human bone. There is still the discrimination factor.

Dr. LIBBY. These are sunshine units in the soil. So with Dr. Machta's 22, that is the average soil will be 40-some sunshine units.

Dr. MACHTA. This, then, would represent a sort of lower bound for the average temperate latitude of the amount that would have fallen out from tests conducted essentially to date. One can put this into perspective. According to the latest information, the norhern tier of the United States has about 33 millicuries per square mile.

However, superimposed on a value of 22 millicuries per square mile for the average band in the north temperate latitude, one should take into account the fact that there are areas of heavier rainfall than average. I have stated that the factor taking weather into account should be no more than 2 or 3. I think a reasonable number is two.

Therefore, I have suggested that the lower limit of the total fallout which will occur in the northern temperature latitude will be in the order of 40 or 45 millicuries per square mile, or converted to something under 90 sunshine units in the soil.

There is another way of looking at the matter. In a recent letter from Dr. Brewer, he suggests that stratospheric debris will come out almost entirely in the temperate latitudes of each hemisphere. This reasoning would yield a marked peak in the temperate latitudes.

The number I am about to quote is the upper bound. Mixing in the stratosphere must of necessity make this lower. I am not offering it as a number that would occur. I would like to make this clear. But since this an absolute extreme, the true number would be less than it.

The number I would like to offer is something of the order of 60 or 70 millicuries per square mile on the average. And again multiplying by the factor of 2, would bring the answer up to 120 or 140 millicuries per square mile.

As I say, I am not quoting this as a number we expect to have. This is a thing we can expect as the absolute upper bound.

This is what will happen from the tests to date. Fallout of strontium 90 will lie somewhere between 40 and 120 millicuries per square mile.

If we continue our tests at about the same rate, one can also make a calculation. Dr. Campbell, formerly of the National Academy of Sciences, has published an article in Science which shows how to take into account not only the radioactive decay, but the storage in the stratosphere. (See p. 1338.)

Presuming the test rate to continue at approximately 10 megatons per year—and if one desires any other test rate the answer is proportional to it. The assumption is made that the mean stratosphere storage time is 5 years—and I prefer this to 10 years, since it gives a slightly more conservative answer, although the difference is less than 10 percent, which is negligible. Then the answer at equilibrium, which will take many years to achieve, would be something in the order of 350 millicuries per square-mile or a little less, to something in the order of 850 millicuries per square mile. A very conservative picture is one in which the nonuniformity is no greater than a factor of 2 as the ratio of peak to average. A guess on my part—and I must admit this is a guess since I have not gone through any quantitative calculations—calls for a factor of 5 for the case of extreme nonuniformity for the ratio of peak to world average.

These give rise, as I indicated, to a range of 350 to 850 millicuries per square mile.

Mr. HOLLISTER. Is the concept of a mean storage time, in your mind a valid one for stratospheric fallout?

Dr. MACHTA. Well, meterologically speaking, it is not necessarily a valid one. I think it is useful for the purposes for which we are using it.

Mr. HOLLISTER. That is, empirically it is valid, although essentially we agree it offers some problems?

Dr. MACHTA. That is correct, sir.

Mr. HOLLISTER. The numbers 350 and 850 correspond essentially to equilibrium after a long period of time? Many years?

Dr. MACHTA. That is so.

Mr. HOLLISTER. Could we not say that within, shall we say, 35 to 100 years we will be so close to the equilibrium value that from then on we can say we are at equilibrium, assuming a constant rate?

Dr. MACHTA. I think this is roughly in the order of magnitude of time to achieve equilibrium; yes.

Mr. HOLLISTER. What is the factor you used to multiply by, to get this equilibrium number?

Dr. MACHTA. In the case of the most conservative picture, I assumed the peak was in the order of twice the average for the world. At the present time, the peak is something, I think, about two and one-half times the average of the world.

In the case of the least conservative picture, which I again view as an extreme and not likely to happen, it is of the order of five times the average for the world.

Mr. RAMEY. What are those figures, then, translated from your microcuries per square mile to bones, for example? Is that possible?

Dr. MACHTA. It is possible, if the gentleman here would provide the numbers. The first step would be to convert to subshine units in soil, which Dr. Libby has done very simply. The next step would be the discrimination factor. This I do not know how to do.

Mr. RAMEY. Perhaps we might carry on from there, and let possibly Dr. Libby, Dr. Neuman, or Dr. Eisenbud apply these discrimination factors, and see how we come out on these upper and lower limits.

Dr. NEUMAN. In my previous testimony, I said that my personal choice of the discrimination factor, and one that I thought represented the best guess, perhaps, was 8; and that I firmly believe the true discrimination factors lie between twice or half of these limits, between 4 and 16.

As far as I know, the other testimony of people using discrimination factors involved figures that also lay between these limits. I think some used 14.

I think, also, that this represents, as I mentioned, the view of Dr. John Lautit, of England. I believe he is on record as choosing eight for the present.

Really this is pretty good, considering some of the spread we have heard in statements. A factor of 2 is relatively good.

I think we are weak on estimates of effects, but in determining levels we can pretty well agree that it lies between 4 and 16.

Dr. LIBBY. I would comment on Dr. Machta's calculations as follows:

Dr. Machta and I agree that the question of the uniformity of stratospheric mixing is one that has to be settled by further experimentation.

There is a great deal of evidence for a certain amount of stratospheric mixing, as I remarked yesterday, since we do find strontium 90 in fallout, even at the South Pole and we have never found any place in the Southern Hemisphere where it rained at all where we did not find strontium 90.

So I think the extreme calculation is really rather extreme. It is all right to put it down, but I am sure Dr. Machta would agree it is rather an extreme calculation.

We will know this answer, as I said yesterday, by our stratospheric monitoring program. That is, we will know the matter of the horizontal mixing.

Now the possibility of concentration in terms of leakage at a certain latitude is an additional point which we will also learn about in connection with our studies of the age of the fission products that come down in our washtubs. We can determine simply whether the material that we collect is old or young. If it is coming from the stratosphere, it must be old; if it is coming from the troposphere, it must be young. And this program will settle it.

Representative VAN ZANDT. Dr. Libby could you include in your statement a time factor? And if that is not possible, will you know in a year or 2 years?

Dr. LIBBY. I think we will know in less than a year. Maybe I am being optimistic, but a year ago we would not have made the flat statement that rain is it, as far as fallout is concerned. And we all make it now. We have established that firmly, we think, during this past year. I think so.

Representative VAN ZANDT. Can you definitely establish the age of the fallout?

Dr. LIBBY. Yes, sir. It is not the easiest thing in the world to do, and it has not been done very often, but it can be done.

Representative COLE. Does not this fallout have any weight at all?

Dr. LIBBY. No, sir.

Representative COLE. None whatever?

Dr. LIBBY. Negligible. We think the particle size is so small you cannot see them, even by the highest powered microscope. We think the particle sizes are well down in the hundredths of a micron range. They may be as large as a tenth of a micron. They are very, very tiny, too small to see, but we can measure them by virtue of their radioactivity.

Now the matter of the discrimination factor. I would not argue with as eminent an authority as Dr. Neuman, and I certainly think his statement is a fair one from all that I know about this difficult subject, that the discrimination factor between average American diets and the bones should be between 4 and 16. I think certainly that must be right.

There is one little point I would like to make that seems not to have been made by anyone. I think it is just accident.

We find the fallout in the top 2½ inches of the soil; therefore, deep-rooted vegetables get less strontium. This is a point worth thinking about. If you plow it to distribute it, it will still get less strontium, for there will be more calcium to dilute it.

So the short-rooted grasses which produce the milk give us about the worest situation.

So I think there is another factor. It is not what we should call a discrimination factor, I think, in the human body, but there is a sort of physical factor as to the depths of the strontium 90 deposits relative to the length of the roots which pick it out of the soil. I do not know what that number should be, but I think it is certainly a significant number.

Maybe like—I do not know. Certainly a number greater than 1, and probably less than 2. So there is a factor which could be multiplied by the discrimination factor which Dr. Neuman gives.

Senator ANDERSON. Mr. Hollister.

Mr. HOLLISTER. Dr. Libby, then you would agree that we should use equilibrium discrimination factors at the moment rather than, for example, to try to relate soil data to, let us say, Kulp's bone data?

Dr. LIBBY. I think you should do both, Mr. Hollister. That is, you should figure out—we know now, as Dr. Kulp has said, that fresh bone is, I think he testified, 0.7 sunshine units in this country, and this at a time when the average soil was—well, it is hard to say just when the children got their average food—but say, 50 sunshine units. Between 40 and 50 sunshine units probably was the ground concentration.

Now all of these factors have come in to give us a ratio in excess of 50 between the ground concentration and new bone at the present time.

Now, the equilibrium discrimination factors are only part of it. I just mentioned another one.

There is another factor I have not mentioned, and that is that the Imperial Valley and desert regions supply truck crops through irrigation methods where the strontium 90 is lower. So this leads to a decrease in the strontium 90 content of the average diet, you see. That should be in.

Mr. HOLLISTER. I am going to go from an MPC in the body, and let's beg the question whether there is one at the moment. If I want to go from MPC to testing rate, which I think Dr. Neuman showed we could do in principle——

Dr. LIBBY. Yes.

Mr. HOLLISTER. How do we get there? Do we go by way of a factor of 50 or 30, or by a factor of 8?

Dr. LIBBY. I do not think we know the right answer to that question. I think you had better do both of these calculations. It looks to me like we have to admit to a certain ignorance as to the correct way. That is, we have 4 to 16 as the range here. I really think Dr. Neuman knows the factor a little better. But that is a fair range. Then we have to estimate these various additional things I have mentioned, and there are some unknowns in this calculation.

Mr. HOLLISTER. Could I ask Dr. Neuman to comment here?

Dr. NEUMAN. I think everyone will admit to uncertainty, and I would be the last one to say that we are certain of these figures. I stated 8 as the best number and estimated the range of 4 to 16, in terms of the variability in results reported thus far. There may be something basically very wrong with the results on final equilibrium factors, in which case our models are wrong. There may, indeed, be another factor of perhaps as much as 2 either way. If we multiply 16 by 2, we are up to 32. I think the uncertainty over how soon equilibrium is achieved means that the higher relation found in the survey data is not really in disagreement with the experimentally determined discrimination factors.

I would like also to say that I felt the best model to use is natural strontium, something with which we truly are in equilibrium. However, I feel the data are much less certain than some of our experimental data with isotopes, because I have distrust of emission spectro-

scopy as applied to a wide variety of samples and the difficulty of estimating what is actually the average diet.

I think there is an important document that received no attention (but it is in the record) submitted by Norman McDonald, of UCLA, comparing his best estimate of the Sr/Ca ratio of average diet and of human bone, determining natural strontium by suspected emission spectroscopy. He came out with an estimate of the equilibrium discrimination factor as 15. This is right in between my estimate of 8 and some of the higher estimates from the survey data. (See p. 720.)

So I will confess to this uncertainty, and I think, for purposes of calculation, that we should take them at several levels. I would suggest 8 be considered one reasonable value, and 32. Well, that's too high. I would say 16, right now, is not an unreasonable guess. It might also be 4.

Would you agree, Dr. Libby?

Dr. LIBBY. I think so. If you have to pick a best number, something like that (8 or 16) might be the best one.

Senator ANDERSON. Dr. Langham, I wonder if you would like to run through that table now of the calculations showing strontium 90 in the bone in relation to maximum permissible concentration?

Dr. LANGHAM. This is a repeat of numbers in essence that were placed before you last week. What it amounts to is taking the various estimates of present levels in the bone, or present levels in the soil, and applying ecological discrimination factors, and estimating what the equilibrium level will be when our entire environment is in equilibrium with the present test rates, assuming 10 megatons of fission yield per year injected into the biosphere.

All this amounts to is taking the various estimates of present bone and soil levels and applying Dr. Libby's factor of 8, which is about what these levels will be increased by when equilibrium is reached, assuming that we can continue to test at 10 megatons fission yield per year.

First, we can take Dr. Libby's estimate given in his recent speech, in which he estimated that the United States or rather the north temperature population belt, would reach equilibrium bone levels of somewhere between 5 and 20 sunshine units. These values were derived by assuming different ecological discrimination factors: 1 in the region that Dr. Neuman is talking about, and 1 in the region which Dr. Libby has talked about on occasion, up around 80.

If we take Dr. Kulp's estimate of two for the equilibrium bone level assuming no more tests, which, of course, is for the general population of the latitudinal belt, and multiply it by 8, we obtain an equilibrium bone level of 16 for continued testing at the present rate.

I have taken Kulp's bone data and applied certain corrections that I think refine it to some degree.

Mr. HOLLISTER. At the present rate of testing, and assuming the rate continues, or at the present rate of testing, assuming no more testing?

Dr. LANGHAM. These are numbers assuming we continue to test at the equivalent of 10 megatons of fission products injected into the biosphere per year until we reach equilibrium. This is until the decay of strontium is equal to the amount we are putting up.

So we have 5 to 20 sunshine units by Dr. Libby. Dr. Kulp's numbers give 16.

Our numbers from Dr. Kulp's bone data is 25.

Mr. Eisenbud, in this meeting, estimated about 40 from New York milk data.

My estimate on the basis of other milk data gives an equilibrium bone level of 29.

If we take the soil data, which is the 22 millicuries per square mile that Dr. Libby has estimated at present, multiply by 1.8 to give sunshine units in the soil and apply a discrimination factor of 10 in going from soil to bone, we come out with 42.

Now, Dr. Machta's numbers that he just quoted are handled in this fashion. That is, we take the soil numbers he gave, multiply them by about 2 (1.8 to be exact) to get them to sunshine units in the soil, and then take one-tenth of that, assuming a discrimination factor of 10, and we come out with an estimate of from 70 to 170 sunshine units ($\mu\mu c$ Sr–90/g bone Ca) as the equilibrium bone level that will be reached if biospheric contamination continues at the present rate.

Mr. HOLLISTER. Which soil data are you taking now?

Dr. LANGHAM. The ones Dr. Machta just gave as the equilibrium level with continued testing (350–850mc/mi^2). Is that not right?

Dr. MACHTA. Yes.

Dr. LANGHAM. The ones he just gave would predict, then, for the United States on this basis, assuming a discrimination factor of 10, 70 to 170 sunshine units in bone at equilibrium.

Now the maximum permissible level that we have been talking about is 100 of these units. So I think you can see that at the present rate of testing, if we assume all numbers other than Dr. Machta's are correct, the average is a factor of 3 below what we have accepted as a maximum permissible level, meaning that we can assume that some people will be 3 times the average value, and still not exceed what we have set as a maximum permissible level.

If we take Dr. Machta's number of 70 sunshine units which is his best or average number—is it not—for this?

Senator ANDERSON. The lowest number.

Dr. LANGHAM. The lowest number. Then our present test rates would not allow a factor of three safety. If we take his most pessimistic guess, then our present test rates could not be permitted.

Senator ANDERSON. Actually, Dr. Neuman's testimony of 2.2 megatons of fission products to be somewhat safe—I hope I do not misstate you, Doctor—would indicate also we would be a little bit above what he regarded as a safe level. I should not have said it. Maybe you should.

Dr. NEUMAN. I made two calculations, one using the MPC of 50, and one using the MPC of 100. Using the 100 value, I came out with a maximum equilibrium test rate of 4.4 megatons of fission products injected annually into the stratosphere. But in that calculation I assumed Campbell's model, and 20 percent overall decay due to the reservoir, and I used a factor only of 2 to take care of the variation in individuals, but I used a factor of 3 for nonuniformity.

So, in a sense, my calculation really anticipated Dr. Machta's, although I did not know I was anticipating it at the time I made it.

Senator ANDERSON. It is very interesting that the two worked out as they did. Mr. Hollister.

Mr. HOLLISTER. I would like to introduce for the record an article by Dr. Charles I. Campbell, entitled "Radiostrontium Fallout from Continuing Nuclear Tests."

(The matter referred to is as follows:)

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RADIOSTRONTIUM FALLOUT FROM CONTINUING NUCLEAR TESTS

In spite of widespread comment on the problem of the fallout of radiostrontium from testing thermonuclear weapons, confusion persists in the public mind and perhaps among many of the readers of Science as well regarding the relationship of Sr–90 accumulation on the ground to such factors as assumed mean storage time in the stratosphere and the rate of testing of thermonuclear weapons. Libby's recently published report on the AEC's studies of the Sr–90 problem (1) was not addressed to the effects of continuing weapons tests. Yet his conclusions have recently been quoted in the press as if they were valid if tests continue provided only that test rates remain unchanged.

Libby's analysis considered essentially the question whether nuclear weapons tests to date may have committed us already to an intolerable accumulation of Sr–90. Happily they have not. Speaking to that point, the meteorologists on the National Academy of Sciences study of the biological effects of atomic radiation stated, "At present, the amount of Sr–90 in the stratosphere from nuclear weapon tests is far too small to approach maximum permissible concentration even if it all were to be deposited now. However, if the testing programs of the several countries producing thermonuclear weapons were to intensify, stratospheric storage time may become a critical item in terms of hazard to mankind. For this reason, a continuing program to investigate this phenomenon is needed, including actual measurements of the radioactivity in the stratosphere and improved and more representative methods of observing fallout" (2, p. 60).

The consequences of continued tests can be discussed in terms of a simple mathematical model which is generally accepted by Libby and others in this country as well as in England (3). Assume that Sr–90 is introduced at a constant rate n into the stratosphere, where it is immediately mixed uniformly over the entire glode. According to British data, mixing is evidently reasonably rapid (3, p. 11). Assume further that fallout occurs at a rate $R=kQ$, where Q is the instantaneous stratosphere storage and k is the reciprocal of the mean stratospheric storage time.

Accumulated radiostrontium on the ground, M, can then be shown to be

$$M=\frac{n}{\lambda}\left[\frac{k}{k+\lambda}+\frac{\lambda}{k+\lambda}e^{-(k+\lambda)t}-e^{-\lambda t}\right] \qquad (1)$$

where λ is the radioactive decay constant of radiostrontium. If the constants are expressed in years and the rate of testing is expressed in terms of millicuries of Sr–90 per square mile of the earth's surface introduced per year into the stratosphere, M is given in terms of millicures of Sr–90 per square mile at t years. When $t=\infty$

$$M_{max}=\frac{nk}{\lambda(k+\lambda)} \qquad (2)$$

and the maximum accumulation of fallout is seen to be proportional to the test rate.

Using Libby's best estimate for the mean stratospheric storage time of 10 years and a conservative estimate of the test rate corresponding to the introducion of 2.5 mc/mi^2 yr, per year as a reasonable value for n, the maximum accumulation of radiostrontium would be about 80 mc/mi^2. Libby, *considering only the Sr–90 produced up to 1955*, predicted maximum fallout of less than one-tenth this amount. The two figures should not be confused.

It is not yet known what fraction of the total radiostrontium produced from a thermonuclear weapon reaches the stratosphere and becomes involved in the fallout process discussed here. For this reason, we do not know how to interpret available data on test rates and accumulation of Sr–90—for example, whether little has reached the stratosphere and has subsequently fallen out again relatively quickly or whether much has entered the stratosphere but has been held back by a long storage time. It cannot be said with much confidence, therefore, what rate of weapons testing would result in a given accumulation of Sr–90.

Assuming a 10-year storage time and a continuing test rate about twice that mentioned in a previous paragraph (corresponding to estimates made by Stewart, Crooks, and Fisher in the United Kingdom), the Sr–90 accumulated on the ground after about 35 years would be 80 mc/mi². This would correspond to about 0.14 MPC (maximum permissible concentration) unit in the soil. According to Libby (4), Sr–90 levels in soils are converted to levels in bones of young children at about 70 percent efficiency. This reduces the figure for levels in young children after 35 years of continuous tests to about one-tenth the permissible levels as established for *occupational* exposures. The concentration would not fall much below 0.07 MPC unit even if storage time were found to be 20 years instead of 10. Recently committees of the National Academy of Sciences (2, p. 39) and the British Medical Research Council (5, par. 281) have expressed their belief that only 0.1 MPC unit or less should be permitted for the population at large. In fact, the British report stated (5, par. 360): "So far as radioactive fallout may affect the individual, we believe that immediate consideration would be required if the concentration of radioactive strontium in bone showed signs of rising greatly beyound that corresponding to one-hundredth of the maximum permissible occupational concentration." The rate of introduction of Sr–90 into the stratosphere assumed here is close to that estimated by Libby for the past 3 years. On the assumptions made here, therefore, a long-term test program could conceivably reach or exceed the levels of Sr–90 considered safe for the whole population.

There is little reason to hope that what may be learned about storage time, *k*, will change this situation much. We must hope that new information may allow us to increase the maximum permissible concentration of radiostrontium in the bodies of the people of the world, that means may be found to decrease the input of Sr–90 to the stratosphere from tests, or, preferably, that a new attitude among the people of the world will permit us to lower the test rate, *n*.

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REFERENCES AND NOTES

1. W. F. Libby, Proc. Natl. Acad. Sci. U. S. 42, 365 (1956).
2. Biological Effects of Atomic Radiation. Summary reports (NAS–NRC, Washington, D. C., 1956).
3. N. G. Stewart, R. N. Crooks, E. M. R. Fisher, British Atomic Energy Research Establishment (Harwell) Rept. HP/R 1701 (1955).
4. Note added in proof: In an address before the AAAS, on 12 Oct. 1956, Libby presented data indicating that the efficiency is more likely between 10 and 30 percent. He also estimated that in the past 2 years Sr–90 was introduced into the stratosphere at an average rate of 6 mc/mi² yr, or more than twice the rate assumed in the calculations of this paper. Libby's address will be published in Proceedings of the National Academy of Sciences.
5. Hazards to Man of Nuclear and Allied Radiation (Her Majesty's Stationery Office, London, 1956).

Mr. HOLLISTER. I would like to read a paragraph from Dr. Libby's speech before the American Physical Society on April 26, 1957. This is on page 23, the third paragraph.

Of course, as testing continues and more fallout occurs, the levels will rise. The strontium 90 that still resides in the stratosphere at the present will fall out according to our expectations at a rate which just about compensates for the decay of the material already deposited, so that no great additional increase from this source is to be expected from weapons fired in the past. If this testing should continue at about the same rate as it has averaged over the last 5 years, then we should at equilibrium, after an infinite time, approach a level of 8 times the present rate, since the average life of strontium 90 is 40 years. This assumes that the future testing will be conducted so as to give in each future 5-year period the same as the last 5 have. And so we would expect in the United States at that time an average human strontium 90 concentration of 20 sunshine units with the conservative factor of 20 between the topsoil concentration and the concentration in human bone, or 5 sunshine units if the factor of 80 is used. In other words, in the United States something between 5 and 20 sunshine units would be the equilibrium concentration of human bones if testing continued

indefinitely at the average rate of the past 5 years. This level would be approached only after a few decades. After 28 years the level would be half of this equilibrium value, and after another 28 years, 56 years total, from an arbitrary beginning which we have set as 1952, we would expect in the year 2008 three-fourths of the equilibrium figures. So somewhere between 4 and 15 sunshine units of strontium 90 in human bones in the United States might result from the present type of testing being continued for the next 50 years.

I would like to ask Dr. Libby how he derived the factor of 8 that would represent the levels at equilibrium as compared with the levels now.

Dr. LIBBY. Mr. Hollister, as you probably know, the strict derivation of this is somewhat complicated mathematically. But let me try to express it in words.

If you take a rate of testing which is equal to the average over the last 5 years, then per year you have got one-fifth of the total we have now.

Now, if you continue this indefinitely, you will accumulate a final total amount which is equal to the average lifetime of the radioactive strontium.

The average lifetime of radioactive strontium is 40 years, not 28 years. Twenty-eight years is the halflife. So the calculation is after an infinite length of time you will have 40 years' worth if no strontium decays.

The reason that you do not get more than 40 years' worth is that the decay of the strontium compensates for the new accumulation. Now, this holds for both stratosphere and for the stuff that is on the ground.

Now, actually the calculation is not strictly rigorous, in that, as Dr. Machta pointed out, and as I have pointed out, too, the strict and rigorous calculation is a bit more complicated. But, considering all of the factors that are involved here, and restricting the consideration to the United States, which was the condition of that paragraph—I am not sure that paragraph made that clear. We were talking about the United States——

Mr. HOLLISTER. I think it would be implied in the sentence that, "So we would expect the United States," et cetera.

Dr. LIBBY. Yes. I consider that this factor of 8 is good enough and a good-enough approximation to the truth to give. The exact truth is not known to us.

We do not know, for example, whether after a long time the strontium 90 that lies in the soil will not act in the way that calcium acts, in that a portion of it will become unavailable to the plant. We do not know whether the strontium 90 that has been there for 5 or 10 years may not be less available to contaminate plants. Chemically, it is quite likely that this will happen. I had in mind, in using this factor of 8, to load the calculation a little bit for these factors which we have not yet known. I was trying to get a number which I think is the solidest number.

Mr. HOLLISTER. You would agree, then, it is a loaded number?

Dr. LIBBY. It is loaded on the side of truth, in the side of actuality, in the direction of what is most likely to happen.

Now, it is very difficult to estimate the effect of this precipitation out, or removal from the biosphere of fallout strontium, which I think we will discover, and I already see evidence in our actual data.

Mr. HOLLISTER. Would you agree that the Campbell model is correct?

Dr. LIBBY. The Campbell model is strictly correct on the assumptions made, surely. I believe it is. I have not gone through all of the derivations in detail.

Mr. HOLLISTER. I went through the Campbell model in trying to justify this factor of 8, and using a half life of strontium 90 of 28 years, the only way I could justify this factor of 8, 8 now being the ratio of the level on the ground at infinite time, and the ratio now being 5 years after we started the constant rate of testing——

Dr. LIBBY. Right.

Mr. HOLLISTER. The only way I could justify this factor of 8 was to assume the storage time as zero.

Dr. LIBBY. That is strictly mathematically correct.

Mr. HOLLISTER. If I assume the storage time is 10 years, which I believe is sort of an average number that we have seen in the literature, this factor of 8 becomes a factor of 32 or 33.

Dr. LIBBY. That is certainly wrong. Not that your calculation is not right, Mr. Hollister, but for the reasons I have pointed out. You see, we now have on the ground about—what is it—two times as much as we have in the stratosphere, something like that, in the United States. And this is a very important consideration in your calculation. I cannot do it in my head and get the strict mathematical number. But we have to consider the biological aging of the strontium that goes into the soil. It is very difficult to know exactly what to do with this, but is seems to me that it was a reasonable thing to do to say that this aging out was about compensated for by the stratospheric—the fact that the stuff is stored in the stratosphere.

Mr. HOLLISTER. Would you say then that one of the problems here is the Campbell model is not strictly applicable?

Dr. LIBBY. Well, I would not put it quite that way. I would say that we do not know enough to make this calculation with complete certainty. But I do believe that the numbers given in the first row (on the blackboard) are just about right, and that the schedule that will be attained, that is, half of it by 1985, is just about right.

Mr. HOLLISTER. You would agree, though, that if, somehow in this loaded calculation we loaded it wrong, it is possible by virtue of the Campbell model argument that that 5 to 20 could be greater?

Dr. LIBBY. I think this is very unlikely. You see Dr. Campbell did not consider the aging, the removal of the strontium 90 from the biosphere. I believe that is certain to be an appreciable effect. But we do not know enough yet to know how important it is.

Mr. HOLLISTER. Would the others like to comment either on this discussion or on the numbers?

Dr. NEUMAN. I would like to say one thing. If the Campbell model is 32, this is a factor of 4, and yet you said earlier, Dr. Libby, you thought the aging was only a factor of 2. We still have a factor of 2 running loose.

Dr. LIBBY. I cannot check in my head the factor of 32 of Mr. Hollister. I do not distrust Mr. Hollister's figure, though.

Dr. MACHTA. My numbers on the board obviously stick out like a sore thumb. The reason lies partly in the factor of 8 used to increase the other values. The aging factor that Dr. Libby just indicated as

being included in the 8—when you put that in my figures, they would be reduced to the same order of magnitude as the others. So those numbers are not comparable. If you do not take into account Libby's aging factors, and if you make the assumption, for example, that half of what fell out in the United States is tropospheric, and half stratospheric, then instead of a factor of 8 you get something like 14 for the conversion of the 5-year fallout to equilibrium fallout.

Further, if you assume what came out of Castle is only 3 years old instead of 5 years old, the number 114, becomes even higher.

So this is the reason for the difference. Dr. Libby has incorporated in the 8 other factors. These and other differences should be included in the 70 and 170, and the changes will bring my answers down to the others. So as now written they are not comparable and I do not wish the record to show this comparison.

Dr. LIBBY. Should they not be still higher?

Dr. MACHTA. No, I do not think the answers will come out higher if you take into account the aging and other factors.

Mr. HOLLISTER. Do we not all agree on the discrimination factors? So there is only the question of uniformity and nonuniformity, and the upper limit would have to be higher; isn't that so.

Dr. MACHTA. The upper limit, yes.

Senator ANDERSON. I am going to recognize Mr. Durham in just a second.

Would you submit a subsequent calculation for our record that might take into consideration some of these other things? We are trying to find where the truth lies. There are all kinds of truths: There are truths, half-truths, the whole truth and nothing but the truth, so help you God.

Dr. MACHTA. I have given the amount falling on the ground. When that is converted to what goes into the bone, others are contributing, what are the conversion factors? If someone would tell me, then the numbers can be made comparable. But this has not been done, sir.

Senator ANDERSON. I just thought you might say, assuming a certain factor, which everybody seems to agree on, 8 or some other figure, is proper, your figures would come to this point. If you can do it, fine, we will appreciate it. We have appreciated your cooperation thus far so much. If you can do it, fine. If you cannot, nobody is going to put you in the penitentiary.

Mr. Durham.

Chairman DURHAM. My question may be irrelevant, but I am going to ask it anyway. Is it in general agreed by all the scientists worldwide that no strontium existed until we developed a thermonuclear weapon?

Representative COLE. How can you be sure? I am intrigued by the question, because I am curious to know.

Chairman DURHAM. I was thinking of the possibility. Of course, this is getting rather high into the problems above the stratosphere and biosphere and all the rest of the spheres, and getting up into the satellites. Is there any scientific data on the possibility this could have happened over the many, many years of existence of the satellites that some chemical reaction possibly took place, and now we are getting this down on the earth?

Dr. LIBBY. You asked two questions, Mr. Durham.

To answer the first one, well, you stated, and Mr. Cole asked if you knew your statement to be correct.

Representative COLE. No. You misunderstood. You gentlemen were nodding your heads when Mr. Durham asked if there were general agreement worldwide among the scientists that strontium 90 did not exist prior to the atomic age. You were all nodding your heads. It was because of that that I said, "How can you be sure?"

Dr. LIBBY. Yes. I will tell you.

Representative COLE. Referring to strontium 90.

Dr. LIBBY. I will tell you how we can be sure. We looked for it before the atomic bomb was invented, and we could not find it, sir. That is how we know and we can still look for it by taking old material. As a matter of fact, the other day I got a can of old tuna fish from a neighbor of mine, which was canned before July 16, 1945, and we will look for strontium 90 in that. Dr. Langham has been collecting milk samples. I do not know whether he has been successful. But just to be sure that there was no strontium 90 before the atomic bomb was first fired we have done these things.

We have looked in old bodies and there is not any. We have looked in the ground below this 2½-inch level and there is not any. This is how we know, Mr. Cole.

Chairman DURHAM. How high have you looked for it?

Dr. LIBBY. Well, sir, the highest we can look is as high as we can sample air, which is—I do not know the exact answer, but it is a few miles.

Chairman DURHAM. Will we get any data from the satellites we are building?

Dr. LIBBY. Now on your satellite question, it is a very interesting point. A few months ago we would all have given a categorical "No" to your question and said there could not be any up there. But recent evidence has indicated that we did not know as much as we thought we knew about the contribution of the sun to the earth. We learned a year ago last Washington's Birthday during the incidence of a great solar flare that matter comes to us from the sun directly, and that the so-called cosmic rays, which we have been speaking about so much, come in part from the sun. Now, if these can come from the sun, I am not so sure that a tiny little negligible amount of strontium 90 might not possibly come from the sun.

The only thing I can say now is that it must not be too large an amount, or we would find it in preatomic bones and soil, and we have not. So it is not too large, but it still might be up there in detectable quantities. I am quite sure it will not be enough to in any way upset our considerations.

Chairman DURHAM. Then we cannot convict man entirely at the present time of placing this burdensome problem upon the world?

Dr. LIBBY. I am not sure that it is there, either, Mr. Durham, but there is a possibility.

Chairman DURHAM. A possibility.

Senator ANDERSON. Off the record.

(Discussion off the record.)

Senator ANDERSON. I think if there is no violent objection, I am going to take advantage of the suggestion here and adjourn. We will not have a session this afternoon because the House is very busy with

a long series of votes, and it makes it most inconvenient for the Members to come and go. They want to be sure to hear it all.

Therefore, tomorrow morning at 10 o'clock, in the old Supreme Court room in the Capitol, the final session will be held. There will be no afternoon session.

That is the final session only as to these hearings.

The chairman of the subcommittee will undoubtedly, I think, recommend to the full committee that the hearings be adjourned at that time for the development of subsequent testimony, if it seems desirable. Is that not correct?

Representative HOLIFIELD. Yes.

Senator ANDERSON. Therefore, we will meet tomorrow morning in the old Supreme Court room, at 10 o'clock, and we will have only a morning session, because some of these witnesses who have been kind enough to stay with us for several days are going to find it necessary to leave, and we want their contributions to us as we we go along.

Mr. Durham.

Chairman DURHAM. I would like to compliment the panel this morning for a fine display of knowledge. I feel it did a lot of good here and throughout the world.

Senator ANDERSON. Just before we go, Dr. Machta, you said your figures stick out like a sore thumb?

Dr. MACHTA. Yes. I do not think they should be put on the board and quoted. They are not comparable because the proper discrimination factors have not been applied. Therefore, I do not believe the press should quote them in any way whatsoever, sir.

Senator ANDERSON. Let us ask the newspapermen present not to report the Machta figures which he desired not to be put on the board. I would not want to take advantage of any witness here. I know Dr. Langham well enough to know that he put them on with the best of intentions, and only to call attention to the fact discrimination factors is not in it.

If a hundred is permissible, and he has a 170 figure, it might look dangerous, which he does not desire to have it look. I am very anxious we do not distort the comments of a very fine scientist who has been willing to come to us and help us in this discussion.

Thank you very much.

(Below are several items of correspondence following the day's discussion and a comprehensive report on the potential hazards of strontium–90 prepared by Dr. Wright Langham and Dr. E. C. Anderson.)

[Telegram]

JUNE 7, 1957.

HAL HOLLISTER,
Joint Committee on Atomic Energy,
The Capitol, Washington, D. C.:

Regret unable to remain for further discussion at Thursday morning panel. In view of importance of question as to whether National Academy of Sciences pathology report is or is not out of date, suggest Joint Committee request answers to following questions from Drs. Warren, Brues, Neuman, Langham. To wit on page 7 of the pathology report it is stated that "there seems no reason to hesitate to allow a universal human strontium burden of one-tenth the (occupational) permissible level." That is, of 100 sunshine units. Further, on page 2–9, it is stated that "it was felt that an amount of internal radiation

sufficient to double the large population background could certainly be considered safe." That is, of approximately 40 sunshine units. On the other hand, the British Medical Council report stated on page 68 that immediate consideration would be required if the strontium concentration in the human bones showed signs of rising greatly beyond 10 sunshine units.

According to some of the testimony presented to the Joint Committee it is a reasonable although unproven inference from available data that a worldwide average strontium 90 level in human bones of 100 sunshine units could be expected to produce several million cases of leukemia and bone cancer over a few decades. In view of these comparisons, do you feel that the existing pathology reports, prepared in early 1956, gives a balance view of the strontium 90 problem and serves as a proper guide for consideration of it?

Dr. WALTER SELOVE.

UNITED STATES ATOMIC ENERGY COMMISSION,
Washington, D. C., June 7, 1957.

Mr. H. L. HOLLISTER,
Staff Member, Joint Committee on Atomic Energy,
Congress of the United States, Washington, D. C.

DEAR MR. HOLLISTER: The point about the factor by which the soil burden of strontium 90 would be expected to increase if testing were to continue at the average rate and in the same type as during the last 5 years was discussed by yesterday's panel, and you asked why I had given the factor to be 8 in my April 26 address before the American Physical Society when this would be strictly true only if the stratospheric residence time were zero.

I told you that the factor of 8 was given because the aging of deposited strontium 90 in the ground would gradually render a considerable portion of it unavailable to plants and that the lower factor of eight was chosen to compensate for this. The aging effect is analogous to the incorporation of the strontium 90 into insoluble solids form which will not feed the plants. For example, calcium occurs in soils in two forms—the "available" and the "unavailable" and the distinction always is made in soil analysis.

The factor of increase in the absence of any aging appears to be about 11 instead of 32 as given yesterday. (The cause of the difference between our calculations is not clear to me. I did not use Dr. Campbell's equations but derived my own as given below. Perhaps some inadvertent error is involved. I would be pleased to check this out with you.)

I believe that the decrease from 11 to 8 to account for about 30 percent of the strontium 90 becoming unavailable to the plants is reasonable.

The detailed calculation is appended.

Very sincerely yours,

W. F. LIBBY.

APPENDIX

Amount of Sr^{90} in the stratosphere after t years is, y.

Amount of Sr^{90} on the ground is, x.

Since the rate of accumulation in the stratosphere, dy/dt, is the deposition rate, S, less the fallout rate, $y/10$, and the decay rate, $y/40$,

$$dy/dt = S - y(1/10 + 1/40), \text{ or}$$

$$y = S\left(\frac{1 - e^{-t(1/10+1/40)}}{1/10 + 1/40}\right).$$

Similarly,

$$x = 40(S+T)(1 - e^{-t/40}) - y.$$

In these equations, T is the rate of tropospheric fallout. In order that at five years the ratio of the amount deposited (30 mc/mi^2 in the northeastern United States) to that in the stratosphere (12 mc/mi^2) be given correctly the ratio of S to T must be 4 to 7. With this the ultimate amount on the ground relative to that deposited at 5 years is 11.

UNIVERSITY OF ROCHESTER,
Rochester, N. Y., June 12, 1957.

Dr. W. F. LIBBY,
Commissioner, United States Atomic Energy Commission,
Constitution Avenue, Washington, D. C.

DEAR DR. LIBBY: Thank you for the copy of your letter of June 7, 1957. My associates and I have gone over the equations and find them to be mathematically correct. I believe I understand the origin of the difference between your number 11 and Hollister's number 32. Hollister, perhaps, will agree. I believe he was using Campbell's equation which is derived for stratospheric fallout only. The number 32 applies to ground levels due to stratospheric fallout only not to the total on the ground at $T=5$ years.

I think, though probably obvious to most, it should be pointed out that predictions of equilibrium from these equations are based on two assumptions: first, that the overall test rate will be constant and second, that the proportion of trapospheric to stratospheric fallout will remain the same.

Is it correct to say you now predict ultimate ground levels of between 432 to 600 sunshine units (depending on the importance of your postulate of ageing) if testing continues? If so, and if you agree with me that the population average should not exceed 40 S. U. (MPC=100 S. U.), then discrimination factors of 10 to 15 would be required for the present test rate to be permissible indefinitely. A figure of 80 percent of the "present test rate" would seem to me to be a "permissible test rate" compatible with your figures.

I would appreciate your comment since my estimate, though only differing by a factor of two (4.4 Megations), presumed ideal conditions of worldwide fallout.

Sincerely yours,

WILLIAM F. NEUMAN,
Associate Professor, Pharmacology and Biochemistry.

cc: Mr. Hal Hollister.

UNITED STATES ATOMIC ENERGY COMMISSION,
Washington, D. C., June 24, 1957.

Prof. WILLIAM F. NEUMAN,
School of Medicine and Dentistry,
The University of Rochester, Rochester 20, N. Y.

DEAR PROFESSOR NEUMAN: You are correct in finding that my equations predict ultimate ground levels of between 400 and 600 S. U. (depending on the importance of the aging factor) if testing were to continue indefinitely. A population average concentration of 40 S. U. then would correspond to discrimination factors of 10 to 15. Of course, these discrimination factors would include the ploughing factor which I mentioned the other day. Of course, one normally does not plough range land but, over a period of many years, I believe that Sr^{90} would gradually mix itself with deeper lying layers of soil. This factor should be included in the overall discrimination factor. I believe in your discussion of discrimination factors, you begin with vegetation and then consider the discrimination factor between vegetation and human bone. Do you not?

I hope we can meet again before long to discuss these problems.

Sincerely yours,

W. F. LIBBY.

cc: Mr. Hal Hollister.

UNITED STATES DEPARTMENT OF COMMERCE,
WEATHER BUREAU,
June 27, 1957.

Hon. CHET HOLIFIELD,
United States House of Representatives,
Washington, D. C.

DEAR MR. HOLIFIELD: In response to a request by Senator Anderson for information on the morning of June 6, 1957, I am enclosing the following remarks for the clarification of my testimony.

The strontium^{90} bone content at equilibrium (i. e., if tests are continued at the current rate of 10 megatons of fission products per year and after a very long time) can be obtained from the predicted fallout. In my testimony, I estimated

that the fallout would lie between the 350 to 850 millicuries per square mile. These numbers were converted to strontium^{90} bone content in sunshine units by Dr. Langham to provide a range from 70 to 170 sunshine units. They were compared with numbers also estimated by Dr. Langham for other investigators which lay between 5 and 40 sunshine units. There are four reasons why the human bone content of strontium^{90} based on my fallout estimates were higher than the others:

1. The conversion from millicuries per square mile to sunshine units in average soil was a factor of 2. It should have been 1.8.
2. The conversion of fallout in 1956 to fallout after equilibrium has been established was performed by Dr. Langham through a multiplication of 8 for the numbers assigned to the other analysts. The factor of 8 should have been somewhat higher. The 8 was used to compensate for the aging of strontium^{90} in the soil and the effects of plowing.
3. Many of the other investigators (and perhaps all) use discrimination factors in converting from soil to human bone which are greater than 10, that is, gave lower bone content from the same soil. The value of 10 was used by Dr. Langham in converting from soil to human bone for my numbers.
4. The upper bound quoted in my fallout figures is offered only as an extreme. Other investigators, except for Mr. Eisenbud, did not offer upper bounds.

A review of the situation during the past several weeks has indicated two features. First, the use of comparable conversion factors yields final answers for my fallout estimates which are essentially the same as those for Dr. Libby and Mr. Eisenbud. I was unable to check the numbers assigned to the other investigators. The reason for the similarity is evident. Both Dr. Libby and Mr. Eisenbud have made their predictions for future fallout based on fallout in northeast United States where there is nonuniformity of fallout. That is, the fallout in northeast United States is over 3 times the worldwide average in 1956. Second, I am unable to provide a set of predictions for human bone content of strontium^{90} at equilibrium if the test rate is continued at about 10 megatons per year, because I cannot choose between the various conversion factors which have been quoted during the fallout hearings. For example, the discrimination factor from soil to bone has been variously estimated as lying between 8 and 80.

I feel that the uncertainty in nonuniform fallout over the globe represents only one of many of the uncertainties in the final prediction of human bone content. Other factors are: The importance of transportation of food from one area to another, the variability of calcium content of soil, the relative importance of plowing and aging, variability among people, etc. It would seem to me that some kind of a statistical treatment is necessary. The use of extremes of each of these factors does not obtain a realistic upper bound for the likely bone content of human beings.

In conclusion, I should like to repeat that the estimate of 850 millicuries per square mile, the equilibrium fallout if test rate is continued at 10 megatons per year, is offered only to show that even with the uncertainty in meteorological factors, a reasonable upper bound can be provided. It is a guess which we hope will be refined in the next few months.

Sincerely yours,

LESTER MACHTA,
Chief, Special Projects Section, Office of Meteorological Research.

UNIVERSITY OF ROCHESTER,
Rochester, N. Y., July 2, 1957.

Dr. W. F. LIBBY,
Commissioner, United States Atomic Energy Commission,
Constitution Avenue, Washington, D. C.

DEAR DR. LIBBY: This letter has as its purpose the possible resolution of some of the differences in the various calculations presented at the congressional hearings. I understand from Dr. Potts that an attempt is being made to recall at least some of the participants of the roundtable discussion for this same purpose.

Unless I am mistaken, Machta and I derived independent approaches to the problem. Eisenbud, Kulp, Langham, and you gave independent calculations of bone levels, but all of these were based on your predictions of ground levels at full equilibrium.

Machta and I were, I believe, in substantial agreement provided we used similar discrimination factors. All the values based on your ground-level estimates, however, were somewhat lower and the question is "Why?"

I have, to my own satisfaction, answered this question. Using your equations as given in your letter of June 7 to Mr. Hollister, I have substituted in the same values with one exception: I used $t=3$ years for stratospheric fallout and $t=5$ years for tropospheric fallout. After all, there was inappreciable stratospheric fallout prior to test Castle. On this basis, $T=6$ mc/mi^2/yr. and $S=4.8$ mc/mi^2/yr. and x, at $t=\infty$, is 396 mc/mi^2, or about 710 sunshine units. Using as I did an overall discrimination of 8, equilibrium bone values would then average 89 sunshine units which is 2.2 times too high (MPC=100, av.=40).

The so-called present "level of testing," said to be 10 megatons per year, should thus be reduced, on these assumptions, to about 4.5 megatons. This agrees with my testimony giving the figure of 4.4 megatons per year, whether we base our predictions on rate equations or on a presumed equilibrium. This is true, of course, only if you find my application of your equations to be acceptable. If you do agree, then, the equilibrium values of Eisenbud, Kulp, and Langham should be multiplied by 13.2/8, before they are corrected for "aging," "plowing," etc.

You asked (June 24) whether the discrimination factor of 8 covered only the area from vegetation to bone. In my testimony, I specified this to be the case. I said further that other more or less favorable factors (fallout to vegetation) may soon be discovered, but the experimental data were, in my opinion, as yet too inadequate to permit the assignment of numerical values. I then asked for further discussion of this point. In summary, then, I used 8 as an overall factor, but it admittedly is based only on the transfer from vegetation to bone.

Your comments will be appreciated.

Very truly yours,

WILLIAM F. NEUMAN,
Associate Professor, Pharmacology and Biochemistry.

cc: Mr. Hal Hollister.

UNIVERSITY OF PENNSYLVANIA,
Philadelphia, Pa., July 23, 1957.

Reference: EM: FN

Division of Biology and Medicine,
Dr. C. L. DUNHAM,
Atomic Energy Commission, Washington, D. C.

DEAR MR. DUNHAM: Thank you for your letter regarding the July 29 symposium. It is very fine that the unfinished business of the congressional hearings is being pursued by the Commission.

In addition to the problem of prediction of future levels of strontium #90, I wonder whether the plans for the meeting include discussion of the "permissible level" of strontium #90 in humans. This subject also was not fully covered in the hearings. Because of its great importance, discussion of this subject should be carried out to a more satisfactory stopping point.

Sincerely yours,

WALTER SELOVE.

cc: H. Hollister.

POTENTIAL HAZARD OF WORLD-WIDE Sr^{90} FALLOUT FROM WEAPONS TESTING

By Wright H. Langham and Ernest C. Anderson

Los Alamos Scientific Laboratory, University of California,
Los Alamos, New Mexico

1. INTRODUCTION

During the past year public attention has been increasingly focused on the potential hazard to the general population of wide-spread, low-level, radioactive fallout from nuclear weapons testing (1–5). Although a number of radioisotopes are present in the fission mixture, Sr^{90} is the major concern. It is believed to be the most important radionuclide because of its similarity to calcium, long physical and biological half-life, and high relative fission yield. These factors

lead to high incorporation in the biosphere and a long residence time in bone. General contamination will result in the bones of the population eventually reaching an equilibrium state with Sr^{90} in the biosphere. The predominance of Sr^{90} over other long-lived radioelements as a potential hazard can be deduced in part from data in Table I, which show that it is the only isotope that combines high fission yield, long half-life, high absorption rate and a low maximum permissible level (MPL). These data suggest Cs^{137} by at least an order of magnitude fission product, and its presence in people and foodstuffs has been reported (6, 7). However, for reasons not discussed here, its potential hazard to the population is believed to be less than that of Sr^{90} by at least an order of magnitude (8).

TABLE I.—*Radioelements of importance to long-term fallout problem*

Radioelement	Type radiation	Fission abundance [1]	Radiological half-life	Absorption on ingestion	MPL (μc)
		Percent		*Percent*	
Pu^{239}	α	----------	24,000 years	3×10^{-3}	0.04
Cs^{137}	β,μ	6.2	28 years	100	98
Sr^{90}-Y^{90}	β	5.1	28 years	35	1
Pm^{147}	β	2.6	3.7 years	3×10^{-2}	25
Ru-Rh^{106}	β,μ	.5	1.0 year	5×10^{-2}	4
Ce-Pr^{144}	β,μ	5.3	275 days	3×10^{-2}	1

[1] Slow neutron fission of U^{235}.

Appraisal of the potential hazard from world-wide fallout of Sr^{90} requires consideration of the extent and rate of fallout, its method of incorporation into the biosphere and the human body, present and predicted levels in soils and people, the basis of presently accepted maximum permissible body levels, and the biological significance of present and future body levels in terms of the megatons of fission weapons detonated. Information on all of these factors is somewhat inadequate at the present time. This paper is an attempt to present a general summary of the present thinking with regard to the above factors.

2. GENERAL WORLD-WIDE FALLOUT FROM BOMB TESTING OPERATIONS

Based on measurements of world-wide fallout, Libby (2, 3) proposed a mechanism by which atomic debris is disseminated throughout the world. This theory leads to three kinds of fallout, which are illustrated in Fig. 1. First is local fallout which is deposited in the immediate environs of the explosion during the first few hours. This debris consists of the large particles from the fireball and includes residues from the soil and structures which are swept into the cloud in wholly or partially vaporized state. The fraction of the total radioactivity which falls out locally depends very much on those conditions of firing which govern the amount of soil and extraneous debris incorporated in the fireball.

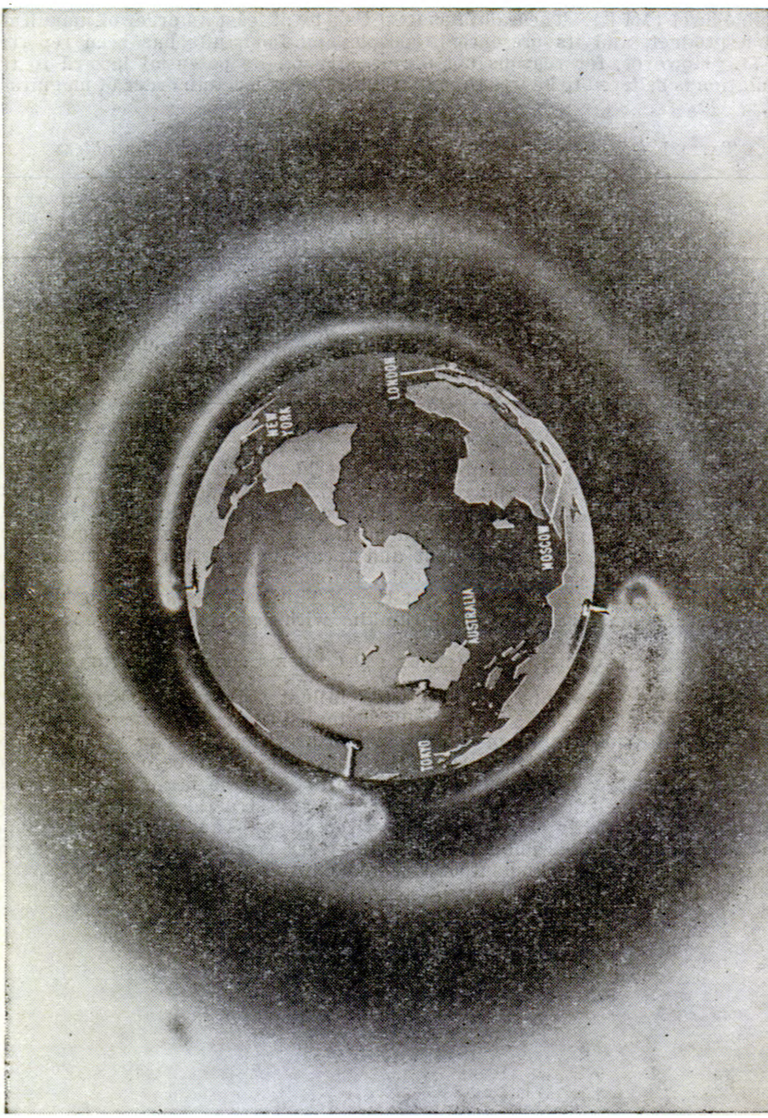

FIG. 1.—Types of radioactive fallout from weapons tests (local, tropospheric and stratospheric).

The second type (stropospheric fallout) consists of that material injected into the atmosphere below the tropopause which is not coarse enough to fall out locally. This debris is sufficiently fine that it travels great distances, circling the earth in the general latitude of the explosion, until removed from the atmosphere by rain, fog, contact with vegetation, and other meteorological and/or physical factors. The average tropospheric fallout time is estimated as 20 to 30 days. The fraction of the fallout which is in this category depends mainly on the size of the explosion and the conditions of firing. If the explosion exceeds a certain minimum size (about one megaton (MT)), the fireball will have enough energy to penetrate the tropopause carrying fission products into the stratosphere. Smaller detonations leave in the troposphere all debris not deposited locally. The fraction of the fission products from a large weapon that remains in the tropopause depends on the size of the explosion, conditions o' firing, and meteorological factors.

The third type (stratospheric fallout) is composed of fission products that are carried above the tropopause and can result only from large weapons. Libby (3) has postulated that the activity is mixed rapidly throughout the stratosphere and falls back uniformly into the troposphere (see footnote page 10), where it is deposited over the earth's surface in relation to meteorological conditions. The over-all mean deposition time is estimated at from 6 to 10 years.

The above mechanism leads to a general distribution pattern of radioactivity over the surface of the earth as shown in Fig. 2. Libby's estimates (3) of Sr^{90} levels in the fall of 1956 suggest 22 mc/mi² for the midwestern section of the United States, 15 to 17 mc/mi² for similar latitudes elsewhere in the world,[1] and 3 to 4 mc/mi² for the rest of the world. The higher value for the upper midwestern United States is attributed to greater local and tropospheric fallout because of the proximity of our own continental test site.[2] The 15 to 17 mc/mi² deposited between about 60° N and 10° N latitude is due to tropospheric fallout from all shots of less than 1 MT conducted in the northern hemisphere plus stratospheric fallout from all weapons greater than 1 MT. Actually, this general picture is greatly oversimplified. Once fission products are suspended in the troposphere (either directly by the detonation or by air exchange, regardless of mechanism, between the troposphere and the stratosphere), meteorological conditions play a major role in their deposition. Libby (3) has stressed the importance of rainfall, fog, and mist. Within any major fallout area one might expect to find fluctuations in the level of surface deposition which correlate with local meteorological conditions. Higher deposition in a local area does not correlate necessarily with total precipitation but rather with frequency of rainfall.

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[1] The latitude position and width of the north temperate tropospheric fallout belt is variable and hard to estimate. Earlier Libby mentioned 60°N–10°S (2); later he mentioned 50°N–10°N (3); and most recently he referred to 60°N–10°N (10). The area between 60°N–10°N latitude agrees better with soil data and it is assumed by the authors that the 15 to 17 mc/mi² applies to this area.

[2] Machta (47), in testimony given during the Open Hearings of the Joint Committee on Atomic Energy, postulated that stratospheric mixing is slow and that stratospheric distribution of fission products is still nonuniform. He feels that a major portion of the nuclear debris is still in the northern portions of the northern hemisphere, rather than uniformly spread over the entire globe or even uniformly dispersed in the northern hemisphere itself. He feels also that stratospheric movement of the fission products is largely by direct transport from west to east in the general latitude of the point of injection with very slow vertical mixing. Slow polewards circulation of stratospheric air from equatorial regions provides some mixing towards the poles. The higher concentrations of fallout in the temperate latitudes was explained on the basis of air exchange between the troposphere and stratosphere through the break in the tropopause frequently found in the vicinity of the jet stream. A large part of the higher concentration of Sr^{90} found in the northern part of the United States may result from such uneven stratospheric leakage instead of the proximity of the Nevada Test Site. In either case, the general distribution of Sr^{90} is relatively the same qualitatively. Quantitatively, Machta's model predicts a greater degree of nonuniformity of fallout over the earth with higher concentrations in the temperate latitudes.

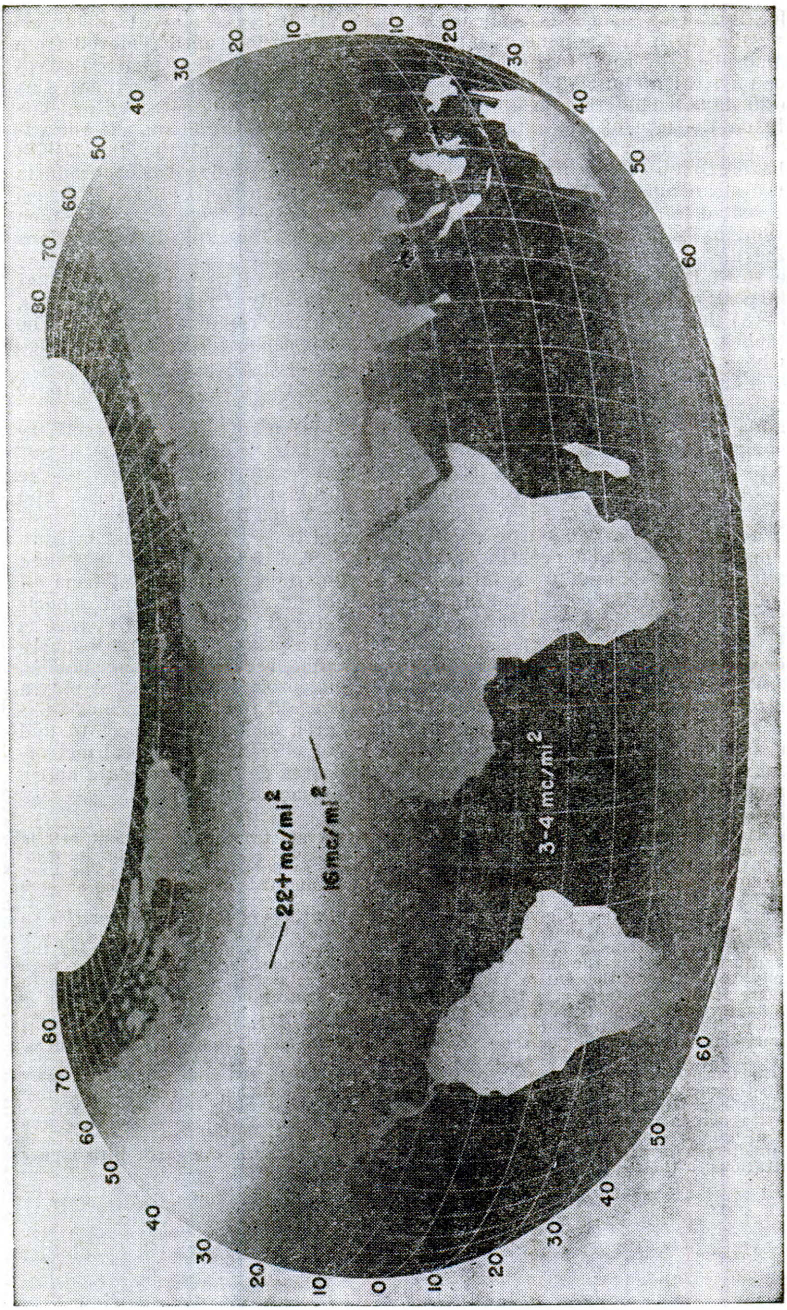

FIGURE 2.—General levels of world-wide fallout deposition.

3. PRESENT AND PREDICTED MAXIMUM LEVELS OF SURFACE DEPOSITION

Libby (3) estimated that the stratospheric reservoir (in the fall of 1956) contained the products of about 24 megatons of fission. One megaton of fission results in the formation of enough Sr^{90} to give a surface deposition of 0.5 mc/mi^2 if uniformly distributed over the entire surface of the earth. If all material presently in the stratospheric reservoir were deposited instantaneously and uniformly over the earth, present values would be increased by 12 mc/mi^2, and the maximum surface deposition of Sr^{90} would result. Maximum deposition, however, will not occur because of the relatively long average stratospheric storage time (6 to 10 years), which will allow some of the strontium to decay before deposition. Figure 3 shows, however, that the predicted maximum level is not highly dependent on the mean time of fallout. Although British investigators (9) appear to favor a fallout half-time of about 5 years, Libby (2) has chosen to use a value of 7, which corresponds to a mean time of about 10 years. With a mean time of 10 years, the maximum predicted level of Sr^{90} surface contamination from fission products alreadmy present should occur in about 1975. Table II shows the estimtaed present levels (October 1956 (3)) and the maximum predicted levels that might be expected at that time.

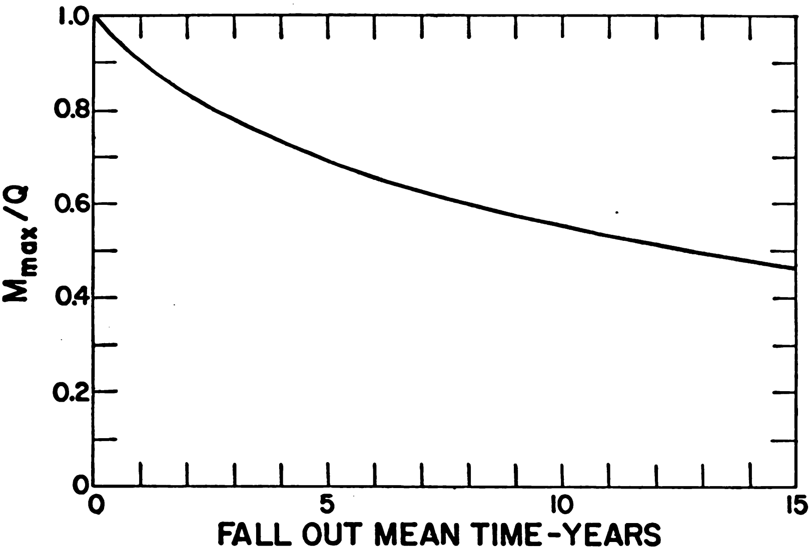

FIGURE 3. Dependence of maximum level of deposition on mean time of fallout.

TABLE II.—*Present and predicted maximum levels of Sr^{90} surface deposition* [1]

Area	Level October 1956 (mc/mi^2)	Maximum level 1975 (mc/mi^2)
Midwestern United States	22	29
Between 60° N. and 10° N. latitude	16	23
Rest of world	3.6	10
World average	[2] 8	[2] 15

[1] Assuming products of 24 MT fission in the stratosphere Jan. 1, 1957, and a fallout mean time of 10 years.
[2] Calculated by weighting for respective areas, taking 35 percent of earth's surface as lying between 60° N. and 10° N. latitude.

As stated previously, these figures assume uniform world-wide distribution of the material now in the stratospheric reservoir and no more weapon tests. Under these conditions, the area in the midwestern United States would be expected to reach a level of about 29 mc/mi^2. The area between 60°N and 10°N latitude may reach about 23 mc/mi^2, and the rest of the world may reach a

level of about 10 mc/mi². These values are general levels only, assuming uniform distribution within the respective areas. Local meteorological conditions will produce nonuniformities within these general regions. Recent data (10) suggest that some areas of the United States (South Dakota, Iowa, Michigan, New York) already may have deposition levels of about 29 mc/mi² (January 1957).

4. INCORPORATION OF SR^{90} INTO THE BIOSPHERE

When Sr^{90} falls upon the earth's surface, it is taken into plants through the root system in relation to the available calcium in the soil. That which settles directly on vegetation may remain as surface contamination, or a part of it may enter the plant through foliate absorption. When plants are eaten by animals, Sr^{90} deposited directly on the surface or incorporated in the plant (by foliate absorption or from the soil) is absorbed by the animal along with calcium. When plant and animal products (e. g., milk) are eaten by man, the Sr^{90} they contain becomes incorporated with his body calcium and deposits predominantly in the bone.

It is reasonable to assume that strontium may be discriminated against with respect to calcium in passing along the ecological chain. For example, the Sr^{90}/Ca ratio in the bones of people may be expected to be lower than the Sr^{90}/Ca ratio in the soil, which is the beginning of man's food chain.

Information regarding Sr^{90} in relation to man and his environment may be obtained from data on stable strontium. Turekian and Kulp have reported stable strontium to calcium ratios in human bone (11) and in sedimentary and igneous rocks (12). If it is assumed that the average stable strontium to available calcium ratio in the world's soils is essentially equal to that of the rocks from which they are formed, the over-all discrimination ratio (OR) against strontium over calcium in passing along the ecological chain from soils to bone is about 0.1, $\left[\text{i.e., } \frac{(Sr^s/Ca)_{bone}}{(Sr^s/Ca)_{soil}} = \sim 0.1\right]$.

Data on stable strontium content of human bone ash were reported by Hodges *et al* (13) and show conclusively that, under equilibrium conditions, stable strontium is equally distributed throughout the skeleton (Table III). Their results were confirmed by others (11, 14), and leave no doubt but that man's bones will eventually come into equilibrium with the Sr^{90} contamination in his environment.

TABLE III.—*Stable strontium content of human bones*

Sample	Sr in bone ash (percent)			
	Parietal	Vertebra	Rib	Femur
Fetal [1]	0.016	0.016	0.017	0.017
All ages [2]	0.023	0.022	0.022	0.022
1914 cadavers	0.027	------------	0.027	0.025

[1] Fetal bones showed range of 0.015 to 0.019 percent.
[2] All-age group showed no significant increase with age when analyses were compared in 5 age groups.

Alexander, Nasbaum and MacDonald (15) obtained excellent data on the discrimination against strontium over calcium in going from plants to milk. They compared the stable strontium to calcium ratio of cows' milk with that of the feed the cows consumed and found that $\frac{(Sr^s/Ca)\text{ milk}}{(Sr^s/Ca)\text{ feed}} = 0.13$. These authors (16) also studied the relative uptake of stable strontium and calcium from the diet by a variety of rodents, including rabbits and kangaroo rats from the Nevada desert. They found an average $\frac{(Sr^s/Ca)\text{ bone}}{(Sr^s/Ca)\text{ food}} = 0.24$. They suggested that this retention ratio might be used to predict the skeletal uptake of Sr^{90} by humans, through continued consumption of contaminated food.

Attempts are being made also to determine the over-all Sr^{90} discrimination ratio in going from soils to human bone by radioactive tracer studies of the discrimination factors (DF) that occur at the various steps along the ecological chain.

The discrimination factor most difficult to establish is the one from soils to plants (DF_1). It is dependent, among other things, on type of soil, available soil calcium, type of plant and perhaps on rainfall, all of which may vary greatly with geographic location. Menzel (17) obtained a soil-to-plant discrimination factor $\frac{(Sr^{90}/Ca)\,plant}{(Sr^{90}/Ca)\,soil}=0.7$, for four widely different soil types using both radioactive and stable strontium. Larson (18) and Bowen and Dymond (19) have obtained comparable values.

Comar (20) has determined the discrimination factor from plants to milk (DF_2) using Sr^{85} and Ca^{45} and found that $\frac{(Sr/Ca)\,milk}{(Sr/Ca)\,plants}=0.14$, which is in good agreement with the value of 0.13 obtained from stable strontium data (16). Comar also studied the discrimination factor (DF_3) in going from milk to human bone following single and multiple feeding and found the radio $\frac{(Sr/Ca)\,bone}{(Sr/Ca)\,milk}=0.5$.

Experiments by Laszlo (21) on the discrimination factor (DF_4) from plants directly to human bone gave a value for $\frac{(Sr/Ca)\,bone}{(Sr/Ca)\,plant}=0.25$, which agrees with radioisotope studies in rats conducted by Comar (22) and with stable strontium data on rodents (16).

The over-all ratio ($OR_{bone-soil}$) in going from soil to human bone via the diet may be estimated from the various discrimination factors and the fraction of dietary calcium derived from diary products and directly from other sources. The Department of Agriculture, on the basis of United States retail sales, has estimated that 65 percent of the dietary calcium for all ages comes from dairy products and 35 percent from other sources. On this basis, $OR=(0.65\times DF_1\times DF_2\times DF_3)+(0.35\times DF_1\times DF_4)=(0.65\times 0.7\times 0.13\times 0.5)+(0.35\times 0.7\times 0.25=0.09$. The above value is in agreement with the crude estimate made from stable strontium considerations, and represents a reasonable general average of the values derived at a recent Washington Conference on "Deposition and Retention of Ingested Sr^{90} in the Skeleton" (23). This Committee reported also that the ($OR_{bone-diet}$) for a six-month-old child might vary from 0.041 to 0.13, which would give an average value for ($OR_{bone-soil}$) of 0.06. The above information on the over-all discrimination factor against strontium during passage up the ecological chain from soils to human bone via the diet is summarized in Figure. 4 and may be used to estimate the average present and future maximum Sr^{90} levels in bone as a result of weapons tests to date. In so doing, however, it must be re-emphasized that these values are for *ecological* discrimination and apply only to discrimination against Sr^{90} in progressing up the life cycle. An ecological discrimination factor automatically assumes that the calcium and strontium are uniformly mixed in soil and are equally available to the depth of the plant feeding zone. No allowance is made for direct foliar absorption of Sr^{90}, for its dilution with a greater reservoir of available soil calcium through plowing, or for the possibility that it may become less available with time through soil binding and leaching. Much of the disagreement between the over-all discrimination factors reported by Libby (10) and those given above is more apparent than real, because of failure to make clear distinction between over-all discrimination and over-all *ecological* discrimination.

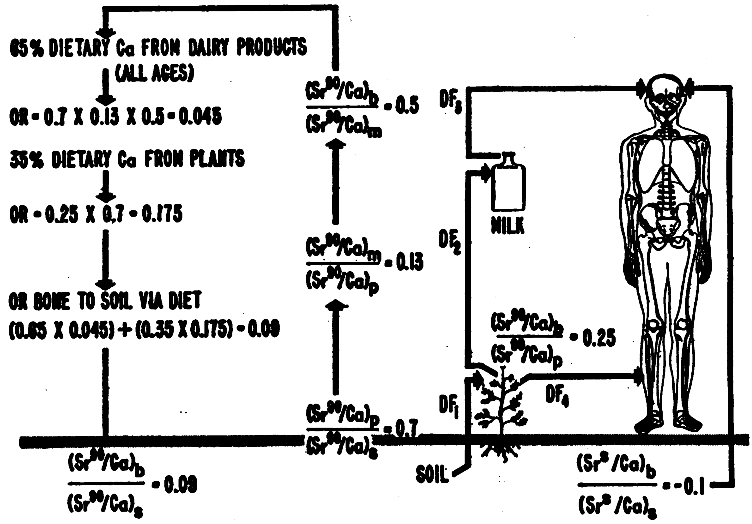

FIGURE 4. Discrimination against strontium with respect to calcium in passing up the food cycle from soil to man.

5. PREDICTED PRESENT AND FUTURE Sr^{90} AVERAGE MAXIMUM LEVELS IN BONE

Sr^{90} content of the bones of young children at the time of maximum biospheric contamination is assumed to be of major concern because children are believed to be more sensitive to radiation and their entire skeletons will be formed under steady state conditions with the Sr^{90} contamination of the environment.

From the present and predicted maximum Sr^{90} surface deposition levels (Table II) and the Sr^{90} soil-to-bone discrimination ratios derived in the previous section, present and future average maximum Sr^{90} levels in bone can be predicted.

Assuming an average of 20 g available Ca/ft² of soil to a depth of 2½ in. (2), 1 mc of Sr^{90}/mi² is equivalent to 1.8 $\mu\mu$c/g available calcium. Multiplication of the Sr^{90} surface deposition levels in Table II by 1.8 gives the specific activity of the available soil calcium. The specific activity of the soil calcium times the Sr^{90} discrimination ratio of 0.06 (for six-month-old child) should give the average maximum specific activity (in $\mu\mu$c/g) of the bone calcium of young children under steady state conditions. Multiplication of the specific activity of the available soil calcium by the Sr^{90} discrimination ratio of 0.09 should give the average maximum specific activity of calcium laid down in the adult skeleton through exchange and bone remodeling during the period of environmental contamination. Present and future average maximum Sr^{90} levels in the skeletal calcium of young children at equilibrium and in newly formed adult bone calculated in the above manner are given in Table IV. As the diet of children changes, they may be expected to approach a Sr^{90} equilibrium level comparable to the level predicted for newly formed adult bone.

TABLE IV.—*Predicted present and future average maximum Sr^{90} levels in bone*

Area	Predicted maximum, fall 1956		Predicted maximum, 1975	
	Children at equilibrium [1] ($\mu\mu$c/g Ca)	New adult bone [2] ($\mu\mu$c/g Ca)	Children at equilibrium [1] ($\mu\mu$c/g Ca)	New adult bone [2] ($\mu\mu$c/g Ca)
Midwest United States	2.4	3.6	3.1	4.7
60° N. to 10° N. latitude	1.7	2.6	2.5	3.7
Rest of world	0.4	0.6	1.1	1.6
World average	0.9	1.3	1.6	2.4

[1] Assuming steady state conditions, 20 g available soil Ca/ft² of soil to depth of 2½ in. and ($OR_{bone-soil}$) = 0.06.

[2] Specific activity of calcium of new bone formed by exchange plus skeletal remodeling, assuming $OR_{bone-soil}$) = 0.09.

These data suggest a present average maximum equilibrium level of 1.7 $\mu\mu c$/g Ca in bones of young children and 2.6 $\mu\mu c$/g Ca in newly formed adult bone in the world population belt between 60°N and 10°N latitude. Calculation of average maximum equilibrium levels in about 1975 from the predicted soil levels gives 2.5 and 3.7 $\mu\mu c$/g Ca for young children and adult bone, respectively, assuming no more detonations after Operation Redwing in the summer of 1956.

The data in Table IV are subject to the uncertainties in predicted present maximum levels of surface deposition and to the uncertainties involved in the derivations of the bone-to-soil discrimination ratios. The greatest uncertainty in the values is probably due to their dependence on available soil calcium with which the Sr^{90} is mixed.[8] Available soil calcium may vary within the United States from about 1 to 100 g/ft^2 to a depth of 2½ in. The relative Sr^{90} uptake would be higher in areas with abnormally low available soil calcium. The available calcium with which the Sr^{90} is actually mixed is dependent also on the average depth of the feeding zone of all the various types of plants responsible for the introduction of calcium into man's food chain.

Derivations of the discrimination ratios assume also that average infant and "all-age" diets of the world population are comparable to those of the United States and assume Sr^{90} is in equilibrium in the soil and make no allowance for the fraction entering the food chain through direct fallout on vegetation.

Kulp *et al.* (24) recently reported Sr^{90} analyses of 484 bone samples from persons of all ages collected at 17 stations in a worldwide sampling network. Most of these samples came from the area between 60°N and 10°N latitude, and the majority were collected during 1955 and the spring of 1956. The average Sr^{90} value for all ages was 0.12 $\mu\mu c$/g Ca. A few results were ten times the average and a definite age effect was observed. The bones of young children showed Sr^{90} values three to four times the average, which was attributed to the greater portion of active bone in children. The average Sr^{90} content of 64 bone samples in the 0- to 4-year-age group was reported as 0.31 $\mu\mu c$/g Ca, after dividing results from rib samples by 2 and those from vertebra by 4 to obtain an average for the total skeleton. This adjustment to obtain a skeletal average was predicated on Sr^{85} distribution studies in adult cancer patients given a single injection. The data in Table III suggest that adjustment of rib and vertebra results to obtain a skeletal average may not be entirely valid for children, in which a major portion of the skeletal calcium was laid down during the period of Sr^{90} environmental contamination. The adjustment, however, in the case of adult bone samples (in which the majority of the Sr^{90} was laid down during a contamination period relatively short compared to the age of the individual) might be justified. These adjusted values, in the case of adult bone, might be a measure of new bone formation by exchange plus skeletal remodeling during the period of contamination. On the basis of these data, it should be possible to postulate an internally consistent model for Sr^{90} deposition in the skeleton of persons of all ages, taking into consideration the rate of increase of environmental contamination and the rate of calcium deposition by skeletal growth and bone remodeling plus exchange as a function of age. Figure 5, derived from data given by Mitchell (25), shows the total deposition of skeletal calcium in males as a function of age and their rate of skeletal accretion in per cent of calcium deposited per year. This figure also shows the rate of increase in integrated Sr^{90} fallout in the Chicago milk-shed area from 1953 through 1957 and suggests a doubling time of about one year (3).

Using the rate of skeletal accretion (Table V, Fig. 5), increase in integrated fallout and Kulp's average Sr^{90} bone values for the various age groups, it should be possible to calculate the maximum average equilibrium level of Sr^{90} as of the fall of 1955. The specific activity of the calcium in the bones of children should depend on the fraction of the skeletal calcium laid down by all mechanisms during the period of environmental contamination, and the specific activity of the bone calcium of adults should depend on the per cent of skeletal calcium equilibrated during the same period by new bone formation through bone remodeling plus exchange.

[8] The assumption of 2½ in. as the effective depth for Sr^{90} is subject to great uncertainty. Plowing will result in mixing to a depth of about 6 in., and no allowance is made for the possibility that some of the Sr^{90} may become unavailable to plants (as is some of the soil calcium) and over long periods of time some may be removed from the soil by leaching.

TABLE V.—*Yearly accretion of skeletal calcium in males*

Age (years)	Calcium in total skeleton [1] (g)	Increase per year (g)	Fraction of equilibrium Sr^{90}/Ca	Age (years)	Calcium in total skeleton [1] (g)	Increase per year (g)	Fraction of equilibrium Sr^{90}/Ca
0	28		[2] 0.50	13	624	85	0.23
1	100	72	0.79	14	715	91	0.22
2	147	47	0.59	15	806	91	0.21
3	179	32	0.42	16	894	88	0.19
4	201	22	0.29	17	973	79	0.16
5	219	18	0.20	18	1035	62	0.13
6	239	20	0.16	19	1073	38	0.09
7	264	26	0.16	20	1078	5	0.045
8	297	33	0.18	21	1078	0	0.019
9	341	44	0.20	22	1078	0	0.006
10	396	55	0.22	23	1078	0	0.001
11	463	67	0.23	24	1078	0	0.000
12	539	73	0.24				

[1] From reference (25).
[2] Assuming 50 percent fetal protection (26).

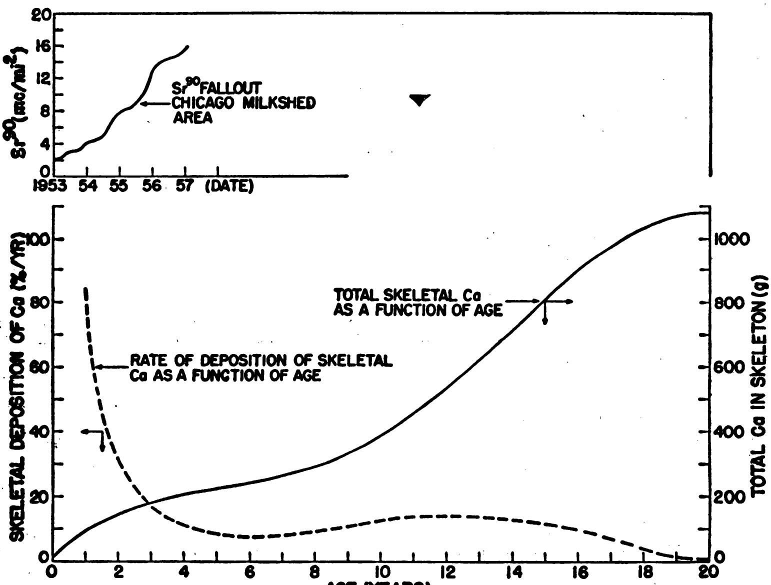

FIGURE 5. Rate of accretion of skeletal calcium in relation to rate of environmental contamination.

It is assumed that each yearly increment of skeletal growth will contain Sr^{90} at a concentration corresponding to the Sr^{90} build-up in the biosphere for that year. For a first approximation, the skeleton will be regarded as a unit and the Sr^{90} burden averaged over the entire skeleton. As better data are available, it will be profiable to consider the individual bones separately, both in terms of their Sr^{90} burden and their radiosensitivity.

Calculated values for the apparent fraction of equilibrium Sr^{90}/Ca ratio as a function of age, based on skeletal growth rate alone and a yearly doubling time of the Sr^{90} level, are given in the last column of Table V and are shown by the solid curve of Fig. 6. The method of calculation is best explained by an example. For an eight-year-old skeleton (Table V) in 1956 the last 33 g of calcium contain an equilibrium concentration C of strontium, and thus a total amount of strontium equal to 33 times C. The previous year's deposition of 26 g would have been formed with a concentration C/2 of strontium and would con-

tribute 13 C. The fifth and fourth years' growth would incorporate 20 C/4 and 18 C/8 units of strontium, respectively. The total strontium in the skeleton at age eight is, therefore, (33+13+5+2.5) C, or 53 C. (A four-year cut-off is used since large-scale testing began in 1952. Actually, the series is converging so rapidly that the cut-off has little effect.) If the entire skeleton had been in equilibrium with the Sr^{90} level of the environment in the eighth year, it would have contained 297 C units of Sr^{90}. The fraction of equilibrium is, therefore, 53 C/297 C, or 0.18.

The points in Fig. 6 represent Kulp's unadjusted values for subjects under 20 years of age and his adjusted data for adults normalized to the 0- to 4-year age group as representing 59 per cent of equilibrium Sr^{90} concentration.

At age 24 (4 years beyond the age at which skeletal growth stops) these data show that about 10 per cent of the skeletal calcium was involved in bone remodeling plus exchange during the period of environmental contamination. The data further suggest that the amount of calcium involved decreases with age, which is in keeping qualitatively with classic concepts of bone physiology. If a similar fraction of the skeletal calcium of growing subjects is involved in exchange plus remodeling, then the Sr^{90} levels in children would be proportionally higher than the curve based on skeletal calcium accretion alone. This indeed appears to be the case and indicates that the major factors have been considered in constructing the model.

If the fraction of remodeling and exchange for children's bones is similar to that observed for adults, then the accretion curve below age 20 should be raised proportionately, to give the over-all apparent fraction of equilibrium Sr^{90}/Ca ratio as a function of age (dashed line, Fig. 6). This curve permits the use of adequate bone data from any age group to predict the average maximum equilibrium Sr^{90} bone level and indicates an average maximum equilibrium level of 0.9 $\mu\mu c/g$ Ca at the end of 1955.

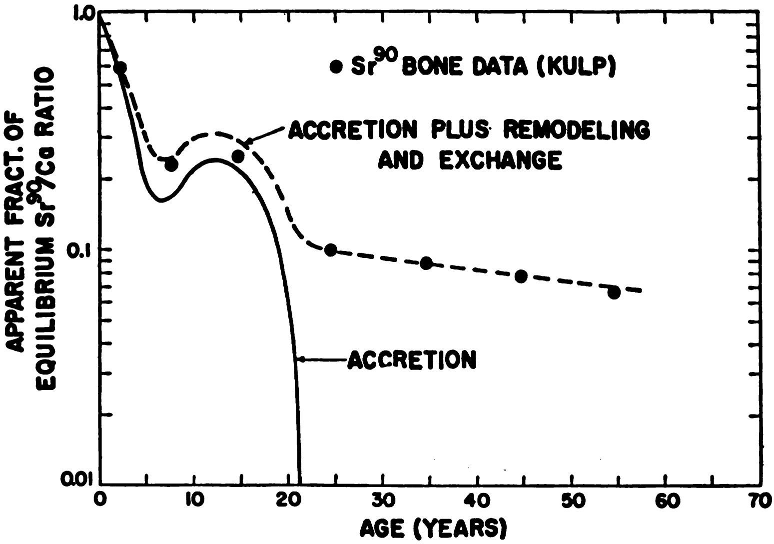

FIGURE 6. Apparent fraction of equilibrium Sr^{90}/Ca ratio as a function of age.

Sr^{90} content of skeletons of stillborns (3) during 1955 averaged about 0.5 $\mu\mu c/g$ Ca, which gives an average maximum equilibrium level of 1.0 when the placental discrimination factor of 0.5 (26) is considered. Bryant *et al* (27) in England reported analyses of 28 bone samples from subjects of all ages collected about January of 1956. Eight samples from persons ranging from 3 months to 3½ years old (average 1⅓ years) average 0.9 $\mu\mu c$ Sr^{90}/g Ca, and 11 subjects ranging from 20 to 65 years of age (average 36 years) averaged 0.07 $\mu\mu c$ Sr^{90}/g Ca. after dividing all rib results by 2. The predicted average maximum Sr^{90} equilibrium level about January 1956, based on these age groups, is 1.0 and 0.9 $\mu\mu c/g$, Ca, respectively.

Since the Sr^{90} environmental contamination level continued to rise during 1956, the predicted average maximum Sr^{90} equilibrium level of new bone as of the first of 1957 is about 1.8 $\mu\mu c/g$ Ca, which agrees quite favorably with the value of 1.7 for October 1956 derived from ecological discrimination factors (Table IV). Extrapolation to 1975, assuming no more weapons tests, gives a maximum average equilibrium level in young children of 2.6 $\mu\mu c/g$ Ca. If it is assumed that 1.8 $\mu\mu c$ Sr^{90}/g Ca represents present equilibrium conditions for the area between 60° N and 10° N latitude, values for the upper midwestern and eastern United States and the rest of the world can be calculated from the present and predicted average maximum levels of Sr^{90} surface deposition given in Table II. Results calculated in this way are compared in Table VI with young children's equilibrium values (Table IV) derived from ecological discrimination factors.

TABLE VI.—*Comparison of average maximum Sr^{90} equilibrium levels in skeletons of young children, derived from bone equilibrium and ecological discrimination considerations*

Area	Predicted maximum 1957		Predicted maximum 1975	
	Bone data ($\mu\mu c/g$ Ca)	Ecological data ($\mu\mu c/g$ Ca)	Bone data ($\mu\mu c/g$ Ca)	Ecological data ($\mu\mu c/g$ Ca)
Upper Midwestern and Eastern United States	2.5	2.4	3.2	3.1
60° N. to 10° N. Latitude	1.8	1.7	2.6	2.5
Rest of world	0.4	0.4	1.1	1.1
World average	0.9	0.9	1.7	1.6

These data show good agreement between the two methods of estimation.

Other observers have estimated average maximum Sr^{90} equilibrium levels in bone usually by methods that involved surface deposition levels and various ecological discrimination factors.

Libby first estimated the maximum equilibrium level in the United States and Europe in about 1970 at about 11 $\mu\mu c/g$ Ca (2). This estimate was based on the assumption that bone calcium would reach a Sr^{90} level approximately 70 per cent of that of available soil calcium. He later estimated a maximum average in the 1970's of 4 to 10 $\mu\mu c/g$ Ca (3) on the basis that calcium of bone would come into equilibrium at 10 per cent of the Sr^{90} content of available soil calcium. More recently he has estimated the average maximum bone equilibrium level in the spring of 1957 at 1.7 to 3.9 $\mu\mu c$ Sr^{90}/g Ca. The latter estimate was based on an over-all discrimination ratio of 1/13 to 1/30 for surface deposition levels of 25 mc Sr^{90}/mi^2 between 10°N and 60°N latitude. Because of the rate of decay of Sr^{90}, he concluded that the equilibrium bone concentration would be very little higher in the 1970's when the maximum soil contamination level is reached.

Kulp (24) estimated the world-wide average maximum equilibrium Sr^{90} bone level at 0.6 $\mu\mu c/g$ Ca in the fall of 1955 and at 1.3 $\mu\mu c/g$ Ca in about 1970, assuming no more weapons tests after the 1956 series. His estimates for the United States bone value were 0.9 $\mu\mu c/g$ Ca for the fall of 1955 and about 2$\mu\mu c/g$ Ca by 1970. All estimates were based on Sr^{90} surface deposition levels and ecological discrimination factors.

Eisenbud (4) made an estimate of the maximum average bone level on the assumption that bone calcium would come into equilibrium with the Sr^{90} in calcium from milk. In the summer of 1955, New York milk contained 2.5 $\mu\mu c$ Sr^{90}/g Ca, corresponding to a surface deposition level in the area of 6.5 mc/mi^2. Since he expected a maximum surface deposition in this area of about 21.5 mc/mi^2 in about 1970 when the stratospheric inventory of Sr^{90} is deposited, he estimated an average maximum bone level of 8.3 $\mu\mu c/g$ Ca. Admittedly, his estimate was pessimistic in that it ignored the possibility of transient high Sr^{90} values in milk resulting from cows eating fresh fallout on the surface of plants and assumed no discrimination between strontium and calcium in going from milk to bone. Introduction of the milk-to-bone discrimination factor of 0.5 lowers his estimate to 4.1 $\mu\mu c/g$ Ca.

Sr^{90} data on milk samples might be used to give a general estimate of the average maximum equilibrium level in bone. Chicago (2), United Kingdom

(27), New York (4), and Turkish milk (2) samples during the fall of 1955 and spring of 1956 averaged about 2.5$\mu\mu c$ Sr^{90}/g Ca. Ignoring all factors other than the discrimination factor (DF_3) of 0.5 in going from milk to bone and the one-year doubling rate for environmental contamination, the average maximum Sr^{90} equilibrium bone level in the fall of 1956 would be 2.5 $\mu\mu c$/g Ca. Extrapolated to 1975, the value would be 3.5 $\mu\mu c$/g Ca.

The various estimates of average maximum Sr^{90} equilibrium levels show a trend toward general agreement with those based on the most recent available bone data and the latest opinions regarding ecological discrimination factors (Table VI).

The most troublesome feature of the above considerations is that all values are average maximum equilibrium levels of Sr^{90} and make no allowance for local concentrations of fallout due to meteorological factors, variations in available soil calcium, dietary patterns and habits, nutritional state of segments of the population, etc. Frequency distribution patterns have been reported for stable strontium (11), natural radium (28) and Cs^{137} (7) in man. All these nuclides show essentially normal distributions with standard deviations of about 35 per cent, which suggests that the range ($\pm$ 3σ) of Sr^{90} equilibrium bone levels as of the first of 1957 (based on an average of 1.8 $\mu\mu c$/g Ca) should lie between about 0.3 and 4 $\mu\mu c$/g Ca.

On the basis of the above distribution patterns, Libby (10) has stated (at steady state among people *living in a given locality*) only one person in about 700 will have more than twice the average Sr^{90} burden, and the chances of anyone having as much as three times the average will be about one in 20 million. Presently the Sr^{90} measurements of bone samples from subjects of all ages show a much greater scatter than indicated by a standard deviation of 35 percent. The greater scatter of the observed values is due largely to the fact that samples came from many localities and (because of the relatively short period of environmental contamination and the age dependence of Sr^{90} deposition) represent varying degrees of equilibrium conditions. As stated by Libby, the spread may be expected to decrease as equilibrium is approached, and a study of the distribution of all bone data when normalized to equilibrium according to the upper curve of Fig. 6 is underway.

Local meteorological conditions will result in increased intensity of fallout in certain localities. The worst possible situation that could come about would be for these "hot spots" to coincide with localities of low available soil calcium in which the population grew up and lived in provincial isolation. Libby (10) has considered this problem in view of the averaging which occurs in food distribution systems and has postulated that a factor of 5 encompasses the total variation due to all factors, including soil calcium deficiencies.

6. SIGNIFICANCE OF SR^{90} LEVELS IN THE POPULATION

A. *Basis of Maximum Permissible Levels of Radiation*

Consideration of the basis of maximum permissible levels of radiation is essential to the evaluation of the potential hazard of Sr^{90} contamination to the general population. Human experience through diagnostic and therapeutic use of X and gamma rays and extensive animal experimentation with all types of radiation have demonstrated conclusively the production of deleterious biological effects. These effects are manifest as early physiologic aging resulting in life shortening, mutations, irregularities in hematopoietic function (some of which result in increased incidence of leukemia), and specific organ or tissue changes such as cataracts of the lens of the eye and tumors of bone.

Damage from ionizing radiations may occur from radioactive isotopes deposited in the tissues and organs, as well as from radiations originating from an external source.

Occupational Exposure.—Maximum permissible levels for most radioisotopes as applied to occupational exposure are calculated on the premise that no critical organ or tissue will receive an average dose rate greater than 300 mrem per week (the maximum permissible dose rate for external whole body radiation), assuming uniform distribution of the isotope throughout the tissue or organ. It is recognized that calculations based on average dose or uniform distribution in a critical organ or tissue may lead to considerable error, since some radioisotopes are unevenly deposited. Although the average dose rate to a critical organ may be 300 mrem per week, some portions of the organ may receive considerably less than the average and others correspondingly more. This error, however, is be-

lieved to be offset at least in part by the fact that 300 mrem is considered the acceptable weekly exposure to the entire body or the blood-forming organs, and therefore may be overly conservative when applied to a small element of tissue.

The maximum permissible levels of radionuclides that localize in bone (i. e., Sr–90) are determined by direct comparison with the 0.1 μc maximum permissible burden for radium. Limited human experience has indicated conclusively that small amounts of radium fixed in the skeleton will produce osteoporosis, necrosis, and sarcoma (29). Bone changes in radium dial painters and persons who received radium therapeutically provide the basis for the value of 0.1 μc. The maximum permissible levels of other bone-seeking radioisotopes are established on the premise that the amount fixed in the bone will not result in greater probability of biological effect than that produced by 0.1 μc of fixed radium.

Derivations of formulae for the calculation of maximum permissible levels, their parameters and pertinent information on individual nuclides are given in the Handbooks of the International (8) and the National (30) Commissions on Radiological Protection. These handbooks provide the only official sources of maximum permissible levels for the various radionuclides from the standpoint of internal absorbed dose. All values presently published in the handbooks refer to continuous occupational exposure.

Nonoccupational Exposure.—Maximum permissible levels for nonoccupational exposure or exposure of a large segment of the general population were established by taking arbitrarily one-tenth of the value for working personnel (8, 30, 31).

The rationale behind a lower value for the general population is based on the numbers involved in the two groups at risk and the increased heterogeneity of the general population over that of the select working group. The latter group is composed of supposedly healthy workers (over 20 years of age), while the general population group may contain children, pregnant women, the undernourished, the sick and the old. On the assumption that frequency of response to radiation stress follows a Gaussian distribution (Fig. 7), the probability of injury of a few individuals from a specified dose increases with increase in size and heterogeneity of the group at risk.

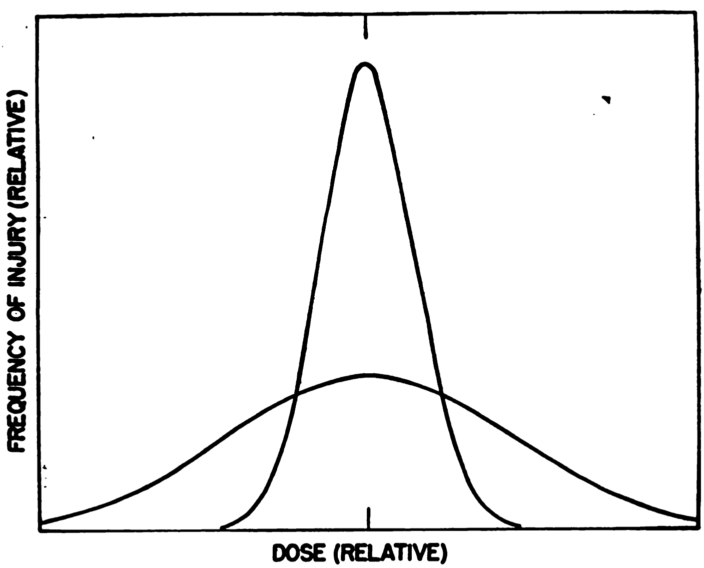

FIG. 7.—Effect of increased heterogeneity of the population on Gaussian distribution of frequency of injury as a function of dose.

The maximum permissible level of Sr^{90} for workers is set at 1 μc in the total adult skeleton, and the recommended level for the general population is set at 0.1 μc[4] (9, 32, 33). The permissible levels of radiation exposure (including that from Sr^{90}) is predicated on the assumption that chronic and/or delayed effects of radiation are threshold phenomena (Fig. 8). That is to say, there is a threshold dose below which effect rapidly becomes insignificant and above which effect increases expenentially over a limited dose range. If this is indeed the case, 100 $\mu\mu c$ Sr^{90}/g Ca must be looked upon as a true maximum permissible level and not an average value for the general population.

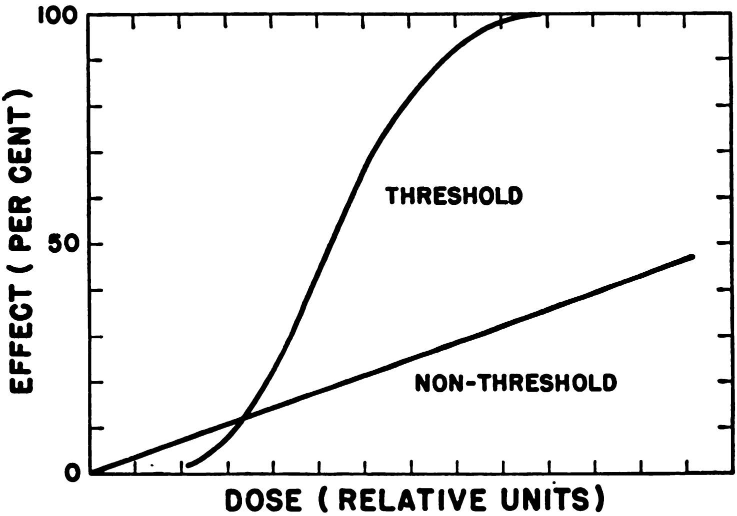

Figure 8.—Threshold versus nonthreshold effect in relation to increasing radiation dose.

Threshold versus Nonthreshold Response.—At the present time it is impossible to say whether leukemogenic and sarcogenic responses to chronic radiation dosage are threshold or nonthreshold relationships. Argument for a linear relationship between incidence of leukemia and radiation dose was presented recently by Lewis (34). His argument was based on all major sources of human data and included a consideration of the Japanese atomic bomb survivors, the British cases of X-ray treated spondylitis patients, X-ray treated cases of thymic enlargement, practicing radiologists and spontaneous incidence of leukemia in Brooklyn, New York. Radiation as a carcinogenic agent has been discussed at length by Brues (35), who stated that the relation between radiation dose and carcinogenic effect is not easy to find and a critical experiment has yet to be done which will clearly indicate, even in a single instance, what the relation is over more than a small range of dosages. While admitting that it is not known, he proposes that a threshold relationship between radiation dose and tumor incidence does exist (36).

Genetic response to external radiation indeed appears to be linear and a given increment of dose produces a corresponding equal increment of effect, regardless of position on the dosage scale (Fig. 8). If it is assumed that all chronic effects of radiation are linear, it would seem more reasonable to establish permissible levels for the general population on the basis of probability of risk averaged over the entire group. Present incidence of bone sarcoma and of leukemia averaged over the entire population is about 2 and 6 per 100,000, respectively. About 10 percent of the natural incidence of leukemia (34) (and perhaps of bone sarcoma) may be attributable to natural radiation background. If this is true, doubling the natural background dose to the bone might be expected to increase the inci-

[4] There is about 1 kg of calcium in the adult human skeleton; therefore, the MPL of Sr^{90} in the general population is equivalent to 0.1 μc Sr^{90}/kg Ca = 100 mμc/kg Ca = 100 $\mu\mu c$/g Ca = 100 Sunshine Units (2).

dences of bone tumors and leukemia to 2.2 and 6.6 per 100,000, respectively. Such a small increase distributed through the general population may be undetectable, and 100$\mu\mu$c Sr^{90}/g Ca (which would about double the background skeletal dose) may be regarded by some as an acceptable average maximum equilibrium for the general population.

B. Hazard from Present and Predicted Sr^{90} Levels

The significance of the general hazard of present and predicted levels of Sr^{90} in bone can be evaluated only in relation to human experience, which is indeed inadequate. Bone sarcoma has resulted from a fixed skeletal burden of 3.6 μc of pure Ra^{226}, and nondeleterious bone changes have been observed in persons having only 0.4 μc for a period of 25 years (37). Necrosis and tumors of the bone have occurred also several years after large doses of X-ray (38), and consideration of human experience with leukemogenic effects of X and gamma radiation (9, 34, 39) suggests that about 80 rads may double the incidence of leukemia.

The only other human experience with which present and predicted levels of Sr^{90} may be compared is that arising from natural background radiation. Natural background dose to the bone (during a 70-year lifetime) may vary from about 8 to 38 rem (40). The major contribution to background variation is differences in the radium levels of soils and minerals. The average natural skeletal radiation dose rate was carefully evaluated by Dudley and Evans (41) and their data are shown in Table VII.

TABLE VII.—*Average natural background radiation dose rate to the skeleton (Dudley, Evans)*

Source of radiation	Skeletal dose rate (mrem/year)	Total dose to age 70 (rem)
K^{40} (internal)	8	0.56
Ra^{226} (internal)	12	0.84
MsTh (internal)	12	0.84
RaD (internal)	12	0.84
Cosmic rays (external)	30	2.1
Local gamma rays (external)	60	4.2
Total	134	9.4

Table VIII (after Brues (42)) gives a general summary of estimated skeletal radiation doses from accepted maximum permissible levels and from present and predicted Sr^{90} burdens in relation to human experience. The maximum permissible level of Sr^{90} (100 $\mu\mu$c/g Ca) is estimated to deliver about 8 rads [5] to the skeleton during a 70-year life-time. This is comparable to the average natural background dose to the bone for the same time period and a factor of ~4 below the maximum natural background dose to which small segments of the general population may be exposed as a result of differences in altitude and natural radium content of soils and minerals. It is a factor of 40 below the lowest skeletal dose which has produced minimal nondeleterious bone changes. These data suggest that the present average maximum Sr^{90} equilibrium level in children will result in a life-time radiation dose of approximately 2 per cent of the accepted maximum permissible level for the general population. The predicted average maximum level of Sr^{90} (from bone data) in about 1975, assuming no further weapons tests, corresponds to a skeletal radiation dose of about 2.6 per cent of the maximum permissible level with a spread ($\pm 3\sigma$) of about 0.5 to 6 per cent.

The biological significance of present and predicted Sr^{90} average maximum equilibrium levels and maximum permissible levels for occupational and non-occupational exposure is summarized in Table IX.

[5] Eight rads is the calculated dose assuming incorporation to age 20 and decay to age 70 with no more incorporation. If equilibrium were maintained, the calculated skeletal dose would be about 21 rads. Since some but not all of the skeleton undergoes remodeling plus exchange, somewhere between 8 and 21 rads is probably more correct.

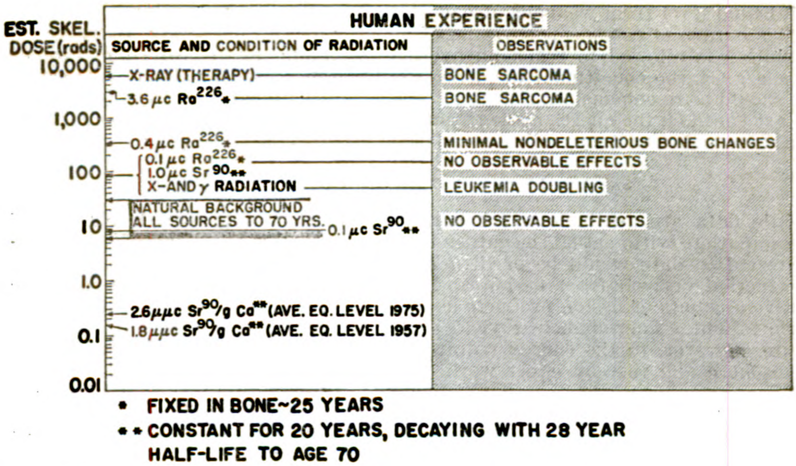

TABLE IX.—*Biological significance of present and predicted Sr^{90} average maximum equilibrium levels and maximum permissible levels for occupational and nonoccupational exposure*

Sr^{90} level	MPL nonoccupational exposure (100 $\mu\mu$ c/g)	MPL occupational exposure (1,000 $\mu\mu$ c/g)	Minimum bone changes	Minimum sarcoma dose	Leukemia doubling dose
Present (1.8 $\mu\mu$ c/g Ca)[1]	1/50	1/500	1/2,000	1/20,000	1/500
Predicted (2.6 $\mu\mu$ c/g Ca)[1]	1/40	1/400	1/1,400	1/14,000	1/360
100 $\mu\mu$ c/g (MPL nonoccupational exposure)	----------	1/10	1/40	1/400	1/10
1,000 $\mu\mu$ c/g (MPL occupational exposure)	10	----------	1/4	1/40	1

[1] Average maximum equilibrium level of Sr^{90}, probability slight that many will run more than 3 times average.

The most interesting comparison made in Table IX is that between Sr^{90} levels and the leukemia doubling dose, assuming a nonthreshold relation between incidence and radiation exposure. These data indicate that the predicted average maximum equilibrium level of Sr^{90}, assuming no more weapons tests after Operation Redwing (Fall 1956), is 1/360 of the leukemia doubling dose. Theoretically, this level is equivalent to an increase in leukemia incidence of 1.7 cases per 10 million population. The rate in some localized areas may be several times higher, but averaged over the world population of 2.6 billion, this would produce an increased lukemia burden of about 400 cases per year. If the entire world population is allowed to reach an average maximum Sr^{90} equilibrium level of 100 $\mu\mu$c/g Ca, the average increase in world's leukemia burden would be about 16,000 cases per year, or about 5 to 10 per cent (34).

The above discussion entails the assumption that Sr^{90} beta radiation induces leukemia of the bone marrow origin at the same rate (per unit of absorbed dose) as X and gamma rays. Much of the beta radiation from Sr^{90} will be absorbed in the bone and not reach the hematopoietic tissues at all. Experiments by Brues *et al* (43) suggest that Sr^{89} (half-life 55 days, $E\beta=1.5$ Mev.) administered to mice is relatively more spectacular as an osteosarcogenic agent than a leukemogenic agent. Furthermore, leukemia was not a significant finding in the radium dial painters (29, 44) or in the radium-injection cases (37).

Human data on radiation-induced osteogenic sarcoma are not adequate to provide a basis for a sarcoma doubling dose or for an estimation of the per cent of normal incidence attributable to natural background. If, however, the same assumptions used for leukemia are applied to osteogenic sarcoma (nonthreshold response, 10 per cent of normal incidence of 2 per 100,000 attributable to natural background and a doubling dose of 80 rads), the predicted average maximum Sr^{90}

equilibrium level from weapons already tested would produce an increase in the world's burden of osteogenic sarcoma of about 150 cases per year. If the world population is allowed to reach an average maximum Sr^{90} equilibrium level of 100 $\mu\mu c$/g Ca, the bone tumor incidence would be increased by about 5,000 cases.

It should be re-emphasized that the above considerations are extremely tenuous and are based on the questionable assumption that the incidence of leukemia and bona sarcoma bear a linear relationship to radiation dose.

7. SR^{90} LEVELS IN RELATION TO WEAPONS TESTING

Little data are available which permit correlation of present levels of Sr^{90} contamination with actual megatons of weapons tested. Total of all weapons tested to date would be of little value without additional information on the fraction derived from fission and on conditions of firing which influence relative amounts of fission products deposited as local, tropospheric and stratospheric fallout. Thermonuclear yield *per se* does not produce Sr^{90}; it does contribute, however, to the energy required to carry the fission products into the stratosphere and thereby effect world-wide distribution. Libby (2, 3, 10) has provided estimates of the megaton equivalents of fission products that have been injected into the stratosphere and deposited over various regions of the earth. Based on these values, Kulp (24) estimated that present levels (Fall 1956) of environmental contamination (including the stratospheric reservoir) was the result of injection of products from about 50 MT of fission yield.

By simple proportionality he estimated biospheric injection of Sr^{90} from 35,000 MT of fission would bring the average maximum Sr^{90} equilibrium bone level of the world's population up to 100 $\mu\mu c$/g Ca (the MPL for occupational exposure). He used 1.3 $\mu\mu c$ Sr^{90}/g CA as the average maximum equilibrium level for the world population in 1970, assuming no more weapons tests. Kulp did not say that the average maximum level of the world population should be allowed to reach 1000 $\mu\mu c$ Sr^{90}/g Ca,[a] but apparently tried to show the relation between megatons of fission tested to date and a MPL familiar to all (45). Libby (2, 3) also has used the occupational MPL as a reference point in discussing the hazard to the world population. This practice has led to confusion of the public and criticism of the Atomic Energy Commission (46). The data in Table X (based on various estimates of 1970 equilibrium bone levels) show the estimated megaton equivalents fission yield that may be injected into the biosphere (all at once) to bring the Sr^{90} average maximum equilibrium bone values in the United States, the northern hemispheric fallout belt and the world up to the limits set for occupational and nonoccupational exposure. The table also shows the influence of various factors for nonuniformity of distribution and uptake on the estimates for the northern hemispheric fallout belt (in which the majority of the world's population is distributed) based on the average maximum level derived from bone data. *These data are presented primarily to emphasize the principal areas of uncertainty responsible for apparent disagreements among various authorities.*

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[a] The Sr^{90} maximum permissible level accepted by the National and International Commissions on Radiological Protection as being applicable to large segments of the population is equivalent to 100 $\mu\mu c$/g Ca.

TABLE X.—*Sr^{90} levels in relation to megaton equivalents of fission products injected into the biosphere*

Source of equilibrium estimate and area	Estimated equilibrium 1970–75 ($\mu\mu c$ Sr^{90}/g Ca)	MT required to produce average MPL	
		(1,000 $\mu\mu c$/g Ca)	(100 $\mu\mu c$/g Ca)
United States (average):			
Libby (10), Ecological data	1.7–3.9	30,000–13,000	3,000–1,300
Kulp (24), Ecological data	2	25,000	2,500
This report, Ecological data	3.1	16,000	1,600
This report, Bone data	3.2	16,000	1,600
Eisenbud (4) [1], Milk data	4.1	12,000	1,200
This report, Milk data	3.5	14,000	1,400
Area 10° N. to 60° N. latitude (average):			
This report, Ecological data	2.5	20,000	2,000
This report, Bone data	2.6	20,000	2,000
World (average):			
Kulp (24), Ecological data	1.3	38,000	3,800
This report, Bone data	1.7	30,000	3,000
This report, Ecological data	1.6	30,000	3,000
Ave. bone data (10° N. to 60° N. latitude)	2.6	20,000	2,000
Ave. × ⅕ (for nonuniformity (10))		4,000	400
Ave. × $\frac{1}{10}$ (for nonuniformity) [2]		2,000	200

[1] Eisenbud's value corrected for discrimination factor of 0.5.
[2] Indicated by spread in current bone data from all ages (24).

Inspection of the data in Table X shows a variation of about 300 in the megaton equivalents of fission products that may be injected into the biosphere, depending on whether one wishes to be ultraconservative and use the highest equilibrium bone value, the nonoccupational MPL and the largest safety factor for nonuniformity, or use the occupational MPL applied to the world average maximum bone level with no safety factor for nonuniformity. The most important point to these data is that they show that the major portion of the variaion is associated with two factors, (1) the maximum permissible level for Sr^{90} as applied to the fallout problem, and (2) the factor for nonuniformity of Sr^{90} distribution and uptake.

The most important question regarding the potential hazard of world-wide fallout to the general population is its relation to future weapons testing. If there is an upper limit to the amount of Sr^{90} that can be tolerated in the bones of the population, then the number of megaton equivalents of fission products that can be contributed per year to the biosphere by all nations must be limited.

Theoretically, the total yearly injection rate should be that amount which, at equilibrium, will not result in a significant fraction of the population exceeding the limit of safety. If a constant yearly injection rate of 1, 10 or 100 MT of fission is adhered to, in about 100 years the amount of Sr^{90} added to the environment will come into equilibrium with the rate of Sr^{90} decay, and continuation of weapons testing at that rate will result in no further increase in the average maximum equilibrium level in the bones of the population. At that time the average equilibrium bone level will be directly proportional to the yearly injection rate, i. e., if 10 or 100 megaton equivalents are injected per year, the average equilibrium bone level will be 10 and 100 times higher, respectively, than it will be if only 1 megaton equivalent is injected.

Only three unclassified reports concerning implications of future biospheric fission product injection rates have appeared. Campbell (5) mathematically related surface deposition to a constant stratospheric injection rate. His equation suggests Sr^{90} surface deposition levels may reach 30 times present values with continuation of the present rate of biospheric contamination for 100 years. His approach, however, makes no allowance for the tropospheric deposition rate. Libby (49) has developed an expression relating Sr^{90} surface deposition to a constant test rate, using a calculated ratio of rates of stratospheric to tropospheric injection of 4 to 7. Libby's calculation is based on a measured value of 30 mc Sr^{90}/mi^2 for the present cumulated fallout in the northeastern United States (one of the more highly contaminated spots in the world) and the assumption that the stratospheric reservoir presently contains the Sr^{90} from 24 MT of fision. Libby first estimated that the maximum equilibrium Sr^{90} deposition level should approach 8 times the present level (10). On re-

calculation of the ratio he obtained a value of 11 (49).[7] Using a build-up factor of 8 and assuming an ecological discrimination factor of 20 to 80 against Sr^{90} in going from soil to human bone, he estimated an average maximum bone equilibrium level of 5 to 20 $\mu\mu c$ Sr^{90}/g Ca for the population of the United States when equilibrium is reached. If the present rate is continued for 28 years, the average maximum level would reach only one-half of the above values (10). The general consensus of opinion (23) of the various specialists in the field of bone and mineral metabolism is that the discrimination factors used above are too high and that a factor of 10 to 20 is more realistic.

Stewart, Crooks and Fisher (48) estimated the concentration of Sr^{90} on the ground in the United Kingdom as 4.5 mc/km^2 on January 1, 1956, and the mean deposition rate as 2.3 mc/km^2/year. From these data the ground concentration in the U. K. as of January 1, 1957, would be about 17 mc/mi^2. They also estimated that an equilibrium value of about 500 mc/mi^2 (200 mc/km^2) of Sr^{90} may be reached in the U. K. in about 100 years if the present rate of biospheric contamination is continued. These data show a build-up factor of about 30 over present levels.

Predicted average maximum Sr^{90} bone levels, assuming continuation of the present biospheric contamination rate for about 100 years, are given in Table XI. These values were calculated from present (Fall 1956) bone equilibrium levels derived either from analyses or from ecological considerations assuming an ecological discrimination factor of 10 against Sr^{90} and Libby's (49) build-up factor of 11 for continued testing. The one British value mentioned above is given for comparison.

TABLE XI.—*Average maximum Sr^{90} bone equilibrium levels assuming continuation of past 5-year injection rate for 100 years*

Basis of estimate and area	Average maximum equilibrium bone level	
	Fall 1956 ($\mu\mu c$/g Ca)	In 100 year [1] ($\mu\mu c$/g Ca)
United States:		
Libby (10), ecological data	1.7–3.9	[2] 19–43
Kulp (24), ecological data	1.4	15
This report, ecological data	3.6	40
This report, bone data	2.5	28
This report, milk data	2.5	28
Eisenbud, milk data	4.1	45
Area 60° N. to 10° N. latitude:		
This report, ecological data	2.6	29
This report, bone data	1.8	20
Stewart, et al. (48), present soil data United Kingdom [3]	3.1	34
Stewart, et al. (48), United Kingdom soil in 100 years	------------	[4] 90
World average:		
This report, ecological data	1.3	14
This report, bone data	.9	10
Kulp (24), ecological data (1970)	1.3	14

[1] Based on, or 5MT of fission yield per year assuming testing for infinite time, an equilibrium level, with continued testing, of 11 times the present level.
[2] Libby's estimate (10) was 5 to 20 $\mu\mu c$/g Ca.
[3] Assuming an ecological discrimination factor of 10 against Sr^{90}.
[4] This value corresponds to a buildup factor of about 30.

The discrepancy between the British value and the others is immediately apparent. No details as to the basis of their estimate were given but it is possible that no consideration was given to the ratio of rates of stratospheric and tropospheric injection, since it compares favorably with the value that would be predicted from the derivations made by Campbell (5).

The data in Table XI (excluding the British value) suggest that, continued biospheric contamination at the present rate for 100 years might result in average maximum equilibrium bone levels of about 30, 25 and 12 $\mu\mu c$ Sr^{90}/g Ca for the United States, the area between 60°N–10°N latitude and the world, respectively.[8]

[7] R. K. Zeigler of LASL Theoretical Division has confirmed the calculation using Libby's assumptions.
[8] If Machta's concept of uneven stratospheric fallout is indeed the case, the average Sr 90 values for the United States and the area between 60° N–10° N latitude may be increased by about a factor of 2.

The upper limit that might be expected in the United States, assuming a factor of 5 as adequate to make allowances for non-homogeneities of Sr^{90} deposition and uptake, would approach about 150$\mu\mu$c Sr^{90}/g Ca, or 150 percent of the accepted maximum permissible level. After 30 years of testing, average maximum equilibrium bone levels may approach one-half of the above values, which may result in an upper limit for the United States of about 75 percent of the maximum allowable level. Assuming the average yearly injection of fission products during the past five years equal to about 10MT of fission yield, it would seem that the testing of 10MT of fission per year by all nations for 30 years should probably be considered the upper limit, or 5MT of fission yield per year assuming testing for infinite time. If these are the limits of acceptable injection rate, international agreement not to exceed these levels seems desirable. Present levels of Sr^{90} contamination are due almost entirely to tests held by only two nations. Present and predicted future Sr^{90} levels, even if weapons tests are continued at the present rate for a few years, does not seem dangerous. However, indiscriminate testing of high-fission yield weapons by many nations could result in serious levels of worldwide contamination.

SUMMARY

What does the accompanying mass of technical data mean with regard to the controversy over cessation or continuation of nuclear weapons tests? Nowhere in this report has a recommendation been made either to stop or to continue testing. Such a recommendation requires a careful weighing of the importance of the nation's nuclear weapons capability in averting a nuclear war, against the probability that a few people might get leukemia or bone sarcoma or manifest a genetic abnormality who otherwise might not have done so. Therefore, the decision to stop or continue tests requires a value judgment involving knowledge of the potential seriousness of present and future threats to the national security, and whether they should or should no be stopped on the basis of moral and humanitarian principles is not readily amenable to solution by the scientific method.

The purpose of this report is to evaluate, as factually as possible from existing data, the potential hazard of Sr^{90} fallout. Evaluation of existing data supports the following general conclusions:

(1) Radioactive isotopes deposited in the bone in sufficient quantity will produce serious consequences, including bone cancer and leukemia. Present Sr^{90} levels in the bones of the population are quite low. The present average maximum equilibrium Sr^{90} radiation dose to the bones of young children is greater than that for adults and is about 2 per cent of the average dose received from unavoidable natural background radiation contributed by cosmic rays and by radium, thorium, uranium, etc., in the environment. It is about 2 per cent of the maximum permissible level adopted by the National and International Commissions on Radiological Protection as acceptable for large segments of the general population. The present Sr^{90} radiation dose to adults, averaged over the total skeleton, is about one-tenth of that to children. Because of nonuniformity of fallout and individual variations in uptake and deposition of Sr^{90} in bone, a very small number of people may accumulate a skeletal dose that will be about five times the average, and an equal number will accumulate only about one-fifth the average. Since in the stratosphere there is still some Sr^{90} from past weapons tests, the average radiation dose may continue to rise until about 1975 even if no more weapons tests are held. At that time the equilibrium level may be 3 to 4 per cent of the average natural background.

(2) If Sr^{90} contamination from weapons testing by all nations continues at the same rate as has occurred during the past five years (about 10 megaton equivalents of TNT fission yield per year), equilibrium will be reached in about 100 years. At equilibrium the amount of Sr^{90} which will disappear each year from our environment due to radioactive decay will just about equal the amount that is being produced each year. At this time continuing weapons tests will not result in any further increase in Sr^{90} in the bones of the population. Assuming a buildup factor of 11 for equilibrium levels, the average Sr^{90} radiation dose to the bones of the population of the United States is predicted to be about 30 percent of the average radiation dose from natural background, or about 30 percent of the maximum permissible level adopted by the National and International Commissions. Since a factor of 5 may be necessary to allow for nonuniformities in fallout and bone uptake, the Sr^{90} radiation dose to a few individuals may approach 150 per cent of the recommended maximum level as an upper limit. If strato-

spheric fallout is not as uniform as predicted by the Libby model, the above values may be somewhat higher. Thirty years of testing will result in an average Sr^{90} level of about one-half of the equilibrium value, which may result in a few people approaching 75 percent of the recommended maximum permissible radiation dose. On this basis, limitation of biospheric contamination by all nations to about 10 megaton equivalents of fission yield per year for 30 years, or 5 megaton equivalents per year, indefinitely, might be desirable.

(3) The existing data support the conclusion that the present rate of biospheric Sr^{90} contamination, if continued for 20 to 30 years, will not produce average maximum population bone levels that will exceed the maximum permissible levels accepted by the National and International Commissions on Radiological Protection. The data also show that many nations cannot test high fission yield weapons indiscriminately and indefinitely without running the risk of seriously exceeding these recommended levels. For this reason, international agreement to limit testing might be desirable while negotiating for agreement to stop testing altogether.

(4) The data presently available definitely show that the greatest question concerning world-wide Sr^{90} contamination concerns the decision as to the *ACCEPTABLE* maximum permissible body dose for the general population in terms of the individual and in terms of world health. The answer to this question involves moral and humanitarian principles, as well as scientific uncertainties as to the biological consequences.

Some deductions as to the worst biological consequences of Sr^{90} fallout (which excludes the genetics question) can be made by accepting two rather pessimistic assumptions, neither of which has been proved, but both of which seem conservative. The natural yearly incidence of leukemia in the United States is about six cases per 100,000 population, and the incidence of bone tumors is about two cases per 100,000. Therefore, normally there are about 10,000 cases of leukemia in the United States per year and about 3,000 cases of bone tumors. This was the natural incidence even before any radioactive fallout had occurred; so atomic bombs have had nothing to do with it. The first assumption is that any amount of radiation has a small chance of producing tumors and leukemia. That is, the assumption is made that there is no *absolutely safe* radiation dose and any amount is theoretically bad. This assumption is open to serious question and is one of the major points of disagreement among scientists. If all radiation is bad, the natural background radiation which we cannot avoid must be responsible for a fraction of the six leukemia cases and the two bone cancer cases per 100,000 that occur in the population. The second assumption is that about 10 percent of the normal incidence of bone tumors and leukemia is due to natural background radiation. There is some scientific evidence from X and gamma radiation in support of this assumption as applied to incidence of leukemia, but none for Sr^{90} radiation. There is no evidence to support its application to bone cancer. We do know, however, that there are things in our environment other than radiation which will cause bone cancer; so it certainly would not be right to attribute 100 per cent of the bone cancer incidence to natural background.

On the basis of the assumptions given above, the incidence of leukemia in the United States might increase from 10,000 cases per year to about 10,030 and the incidence of bone cancer might increase from about 3,000 to about 3,010. This suggests a total increase of 40 cases out of a population of 165 million people as the maximum biological consequence of Sr^{90} fallout from weapons tested to date *by all nations*. If the present rate of biospheric Sr^{90} contamination continues for 30 years, the biological consequence of Sr^{90} fallout may be a total increase in the United States population of about 250 cases per year of these two diseases. There is a good chance that this prediction may overestimate the population risk. The assumption that even the smallest amount of Sr^{90} deposited in bone carries a small probability of harm is seriously questioned, and there is a possibility that a threshold below which no leukemia and bone cancer will be produced does exist. Animal experiments with radioactive strontium definitely indicate that Sr^{90} may not be as bad as anticipated for producing leukemia because the beta radiation from the Sr^{90} gets absorbed in the hard bone and does not radiate the bone marrow where leukemia begins. These same experiments do suggest that production of bone tumors by Sr^{90} is about as expected on the basis of human experience with radium.

(5) The present data also indicate that much more research on both the physical and biological factors of fallout must be done if weapons tests are to be continued. This research is necessary to narrow the limits of error and uncertainty in existing data and to permit predictions to be based on facts instead of on what appears to be reasonable assumptions. More research on the biological and

medical effects of radiation and radioactive materials is essential, even to the future of the power reactor program which also produces Sr^{90} and other fission products.

Although present knowledge of the biological effects of radiation and radioactive materials is not all it should be to allow plunging ahead recklessly and without worry into all aspects of nuclear technology, it is adequate to dispel an attitude of gloom and doom. Radiation is not the only potential hazard man is facing as part of the price of living in a highly developed society nor is it the insurmountable one.

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93299°—57—pt. 2——24

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The committee will stand adjourned.

(Whereupon, at 12:15 p. m., Thursday, June 6, 1957, the committee adjourned, to reconvene at 10 a. m., tomorrow, Friday, June 7, 1957, in the old Supreme Court chamber, the Capitol.)

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

FRIDAY, JUNE 7, 1957

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION
OF THE JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The special subcommittee met, pursuant to recess, at 10 a. m., in room P–63, the Capitol, Hon. Chet Holifield (chairman of the subcommittee) presiding.

Present: Representatives Holifield, Durham (chairman of the Joint Committee), Cole, Price, Van Zandt; Senators Anderson and Jackson.

Also present: Professional staff members: James T. Ramey, executive director; George E. Brown, Jr., Hal Hollister, staff technical adviser, and Paul C. Tompkins, consultant.

Representative HOLIFIELD. The committee will come to order.

The Chair would like to explain something which occurred yesterday for the benefit particularly of the press.

Dr. Libby in response to a request of mine on a previous day handed in a short statement regarding the scientific reasons for testing weapons. Shortly after this, one copy which was handed to me was turned in, I was called to another meeting, and I had to stay in it all morning, because it was an executive session. Senator Anderson took over the chair. There was some confusion between Senator Anderson and me, and neither one of us read the statement of Dr. Libby at that time or before adjournment, I should say. As there were no other copies handed out, there were some reporters, apparently, who noticed it and asked to see it. They read it before the papers were taken to the committee room. I have asked the staff to prepare some copies which have been handed out this morning, and I am going to ask, since it is a short statement—and it was not accepted officially—Dr. Dunham to read this into the record at this time.

STATEMENT OF DR. WILLARD F. LIBBY, UNITED STATES ATOMIC ENERGY COMMISSIONER, READ BY DR. CHARLES L. DUNHAM

Dr. DUNHAM (reading):

I have been asked by the committee to explain briefly in my statement for insertion in the record why I believe nuclear tests must continue.

For the survival of our Nation and that of the free world, heavy reliance is placed on the United States' defensive and deterrent capabilities. These capabilities are inextricably bound to our nuclear warheads and to the weapons systems which would carry these warheads.

We are today in a period of radical transition in the weapons systems available and becoming available. A very few years ago the possibility of a surprise nuclear attack upon the United States and the other free nations was small. Our own weapons could be readied carefully and the carrying vehicles could take time in delivering the warheads to destination.

Now, however, with the gains made by nations in delivery systems the technical possibility of surprise attack against us has increased. This then is forcing a change in our defensive systems. We require new weapons of smaller size, capable of withstanding extreme conditions, instantly ready and tailored to give the desired effect. These characteristics are particularly important and stringent in the case of weapons for defense of our Nation and our forces against possible enemy attack. Generally at present, each new weapon-delivery system requires a new or modified warhead design.

Naturally, in view of the reliance we must place on our nuclear-weapons systems, new or modified weapons and design changes must be tested if we are to rely on them in the emergency. Unfortunately, there is no substitute for testing to determine the reliability of a weapon, conventional or atomic. To cut off testing, therefore, means the cutting off of the introduction of improved nuclear-weapons systems. Though there can be very limited extrapolation of present information without testing, cessation of tests would, to all intents and purposes, end shortly our developmental work. It would mean the cutting off of attempts to achieve further improved designs—designs which would lessen still further radioactive contamination from detonations, designs to make most efficient use of materials, designs which could function under the extremes that tney would be called upon to face. It would mean that systems being developed aand urgently needed for our defense would be without the most effective warheads.

As you know, one cannot enumerate in an unclassified statement and without divulging to the world the status of our nuclear armaments the known defense and deterrent weapon systems which we would forego by stopping tests. From past test series, we have found, sometimes unexpectedly, means of increasing the efficient use of materials, reducing the size and complexity of warheads, increasing their deliverability and yield, and reducing the radioactive contamination from our larger yield devices. The committee is familiar with these past developments and with the fact that additional systems now scheduled could not be brought into being without further tests. In a public statement, I can only emphasize that our weapons development would be crippled by a cessation of tests.

Naturally, should we interrupt our testing and others do not so do, we could find ourselves shortly with systems whose nuclear warheads were not adequate for the intended purposes. With the rapidity with which weapons systems are now changing we might find ourselves in such a position in a relatively short period of time.

As I stated earlier, therefore, cessation or interruption of testing is in itself a form of disarmament. Such a step should only be taken, therefore, as a part of a comprehensive disarmament plan. It should only be taken when there are means in existence of assuring that other nations similarly will cease their testing and hence their development of new systems.

We must continue our efforts to find an assured way of disarmament, as part of which all would stop testing of nuclear weapons. In the meantime, we as a Nation should continue to limit our test shots to those essential to our development program for the weapons so vital—in the absence of safeguarded disarmament—to assuring the survival of the free world. We should continue by all possible means our efforts to reduce the amount of residual contamination from these fully justified shots.

Representative HOLIFIELD. The request of the Chair was that Dr. Libby give the scientific reasons for the continued testing of weapons. While there are some scientific reasons in the statement, there are also statements of policy and conclusions on Dr. Libby's part. In view of the fact that Dr. Libby is not here, I have no questions on the statement. Do the other members of the committee have any questions?

Chairman DURHAM. No.

Representative COLE. Except to point out, as I am sure the Chair will agree, that Dr. Libby could not discuss the scientific considera-

tions and justifications of continuing weapons tests without disclosing vital security information.

Representative HOLIFIELD. I certainly agree with that statement.

Dr. Dunham, I believe you have some documents to insert in the record.

Dr. DUNHAM. Yes; I have two statements prepared by Dr. Libby, advance copies of which the committee already has, but these are up-dated material on radioactive fallout on soils and the radioactivity of rain, and the other statement has to do with natural occurrence of radioactivities.

I would like to on Dr. Libby's behalf introduce these in the record.

Representative HOLIFIELD. I do not believe the members have had a chance to see these statements. Did you present the copies this morning?

Dr. DUNHAM. I believe Mr. Hollister has copies of these. Yes; they were brought up this morning.

Representative HOLIFIELD. For the present we will accept them without knowing exactly what they are.

Dr. DUNHAM. I would also like to take the opportunity at this time to introduce a statement prepared by Dr. Harry Wexler, of the Weather Bureau, entitled, "Radioactive Fallout in the Stratosphere," which is essentially a discussion of research approaches in meteorology to solving some of the fallout problems.

Chairman DURHAM. Mr. Chairman, this is a very interesting statement. It will not take but a few minutes to read it. Would you like to read it?

Dr. DUNHAM. I will be very happy to read it if the committee so desires.

STATEMENT OF DR. HARRY WEXLER,[1] DIRECTOR OF METEOROLOGICAL RESEARCH, UNITED STATES WEATHER BUREAU (READ BY DR. CHARLES L. DUNHAM)

Since the large nuclear explosions deposit a considerable portion of their radioactive debris in the stratosphere, these questions arise: What happens to this radioactivity? Does it mix uniformly over the world after a year or two? Is there much vertical mixing upward and downward? Are there important

[1] Chief scientist of the USNC-IGY Antarctic Program and is responsible for its entire scientific effort to be undertaken in 1957 and 1958 in Antarctica. He is a member of the USNC-IGY Antarctic Committee, the USNC-IGY Technical Panel on Meteorology, the ad hoc Arctic Committee and the ad hoc Equatorial Committee. He is a member of the United States National Committee for the International Geophysical Year and an alternate member of its Executive Committee. Received his B. S. degree in mathematics from Harvard University in 1932. He did graduate work at the Massachusetts Institute of Technology from 1932 to 1934 and from 1937 to 1938 and obtained his Sc. D. in meteorology in 1939. During the intervening period, 1934 to 1937, he worked at the United States Weather Bureau in Chicago, Ill., and Washington, D. C. He was appointed assistant professor of meteorology at the Institute of Meteorology of the University of Chicago in 1940 and left this position in late 1941 to return to defense work at the Weather Bureau in Washington. In November 1942, he joined the Army Air Force's Weather Service as a captain and was appointed senior instructor in meteorology to the Army Air Force Aviation Cadet School in Grand Rapids, Mich. While there he served as a member of the wartime University Meteorological Committee established to assist the military services in matters related to meteorological training.

In late 1943, Dr. Wexler was transferred to the Weather Division of the Army Air Forces in Washington, where he served as research executive and initiated and fostered a program of research in weather which later developed into the large Air Force program in geophysics. In September 1944, with Colonel Floyd Wood as pilot, made the first penetration of an Atlantic hurricane, which because of its unusual intensity became known as the Great Atlantic Hurricane. In 1946 Dr. Wexler became Chief of the United States Weather Bureau's Science Services Division. In 1955 he was named Director of Meteoro-

seasonal and geographic effects? Are there channels connecting the stratosphere to the troposphere through which large quantities of radioactvity can flow downward with the air and ultimately reach the precipitation layers and thus be rained out?

Dr. Machta has presented testimony pointing strongly in the direction of nonuniformity of stratospheric fallout which is corroborated by analysis of strontim 90 in soil samples collected by Dr. Alexander. This is a surprising result to most meteorologists who would believe that after a year or two, airborne contaminants released in the stratosphere should be uniformly mixed over the world. Admittedly, there has been little evidence to go on; for example, Krakatao volcanic ash apparently spread uniformly over the world within 3 to 6 months as best as could be judged from fragmentary optical and solar radiation effects available in 1883.

In recent years, observations of the atmospheric content of ozone, most of which is found in the lower stratosphere, show decided geographic and seasonal nonuniformity over the world, although the interpretation of this gas in terms of mixing is complicated by geographic and time variations in solar ultraviolet radiation, air temperature, and large-scale vertical air motions.

Against this background of conflicting evidence, one thing stands out. In view of great quantities of radioactive debris now present in the stratosphere and the possibility that as the years go on, new and larger amounts may be placed there, we must learn much more about the lateral and vertical transport and mixing mechanisms in the stratosphere. In addition, further understanding of atmospheric removal processes is needed. An outline of proposed research and development is presented below.

1. METEOROLOGICAL OBSERVATIONS IN THE STRATOSPHERE

Only in recent years have meteorological balloons penetrated systematically into the lowest layers of the stratosphere—but their average height (60,000 feet) is well below the top of the large thermonuclear bomb mushroom clouds (100,000 feet or higher). With increased effort during the International Geophysical Year, the average height may approach closer to 100,000 feet, but there will be large areas, particularly in regions of low temperatures, where the balloons become brittle and burst before rising appreciably in the stratosphere or even reaching the stratosphere.

Recommendation: Development of a relatively inexpensive, lightweight, solid propellant, frangible cased rocket, capable of carrying meteorological instruments to 200,000 feet and dropping them by parachute which can be followed by radar. This offers the only hope of establishing a widespread network of meteorological observations in the important 100,000–200,000 foot layer to observe stratospheric winds, temperatures and densities, so that the higher yield atomic clouds can be tracked.

2. AIR SAMPLING AND ASSAYING IN THE STRATOSPHERE

Although the availability of stratospheric meteorological observations will aid in determining the bulk transport of clouds of radioactive debris, they will not usually be sufficient to help estimate the degree of mixing between the

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Footnote continued from preceding page.

logical Research. The Weather Bureau has made Dr. Wexler available to the USNC–IGY for scientific direction of the Antarctic program.

In 1945 the Institute of Aeronautical Sciences presented Dr. Wexler the Losey Award in recognition of his outstanding contributions to the science of meteorology as applied to aeronautics.

A member of the Advisory Committee on Reactor Safeguards for the Atomic Energy Commission; Chairman, Committee on the Meteorological Aspects of the Effects of Atomic Radiation of the National Academy of Sciences; a member of the Meteorological Operations Subcommittee of the National Advisory Committee for Aeronautics. He is a councillor of the American Meteorological Society, a fellow of the American Academy of Arts and Sciences, a member of the Royal Meteorological Society and the Washington Academy of Sciences. He is a member of the American Geophysical Union. In August 1955, Dr. Wexler was a member of the United States delegation to the Atoms for Peace Conference in Geneva, Switzerland.

As an internationally recognized authority on meteorology, Dr. Waxler has published some 50 papers in scientific journals treating such subjects as the radiative cooling of the air, polar anticyclones, atmospheric turbidity, structure of hurricanes, and upper atmosphere temperatures and dynamic connections with the lower atmosphere.

In November 1956 was the recipient of the Department of the Air Force Exceptional Service Award in recognition of distinguished patriotic service. (Submitted by U. S. Department of Commerce.)

radioactive clouds and their environments. The existing limited program of sampling the stratosphere for natural and artificial constituents has thrown some light on the incompleteness of mixing in the stratosphere. Strontium 90, carbon 14, water vapor, and ozone have been sampled or assayed directly, and the few observations available, especially of the vertical profile of water vapor and ozone, reveal a marked stratification in the lower stratosphere, indicating very weak vertical mixing—at least during the few times soundings were made. There are not sufficient observations geographically or seasonally to arrive at similar conclusions regarding the degree of lateral mixing.

Recommendations: An intensified program of air sampling and analyzing to a height of 150,000 feet by aid of large "sky-hook" balloons should be inaugurated over large areas to determine the lateral and vertical mixing efficiency of the lower stratosphere and exchange mechanisms with the troposphere.

3. PREDICTION OF STRATOSPHERIC TRANSPORT AND MIXING

After an adequate program of stratospheric observations has been inaugurated so that one has a reasonably accurate picture of the present state of the stratosphere and its distribution of trace elements, then the question of the future state arises.

The few day-to-day weather analyses that have been carried on in the lower stratosphere suggest strongly that the flow patterns are so different from those existing below that inference or extrapolation from lower level happenings or predictions are of little or no value. The electric computer, with its capacity for analyzing large quantities of data quickly, has proved its value in forecasting important changes in flow pattern in the troposphere such as the change from a basically zonal (west and east) flow to largely meridional (north and south) flow. Such changes, occurring very rapidly, can have an important effect on the distribution of airborne material in the troposphere. It is suspected that there may be similar large scale changes in flow patterns in the stratosphere.

Recommendations: A special research unit should be established to plot and analyze daily stratospheric charts, using all the data described above, and having access to a modern high speed automatic computer to develop prediction techniques for stratosphere transport, mixing, and exchange with the troposphere.

4. REMOVAL PROCESSES

The hazard from strontium 90 stems from ingestion. The radioactive particles must, therefore, be removed from the atmosphere onto the ground or foliage. Evidence suggests precipitation as the prime removal process, although not the exclusive one. Impaction on obstacles and, to a lesser extent, gravitational settling are other known mechanisms.

The details of the precipitation scavenging process are a matter of speculation. Empirical correlations with fallout are inadequate for predictive purposes under different conditions.

The removal of small particles over water bodies is especially important since water surface constitutes almost three-fourths of the earth's surface.

Recommendations: The study of removal processes by impaction, scavenging, etc., should be intensified. Ocean samples should be analyzed for strontium 90 and cesium 137 in order to determine whether the oceanic fallout pattern follows that over land.

Representative HOLIFIELD. Are there any questions on this statement?

Dr. Dunham, does the AEC have any comments on this statement?

Dr. DUNHAM. No, it seems like a very sound recommendation.

Chairman DURHAM. You have made some fine recommendations here. Are we equipped to carry out the recommendations, as far as you know?

Dr. DUNHAM. Maybe Dr. Machta, who is here, would like to speak to that point. I gather his item 1 would require some development work. I am sure Dr. Machta is better prepared to answer that question.

Representative HOLIFIELD. Dr. Machta, would you like to come forward?

Chairman DURHAM. Have you read Dr. Wexler's statement?

Dr. MACHTA. Yes, I have.

Chairman DURHAM. Do you concur in his recommendations?

Dr. MACHTA. Yes, sir, I do.

Chairman DURHAM. Is any agency of the Government equipped to carry out his recommendations at the present time?

Dr. MACHTA. Yes, I think the Weather Bureau, if asked to do so, would be equipped to do it.

Chairman DURHAM. You are qualified to do it?

Dr. MACHTA. Yes, sir. We have a high-speed computer which is used for our everyday forecasting which would be devoted to this purpose, and we have in mind certain other projects which Dr. Wexler indicated.

Chairman DURHAM. You think it is important that we proceed to do this type of testing?

Dr. MACHTA. Yes, sir; I do.

Representative HOLIFIELD. This statement, as I read it, has to do with nonuniformity of stratospheric fallout. Does that pertain to the matter while it is in the stratosphere or does it pertain to the nonuniformity of its descent to earth?

Dr. MACHTA. The statement includes both aspects. In making a prediction, one must know where it is in the atmosphere, and how it would be removed.

Representative HOLIFIELD. At the present time the research you recommend is to find out the conditions in the upper stratosphere?

Dr. MACHTA. This primarily, but secondly the removal processes. For example, does it, as we really suspect happens, come down primarily in rainfall and factors of this sort?

Representative HOLIFIELD. It goes to sustain the theory of nonuniformity of deposit on the earth's crust, does it not?

Dr. MACHTA. I do not believe I would say that. What we want to do is to conduct more research to confirm the suspicions we have.

Representative HOLIFIELD. His suspicions, if you want to call them that, are along that line?

Dr. MACHTA. Yes, sir.

Chairman DURHAM. Is any of this type of sampling now being done?

Dr. MACHTA. Yes. During the International Geophysical Year, a very much more extensive program of ozone sampling in the upper atmosphere is to be conducted. We believe this will throw great light on the exchange in the stratosphere and the exchange between the stratosphere and troposphere. In addition, the AEC is undertaking a very good sampling program of strontium 90 and other fission products in the stratosphere which would be very important.

Chairman DURHAM. How about the oceanic testing?

Dr. MACHTA. According to my information, the Woods Hole Oceanographic Institute is sampling the Atlantic Ocean to determine the content of strontium 90. Their preliminary report will be out very shortly.

Representative HOLIFIELD. Before we proceed further, it has been called to my attention that we have Dr. Waterman, the Director of

the National Science Foundation, present this morning to partake in the panel discussion. We are glad to have you here, sir.

Dr. WATERMAN. Thank you.

Representative HOLIFIELD. Dr. Dunham, yesterday Senator Richard L. Neuberger released a press release which reads as follows:

Senator Richard L. Neuberger today urged that the Atomic Energy Commission make public a report on strontium 90 effects submitted to the AEC by an advisory committee.

The Oregon Senator charged in a Senate speech that the report, requested by the Commission over 2 years ago, has not yet been issued in final form due to "official reluctance."

Dr. H. Bentley Glass, professor of biology at Johns Hopkins University, indicated Tuesday that release of the study prepared by the Advisory Committee on Biology and Medicine has been delayed by the AEC. Glass is a member of the advisory group.

"The American public has a right to know of the dangers inherent in exposure to radioactive materials," Neuberger declared. "Suppression of this report on the part of the Atomic Energy Commission represents a betrayal of public trust."

"This subject is one of vital concern to all Americans," the Senator said. "It affects not only the present population of the United States, but future generations as well. Any attempt to shield the facts from public scrutiny is inexcusable."

The 44-year-old Oregon Senator, sponsor of a bill to establish a National Radiation Health Institute, said that the AEC owes the public an explanation as to why the report was not previously released. "Full disclosure of unclassified information regarding radiation exposure is vital to establishment of informed public opinion and the rational discussion of this issue which looms paramount in the minds of many Americans" he said.

In view of the fact that this release was made during these hearings, and in view of the fact that there has been no evidence presented to the committee that there has been such a study or report, I will ask you at this time to comment on this in order that the Atomic Energy Commission may have an opportunity to state its views.

Dr. DUNHAM. Thank you very much, Mr. Holifield.

First I would like to make it perfectly clear that there is no such report. The Commission has never asked that the Advisory Committee on Biology and Medicine make a report.

On the other hand, as I indicated the other day in testimony, during the last 2 years there has not been a meeting of that committee at which the matter of the fallout studies was not discussed, and a brief bringing up to date of the commitee on the facts given by a member of my staff. The committee has considered this matter very, very carefully. They have as yet prepared no statement on the subject.

Representative HOLIFIELD. There has been a study made of this subject?

Dr. DUNHAM. Not by the committee as such. They have reviewed the status of the material being developed at each of their meetings each year. When on 1 or 2 occasions I neglected to put this item on the agenda, one or another of the Commissioners called my attention to it, and asked that it be put on the agenda, bringing the committee up to date on this information.

Representative HOLIFIELD. Has this committee been presented with the results of your studies on strontium 90?

Dr. DUNHAM. Yes. They have been presented, as I say, at every meeting, and they have been brought up to date on the new material as it developed.

Representative HOLIFIELD. I am speaking of the congressional committee. Has this committee been presented with all of the pertinent information in regard to strontium 90 which is in possession of the AEC?

Dr. DUNHAM. I believe this is so. With the material introduced in the record by Dr. Libby and material presented by other people who have appeared before you, I think this constitutes the present body of knowledge of the Atomic Energy Commission on the contamination of the environment by strontium 90.

Representative HOLIFIELD. Is there an area of knowledge on the subject of strontium 90 which is in the classified area and which has been or will be presented to the committee in executive session?

Dr. DUNHAM. The only material pertinent to this question which is classified is that to do with actual fission yield of specific devices. This material, I believe, has been in the hands of the committee. You are brought up to date at each series of tests as to what these fission yields are.

Representative HOLIFIELD. So there has been no withholding of your overall calculations as to the amount that is in the stratosphere, and which has been deposited, according to your measurements, in various places throughout the earth's crust?

Dr. DUNHAM. That is right. There has been no withholding at all.

Representative HOLIFIELD. That information, to the best of our present ability to measure, has been presented not only on the continental United States, but other measurements in other lands?

Dr. DUNHAM. Yes. We have presented you with all the material we have developed.

Representative HOLIFIELD. Are there any further questions on this point?

Chairman DURHAM. Then it is a fact, Doctor, that all the information has been made public through the present hearings and other methods?

Dr. DUNHAM. That is correct, sir.

Representative HOLIFIELD. Dr. Dunham, we will now let you read your statement.

STATEMENT OF DR. CHARLES L. DUNHAM, DIRECTOR, DIVISION OF BIOLOGY AND MEDICINE, ATOMIC ENERGY COMMISSION

Dr. DUNHAM. Thank you. I would like first to say what I am going to do is to review briefly the present program of research of the Division of Biology and Medicine which relates to fallout. I understand the committee has invited representatives of other Government agencies who are making very important contributions to the general information on this subject to present their programs. Therefore, I will let them speak for themselves.

I have with me today, Dr. Shields Warren, from the New England Deaconess Hospital. I would like to call upon him, after I have finished, to speak from the standpoint of a university scientist, plus his great background in this particular area. Also, Dr. Austin Brues, who is the Director of the Division of Medicine and Biology of the Argonne National Laboratory, to speak to you from the standpoint of a director of such a program at one of our national laboratories.

That part of the research program of the Division of Biology and Medicine which relates to fallout falls into four major categories:

1. Collection and analysis of samples to determine distribution of strontium 90 in the atmosphere and the biosphere.
2. Further refinement of our knowledge of the radiotoxicity of strontium 90 and cesium 137 and other radionucleides and of the long-term effects of external gamma radiation.
3. Research into methods of treating and ameliorating radiation injury.
4. Civil effects test programs at weapons tests.

With the exception of the activities of the Health and Safety Laboratory in New York, all work is done by contract either at national laboratories or at private laboratories, both university and commercial, or with Government laboratories by transfer of funds to Naval Radiological Defense Laboratory, Naval Research Laboratory, the Air Force, and so forth.

With the exception of experimental studies done at weapons tests, none of this work is classified even at the time it is done. In fact, we have made deliberate efforts to see that this is so, so as to avoid delays in making the information available. That work undertaken at weapons tests is now carefully planned so that insofar as possible the first preliminary reports are written in unclassified form and the final complete reports whenever possible are unclassified or unclassified versions are written simultaneously.

I. COLLECTION AND ANALYSIS OF SAMPLES

The gummed-paper network of the Health and Safety Laboratory now comprises 94 stations in the United States and 75 in foreign countries. Stainless-steel pot collections are being made at 7 stations in the United States and in the following countries: Hawaii (which is not a country), Chile, French West Africa, Austria, Union of South Africa, Thailand, South Rhodesia, Peru, Pakistan, Kenya, Japan (2), Colombia, and Brazil. This network is to be expanded as cooperative arrangements are developed with other countries.

Soil sampling on a worldwide basis is under Dr. Lyle Alexander, United States Department of Agriculture, and collections in 17 countries were made in 1955 and in 39 countries in 1956 and annually or more frequently collections are made in various parts of the United States. These are supplemented by soil collections in the New York area by the Lamont Laboratories of Columbia University. In the future, we hope to be able to undertake simultaneous soil sampling in North and South America on an annual basis.

Sampling of the Pacific Ocean in relation to weapons tests is extensive, and is supplemented by excellent Japanese sampling and analysis, with frequent exchanges of data.

The ocean is large, and we cannot sample the whole ocean, but we do endeavor to follow the radioactive material as it moves along with the current. In cooperation with such organizations as the NORPAC and the Japanese, we have been able to get quite a lot of information on the movement of these water masses.

Followup studies on the Rongelap Atoll are annual, and include analysis of soils, edible plants, animals, and seafood. Both the Naval Radiological Defense Laboratory and the University of Washington

School of Applied Fisheries, and the biological laboratories established by the AEC at Eniwetok have taken part in this with assistance from the Walter Reed Medical Center. The medical followup on the people of the Rongelap and Uterik Atolls is under the direction of a group at Brookhaven National Laboratory with assistance from the National Institutes of Health, Walter Reed Medical Center, and the Department of the Navy.

The human bone sampling program is largely carried out by the Lamont Laboratories in New York City with supplementary collections by the Argonne Cancer Research Hospital and other groups in this country. Analyses are carried out by commercial laboratories with careful cross checks by Lamont Laboratories and by the Health and Safety Laboratory of AEC for analytical accuracy.

Representative HOLIFIELD. I understand that Dr. Schulert is here to talk about the bone samples.

Dr. DUNHAM. That is fine.

Stratospheric sampling: Techniques are being developed to make possible the monitoring of radioactive fission products in the stratosphere. Such measurements would provide important information on quantities of weapons debris reaching the stratosphere, the distribution and retention of such materials in the stratosphere, and their release to the lower atmosphere. In experiments now being conducted with the Department of Defense, balloons were used to carry sampling equipment to altitudes of 50,000 to 90,000 feet, where radioactive particles were filtered from a defined volume of air. Balloons were being launched at Minneapolis, Minn., San Angelo, Tex., and at France Air Force Base in the Panama Canal Zone.

Radiochemical analyses of the samples are presently being made on a pilot scale by the Commission's Health and Safety Laboratory, New York, until arrangements can be made with commercial laboratories to perform this work. Results of these studies will be useful in planning a worldwide network for the stratospheric monitoring of long-lived radioistotopes.

In addition, we have work at the Midway Laboratories in Chicago in attempting to develop a more adequate method of sampling. The present sampling devices are filter devices and, although we believe they collect 25 percent of the material present in the stratosphere, we want to be able to be sure we are actually getting all of the material from a given volume of air.

The milkshed sampling program includes regular sampling in the New York and Chicago milksheds, with less frequent sampling by the Health and Safety Laboratory and the Los Alamos Scientific Laboratory of other milk supplies.

Food sampling in the United States is related to milksheds and the location of United States agricultural experiment stations and carried out largely under Dr. Alexander in the United States Department of Agriculture.

Representative COLE. May I interrupt here, Dr. Dunham, to ask you to explain to what extent you conduct continued sampling of milk in the area of the Nevada tests?

Dr. DUNHAM. We have some samples of that.

Representative COLE. The reason for the question is that, presumably, the fallout would be greater in the area near to the Nevada test.

Dr. DUNHAM. Yes, sir.

Representative COLE. Some people may feel and I am curious why you select the Chicago and New York milksheds for your milk tests.

Dr. DUNHAM. Those are the two largest milksheds, of course. As to the milk within hundreds of miles of the Nevada test site, much of that is imported. Some is developed locally. But the Los Alamos Scientific Laboratory has been sampling milk in communities there.

Representative COLE. That answers the question. The Los Alamos Laboratory is the agency responsible for sampling the milk in the area of the Nevada test.

Dr. DUNHAM. That is correct, sir.

Representative COLE. That is, I assume, a reasonably continuous sampling process?

Dr. DUNHAM. That is correct. We are in the process of initiating a worldwide food sampling program with the assistance of the Interdepartmental Committee on Nutrition, aimed at checking especially the food constituting the principal source of calcium in the diet. Analyses will be by the Health and Safety Laboratory or contracted out.

There are two ways in which the information thus developed reaches the public: One, as unclassified Atomic Energy Commission reports, second, the publication in the scientific literature. The material often appears first as an unclassified AEC report and later is published in a scientific journal. This is in part because of backlogs of accepted papers in the better scientific journals. This material is also disseminated to the United Nations Scientific Committee on Radiation Effects as soon as it has become available in printed form to United States scientists. It is our hope that many of the worldwide collections and analyses can be done in the countries of origin. The AEC Health and Safety Laboratory is training scientists from a number of countries in these techniques. Pending their taking over, we are getting excellent cooperation from the countries which have expressed an interest in these matters. Meanwhile we are analyzing any samples submitted from foreign countries.

Representative VAN ZANDT. At this point, Dr. Dunham, is it not true that spokesmen from the AEC are constantly making speeches and copies of the speeches are given a pretty good coverage, and they contain firsthand information taken from the files of the AEC?

Dr. DUNHAM. That is one of the reasons these speeches are given. By the time you get the reports accepted and printed by the scientific journals, much time has elapsed. Many of these speeches are done deliberately to get the material out as fast as it is available.

Representative VAN ZANDT. I might say I read these speeches religiously and as a result I feel I am being kept up to date in regard to the developments in this field.

Representative COLE. Let me say in response to that that if my good friend, Mr. Van Zandt, reads all the speeches coming to his desk emanating from the Atomic Energy Commission, he does not do anything else.

Representative VAN ZANDT. I am speaking about radiation hazards.

Representative COLE. Because they come to my desk, and I admit that I do not read them all. They come too fast.

Representative VAN ZANDT. I agree with my colleague from New York. I am especially interested in the radiation hazard.

Dr. DUNHAM. I know you have a special interest in that, Congressman.

The worldwide and national bone sampling program will be stepped up as more sources of material can be found.

II. RADIOTOXICITY STUDIES

The radiotoxicity studies include extensive small animal studies at the University of Rochester atomic energy project, Argonne National Laboratory and Los Alamos Scientific Laboratory. Lifetime studies in dogs at the University of Utah to compare in mature animals radium, plutonium, mesothorium, and strontium 90 have been underway for 6 years. The University of California, Davis Campus, is just commencing a womb-to-tomb strontium 90 experiment in dogs. That means the mothers are fed strontium 90, and as soon as the pups are born, they are given strontium 90 throughout their whole lifetime. That is the way that experiment is planned.

Representative COLE. Doctor, would you tell me why dogs are selected for this experiment, rather than any other animals?

Dr. DUNHAM. Yes. We have quite a lot of information on mice and rats. But these substances we are talking about here are bone seekers. A rat bone or a mouse bone never really ceases to grow. Furthermore, they are very short-lived animals. It is a matter of a year or 2 or 3 at the most. Dogs are chosen because they are longer lived and, secondly, because their bone development and maturation follow very closely the human pattern. So when they are mature and the epiphyses have closed the bone structure of those dogs resemble exactly what we know to exist in the radium dial workers and the people who were given radium as treatment which are the basis of our information on radiotoxicity of internal emitters in man.

Representative COLE. What is mesothorium?

Dr. DUNHAM. Mesothorium is one of the products in the decay scheme of the thorium series. It is the material which Dr. Looney mentioned as being in the radium dial paint, and which has confused some of the data there because the early work was done sort of like somebody's cook. They might put more or less mesothorium in the mixture. Dr. Evans' group at Cambridge has worked very hard and now I believe has developed a method which he feels is satisfactory so he can work back on the living patients and estimate how much of the thorium series was introduced with the radium. This has been a very difficult problem.

Three more large-scale dog studies are planned with strontium 90, cesium 137, and mixed fission products.

Studies on the discrimination between strontium and calcium in animals and humans are underway at the Oak Ridge Institute of Nuclear Studies, University of Rochester, University of California at Los Angeles atomic energy project, Sloan-Kettering Research Institute, and Montefiore Hospital in New York. Additional work will be undertaken by the Argonne National Laboratory and the Argonne Cancer Research Hospital as low-level counting facilities become available at Argonne National Laboratory.

III. TREATMENT OF WHOLE BODY RADIATION INJURY AND METHODS OF AMELIORATING RADIATION EFFECT

Currently there are three approaches to a more specific treatment for whole-body injury:

1. Prophylaxis: This approach is useful only if given prior to exposure. In other words, it would have no practical value in the event of an atomic catastrophe except if a person knew he had to go in and take two or three hundred roentgens, he would be able to take some prophylactic measure to reduce his injury.

Representative VAN ZANDT. Dr. Dunham,, would it be possible to innoculate military personnel with this preventive, whose duties kept them in close proximity to reactors on board naval vessels?

Dr. DUNHAM. What we have been working with now, which shows promise, you have to take a few hours before exposure. It is questionable whether a person could take it indefinitely so I don't think it would be useful there. But if a person had to go in in the event of an accident and had to take 300 roentgens to save a person's life, this would be very useful.

Representative VAN ZANDT. What would be the life of this preventive?

Dr. DUNHAM. It can be taken in experimental animals. This is the point I want to make very definitely. This can be taken for a matter of several days. You have to begin and give a good-sized dose within a few hours prior to beginning of the exposure.

Chairman DURHAM. What is the combination of the bromide?

Dr. DUNHAM. That means that this long name here is a salt just like sodium chloride. The chloride is what makes it the salt.

Chairman DURHAM. I could not tell from that whether it was a combination of 3 or 4 other salts or not.

Dr. DUNHAM. It is the bromide of this compound.

Representative COLE. Is there reasonable reason for encouragement in this field of pretreatment?

Dr. DUNHAM. Yes. We found this particular material, which I abbreviate AET, developed at the Oak Ridge National Lab to be quite effective in essentially doubling the resistance of rats, mice, and monkeys. The only question is what is the toxicity of the material in man? We found in dogs they do not tolerate it. You cannot give enough to protect the dog and similarly the rabbit. This will have to be determined and some studies are being done. Some work is being done using this material at the National Institues of Health and other hospitals, not in the doses required to protect, but in the treatment of what is called radiation sickness, the nausea and vomiting that goes with radiation therapy, so we can begin to get a feel of how sensitive human beings are to this material.

We don't know at this point. It may be like one of the new drugs for TB. You can cure monkeys because they are 30 times less sensitive than humans to the drug. But you can't give enough to the human to totally wipe out the disease.

Representative VANZANDT. How much of the dose could the body of of the animal absorb if this preventive was applied beforehand?

Dr. DUNHAM. I don't recall the exact doses for any specific species of animals. It is a matter of a few hundred milligrams. I don't recall the exact dose.

AET (S-B-aminoethylisothiouronium bromide) pretreatment developed at Oak Ridge National Laboratory holds great promise. Its toxicity in humans has yet to be fully evaluated. In experimental animals it will roughly double resistance.

2. Treatment which is effective if given up to 48 hours after exposure begins: (*a*) Bone marrow transplants have proven very effective in a variety of mamalian species—mice, rats, and monkeys. Intensive efforts to establish the value of this treatment in humans suffering from aplastic anemia are currently underway. There are several hospitals in this country which are attempting to do this in humans who have had their bone marrow injured by radiotherapy which was necessary to treat a cancerous condition. This has only been going on for a few months. I think we will have a pretty good feel for this 6 months from now as to whether this is a feasible approach.

Representative VAN ZANDT. Dr. Dunham, some years ago at Los Alamos there was an accident that involved Dr. Graves and another physicist.

Dr. DUNHAM. Yes.

Representative VAN ZANDT. The other physicist died as a result of the dose.

Dr. DUNHAM. That is correct.

Representative VAN ZANDT. How much knowledge did the Commission gain as a result of that accident which caused the death of the physicist?

Dr. DUNHAM. I would say that the report which has been published in the Annals of Internal Medicine of both accidents—there were 2, 1 a little earlier in the game, and 1 about 1945—constitutes the most carefully documented, from a scientific standpoint, group of cases of whole body radiation exposure, complicated by skin burns, that exists anywhere.

Representative VAN ZANDT. Do you recall the name of the physicist?

Dr. DUNHAM. It was Dr. Sloton who died in one accident. The other one was an Armenian name. Dr. Sloton is the one I think you are thinking of.

Representative VAN ZANDT. How long did he live after the accident?

Dr. DUNHAM. About 3 or 4 weeks.

Representative VAN ZANDT. Can you describe the body reaction to the exposure?

Dr. DUNHAM. What essentially happened is that he had his hands quite close to the criticality experiment that got out of control, so he received probably 60,000 to 80,000, or better, rep equivalent to the skin and forearms. He received a very extensive burn. It was almost as though you immersed his hands in a tub of boiling water and held them there for some time. In addition there was a complete cessation of the formation of all the blood constituents. That was the whole body effect of the mixed gamma and neutron irradiation.

Representative VAN ZANDT. Was he unconscious immediately after the accident?

Dr. DUNHAM. I don't recall. I don't think anybody was unconscious immediately after the accident.

Representative VAN ZANDT. I understand there was ample time to study the effects?

Mr. DUNHAM. These were done by Dr. Louis Hempelmann, who is now at Rochester. I might say that this matter of bone-marrow

transplants was originally developed by Dr. Lorenz, the late Dr. Lorenz, at the National Cancer Institute, who was working during the Manhattan District days very closely with Dr. Leon Jacobson at the Argonne National Laboratory.

(b) Another approach is to concentrate an active substance believed to be present in pregnant cow's blood. This is a very elusive material and all attempts to concentrate it to date have failed. It is fragile and very difficult to keep in its original state. There is no question that there is something in pregnant cow's blood which does convey a certain degree of protection after injury has occurred.

III. REMOVAL OF RADIONUCLIDES FROM THE BODY

As to the removal of radionuclides from the body, and this is of prime interest with respect to fallout and strontium 90, whereas we now have two very valuable drugs, EDTA (ethylenediamine-tetraacetic acid) and zirconium citrate which will divert plutonium in the blood stream away from bone and into the urine. No known preparation will do this for strontium. We have nothing which will selectively remove important amounts of either plutonium or strontium once it is deposited in the bone. Work in this field has been discouraging; nevertheless scientists at the Los Alamos Scientific Laboratory and at the Montefiore Hospital in New York are working hard at the problem.

We have a few projects studying skin burns induced by beta radiation as well as the followup studies on the Sandstone skin burns, and the skin burns in the Rongelapese. There are also a series of studies dealing with the problem of inhalation of radioactive materials. In addition to all this we have a large effort in the genetic effects of radiation and in the biochemical effects of radiation which will provide basic knowledge of the mechanisms of injury which in turn should permit a more efficient approach to the problem of combating radiation effects.

IV. CIVIL DEFENSE ASPECTS

As to the methods of coping with fallout, they fall into two groups.

1. Adequate shelter during maximum gamma radiation hazards: The Division of Biology and Medicine has, since 1952, taken advantage of each Nevada test series to develop information on shelter design. This work is done in cooperation with the Federal Civil Defense Administration and at Operation Plumbob the FCDA is supporting practically all of the work on shelters.

2. Decontamination: This is relatively easy with ordinary detergents and water for small objects and structures. For larger areas it is an economic problem. One approach is to fix with asphalt spray, then bulldoze off. For very large farm areas this becomes very difficult and one must rely on time for decay and deep plowing for more uniform mixing, thus diluting available strontium-90. Small areas of low-calcium soil can be treated with calcium to reduce the strontium-90–calcium ratio. Milk can be treated similarly up to a point, or one can simply remove all calcium and strontium by ion exchange resins and add uncontaminated calcium. Continuing the initial studies at Upshot-Knothole and at Operation Teapot, Operation Plumbob will develop further information on contamination of soils, crops,

and foods, shielding attenuation factors for various structures and structural configurations, and evaluation of decontamination procedures under field conditions.

Mr. Chairman, at this point I would like to ask permission to introduce into the record a summary of the Operation Plumbob civil effects test group project summaries, dated 1957. I would like also, with your permission, to introduce into the record a letter which I wrote you on May 28 outlining the shelter program at Operation Plumbob.

Representative HOLIFIELD. I am glad you are presenting that. I had intended to ask you to, or have Mr. Corsbie do so.

(The material referred to follows:)

MAY 28, 1957.

Hon. CHET HOLIFIELD,
Chairman of the Special Subcommittee on Radiation of the Joint Committee on Atomic Energy,
Washington, D. C.

DEAR MR. HOLIFIELD: This is in reply to your request of May 27, 1957, for information on the number of shelters, and sponsors thereof, being tested in the current weapons-test series.

The program of the civil effects test group includes studies on the blast effect on prototype shelters and test structures, the latter being tested principally to obtain engineering design data. There are 3 reinforced home-type shelters for a few persons; 1 mass shelter capable of holding large numbers of people and serving the dual function of a parking garage; 3 concrete dome structures for obtaining engineering design data; 1 industry-financed prototype vault for records storage; 1 industry-financed windowless reinforced clay-masonry structure; 9 shelters similar to design and capacity of the German structures; and 3 prototype entranceways to test blast-resistant doors, ventilation equipment, et cetera, for France.

All of the above structures are being tested under the sponsorship of the Federal Civil Defense Administration and, except for the industry-sponsored and foreign shelters, are being financed by FCDA.

In addition, the AEC is continuing some blast biology studies, using two shelters which were included in the Teapot series of 1955. These were tested at pressures of approximately 100 pounds per square inch. They are being used to supply further data on biologically acceptable criteria for open shelters.

Also, I am enclosing copy of Mr. Corsbie's remarks prepared for the press briefing in Nevada, which explains in more detail the civil effects test program.

Sincerely yours,

C. L. DUNHAM, M. D.,
Director, Division of Biology and Medicine.

[Preseries briefing, May 1957]

NEVADA TEST ORGANIZATION

OFFICE OF TEST INFORMATION

Las Vegas, Nev.

REMARKS BY ROBERT L. CORSBIE, DIRECTOR, CIVIL EFFECTS TEST GROUP

Organizationally, the Civil Effects Test Group is one of six scientific and technical units reporting to the test director and is a counterpart of the Military Effects Group. It is principally sponsored by AEC and FCDA. Other Government agencies, some private industrial groups, and two foreign nations have projects in this program. The foreign nation and industry projects are proposed and sponsored by FCDA. There are financial arrangements between the participants and the Civil Defense Administration.

The scientific and technical content comprises 10 programs, 54 projects, about 200 shot participations, and requires at NTS a peak population of around 400 scientific and staff personnel over the operational period of several months.

All projects are reviewed by the appropriate scientific and technical test screening and planning committees before acceptance for field testing, and are coordinated with the military effects tests.

The continuing need for effects information parallels and keeps pace with new developments in weapons. The civil effects program stems from this continuing need for up-to-date information on the weapons effects given by a family of nuclear weapons. Our weapons development tests afford an opportunity to augment laboratory experiments with new and useful knowledge from nuclear detonations. Continental tests afford unusually good opportunities to verify in the field various theoretical concepts and laboratory programs which are directed toward complete knowledge of effects on man.

The six general areas of study in the Plumbbob program are as follows:

1. Fallout radiation
2. Prompt-gamma and prompt-neutron radiation
3. Blast effects on structures
4. Blast biology studies
5. Radiological countermeasures and training
6. Instrumentation and supporting services

FALLOUT STUDIES

The fallout studies for Plumbbob represent a continuation of work begun by the Atomic Energy Project of UCLA in the study of the fallout from the Trinity test in 1945. The main purposes of these studies include:

(*a*) Learning to control the availability of radioactive fallout to plants, animals and man;

(*b*) Defining accurately the limits of environmental radiation that can be safely tolerated. Such studies are indispensable also for establishing safety criteria for weapons testing program.

PROMPT-GAMMA AND PROMPT-NEUTRON RADIATION STUDIES

It is necessary to study prompt-neutron and prompt-gamma radiation to obtain data on shielding necessary for shelter design. Better understanding of biological effects of radiation is possible through the advances in gamma and neutron dosimetry. Radiation dosimetry now available makes it possible to measure radiation doses in the field with an accuracy which equals or exceeds measurements in the laboratory. Laboratory and field experiments relating to radiation effects on animals have pointed up the necessity for a better-defined relation between biological effects on animals and on humans. This has reemphasized the value and foresightedness of the medical studies through the Atomic Bomb Casualty Commission that have been underway in Japan since 1946. The ABCC files contain clinical records of more than 4,000 well-documented cases of survivors. These afford data which would be more meaningful to all radiation medicine in the world if we knew the varying doses that individual cases received under the known shielding conditions and distances that prevailed.

During Operation Plumbbob we will initiate studies that are expected to establish the angular distribution of radiations at several distances in air so that total effect on persons inside structures can be evaluated. These basic data will permit the next phase of a long-range program to begin. This will be the determination of the attenuation and scattering by structural and other shielding materials, including terrain. Around 65 percent of the survivors whose cases are adequately documented were shielded in light wood houses of unique construction and geometry, and it is therefore necessary and important that these materials and geometrical configurations be studied to evaluate the shielding afforded to individuals in such structures.

The results of the Plumbbob studies, plus subsequent pilot and laboratory experiments, will permit detailed planning for a third phase of the overall program, the end-product of which is expected to establish the individual doses in terms of gamma and neutron radiation for the medical records developed by ABCC through 10 years, study of survivors. The benefits that could result from this are enormous. Obtaining new data on doses, and through use of the ABCC medical records, their subsequent effects may well provide information that will permit the administration of improved treatment of radiation effects on man. Two of the major laboratories of the AEC are cooperating on this program. They are Oak Ridge National Laboratory and Los Alamos Scientific Laboratory. In addition, the Division of Biology and Medicine of the AEC is being assisted in

this program by the Brookhaven National Laboratory, the Air Force School of Aviation Medicine, the Army Surgeon General's Office, and the Naval Medical Research Institute.

BLAST EFFECTS ON STRUCTURES

Studies of blast effects on shelters and structures are sponsored principally by FCDA. Included are reinforced-concrete home-type shelters for a few persons and so-called mass shelters, capable of holding large numbers of people as well as performing dual functions, such as serving as a parking garage in ordinary times. Also, there are some industry-sponsored items; namely, a protective vault for records, and a windowless reinforced-clay masonry structure. Three reinforced-concrete dome structures are being subjected to overpressures in several ranges to obtain engineering design data for use in future mass shelter designs. Through a United States architect-engineering firm and FCDA, France and West Germany are testing a number of shelters of their design. This represents the first time other nations have included structures or otherwise participated in the Civil Effects Test Group. In addition, a variety of valves, devices, and equipment will be tested for use as shelter components.

BLAST BIOLOGY

The present state of knowledge makes mandatory further studies relating to blast biology. These investigations are being carried out by the Lovelace Foundation and are directed toward obtaining more information on the primary, secondary, and tertiary effects of blast. They are a continuation of the work begun during 1953–55, where together with other valuable data for the first time a means was devised of obtaining usable information on numbers and types of missiles (flying bricks, timber, glass, etc.) per unit area and on the penetrability of glass and masonry fragments and other small missiles likely to be produced in an urban area that has been subjected to a nuclear blast. It is expected that the studies during plumbbob will provide equally valuable information on the problems associated with biomedical effects of static pressures and dynamic pressures sufficiently strong to translate bodies the size and weight of a man from a state of rest to a state of motion.

COUNTERMEASURES AND TRAINING

One of the most important new programs that will be initiated during Plumbbob is work by the Naval Radiological Defense Laboratory on countermeasures against fallout radiation. The proof-testing of radiological shelters and typical buildings is expected to produce data useful in practical applications and guidance for planning a long-range program on methods of survival and continuing occupation of areas that have been subjected to heavy radioactive fallout. This program is designed to provide confirmation and applicability of laboratory theories and methods of decontamination to the large-scale recovery of areas contaminated by radioactivity and in addition to develop data on scaling from low yield to megaton detonations.

Our program includes several training exercises and an offsite radiological defense project of especial importance to civil defense. In addition, other projects will include the field testing of aerial monitoring equipment, and the indoctrination and training of radiological defense personnel drawn generally from State and community civil defense organizations. The field testing of commercially produced radiation detection instrument is included in these projects.

INSTRUMENTATION AND SUPPORTING SERVICES

The instrumentation and supporting services concerned with radiation, blast, thermal, technical photography, are self-explanatory and are designed to give necessary measurements and records to project authors for use in reaching conclusions and in the preparation of technical reports. To supplement and make these physical measurements more meaningful, biological materials, and dosimeters are used to provide data for correlating exposures with effects and extrapolating the findings to probable physical effects on human beings. This requires for Plumbbob several species of animals such as mice, guinea pigs, primates (monkeys), dogs and swine—all of which you will recognize as routinely used in biomedical research.

SUMMARY

It is emphasized that probably the most significant aspect of the Civil Effects Test Program, Operation Plumbbob, is the coordination of continuing laboratory research and less frequent test activities in planning projects to provide information essential to an adequate understanding of nuclear effects on life in all its phases. This coupling provides a continuous flow of basic data usable in immediate practical application and in planning of future research into the means of national self-protection, individual survival, and accommodations of medical practice to the atomic era.

In any case, peace or war, we are already well into the atomic age. Learning to live with the byproducts of nuclear reactions is now necessary and urgent. In addition to the necessity for developing military strength, the weapons testing programs furnish a unique opportunity for providing indispensable information to this end. Additionally, opportunity is provided for the training of key personnel in the theoretical and practical aspects of dealing with environmental radiation in the great variety of situations in which it occurs.

Dr. DUNHAM. He is so busy at the tests. I have also the remarks he made at a press conference at the beginning of Operation Plumbob, which again outlines the civil-effects program.

Representative HOLIFIELD. I think it is very important that the American people know that these tests in Nevada have a much greater effect than just the testing of weapons. There is the testing of materials, the learning of how to protect from radiation, the effect on animals and all of these things which go toward furthering our knowledge of radiations involved in these Nevada tests.

Dr. DUNHAM. That is very true. It not only gives us basic knowledge for all radiation problems, but as long as there is a possible threat of nuclear warfare, it gives us information that is absolutely vital for the defense of the country.

Representative HOLIFIELD. I think the listing of the research projects which you have made in this speech and also the listing of contracts, which are another part of the record, will assure the American people that there is a deep concern and a very wide range of studies in these fields, and that we are not being careless or indifferent regardless of differences of opinion as to what the effects mean. We are at least trying to scientifically get these effects.

Dr. DUNHAM. Yes.

Representative VAN ZANDT. Dr. Dunham, in connection with the use of water for the purpose of attacking the radiation hazard, is that possible? What I have in mind is a pressurized water hose being used to wash off the contamination.

Dr. DUNHAM. That is no question you can hose the fallout material off. Certainly the type of material which came down in the Marshall Islands, which was visible large particles, one could get rid of a great deal by ordinary pressure.

Representative VAN ZANDT. What about the ordinary sprinkler?

Dr. DUNHAM. You have to watch that if you hose it off the roof and sides of the building, and it collects at the edge of the building, you may actually build up more radiation intensity in the building than if you left it alone. One actually has to move it some distance. This becomes quite a problem sometimes.

Representative VAN ZANDT. I understand this practice has been very effective aboard ship.

Dr. DUNHAM. Very effective. I think NRDL has done extensive studies.

Representative HOLIFIELD. I think that distinction between a ship and land is very important because as you say the removal from the walls or roofs of the building means that you are removing it into the soil where people walk.

Mr. DUNHAM. That is right.

Representative HOLIFIELD. Previous testimony from, I believe, Dr. Alexander from the Agriculture Department, was to the effect that it was very difficult to leach this material from the soil.

Dr. DUNHAM. That is correct. It takes years.

As I see it, the areas of greatest immediate need for information are:

1. Better predictability of the properties of nearby strontium 90 fallout: By that I mean whether it is soluble or relatively insoluble, and there is evidence that the near-in fallout is less soluble, and would be less available to plants than the fine particles—better predictability of the properties of nearby strontium 90 fallout, that having tropospheric dissemination, and that getting into the stratosphere for each type of weapon and for each circumstance of burst (ground, low air, high air, etc.). This is very difficult and can only be learned at weapons tests.

2. Amount and distribution of strontium 90 and other fission products in the stratosphere and more precise estimates of holding time in the stratosphere: We will know much more about the first of these in a year's time.

As Dr. Libby indicated, we will know considerable more about that in a year's time as a result of our balloon sampling.

3. Strontium 90 toxicity: In 10 to 15 years we will have experimental data in dogs which will firm up the maximum permissible body burden of strontium 90 for all ages and will have determined whether strontium 90 is ever leukemogenic in dogs and presumably in man, and will have gone a long way to settling whether or not the bone tumor effects of strontium 90 have a threshold.

4. True doubling dose for mutation rate in human germ cells; this will take many years to accomplish, if ever.

5. Tolerable mutation rate for the human race: I use the word "tolerable" advisedly. This again will take many years and will never be exact because of human social patterns.

6. Leukemogenesis and effects on life span by low level external radiation exposures: A study on the latter in several thousand mice at Argonne National Laboratory will be completed in 2 to 3 years. A small scale study in dogs has been going at the University of Rochester for 6 years. This has another 8 to 10 years to go. Related to this is an intensified effort to get more acurate estimates of the true exposures of the Japanese irradiated at Hiroshima and Nagasaki. We attack this from two standpoints. One is to get more exact information about the exact location of the survivors, the type of structures, by aerial photography, and by studies now going on in Nevada attempting to get a much better idea as to what the shielding properties of different materials are, housing materials and the like, to the various types of mixtures of neutrons and gamma rays characteristic of the two weapons detonated in Japan.

I have transmitted separately a more detailed summary statement of the Atomic Energy Commission's biomedical program related to biological hazards of radiation.

(The detailed summary referred to follows:)

U. S. ATOMIC ENERGY COMMISSION, DIVISION OF BIOLOGY AND MEDICINE

Summary—Studies and research projects on fallout problems and related research on the biological hazards of radiation

	Scientific man-years	Amount (in thousands)
1. Sampling and analysis of radioactive fallout, including fission product toxicity and transport:		
(a) Sampling and analysis of radioactive fallout (Sunshine project)	253	[1] $1,193
(b) Research on the biological hazards of radioactive fallout (Sunshine project)		[1] 4,424
2. Effects of radiation on humans, mammals, and other organisms (exclusive of genetic studies)	449	9,168
3. Treatment and methods of ameliorating radiation effects	60	1,338
4. Genetic effects of radiation:		
(a) Studies of human genetics and of genetic effects of radiation on human cells and tissue culture	12	154
(b) Experimental studies of the genetic effects of radiation on species other than man	71	1,314
5. Biochemical and microbiological studies of radiation effects	110	1,472
6. Environmental studies	4	52
7. Dosimetry research: The development of improved methods of measuring radiation	47	1,026
Total	1,006	20,141

[1] A breakdown of these totals appears in the following pages.

In addition to the research programs recapitulated above as concerned with fallout problems and related research on the biological hazards of radiation, the AEC's Division of Biology and Medicine supports a sizable program of research involving the utilization and better understanding of nuclear energies.

This portion of the program involves cancer research and other atoms for peace uses; radioisotopes in medicine; improvement of crops in agriculture; and many other projects less closely related to the fallout problem.

[In thousands of dollars]

Installation	Radiation effects on biological systems (1)	Combating radiation detrimental effects (2)	Beneficial applications (3)	Biomed problems (4)	Dosimetry and instrumentation (5)	Total
1. Argonne Cancer Research Hospital			1,240			1,240
2. Argonne National Laboratory			440		180	620
3. Brookhaven National Laboratory	300		1,197		110	1,607
4. University of California Radiation Laboratory	137	170	1,117		73	1,497
5. University of California Medical School			235			235
6. University of California at Los Angeles	120	182	140	225	50	717
7. GE Co., Hanford Works					286	286
8. ORINS			667			667
9. University of Rochester			115	88	41	244
10. Knolls Atomic Power Laboratory				88		88
11. University of Tennessee			50			50
12. Health and Safety Laboratory, New York				538		538
Total	557	352	5,201	939	740	7,789
13. University and other institution laboratory research projects						3,668
Total, other biological and medical research projects						11,457
Total AEC biology and medicine research program for fiscal year 1957						31,598

SUNSHINE PROJECT: STUDIES AND RESEARCH PROJECTS ON FALLOUT PROBLEMS AND RELATED RESEARCH ON THE BIOLOGICAL HAZARDS OF RADIATION

1. *Sampling and analysis of radioactive fallout, including fission product toxicity and transport*

(A) SAMPLING AND ANALYSIS OF RADIOACTIVE FALLOUT

Institution	Title	Investigator	Number of scientists	Number of supporting personnel	Fiscal year 1957 project amount
Agriculture, U. S. Department of, Soil Conservation Service.	Collection and Preparation of Samples of Soils, Plants and Animals for Calcium and Strontium Analyses.	T. Alexander	3	3	$29,000
Armour Research Foundation of the Illinois Institute of Technology.	Efficiency of Scavenging Devices Used in Determining Fallout.	J. Rosinski	6	1	25,000
California, University of, Scripps Institute of Oceanography.	Proposal for Use of Nuclear Tools in Oceanographic Research.	H. E. Suess and H. Craig	4	1	37,000
California, University of, College of Agriculture	Study of the Decontamination of Soils Containing Radioactive Elements and Salts.	R. Overstreet	6	2	17,000
Chicago, University of, Chicago Midway Laboratories.	Study of High Altitude Sampling Techniques	R. Hogness	4	4	47,000
Columbia University, Lamont Geological Observatory.	Distribution of Certain Fission Product Activities	J. L. Kulp	5	8	105,000
Commerce, U. S. Department of, Weather Bureau.	I. Transport of Bomb Debris, and II. A-Bombs and Weather.	H. Wexler	3	3	43,000
General Mills, Inc	Texas Balloon Flight Operation	H. Demorest	1	1	41,000
	Upper Atmospheric Monitoring Program	J. Gravelle	5	10	221,000
Health, Education, and Welfare, Department of, Public Health Service.	Establishment of a Radiation Surveillance Network	J. G. Terrill, Jr	2	2	38,000
Interior, U. S. Department of, U. S. Geological Survey.	Airborne Monitoring Program				6,000
Isotopes, Inc	Radiostrontium Analysis of High Altitude Filter Paper Samples.	H. L. Volchok	1	1	6,000
	Radiostrontium Analysis	do	3	3	52,000
Navy, U. S. Department of, Naval Research Laboratory.	Radioactivity Monitoring Program	I. H. Blifford, Jr	3	3	20,000
Nuclear Science and Engineering Corp	Radiostrontium Analysis of High Altitude Filter Paper Samples.	R. A. Brightsen	1	1	18,000
	Radiostrontium Analysis	do	3	3	52,000
California, University of at Los Angeles (atomic energy project).	Factors Influencing the Biological Fate and Persistence of Radioactive Fallout—Operation TEAPOT.	K. Larson, R. G. Lindberg, and J. W. Neel.	21	40	125,000
	Phenomenology of Fallout at Near Distance (within 300 miles of Nevada site).	do			
	Radiochemical Analysis of Certain Marine Samples	do			
	Studies to Document the Occurrence of Radioactive Debris Resulting from Weapons Testing Programs.	do			

	Radio-Ecological Survey of Areas Adjacent to NTS and in Selected Areas up to 600 Miles Distance.	do			
	Environmental Decay Studies	do			
	Study of Strontium and Calcium Relationship in Bone, Plant and Soil—Sampling and Analyses.	R. E. Nusbaum	6	7	39,000
	Correlation of Radiation Intensity from Fallout Material and Weapons Yield.	L. Baurmash	10	11	55,000
	Study of Solubility and Radiostrontium Content of Soils from Various Distances from Ground Zero to Determine Variations Due to Fractionation of Radioisotopes.	do			
	Study of Physical Characteristics of Fallout Material	do			
	Phenomenology of Fallout—TEAPOT SERIES	do			
	Study of the Fluctuation of Radioactivity Levels at Selected Sites in California.	do			
	Fluctuations in Radioactivity Levels in Known Contaminated Areas Adjacent to NTS.	do			
	Studies to Improve Methods of Sampling Radioactive Particulate Materials.	do			
	Studies on Biophysical Aspects of Fallout Phenomenology During NTS Test Series—Proposed in 1957 and 1958.	do			
	Theoretical Studies and Preparation of Summaries on Fallout Using Data Accumulated During Past Test Series.	do			
General Electric Co., Hanford operations office	Pacific Sample Analysis	D. L. Reid	3	3	10,000
Health and Safety Laboratory, New York operations office.	Operation of Gummed Film Network, Collection and Counting of Samples; Other Measurements.	I. B. Whitney	1	1	46,000
	Recording and Analysis of Data from Gummed Network	A. E. Brandt	2	2	35,000
	Collection and Analysis of Samples for Specific Radioisotopes, Uptake of Sr-90.	J. H. Harley and E. P. Hardy, Jr.	5	6	53,000
	Analysis of Marine Samples from the Pacific, Including Tuna, Water, Plankton, Coral.	J. H. Harley	3	3	21,000
	Analysis of Field Samples from Operation Redwing and Related Activity.	R. T. Graveson	1	1	4,000
	Analytical Staff Assistance, etc	I. B. Whitney	1	1	20,000
	Basic Developmental Studies in Trace Radiochemistry	J. H. Harley and G. A. Welford.	3	3	22,000
	Special Studies in Evaluation of Hazards Resulting from Radioactive Fallout at AEC and Contractor Installations.	A. J. Breslin	1	1	6,000
Subtotal, sampling and analysis of radioactive fallout.			107	125	1,193,000

SUNSHINE PROJECT: STUDIES AND RESEARCH PROJECTS ON FALLOUT PROBLEMS AND RELATED RESEARCH ON THE BIOLOGICAL HAZARDS OF RADIATION—Continued

1. Sampling and analysis of radioactive fallout, including fission product toxicity and transport—Continued

(B) RESEARCH ON THE BIOLOGICAL HAZARDS OF RADIOACTIVE FALLOUT

Institution	Title	Investigator	Number of scientists	Number of supporting personnel	Fiscal year 1957 project amount
Agriculture, U. S. Department of, Soil and Water Conservation Branch.	Accumulation and Movement of Fission Products in Soils and Plants.	R. F. Reitemeier	10	3	$30,000
Arizona, University of	Utilization of Phosphorus from Biological Material and Uptake of Strontium by Various Type Crops.	W. H. Fuller and W. T. McGeorge.	2		5,000
California, University of, agricultural experiment station.	Effects of Sr-90 Administered During the Growth of the Dog.	A. C. Anderson	2	2	20,000
California, University of	Study of Internal or Metabolic Factors and the External or Environmental Factors Affecting Ion Absorption by Plants.	L. Jacobson and R. Overstreet.	3	3	10,000
Columbia University, Lamont Geological Observatory.	Studies of Circulation of the Deeper Oceanic Water	M. Ewing	3	4	50,000
Emory University	Long-Range Effects of Radiation on Natural Populations and Communities of the Granite Outcrops.	R. B. Platt	2	3	10,000
Hawaii, University of	Radioisotope Uptake in Marine Organisms with Special Reference to the Passage of Such Isotopes a sare Liberated from Atomic Weapons Through Food Chains Leading to Organisms Utilized as Food by Man.	R. W. Hiatt	3	1	35,000
Idaho State College	Development of Analytical Methods for the Determination of Small Amounts of Strontium, Uranium and Fluoride.	A. E. Taylor	2	2	15,000
Interior, U. S. Department of, Fish and Wildlife Service.	Accumulation of Fission Products by Marine Fish and Shellfish.	W. A. Chipman	5	11	43,000
Jefferson Medical College of Philadelphia	Effects of Radioactive Particulates in Lung Tissues	H. Brieger	3	3	22,000
Johns Hopkins University	Investigation of the Mechanism of Bone Deposition and Related Physiological Studies.	J. E. Howard	4	7	23,000
Kansas, University of	Study of Deposition and Excretion of Bone-Seeking Radioisotopes.	F. E. Hoecker	2	3	20,000
Little, Arthur D., Inc	Studies on the Effects of Natural and Artificial Radioactivity on the Electrical Properties of the Atmosphere.	B. Vonnegut	5		25,000
Marquette University School of Medicine	The Pathological Effects of Radioactive Isotopes of Calcium and Strontium on Bone and Soft Tissue.	J. F. Kuzma	2	2	16,000
Massachusetts General Hospital	Effects of Radioactive Iodine on Biology of the Thyroid Gland.	O. Cope and J. B. Stanbury	5	2	13,000
	The Metabolism of Calcium and Strontium as Disclosed by Tracer Studies on Patients with Thyroid and Related Diseases.	J. B. Stanbury	3	2	20,000

Massachusetts Institute of Technology	Radium, and Mesothorium Poisoning, and Dosimetry and Instrumentation Techniques in Applied Radioactivity.	R. D. Evans	4	5	176,00
Miami, University of	A Study of Factors Affecting the Selective Uptake and Retention of Zn-65 by the Dorsolateral Prostate of the Rat, and a Study of the Role of Zinc in the Physiology of the Prostate.	S. A. Gunn	2	1	11,000
Michigan State University	I. The Absorption and Utilization of Radioactive Minerals Applied to the Leaves of Plants; II. The Absorption and Utilization of Ruthenium by Plants; III. The Leaching of Nutrients from Leaves of Plants.	H. B. Tukey	2	1	25,000
Minnesota, University of	Effects of Radioactive Substances on Plant Pathogens and Other Microorganisms.	J. J. Christensen and E. C. Stakman.	3	7	28,000
Montefiore Hospital for Chronic Diseases	A Study of the Distribution and Excretion of Lanthanum and the Rare Earth Elements.	D. Laszlo	6	3	25,000
	Dynamics of Strontium Distribution in the Body	do	4	3	22,000
Navy, U. S. Department of, Naval Radiological Defense Laboratory.	Radiological Countermeasures System Operations Training and Evaluation.	P. Tompkins	15	17	59,000
	Fallout Distribution and Radiation Contours—Low Yield Detonation.	do			
New Mexico Highlands University	Selective Denuding Action of Atomic Radiation on Vegetation of Some Nevada Sites and Gross Changes in the Surviving Flora.	L. M. Shields	2		13,000
New York, State University of, Research Foundation.	Effects of Irradiation on the Calcifying Mechanism of Epiphyseal Cartilage.	A. Hirschman	2		10,000
New York University, Bellevue Medical Center	The Distribution and Persistence of Radioactive Aerosols in the Lungs of Animals.	B. Altshuler	5	2	19,000
	Tissue Reactions to Intrapulmonary Radiation	M. Kuschner	1	3	15,000
	Aerosol Retention Studies	N. Nelson	5	3	20,000
North Carolina, University of	The Double Isotope Effect of Ca-45 and Sr-90 on the Pattern of Distribution in the Body, Particularly in Bone.	C. D. VanCleave	2	2	10,000
Ohio Agricultural Experiment Station	Detailed Characterization of Soil and Vegetation on Selected Sites to Serve as Basis for Future Evaluation of Effects of Radioactive Contamination.	N. Holowaychuk	3	2	30,000
Pittsburgh, University of	Hazard from Inhaled Radioactive Particulate Matter	H. Cember	3	4	20,000
Presbyterian and St. Luke's Hospital	Mobilization of Radioactive Emitters from Bone	R. D. Ray	2	1	15,000
Rochester, University of	Dynamic Aspects of Skeletal Metabolism	A. R. Terepka	4	2	17,000
Tennessee, University of	The Metabolism of Alkaline Earth Metals by Bone	F. W. Lengemann	1	4	9,000
	Vegetation Studies Related to Disposal of Radioactive Wastes.	R. E. Shanks	4	1	17,000
	Studies in Iodide Metabolism	L. Van Middlesworth	5	2	19,000
Utah State University	Effect of Radioactive Elements and Radiation on Ewes Maintained on Different Levels of Nutrition.	L. E. Harris	3	3	10,000
Utah, University of	Toxicity Studies of Plutonium and Other Radioactive Substances in Animals.	T. F. Dougherty	10	27	250,000
Western Reserve University	A Study of the Physiological Function and Histological Changes of Thyroids Irradiated with Radioactive Iodine.	B. M. Dobyns	2	2	3,000
Woods Hole Oceanographic Institution	Studies on the Background Radiation and Flow of Deep Ocean Currents.	A. B. Arons	2	2	9,000

SUNSHINE PROJECT: STUDIES AND RESEARCH PROJECTS ON FALLOUT PROBLEMS AND RELATED RESEARCH ON THE BIOLOGICAL HAZARDS OF RADIATION—Continued

1. Sampling and analysis of radioactive fallout, including fission product toxicity and transport—Continued

(B) RESEARCH ON THE BIOLOGICAL HAZARDS OF RADIOACTIVE FALLOUT

Institution	Title	Investigator	Number of scientists	Number of supporting personnel	Fiscal year 1957 project amount
Argonne Cancer Research Hospital	Metabolism of Bone Seeking Radio-Elements in Human Beings.	R. J. Hasterlik	2	2	32,000
Argonne National Laboratory	Toxicity of Radiostrontium	M. P. Finkel			
	Study of Sr-90 Metabolism in Laboratory Animals	W. E. Kisielski			
	Toxicity of Calcium 45	M. P. Finkel			
	Toxicity of Sr-90, Ca-45, Sr-89, Ra-226, etc.	do			
	Isotope Toxicology and Pathology, Morphologic Aspects of Radiation Carcinogenesis.	H. Lisco	6	7	
	Study of Radiation Damage in Bone	L. A. Speckman, T. W. Speckman, and W. P. Norris.			
	Dynamics of the Metabolism of the Alkaline Earths in Laboratory Animals and Men.	W. P. Norris	8	9	346,000
	Radioelement Metabolism in Humans	A. E. Stehney and L. D. Marinelli.	3	3	
	Toxicity and Metabolism of Radioactive Ruthenium (Ru-106).	A. M. Brues and H. Walton.	2	1	
	Natural Environmental Radium Levels and Bone Tumors.	H. Auerbach	1	1	
	Radium Levels in Bone Tumor Patients	H. Auerbach and W. P. Norris.	2	1	
	Determination of Permissible Dosage of Isotopes	A. M. Brues	1	1	
	Meteorology Studies	H. Moses and H. A. Schultz.	5	6	
Brookhaven National Laboratory	Meteorological Research	M. Smith and M. Fox	2	2	65,000
California, University of, at Los Angeles (atomic energy project).	Evaluation of Acute Inhalation Hazard from Radioactive Fallout Materials.	G. Taplin	3	3	
	Sr-90 Poisoning and Research for Therapeutic Measures.	N. MacDonald	6	7	
	Fixation and Extractability of Fission Products Contaminating Various Soils and Clays.	E. Romney			
	Effect of Organic Matter on Availability of Fission Products to Crop Plants.	do			
	Effects of Calcium Carrier and Stable Strontium in Plant Uptake Radiostrontium.	do			
	Effects of Cation Balance, Root Temperature, Killing of Roots and Photoperiod on Calcium, Phosphorus and Strontium Uptake by Barley Plants.	do			
	Isolation and Identification of Biochemical Substances in Plant Tissues Labeled with Radioactive Fission Products.	do	13	14	518,000
	Effects of Complementary Ions, Texture and Ph on Plant	do			

	Uptake of Radiostrontium.				
	Chemistry of Fission Products in Plant Tissue	do			
	Effects of Ionizing Radiations from Fission Products on Certain Metabolic Systems in Plants.	do			
	Determination of the Maximum Levels of Fission Product Uptake by Crop Plants Grown on Agricultural Soils.	do			
	Digestibility of Fission Product Elements in the Diet	R. Monroe	6	7	
	Comparative Dynamics of Strontium and Calcium	do			
California, University of, Radiation Laboratory	Radiobiological Studies	J. G. Hamilton	12	13	165,000
	Internal Irradiation and Hematological Response	J. F. Lawrence, et al	4	4	46,000
General Electric Co., Hanford operations office	Absorption of Radioelements into Plants	F. P. Hungate	4	4	60,000
	Fission Product Absorption and Metabolism	R. C. Thompson	4	4	89,00
	Biological Effects of Iodine-131	L. K. Bustad	3	3	100,000
	Effects of Process Effluents on Aquatic Organisms	R. F. Foster	4	4	128,000
	Plutonium Absorption and Metabolism	R. C. Thompson	4	4	94,000
	Toxicity and Metabolism of Inhaled Radioactive Particles	R. W. Wager	7	8	110,000
	Ground Waste Investigations	D. W. Pearce	1	1	48,000
	Soil Chemistry	do	3	3	47,000
	Soil Physics	do	2	2	71,000
	Meteorological Transport and Diffusion	do	3	3	59,000
	Particle Translocation	do	4	4	51,000
Los Alamos Scientific Laboratory	Cesium-137 Studies (and Strontium)	W. Langham	5	6	100,000
Oak Ridge Institute of Nuclear Studies	Metabolism of Radiostrontium in Humans and Experimental Animals.	C. L. Comar	1	1	25,000
Rochester, University of (atomic energy project)	Biological Effects of Internally Deposited Radioactive Isotopes in Rats and Monkeys.	L. Tuttle and R. Baxter	2	2	35,000
	Biological Effects of Inhaled Radioactive Materials	J. Stannard	13	14	190,000
	Mechanism of Bone Deposition, Mobilization and Transport of Radioelements.	W. F. Neuman	18	20	117,000
	Chronic Effects of Sr-90 in the Monkey	L. Tuttle, R. Baxter, and M. Goldman.	3	3	36,000
	Dosimetry of Fission Product Fallout Fields	A. H. Dahl	2	2	21,000
	Plutonium Inhalation Field Study	J. Stannard	3	3	35,000
	Fate of Radium and Its Daughter Products in Living Organisms.	J. Hursh	1	1	15,000
	Aerosol Problems	H. Hodge and K. Lauterbach.	2	1	
	Toxicological Studies of Elements and Compounds of Interest to the AEC.	H. Hodge and E. Maynard	2	1	318,000
	Effects of Heavy Metals on Cells and Tissues	A Rothstein	1	1	
	Biological Transport Mechanisms for Particulates	R. Gosselin	1	1	
	Pharmacology and Toxicology of Thorium Isotopes	J. Stannard	1	1	20,000
	Biological Effects of Internally Deposited Radioactive Isotopes in Rats and Monkeys.	L. Tuttle and R. Baxter	2	1	30,000
Tennessee, University of	Fission Product Metabolism in Animals	J. A. Ewing	1	1	94,000
Washington, University of	Evaluation of Radioactive Contamination in Western Pacific—Radiological Study of Rongelap Atoll, etc.	L. R. Donaldson	14	15	111,000
Subtotal, research on the biological hazards of radioactive fallout.			335	340	4,424,000
Total, sunshine project			442	465	5,617,000

2. Effects of radiation on humans, mammals, and other organisms (exclusive of genetic studies), $9,168,000

Institution	Title	Investigator
California, University of	The Effect of Radiation on Work Capacity and Longevity of the Dog.	A. C. Andersen, G. H. Hart.
Cedars of Lebanon Hospital	Chemical Studies on Connective Tissues of Animals Aged Prematurely by Irradiation (Assessment of Biochemical Age).	H. Sobel.
Chicago, University of	Radiosensitivity of the Lymphocytes	P. P. H. DeBruyn.
	Bacteriological Aspects of Radiation Sickness	C. P. Miller.
	A Study of the Effect on Gastric Tissues of Irradiation Therapy in Peptic Ulcer.	W. D. Palmer.
	The Physiological Factors Involved in Antibody Synthesis and in the Modification of the Immune Process by X-Irradiation.	W. H. Taliaferro.
	The Effect of Whole Body Ionizing Radiation on the Quality of Antibody.	D. W. Talmage.
Children's Medical Center	The Effect of Irradiation on Induction of Pituitary Tumors.	J. Furth.
Columbia University	Effect of Ionizing Radiation on Nerve Tissue	D. Nachmansohn.
Emory University	Effect of Radiation on Learned Behavior, Problem-Solving Ability and Neural Mechanisms of Monkeys.	A. J. Riopelle.
Florida, University of	Effect of Radiation on the Uptake of Large Organic Molecules by the Liver and Spleen of the Mouse.	F. E. Ray, M. F. Argus.
George Washington University.	The Dose-Incidence Relationship of Beta Radiation Induced Skin Cancer in the Rat.	L. K. Alpert.
Harvard University	The Effect of Ionizing Radiations on Peripheral Nerve.	E. L. Gasteiger.
	Radiation Effects on the Lung	S. Warren.
Illinois, University of	A reevaluation of Radiation Injury (B-rays) of the Skin by a Direct Method Approach.	S. R. Rosenthal.
Iowa, State University of	A Quantitative and Morphologic Study of Radiation Induced Cataracts.	T. C. Evans and P. J. Leinfelder.
Johns Hopkins University	The Mechanism of the Activation of Latent Epidemic Typhus Infections in the Laboratory Animals and in Humans by X-ray.	W. H. Price.
Kansas, University of	Immunological Study of Radiation-Induced Damage to Biological Systems.	C. A. Leone.
Kresge Eye Institute	Effects of Neutrons and Other Radiations on the Ocular Lens.	V. E. Kinsey.
Marquette University	Temperature Prevailing During Exposure as a Modifying Factor in the Dose-Response Relationship of X-rayed Mammalian Skin.	J. P. O'Brien.
Massachusetts General Hospital.	A Biochemical Study of the Effects of Radiation on Cells.	P. C. Zamecnik, I. T. Nathanson.
Miami, University of	A Quantitative Study of the Effects of Radiation on the Blood Capillaries of Normal Animals.	E. L. Chambers.
Michigan, University of	Effects of Irradiation on the Localizing Response (to Antigen) of Different Tissues in Immunity.	A. C. Curtis.
Syracuse University	Effects of X-Radiation Upon the Renal-Endocrine System.	W. R. Boss.
Minnesota, University of	Effects of Ionizing Radiation Upon Tissue Metabolism.	W. D. Armstrong, W. O. Caster.
	9Toxic Effects of Irradiation	J. F. Marvin, F. J. Lewis.
Nebraska, University of	Effects of Cranial X-Irradiation on Psychological Processes in Rats.	W. J. Arnold.
Nevada, University of	Range Livestock Production Adjacent to Nevada Proving Grounds.	V. R. Bohman.
New England Deaconess Hospital.	The Effects of Ionizing Radiation on the Developing Mammalian Nervous System.	S. P. Hicks.
	Acute and Chronic Radiation Injury	S. Warren.
New York University	Histochemical Studies of Metabolic Alterations in Rats Receiving Lethal and Sublethal Doses of Radiation, with Emphasis on Terminal Vascular Bed.	B. W. Zweifach, B. P. Sonnenblick.
New York University, Bellevue Medical Center.	Study of the Biological Effects of Ionizing Radiation (Alpha and Beta) on Human Skin.	M. B. Sulzberger.
Nuclear Science and Engineering Corp.	A Toxic Substance Produced by Irradiation	Abraham Edelmann.
Oregon, University of, Medical School.	Studies of Hemic Effects of Radioisotopes, X-rays and of Adrenocortical Hormones.	E. E. Osgood, A. J. Seaman.
Pennsylvania, University of	Changes in the Capillary Fragility and the Colloidal Properties of Blood Following Irradiation.	L. V. Heilbrunn.
Pittsburgh, University of	The Study of the Effects of Radiation on the Immune Response.	F. J. Dixon.
Rochester, University of	Individual Response to Ionizing Radiation in Animals and Patients.	L. H. Hemplemann.

2. Effects of radiation on humans, mammals, and other organisims (exclusive of genetic studies), $9,168,000—Continued

Institution	Title	Investigator
Stanford University	Marine Biological Survey of Western Pacific, with Special Emphasis in the Palau Island (Survey Area Includes New Guinea Region, Philippine, Caroline and Mariannas Islands).	R. R. Harry, Jr.
	Biological and Medical Investigations with the 70 Mev. Linear Electron Accelerator.	H. S. Kaplan, E. L. Ginzton.
Tennessee, University of	The Response of the Reticulo-Endothelial System to X-irradiation.	N. R. DiLuzio.
	A Study of the Effects of Cobalt-60 Gamma Irradiation on Infection and Immunity.	W. M. Hale.
Texas Technological College	The Effects on Rat Behavior of Developmental Aberrations Induced by Ionizing Radiation in utero.	S. J. Kaplan.
Texas, University of, M. D. Anderson Hospital and Tumor Institute.	Physical and Radiobiological Investigations with 22-Mev. X-rays and Electrons, as Compared with Cobalt 60 and 250-kilovolt X-rays.	W. K. Sinclair.
Tufts College	Study of the Effects of Radiation on Growth	D. Rapport.
Vanderbilt University	Fetal Irradiation and the Patterns of Behavior Development.	G. W. Meier.
Western Reserve University	Investigations of the Biological Effects of Internally Deposited Radioisotopes and Related Radio-Biology Studies.	H. L. Friedell.
Wisconsin, University of	The Effect of Various Forms of Irradiation of the Brain on Learned and Unlearned Behavior of Monkeys and Chimpanzees.	H. F. Harlow.
Yerkes Laboratory of Primate Biology, Inc.	Behavioral Effects of Ionizing Radiation on Chimpanzees of Various Ages.	H. W. Nissen.
Argonne Cancer Research Hospital.	Biological Studies with High Energy Sources	E. L. Simmons.
Argonne National Laboratory	Hemolysin Formation in X-Irradiated Rabbits	B. N. Jaroslow.
	Variations in the Haemagglutination, Electrophoretic and Serological Properties in the Sera of Monkeys Exposted to Low-level Gamma Irradiation.	C. W. Leone. G. A. Sacher.
	Radiation Effects on Immunity to Ascites Tumors.	A. M. Brues. A. N. Stroud.
	Effect of Continuous Irradiation on Tumors	A. M. Brues.
	Continuous Radiation Effects on Cell Division in Tissue Cultures.	A. M. Brues, A. N. Stroud.
	Cell Division Effects in Radiated *Paramecium*	E. L. Powers.
	Beta Irradiation of Skin From Point Sources	A. J. Finkel.
	Biological Effects of Cosmic Rays	A. M. Brues, H. Walton.
	Organ Weight Changes in Irradiated Mice	A. M. Brues, A. N. Stroud.
	Studies on the Effects of Ionizing Radiation on Connective Tissue.	F. Wasserman.
	Primate Radiobiological Program	R. J. Flynn.
	Effects of X-Irradiation on Developing Embryos of the Grasshopper.	T. N. Tahmisian.
	The Effect of X-Irradiation on the Oscillation of Developing Egg Nuclei in Grasshoppers.	Do.
	Theory of Radiation Injury and Lethality	G. Sacher, D. Grahn.
	Gamma-ray Toxicity: Lethality	D. Grahn, G. Sacher.
	Gamma-ray Toxicity: Histology, Hematology, Pathology.	G. Sacher, D. Grahn.
	Radiation Effects on Reproduction in the Female Mouse.	M. H. Sanderson, S. P. Stearner.
	Effect and Dose-Exposure-Time Relation of X and Gamma Radiations on Avian Species Other Than the Chicken.	S. P. Stearner, M. H. Sanderson.
	Effect of Dose and Exposure time of Co-60; Gamma Rays on Mortality of Young Chicks.	Do.
	Departure from Additivity when Mixtures of Fission Neutrons and Co-60 Gamma Rays are Administered to Mice.	H. H. Vogel, Jr., J. W. Clark.
	Effects on Mice of Acute Irradiation with Fission Neutrons and Co-60 Gamma Rays.	Do.
	"Recovery" After Irradiation of Mice with Fission Neutrons or Gamma Rays.	Do.
	Chronic Irradiation of Mice by Fission Neutrons	Do.
	Relative Biological Effectiveness (RBE) of Fission Neutrons and Co-60 Gamma Rays.	Do.
	Survival of the Chick Embryo as a Standard Radiobiological Test.	H. H. Vogel, Jr.
	Late Effects of Acute Irradiation with Fission Neutrons and Gamma Rays.	H. H. Vogel, Jr., et al.
	Effect of Natural and X-ray Induced Aging on Susceptibility to Chemical Carcinogens.	H. Ducoff, H. Lisco.

2. Effects of radiation on humans, mammals, and other organisims (exclusive of genetic studies), $9,168,000—Continued

Institution	Title	Investigator
Brookhaven National Laboratory.	Production of Radiation of Desired Character	L. E. Farr.
	Control of Radiation Distribution in Mammalian Organisms.	Do.
	Effects of Radiation	Do.
	Effects of Radiation on Aging in Mice	H. J. Curtis.
	Effects of Radiation on Animal Metabolism	L Nims.
	The Acute Effects of Radiation in Mammals	H. Quastler.
California, University of, at Los Angeles (Atomic Energy project).	Radiation Biology	T. Hennessy.
California, University of, Radiation Laboratory.	Biological Effects of Radiation	C. Tobias, J. Born.
General Electric Co., Hanford operations office.	Beta Irradiation of Skin	L. K. Bust[illegible]d.
	Relative Biological Effectiveness	F. P. Hungate.
	Radiosensitivity of the Gastrointestinal Tracts	R. C. Thompson.
Oak Ridge National Laboratory.	Radiation Effects on Biological Systems	A. Hollaender.
Rochester, University of (Atomic Energy project).	Studies on the Hemorrhagic State and the Metabolism of Animals Exposed to Ionizing Radiation.	L. Tuttle, L. Miller.
	Study of the Morphological and Physiological Alterations of the White Cells Using Normal and Irradiated Animals.	M. Ingram.
	Studies of the Endocrine Imbalances in the Irradiated Animal.	S. Glasser. F. Brayer.
	Effects of X-Irradiation on the Aging Process in the Rat.	J. B. Hursh.
	Acute Radiation Effects on Whole Body X-Irradiation in Animals.	L. Steadman.
	Effect of X-Irradiation on Spermatogenesis in Dogs.	J. Hursh, G. Casarett.
	Clinical and Biochemical Studies of the Irradiated Dog.	W. Mason.
	Drosophila Melanogaster as a Tool in Radiobiologic and Toxicologic Investigation.	L. Tuttle. R. Baxter.
Tennessee, University of	Radiation Effects on Reproductive Functions in Farm Animals, Sperm Physiology.	J. A. Ewing.
	External Radiation Studies With Large Animals.	Do.
National Academy of Sciences Atomic Bomb Casualty Commission.	Studies of the Radiation Effects of the Atom Bomb on the Survivors of Hiroshima and Nagasaki, Japan.	
Columbia University	Biological Action of Ionizing Radiation. Instrumentation for Research.	G. Failla.
Stephen F. Austin State College.	The effect of Radiation on a Natural Population of *Peromyscus gossypinus* (field mouse).	W. M. McCarley.
Los Alamos Scientific Laboratory.	Bio-Medical Problems in Atomic Energy Operations.	
Civil effects test program	Radiological Countermeasures	P. C. Tompkins.
	Biological Assessment of Blast Effects	C. S. White.
	Physical Response to Blast Loadings	L. J. Vortman.
	Radio-Ecological Aspects of Nuclear Fallout	K. H. Larson.
	Instrumentation and Dosimetry	R. L. Corsbie.

3. Treatment and methods of ameliorating radiation effects, $1,338,000

Institution	Title	Investigator
Buffalo, University of	Immunological Factors in Bone Marrow Transplants.	E. Witebsky, M. L. Bloom.
Children's Medical Center, Boston, Mass.	The Nature of Bleeding in Pancytopenia With Special Regard for Thrombocytopenia and the Vascular Defect.	S. Farber.
Jefferson Medical College of Philadelphia.	Transplantation of Preserved Marrow Between Animals and From One Human to Another.	L. M. Tocantins.
Mary Imogene Bassett Hospital.	The Collection, Storage and Use of Human Bone Marrow.	J. W. Ferrebee, E. D. Thomas.
Massachusetts General Hospital.	The Collection, Storage and Use of Human Cadaver Marrow.	B. Castleman, J. W. Ferrebee.
New England Center Hospital	Bone Marrow Research Project	W. Dameshek.
	Physiopathology of Platelets and Development of Platelet Substitutes.	Do.
New York, Research Foundation of State University of.	Experimental Transfusion of Bone Marrow Into Rabbits After Total Body Irradiation	J. H. Ferguson, M. F. Hilfinger.
Parke, Davis & Co	Factors Elaborated by Animal Tissues Which Stimulate Rate of Regeneration of Hemotopoietic Organs of Animals Exposed to Total Body Irradiation With Gamma Rays.	J. K. Weston.
Southwest Foundation for Research and Education.	An Investigation of the Production and the Possible Isolation of Substances Capable of Stimulating Recovery From Radiation by Utilizing Techniques of in vitro Maintenance of Spleen and Other Organs.	N. T. Werthessen.
Tennessee, University of	Physiology of Water and Ionic Balance in Monkeys Subjected to Whole Body Radiation.	R. R. Overman.
Do	Protective Action of Bone Marrow Perfusates and AET in Irradiated Monkeys.	Do.
Yale University	Biologic Implications of Isologous and Heterologous Bone Marrow Repopulation in Irradiated Animals.	J. W. Hollingsworth.
Argonne Cancer Research Hospital.	Radiation Recovery Factor	E. Goldwasser.
	Differential Reduction in Radiosensitivity	L. O. Jacobson.
Argonne National Laboratory	Radiation Protection and Therapy	H. H. Vogel, Jr., J. W. Clark.
	Radioelement Metabolism in Humans; Mechanisms of Spleen Protection.	H. S. Ducoff.
	Protective Factor in Plasma Protein	A. M. Brues, A. N. Stroud.
	Protective Effect of Non-Irradiated Protoplasm in Supralethally X-Irradiated Protozoan Animals.	E. W. Daniels.
	Role of Hydrogen Peroxide and Catalase in Radiation Lethality.	R. N. Feinstein.
	Phenothiazine Derivatives and Other Substances as possible Protective Agents Against the Lethality of Ionizing Radiation.	Do.
	Protective Mechanisms in Radiation Injury	H. M. Pratt.
	Therapy in Radioelement Poisoning	J. Schubert, M. White.
	Radiation Protection of Chicks by Means of Vasoconstrictor Drugs.	J. P. Stearner, M. H. Anderson.
Brookhaven National Laboratory.	Combating Radiation Detrimental Effects	H. Curtis.
Oak Ridge National Laboratory.	Radiation Protection, Living Cells	A. Hollaender.
	Mammalian Radiation Recovery	C. Congdon.
Rochester, University of (Atomic Energy project).	Studies on Therapy of the Radiation Syndrome With Attempted Control of Infection, Hemorrhage and Nutritional and Humoral Imbalances by Standard Medical Procedures.	S. Michaelson.
	Clinical Treatment of Radiation Injury. Tolerance of Animals to Repeated Sublethal Radiation Dosages. Study of Recovery Pattern Following Lethal Dosage of Ionizing Radiation.	J. Howland.
	Effect of Varied Nutritional States on the Survival and Recovery Process of the Irradiated Rat with Particular Interest on Aging, Anemia, and Partial Body Exposures.	T. Noonan.

4. Genetic effects of radiation

(a) STUDIES OF HUMAN GENETICS AND OF GENETIC EFFECTS OF RADIATION ON HUMAN CELLS AND TISSUE CULTURE, $154,000

Institution	Title	Investigator
Johns Hopkins University	The Effects of Ionizing Radiations on Gene and Chromosome Mutation Rates in Normal Human Cells in Tissue Culture.	H. B. Glass.
Long Island Biological Association.	The Study of Spontaneous and Induced Genetic Changes in Mammalian Cells Grown in Tissue Cultures.	M. Demerec and B. P. Kaufmann.
Michigan, University of	Development of Information Concerning (1) Human Mutation Rates; (2) The Accumulation of Deleterious Recessive Genes in Human Populations; and (3) The Manner of Action of Selective Factors on Both Contemporary and Primitive Human Populations.	J. V. Neel.
	Study of Consanguineous Marriage in Japan	Do.
Yale University	Radiation Effects on Mammalian Chromosomes in Tissue Cultures.	N. H. Giles.

(b) EXPERIMENTAL STUDIES OF THE GENETIC EFFECTS OF RADIATION ON SPECIES OTHER THAN MAN, $1,314,000

Columbia University	Studies of Mutations in Populations of Wild House Mice.	L. C. Dunn.
Iowa State College of Agriculture and Mechanic Arts.	Quantitative Study of Lifetime Sickness and Mortality and Progeny Effects Resulting From Exposure of Animals to Penetrating Irradiation.	J. W. Gowen and J. Stadler.
Oregon, University of	Investigations in Population Genetics and Ecology.	D. L. Jameson.
Roscoe B. Jackson Memorial Laboratory.	Quantitative Population Genetics of Mice Under Irradiation.	E. L. Green.
	Attempt to Delineate Inborn Anemias in Mice	E. S. Russell.
Texas, University of	Direct and Indirect Effects of Radiation on Genetic Developmental Systems of Vertebrates.	W. F. Blair.
Amherst College	Genetic Effects of Acute and Chronic Low Level Irradiation with Cobalt 60.	H. H. Plough.
Arkansas, University of	Developmental-Genetic Study of the Effects of X-ray Irradiation in *Drosophila virilis* and *Bufo valliceps*.	F. E. Clayton.
Brown University	Radiation Effects on the Cytoplasm of Habrobracon eggs.	W. Kenworthy.
Chicago, University of	The Genetic Functioning of Heterochromatin	W. K. Baker.
Columbia University	The Population Genetics of Species of Drosophila.	T. Dobzhansky.
Delaware, University of	The Relation of Genome Number to Radiosensitivity in Habrobracon.	A. M. Clark.
Indiana University Foundation.	The Influence of Radiation in Altering the Incidence of Mutations in Drosophila.	H. J. Muller.
Johns Hopkins University	The Action of Radiation and Other Mutagenic Agents—(1) In Inducing Mutation in Drosophila Females; and (2) In Controlling the Action of a Specific Gene Responsible for Suppressing Uncontrolled Growth.	H. B. Glass.
Long Island Biological Association, Inc.	Adaptive Value of Experimental Populations Exposed to Radiations.	B. Wallace.
Mississippi, University of	Chromosome Breakage in Oocytes of *Drosophila*	D. R. Parker.
North Carolina State College of Agriculture and Engineering.	The Genetic and Developmental Effects of Ingested Radioactives in Habrobracon.	D. S. Grosch.
North Carolina, University of	A Study of Genetic Recombination as Influenced by Mutagenic and Nonmutagenic Environmental Agents.	M. Whittinghill.
Northwestern University	Studies on the Radiation Genetics of *Drosophila melanogaster* Females.	R. C. King.
Pennsylvania, University of	Mutation Rates in Mormoniella	P. W. Whiting.
Pittsburgh, University of	Genetic Potential of Certain Populations of *Drosophila persimilis* from the Sierra Nevada of California.	E. B. Spiess.
Texas, University of—M. D. Anderson Hospital and Tumor Institute.	The Effects of Radiations on the Genetic System of Organisms in Relation to Their Physiological and Biochemical Systems.	M. L. Alexander.

4. *Genetic effects of radiation*—Continued

(b) EXPERIMENTAL STUDIES OF THE GENETIC EFFECTS OF RADIATION ON SPECIES OTHER THAN MAN, $1,314,000—Continued

Institution	Title	Investigator
Texas, University of	Research on Direct and Indirect Effects of Radiations on the Genetic Systems of Organisms.	W. S. Stone.
Emory University	Study of the Influence of Oxygen Level and Temperature on the Effects of Ionizing Radiation.	A. V. Beatty.
Florida, University of Agricultural Experiment Station.	Recovery of Radiation Induced Micromutations in Oats by Recurrent Selections.	A. T. Wallace and F. H. Hull.
Harvard University—Bussey Institution.	The Biological Effect of Radiation; Effects of Irradiation on Chromosomes.	Karl Sax.
Johns Hopkins University	Modification by Supplementary Agents of the Rates of Induced Chromosome and Gene Changes.	C. P. Swanson.
Minnesota, University of	The Genetic Basis and Practical Significance of Mutations Induced in Oats and Barley with Ionizing Radiations.	R. S. Caldecott.
Purdue Research Foundation	Genetic Effects of Thermal Neutron Irradiation in Homozygous Tomatoes.	A. B. Burdick.
Washington, State College of	A Study of Factors that Govern Radiosensitivity in Plants.	R. A. Nilan.
Yale University	Investigations on the Cytogenetic Effects of Radiations.	N. H. Giles.
Argonne National Laboratory	Cytology with Special Reference to Radiation Effects in Animal, Plant and Human Nuclei.	B. R. Nebel.
	Genetic Resistance to X-Irradiation in Inbred Lines of Mice.	C. A. Leone and H. M. Patt.
	Genetics of Radiation Toxicity	D. Grahn and G. A. Sacher.
	Radiation—Induced Recessive Mutations in Mice.	H. M. Slatis.
Oak Ridge National Laboratory.	Genetic and Developmental Effects of Radiation on Mice.	W. Russell.
California, University of Radiation Laboratory.	Radiation and Mutation Rate	C. Stern.
Oak Ridge National Laboratory.	Genetic and Cytogenetic Effect of Radiations	A. Conger and R. Kimball.
Brookhaven National Laboratory.	Radiation Mutations in Plants	C. Konzak.

5. Biochemical and microbiological studies of radiation effects, $1,472,000

Institution	Title	Investigator
Arkansas Medical School, University of.	Studies on the Biochemical and Nutritional Aspects of X-Radiation Injury.	P. L. Day.
Arkansas, University of	The Utilization of Radioisotopes by Vertebrate Embryos.	P. M. Jonston.
Brown University	Penetration of the Gut Wall by Intestinal Bacteria After X-Irradiation.	M. H. Hatch.
California Institute of Technology.	The Genetic and Cytological Effects of High Energy Radiation.	G. W. Beadle.
Chicago, University of	Studies on the Mechanism of Action of Ionizing Radiations.	E. S. G. Barron.
Christian Brothers College	Uranium Complexes with Amino Acids and Peptides.	E. J. Doody.
Columbia University	Study of the Action of Radiation on Deoxypentose Nucleic Acids Having Biological (Transforming) Activity.	S. Zamenhof.
Cornell University	Cytological and Genetic Studies of Bacteria as Related to Effects of Radiation.	M. R. Zelle.
Duke University	The Effects of Ultraviolet Light and Gamma Rays on Cell Lipids and the Physiological Action of Irradiated Lipids.	K. M. Wilbur, F. Bernheim.
Florida, University of	Concentration of Mineral Elements in the Fetus and the Relationship to Placental Transfer of These Elements.	G. K. Davis, R. L. Shirley, A. Z. Palmer.
George Washington University.	Studies of the Effects of Radiation on the Biosynthesis and Degradation of Nucleoproteins and Its Modification by Various Agents.	P. K. Smith.
Hahnemann Medical College and Hospital.	The Biochemical Properties of Microsomes and the Effects of Radiation on Them.	J. S. Roth, H. J. Eichel.
Johns Hopkins University	A Study of Metabolism and Active Transport of Certain Divalent Metals in Tissues and in Isolated Mitochondria, with Special Attention to the Possible Role of Complexing Agents in These Processes.	L. S. Maynard.
	Biochemical Changes Resulting from Mutations Induced by X-rays, Ultaviolet, and Nitrogen Mustard.	W. D. McElroy.
Marine Biological Laboratory.	Studies on the Physiology of Marine Organisms Using Radioisotopes.	P. B. Armstrong.
Michigan, University of	Effects of Radiation on the Intermediary Metabolism of Mammalian Skin.	I. A. Bernstein.
	Studies with an X-ray Monochromator and X-ray Irradiation Service Operation.	H. J. Gomberg.
	The Biochemical Effects of Radiation: The Effect of Ionizing Radiation on Nucleic Acid Metabolism.	R. L. Potter.
Notre Dame, University of	Mechanisms Involved in the Action of Radiations on Living Cells.	C. S. Bachofer.
Oklahoma, University of Research Institute.	The Cytology and Genetics of Radiation Resistance in Bacteria.	J. B. Clark.
Pennsylvania, University of	The Internal Organization of Normal and Phage-Infected Cells as Influenced by Radiation.	S. Mudd.
Philadelphia General Hospital.	The Effect of X-ray Radiation of the Lipids of the Skin.	H. P. Schwarz.
Pittsburgh, University of	Study of the Correlation of Radiation Effects with Physical and Chemical Changes in Viruses.	M. A. Lauffer.
Reed College	The Effect of Ionizing Radiation on Biochemical Compounds.	A. F. Scott, A. H. Livermore.
Sloan Kettering Institute for Cancer Research.	Biological Effects of Radiation, and Related Bio-Chemical and Physical Studies.	C. P. Rhoads.
Southern California, University of.	Effect of Radiation on Intestinal Absorption and Metabolism of Fats and Carbohydrates.	R. B. Alfin-Slater, A. L. S. Cheng.
Southern Illinois University	The Effects of X-rays and Ultraviolet Radiation on the Multiple Manifestations of a Gene Together with Genetical Analysis of the Radiation Induced Variations and the Effects of Extracts from Unirradiated Cells on the Repair of the Genotypes of Irradiated Cells.	C. C. Lindegren.
St. Louis University	Study of the Relation of Rickettsial and Viral Infections to Radiation Injury.	D. Greiff.
Tennessee, University of	A Survey of the Effects of Radiation on Animals Parasitized with *Taenia pisiformis*, on Parasites of the Irradiated Animals, and on the Parasites per se.	A. W. Jones.
Texas, University of	The Genetic and Biochemical Effects of Radiation on Bacteria.	O. Wyss.
Vanderbilt University	Study of the Absorption and Metabolism of Lipids and Vitamins, and the Alterations Which Occur in Acute Radiation Injury.	W. J. Darby.

5. Biochemical and microbiological studies of radiation effects, $1,472,000—Con.

Institution	Title	Investigator
Virginia, Medical College of	An Investigation of Certain Tissue Protein Changes in Irradiated Animals.	H. G. Kupfer, N. F. Young.
Virginia, University of	A Study of the Kinetics and Reactivity of Cell Surface Components as Affected by Ionizing Radiation.	H. Jonas.
Western Biological Laboratories.	Further Studies of an Unidentified Factor in Liver which Prolongs Survival of Animals Administered Multiple Sublethal Doses of X-Irradiation.	B. H. Ershoff.
Worcester Foundation for Experimental Biology.	Investigation of the Effects of Radiation on the Biosynthesis and Metabolism of Adrenocortical Steroids.	G. Pincus.
Argonne Cancer Research Hospital.	Effects of Ionizing Radiation in Protein Metabolism in the Human Being and Experimental Animals.	R. J. Hasterlik, E. I. Pentz.
Argonne National Laboratory	Iron Uptake by the Bone Marrow of Irradiated and Control Rats.	H. H. Vogel, J. W. Clark.
	Radiation Effects on Dry Bacterial Spores	E. L. Powers, C. F. Ehret.
	Mutagenic Effects of C-14	N. Williams, N. S. Scully.
	Biochemical Studies of Effects of X-Irradiation	E. K. Bernstein.
	Dependence on Radiation Dose Rate of Oxidation or Reduction of DPN/DPNH in Living Systems.	T. N. Tahmisian.
	Cytochemical Studies of Nucleic Acids in Irradiated Tissue Cultures.	A. M. Brues, A. N. Stroud.
	Radiation Effects on the Mating Reaction in *Paramecium bursaria*.	C. F. Ehret.
	Radiosensitivity of S-adenosylmethionine-forming System in Animal Tissues.	P. D. Klein.
	Biochemical Changes in Irradiated Animals	J. F. Thomson.
Brookhaven National Laboratory.	Effects of Radiation on Enzymes	L. Augenstine.
	Biochemical Effects of Radiation in Mammals	F. Sherman.
	Effects of Radiation on Large Molecules	D. Fluke.
California, University of, at Los Angeles (Atomic Energy project).	Radiation Metabolism	L. Bennett.
	Tissue Transplant—Effects of Irradiation	B. M. Allen.
	Metabolic Radiobiology	O. Schjeide.
California, University of, Radiation Laboratory.	Distribution and Metabolism of Trace Elements	C. Tobias.
Oak Ridge National Laboratory.	Effects of Radiation on Paramecium	R. Kimball.
	Biophysics	J. Kirby-Smith.
Rochester, University of (Atomic Energy project).	Effects of Radiation on the Biosynthesis of Hemoglobin.	K. Salomon.
	The Effect of Various Radiations on Bacterial Systems.	G. Whipple.

6. Environmental studies, $52,000

Institution	Title	Investigator
South Carolina, University of	An Ecological Study of the Fishes of the Savannah River Drainage.	H. W. Freeman.
	An Ecological Study of the Flora and Fauna of the Savannah River Plant Area.	W. E. Hoy.
Texas A. & M. Research Foundation.	A Study of Some Factors Involved in the Disposal of Radioactive Wastes at Sea.	R. G. Bader.
Washington, University of	Determination of Relationships Between Temperature Lapse Rate, Wind Speed, and Wind Shear (Atmospheric Turbulence Study).	F. I. Badgley.

7. Dosimetry research: The development of improved methods of measuring radiation, $1,026,000

Institution	Title	Investigator
U. S. Army Corps of Engineers.	Program of Research and Development on Scintillation Crystals.	N. F. Blackburn.
Commerce, U. S. Department of, National Bureau of Standards.	Radiation Monitoring Systems	L. Costrell.
Commerce, U. S. Department of, National Bureau of Standards.	Radiation Shielding Problems	H. O. Wykoff.
Columbia University	Attenuation of Scattered Cobalt 60 Radiation in Lead and Building Materials.	C. B. Braestrup.
	Evaluation of Aerosol Filters and Impactors Having Chemically Different Surfaces.	V. K. LaMer.
Levinthal Electronic Products, Inc.	Study of Scintillation and Other Related Properties of NaI Crystals.	W. J. Van Sciver.
Argonne National Laboratory.	Dosimetry of Mixed Radiations: Fission Neutrons and Gamma Rays.	H. H. Vogel, Jr., J. W. Clark.
	Tissue Dose Distribution of Soft X-rays	H. Walton, G. A. Sacher.
	Radiation Dosimetry	L. D. Marinelli.
California, University of, at Los Angeles (Atomic Energy Project).	Chemical Dosimeter Development	G. Taplin.
	Scintillation Counter Development for Medical Use.	B. Cassen.
General Electric Co., Hanford area.	Analytical, Bio-assay and Counting Methods	D. W. Pearce.
	Special Studies and Monitoring Methods	Do.
	Gamma and Beta Ray Dosimetry	Do.
	Neutron Dosimetry	Do.
Knolls Atomic Power Laboratory, Schenectady.	Investigation of Fast Neutron Effects on the Electrical Properties of Semiconductors in the Energy Range From About 0.5 to 18.0 Mev.	T. M. Snyder.
	Development of an Electron Microscope Capable of Resolving Individual Atoms.	Do.
Oak Ridge National Laboratory.	Radiation Dosimetry	G. S. Hurst.
Rochester, University of	Measurement of Radiation Dose to Lungs From Radon and Thoron Degradation Products.	W. Bale, A. Dahl.

Dr. DUNHAM. I call your attention to the first page of that in which it says that there are only four people working on environmental studies. The reason that happened is that in the hurry to get this material to you, we failed to take cognizance of the fact that most of the environmental studies now going on are lumped under the first group, sampling and analysis of radioactive fallout. So that this figure here looks very small, but that does not mean that we are not doing extensive studies in Nevada, and elsewhere, but they are listed in item 9.

I would like to at this point call upon Dr. Shields Warren, with your permission, to say a few words about research in this area.

Representative HOLIFIELD. I was going to ask Dr. Warren and Dr. Brues and Colonel Bach and Dr. Shelton if they wanted to comment on your statement.

Dr. Warren, will you lead off?

Representative COLE. May I interrupt for a moment to express my personal admiration for the appearances Dr. Dunham has made throughout these hearings and particularly this morning. I have been tremendously impressed with his grasp of the problem and his knowledge of the details of it. I congratulate you on it. I think the Commission is very fortunate in having a person of your capacity as Director of the Division of Biology and Medicine. The public is fortunate in having you as a public servant. I compliment you in the highest terms. I am very much impressed, and I am amazed that a person of your talents is willing to sacrifice your life to work for the Government for the pittance that you get.

Dr. DUNHAM. Thank you very much, Mr. Cole.

Representative VAN ZANDT. I have a question of Dr. Dunham. For the benefit of the record, can you describe briefly what happened to the animals that were on the Marianas in the 1954 test? There were 45 or 47 of them, as I recall. Are you familiar with the study on those animals?

Dr. DUNHAM. In the Marianas?

Representative VAN ZANDT. On the Marshalls.

Dr. DUNHAM. You mean the animals on Rongelap. Some of them were recovered almost immediately and taken to the Naval Radiological Defense Laboratory where some were sacrified immediately to find the body burden and some were kept for a period of months to discover the excretion rates and more recently a few more animals were found at the recent survey and similar studies have been done at the Radiological Defense Laboratory.

Representative VAN ZANDT. Is it possible for you to prepare a short statement giving the history of those animals and make it a part of the record?

Dr. DUNHAM. I would be happy to do so.

(The information referred to follows:)

UNITED STATES ATOMIC ENERGY COMMISSION
Washington, D. C., June 28, 1957.

Mr. JAMES T. RAMEY,
Executive Director of the Joint Committee on Atomic Energy,
Congress of the United States, Washington 25, D. C.

DEAR MR. RAMEY: Enclosed is a statement giving the history of the animals on Rongelap which Representative Van Zandt requested us to furnish for the record during the fallout hearings on June 7.

Sincerely yours,

CHARLES L. DUNHAM, M.D.,
Director, Division of Biology and Medicine.

Animals (swine, ducks, chickens) present on Rongelap Island were permitted to continue to live on the island and then were collected at time intervals up to about 2 months after the fallout had occurred. They were then sacrificed serially up to 6 months after the time of detonation. Under these conditions it is estimated that intake of the fallout material may be higher from consuming foods having external contamination than through the soil-plant-animal cycle. It would be expected that the eating habits of animals would result in greater intake of such material than for humans. Yet after living in this environment during the 2 months of heaviest contamination there were no significant gross changes nor pathological changes which could be ascribed definitely to radiation at the time of the examination (sixth month). A summary of the data is contained in the tables 1 and 2 attached, taken from Some Effects of Ionizing Radiation on Human Beings.

Analyses of a rooster and some rats that lived for 2 years in that area before collection and sacrifice, yielded similar negative pathological results. Table 3 summarizes these data. It will be noted from this table that the internal deposition of strontium 90, the isotope of greatest concern, is less than the maximum permissible concentration for adult atomic energy workers (1,000 sunshine units). Yet during this period of time the external radiation dose out-of-doors was at least 400 roentgens. These data definitely indicate that for times immediately following a detonation the external gamma exposure would be the dominant hazard.

A full account of these data may be found in Some Effects of Ionizing Radiation on Human Beings, and the forthcoming publication Radiological Contamination of Certain Areas in the Pacific Ocean From Nuclear Tests. Both of these documents can be obtained from the Superintendent of Documents, United States Government Printing Office, Washington, D. C.

TABLE 1.—*Mortality and external radiation dose of animals from the living areas of Rongelap and Utirik*

Animals	Series A			Series B			Series C			Series D			Total		
	200 roentgens (day 8) [1]			330 roentgens (day 25) [1]			340 roentgens (day 33) [1]			340 roentgens (day 51-53) [1]					
	Total received	Dead	Sacrificed	Total received	Dead	Sacrificed	Total received	Dead	Sacrificed	Total received	Dead	Sacrificed	Total received	Dead	Sacrificed
Hens	6	[2] 1	[2] 1				20	[3] 2	[4] 2	11	[5] 5		37	8	3
Roosters	1						2	[6] 1		1			4	1	
Chicks							9	9					9	9	
Ducks							4		[7] 1				4		1
Pigs	1		[8] 1	7		[9] 4				[10] 3			11		5
Cat	1												1		
Total													66	18	9

[1] Day of collection past detonation and external dose.
[2] 23 days past detonation.
[3] 42 and 43 days past detonation.
[4] 44 days past detonation.
[5] Nos. 96, 39, 35, 7, and 24 were dead 67, 74, 92, 99, and 130 days past detonation respectively.
[6] 49 days past detonation.
[7] 56 days past detonation.
[8] 45 days past detonation.
[9] Sow and Nos. 6, 24, and 25 were dead 33, 57, 82, and 82 days past detonation respectively.
[10] Animals from Utirik; all others from Rongelap (group IV area animals received 22 roentgens external dose).

TABLE 2.—*Radiochemical analysis of tissues and urine of pigs from Rongelap on 82d day postdetonation*

BETA ACTIVITY—D/M/TOTAL SAMPLE

Sample	Gross activity $\times 10^{-3}$	Sr^{90} $\times 10^{-3}$	Ba^{140} $\times 10^{-3}$	Total rare earth $\times 10^{-3}$
Pig #24 (25.8 kgm):				
Skeleton (total)	8,890	5,660	660	1,010
Liver	31	0.40	0.33	6.4
Colon and contents	12	5.0	2.4	3.2
Lung (alveolar)	1.5	0.22	0.20	0.8
Stomach	1.2	0.22	1.1	1.3
Intestine (small)	2.3	0.62	0.50	0.51
Kidney	3.3	0.21	0.42	0.74
Remaining tissues	690			
Total	9,630	5,667	665	1,020
Urine sample, 24 hours	13	8.7	1.2	1.6
Pig #25 (22.7 kgm):				
Skeleton (total)	8,600	5,100	530	690
Liver	27	0.53	0.20	5.5
Colon and contents	16	5.0	3.2	4.9
Lung (alveolar)	1.1	0.26	0.23	0.33
Stomach	2.0	0.29	0.13	0.39
Intestine (small)	2.6	0.83	0.88	0.88
Kidney	3.1	0.14	0.19	0.52
Remaining tissues	220			
Total	8,870	5,107	534	702
Urine sample, 24 hours	6.2	4.4	0.40	0.54

SUMMARY

Gross beta activity	Skeleton	Total body	Urine (24 Hrs)
Sr^{90}	62.0	58.0	69.0
Ba^{140}	6.8	6.5	7.9
Rare earth	9.7	9.0	10.5
	78.5	73.5	87.4

All values corrected for decay.

TABLE 3.—*Analysis of rats and a rooster collected on island of Rongelap*

	Wet weight	d/m Sr-90/ sample	February 1956 [1]	
			Ca/sample (gm)	Sunshine units
Rats: carcass [2]	44.7	642±23	0.533	545±19
Do	62.5	315±62	.315	453±90
Do	82.3	367±21	.353	470±27
Rooster:				
Femur	[3] 26.0	1,210±39	5.19	105±3
Tibia	41.0	5,702±119	9.50	272±5

	Sr-90	July 1956 [4]	
		Calcium	Sunshine units
Rats: Bone	245±5 d/m/g/wet	171±9 mg/g wet	644

[1] Naval Radiological Defense Laboratory.
[2] Does not include head, femurs, tibiae, and viscera.
[3] Dry weight of 2 femur halves.
[4] Applied Fisheries Laboratory, University of Washington.

Senator ANDERSON. I would like to have you see a letter that a Senator from Nevada received, and I will read it aloud. You tell me how you would feel if you were the Senator from Wyoming. I will leave the doctor's name out until I have his permission to insert it in the record. I guarantee it is an authentic signature of a radiologist at Reno, Nev.

The May 28 nuclear tests here in Neveda were of considerable concern to me as a doctor and a radiologist. The fallout as measured on my radiation survey meters was anywhere from 3 to 6 times above the acceptable tolerance level (that is 2 milliroentgens per hour or 50 milliroentgens per day) for the first 48-hour period. The local newspapers quoted reassuring phrases from the AEC to the effect that the above intensity of fallout was negligible and without danger and that the halflife of the radiation sources was calculated to be 12 hours.

This last statement is erroneous. One of the rules which the AEC enforces among users of radioactive isotopes is that residues of such isotopes be kept stored under appropriate protected conditions for 10 half lives, which is a period of decay, at the end of which the radioactive material is considered impotent. Yet, I find that after 10 half-life periods here in Reno there is still a 1.2 milliroentgen per hour radiation present. Actually, if the original information were accurate, at the end of the 10 half-life period it should be hardly detectable by my survey meters. This can mean one of two things: Either the estimated half life was in error or we have been revisited by fallout material dependent upon wind changes.

It is quite true that 50 milliroentgens per day (or 2 milliroentgens per hour) is considered to be of tolerance level. However, we must consider that all radiation received by this population is on an accumulative basis; and if we are to be periodically exposed to this much radiation, the accumulation can mount up to levels, which in the light of our present medical knowledge, becomes quite significant and hazardous. Actually, no one knows at exactly what dose level significant genetic changes and malignant tumors may suddenly appear in the human. So, it behooves us to practice utmost caution at all times in this regard.

Politics, physics, and medicine seem to be primarily concerned with nuclear tests and observation. But, from where I sit, only the medical man seems most keenly concerned with the future possibilities of these radiation dangers. So, I wish to add my protest to the situation which exists here in Nevada. I want more assurance that there is justification for continued testing, and I wonder if testing grounds could not be removed to more remote, uninhabited areas of the world.

I am not going to worry about your comments on the latter two questions, but what about these 10 half lives and what his testing devices show?

Dr. DUNHAM. I don't know what testing devices he has used. Dr. Dunning, a biophysicist in my division, who spends a good deal of time at the tests working closely with the radiation safety group and the Public Health Service there, has handed me a note to the effect that a dose of 50 to 100 millirep is now estimated for anybody who would continue to live in Reno for the rest of his life.

Senator ANDERSON. That is the estimate. What does that do to you?

Dr. DUNHAM. I think there has been a great deal of testimony during the past 2 weeks on the problem of whether there is a threshold at very low dose rates. This certainly is a low dose.

Senator ANDERSON. Geneticists all seem fairly well agreed that there was no threshold.

Dr. DUNHAM. That is correct. I don't think there has been any question of the geneticists' concept that the genetic effects are all cumulative, and at any level, high or low.

Senator ANDERSON. Would the Senator who got this letter be justified in answering the doctor and say you just have to grin and bear it?

Dr. DUNHAM. I would like to put this somewhat into perspective. If this doctor were to fluoroscope a patient suspected of having an ulcer of the stomach or a cancer of the bowel, the dose to the gonads—here we are talking about genetic effects—would be somewhere in the vicinity of 20 to 30 or possibly more roentgens, not 50 to 100 milliroentgens.

(NOTE.—These figures, 20 to 30 roentgens, may be high. Pullman and Laughlin, in their preliminary edition of Section III: Gonadal Dose Produced by the Medical Use of X-rays of the Report of the Genetics Committee of the National Academy of Sciences Study of the Biological Effects of Atomic Radiation, give an estimate of the gonadal dose from a study of the colon as from 4 to 15. The gonadal dose from a study limited to the stomach only would be less than 1 roentgen. This presupposes modern X-ray and fluoroscopy equipment and the taking of adequate precautions.)

Senator ANDERSON. As Dr. Selove pointed out in that instance the doctor would weigh the possibilities for good along with the possibilities for damage. In this particular instance there is no possibility for good. As far as the individual is concerned, he just gets damaged.

Dr. DUNHAM. That is correct to the extent that he would be damaged. I think that is one of the purposes of these hearings, that is, to point out and give the committee information on what factors have to be weighed when we talk about testing nuclear devices. Is it worth it to the country as a whole.

Representative HOLIFIELD. That milliroentgen is a thousandth of a roentgen?

Dr. DUNHAM. Yes.

Representative HOLIFIELD. How would you express a millionth?

Dr. DUNHAM. A millionth would be a millimilliroentgen.

Representative HOLIFIELD. You do not get that low in measuring.

Dr. DUNHAM. I don't think one can measure it satisfactorily that low that way and one has to begin to talk about counts per minute.

Representative HOLIFIELD. Thousandths is about the lowest?

Dr. DUNHAM. That is correct. Certainly film badges don't get that low.

Representative COLE. If you did get that low, what name would you give it?

Dr. DUNHAM. A microrentgen.

Representative HOLIFIELD. That is the same as a microcurie?

Dr. WARREN. Yes.

Representative HOLIFIELD. Dr. Warren.

STATEMENT OF DR. SHIELDS WARREN, NEW ENGLAND DEACONESS HOSPITAL, BOSTON, MASS.

Dr. WARREN. Thank you very much, Mr. Chairman.

Representative HOLIFIELD. Dr. Brues, would you like to come forward, and Lieutenant Colonel Bach and Dr. Shelton, if you wish to have anything to say, will you please come forward? Is Dr. Schulert here?

Dr. SCHULERT. Yes, sir.

Representative HOLIFIELD. Dr. Crow's name is here, too. I do not know whether he is still with us or not. I understand that Dr. Shannon and Dr. Burney have separate statements, so we will not ask them to come forward at this time.

You may proceed, Dr. Warren.

Dr. Warren. Thank you. I appreciate the kindness of the committee in permitting me to make a statement relative to the present research program of the Atomic Energy Commission in this field of radiation, and to make comments as to the program and its possible future trends.

First, I would like to restrict myself to the fields of biology and medicine, as I am not competent to venture beyond them. I have some hesitancy in making recommendations as a medical man even in the field of biology. However, I would like to say that the program of the Division of Biology and Medicine of the AEC, which you have just heard outlined so clearly by Dr. Dunham, has within the limits of budgetary approval and personnel, attained as much in the way of research results as could be expected from any program.

Initially in this program there was a very real limiting factor, the scarcity of competent scientists. Scientists are still scarce, but are being trained rapidly. One of the best ways to attract scientists is to have adequate opportunities for them to work in any field. As I said, Dr. Dunham has outlined for you the present program with regard to radioactive fallout and certain suggested additions to it. I heartily second his suggestions, but would go somewhat further since he has restricted his comments largely to the factors that are immediately referrable to radioactive fallout.

Much of our knowledge with regard to the effects of radioactive fallout must be obtained by advancing our knowledge of other types of radiation, since fallout occurs in such very small quantities as to produce recognizable changes only with the greatest rarity. Therefore, there must be much research done in the general field of pathological effects of radiation on the one hand, and the effects of radiation on heredity on the other.

To be more specific, I would recommend expansion of research on the effects of radiation on the cell, both in its resting and its dividing state, and expansion of the study of the effects of radiation on enzyme structure. Additional studies are desirable on the effect on both acute and chronic radiation on animals, both as to specific changes and as to more subtle changes, such as shortening of the life span.

Studies of human population genetics, a relatively new science, should be greatly expanded, and the present studies in mammalian genetics should be multiplied several times. These studies, together with those recommended by Dr. Dunham and others, should not be at the expense of the present research program of the Division of Biology and Medicine of AEC since those existing studies, I believe, are well chosen and significant. Rather, the research budget of the Division should be materially increased. I think this should be done in an orderly fashion. A sudden increase is not a practical thing to undertake.

I would like to emphasize in regard to all of the research done in the universities—indeed practically all research in biology and medicine in this field—that there has been no restriction, no withholding of data. All of us working in this field have been free to publish our results exactly as they have occurred. There is absolutely no effort on the part of AEC to withhold significant information.

I think it might be worthwhile to comment briefly on the very careful studies on the fallout problem that are made both in this country and abroad. As you have heard, the groups that are following this

field of work are several. In this country, of course, the Atomic Energy Commission is the first and most active. The Department of Defense plays a large role. The Federal Civil Defense Administration is keenly interested. The Public Health Service is carrying on a number of very valuable studies. These agents all cooperate with one another and with the Weather Bureau, the Department of Agriculture, and other governmental groups, as you have heard, that have special interests in the field.

I think it is pertinent also that there are in this country several unofficial but nonetheless very well constituted groups that are following this problem of fallout and research reated to it. The most expert are (1) the National Committee on Radiation Protection which in turn is affiliated with the International Committee on Radiation Protection, and the International Committee on Radiation Units. You heard from Dr. Taylor, its chairman, as to the way its work is carried on and what it does.

(2) The Committee on Radiation Effects of the National Academy of Sciences, which, as you know, has made valuable reports and are continuing to follow the problem.

Representative COLE. Mr. Chairman, I wonder if it might not be appropriate—I intended to ask Dr. Warren about it later—at this time to indicate the distinction between the United Nations Scientific Committee on Radiation, the International Committee on Radiation Protection, and the International Committee on Radiological Units There is coming to be an abundance of international committees in the field of radiology, and I would like you to explain the origin, the genesis of each· how they overlap and so forth.

Dr. WARREN. Yes. In the mid-1920's the people working in the field of radiology and radiobiology became concerned over the problems of radiation protection and the fact that many of the people who were going into radiology were being damaged by the radiation they received, and were not being taught how to protect themselves.

Consequently, in several countries National Committees on Radiation Protection were established, first informally. Those in this country were made up of representatives of scientific societies which cooperated with the National Bureau of Standards and at present the activities of the National Committee on Radiation Protection are housed at and aided by the personnel of the National Bureau of Standards.

The chairman of these various national committees joined informally in an international group. This international group first met every 3 years at the International Congress of Radiology. As the problems became more multiple they have met more frequently.

Representative COLE. You say it began in 1920?

Dr. WARREN. That began, I think, in formal fashion in either the late 1920's or the early 1930's. Its last meeting was the end of April in Switzerland. Dr. Taylor, from whom you heard, together with Dr. Wycoff of the Bureau of Standards, were the United States representatives there.

This is a purely informal group which has had such efficiency in its work, such scientific standing, that the AEC and practically all of the governmental and State agencies accept its ruling as official, and through the recording of these in the Federal Register they have the force of law.

These committees are entirely apart from the United Nations committee. If I could reserve comment on that for just a few minutes while I speak of some of the other agencies, I would like to come to that in more detail later.

Among other competent groups working in this country are many research groups in universities, in research laboratories throughout the country; the Federation of Atomic Scientists, who had a representative here, is also keenly interested. Then there are a number of other groups that have committees having to do with atomic energy, and concerned with problems of protection. Even the United States Chamber of Commerce and the AFL-CIO are interested in this field.

Abroad there is much emphasis on the problems of fallout and the official report of the British Medical Research Council contains much useful data which, as you have heard, correlates closely with those of our own National Academy of Sciences.

The World Health Organization has also considered certain aspects of the radiation problem.

At the instance of the United States, the General Assembly of the United Nations has appointed a scientific committee to study radiation problems. This committee consists of representatives of 15 nations. I am the representative for the United States. Dr. Brues, sitting beside me, is one of my alternates and has been extremely helpful. This committee has been studying the problem of fallout and related aspects of radioactivity most intensively, and its report will be presented to the General Assembly in June of 1956.

Representative COLE. May I inquire, Dr. Warren, if the meeting of this committee last fall in New York was the first of its meetings?

Dr. WARREN. No, that was the second of its meetings, Mr. Cole. There was one in Switzerland in April. The next, I believe, is scheduled for New York in January. However, there is a great deal of work done besides that being done at these various meetings. There is a secretariat which is continuing to digest all of the information that has been turned in. All of us receive information from all over the world. At the present time, I am reading through a stack of reports, from virtually every country in the world, that is close to 3 feet high. I have gotten two-thirds of the way through it now. Various members of this United Nations Committee have been given the responsibility with the secretariat of preparing separate chapters of this report which will probabily be about a 200-page report when it gets done.

Representative COLE. How long has the committee been in existence? Has it been about a year?

Dr. WARREN. As I remember it, it came into existence about a year and a half ago. Do you remember, Dr. Brues?

Dr. BRUES. November before last, I believe.

Dr. WARREN. Yes. It will make its report, as I say, for June 1958.

CHAIRMAN DURHAM. Why is it just 15 nations, Doctor?

Dr. WARREN. It was felt that a group of that size would be a group that would represent a high degree of scientific competence from the different countries. It would have available to it information from all over the world and all of the member nations of the United Nations and its allied organizations. It would be a small enough group to

work effectively. If it had been a group representing every one of the members of the United Nations——

Chairman DURHAM. Can you give us the names of the 15 countries?

Dr. WARREN. I believe I can. Australia, Argentina, Belgium, Brazil, Czechoslovakia, Egypt, France, India, Japan, Mexico, Sweden, U. S. S. R., United Kingdom, United States, and Canada.

Chairman DURHAM. Russia is a member of the group?

Dr. WARREN. Both Russia and Czechoslovakia are members of the group.

Representative HOLIFIELD. How many meetings has this committee had?

Dr. WARREN. This has had three formal meetings. The reason I was hesitant is that we have had so many informal meetings that it is very difficult to keep track of them. You might say that those of us working in the group are almost in continual session through contact with the secretariat by mail and so on.

Representative HOLIFIELD. Do you have an adequate secretariat to collate the material that is sent in and is there a wide collation and distribution?

Dr. WARREN. Yes, there is a very wide collation and distribution. The great bulk of the material has come from the United States, because we have accomplished more research than any other of the nations.

Chairman DURHAM. How do you report, Doctor? Do you report directly to the American representative?

Dr. WARREN. As the representative for the United States, I report to Mr. Lodge as the Ambassador to the United Nations. However, our committee as a whole is a creature of the General Assembly and reports to the General Assembly.

Chairman DURHAM. It reports directly to the General Assembly?

Senator ANDERSON. How does this information get to the people of this country?

Dr. WARREN. It is the responsibility of the various representatives to bring all pertinent information which is made available to them—this is all circulated—to the people of their country. The final report of the committee will be an open report to the General Assembly and through them to all the member nations and distributed throughout the world.

Senator ANDERSON. Can you give us the name of the fallout expert in Argentina?

Dr. WARREN. The head of the delegation is Captain Nuñez.

Senator ANDERSON. Is he a radiologist?

Dr. WARREN. He has a background in radiology and radiobiology. I might say that the different countries have chosen people with specific competences which vary with the countries. But each delegation from each country brings all the available information from that country. It is not restricted to the specific competence of the members of the delegation.

Senator ANDERSON. What sort of individual represents Egypt, for example?

Dr. WARREN. In Australia a physicist is the representative. In Belgium a medical man interested in radiobiology is a representative. In Canada a medical man who is a public health official represents the country. In Brazil, a distinguished biophysicist. In Czecho-

slovakia, also a biophysicist. I could go through the list. It is a very varied group.

Senator ANDERSON. This individual from Canada, has he been making any special studies of fallout?

Dr. WARREN. Yes.

Senator ANDERSON. Is that his field?

Dr. WARREN. He is relatively new to this field, but he was chosen by the Canadian Government because they felt him to be thoroughly competent in it.

Senator ANDERSON. Has he been doing work and making studies in fallout? Is that his specialty?

Dr. WARREN. I would say he is essentially an all around public health man who is especially interested in this field and is provided by his Government and the scientists in his country with a great deal of information in this field.

Representative COLE. You spoke of the report of this committee as being a final report. Does that mean that when the report is submitted in June of next year, that the committee will disband and discontinue its work?

Dr. WARREN. We have been appointed by the General Assembly to review the problem, to gather, collate and evaluate information, and to make a report. Our future is entirely in the hands of the General Assembly. I rather assume that they will ask us or successors to us to continue this work. I cannot speak for the General Assembly in any way, of course.

Representative HOLIFIELD. You may proceed.

Dr. WARREN. I would like to leave very briefly the discussion of research and make one further comment. That is this: The ultimate decisions with regard to weapons testing and with regard to the whole development of atomic energy will have to be made, as they have been made in the past, by you and other duly constituted representatives of our people. I believe that the advances in science within the next few years provided research is adequately supported and facilitated will permit obtaining much more conclusive data than now exist as to the feasibility of continued weapons testing. The concern of the world is for disarmament and the elimination of war, of course. I firmly believe as a physician that it is inexcusable for us to jeopardize our own safety and that of the rest of the free world in order to eliminate a risk of as low an order of magnitude as is constituted by any reasonable program of atomic weapons testing.

Senator ANDERSON. Do you think that the proposal made by Dr. Langham which was an overall control of the total tonnage of any fission products going into the atmosphere would jeopardize our production of weapons.

Dr. WARREN. I am not at all an expert in this field, Mr. Anderson. I would not have any opinion. I would hope that it might be feasible to work out some program of this type.

Senator ANDERSON. Would you read again the last paragraph.

> I firmly believe as a physician that it is inexcusable for us to jeojardize our own safety and that of the rest of the free world in order to eliminate a risk of as low an order of magniture as is constituted by any reasonable program of atomic weapons testing.

You there set yourself up as an expert in the field. I am not trying to say it is improper. You testified what you would do.

Dr. WARREN. Yes.

Senator ANDERSON. Having established that, do you thing Dr. Langham's proposal that an amount of 10 megatons of fission production going into the atmosphere each year which we are now doing is about the safe limit?

Dr. WARREN. I feel we would be safe in having that much. I would hesitate to say that is an absolute upper limit. I would think that is a reasonable amount. I would not be worried by a program at that level.

Senator ANDERSON. If you have not made studies in the field yourself, you recognize that the Los Alamos and the Livermore Laboratories have.

Dr. WARREN. Yes. They are most competent.

Senator ANDERSON. If they feel that is a top limit, does that suggest to you that is something we might look to as a proper guide or not?

Dr. WARREN. I would think that this might be very sound indeed. From my own knowledge from the medical standpoint, as I said, I would not be at all worried by a program at this level.

Senator ANDERSON. Almost every time when somebody comments, they talk about limitation on testing as if it meant the elimination of all progress and all testing of every kind. It is like saying to man he should be careful in the amount of protein he takes into his system. But a doctor will say if you do not take any protein at all, many things will happen to you. Somehow we do not get much comment on the suggestion of limitation. It is always that we will abolish it all. This was not the proposal of Los Alamos and certainly has not been my own.

Senator JACKSON. Dr. Warren, what this really boils down to is that we have two risks. One is the risk to the free world if we are not prepared to deal with an enemy that might well bring total atomic hydrogen catastrophe to all free nations. On the other hand, continued testing do present a danger of an undetermined nature to people. We do not have enough scientific data for scientists to speak scientifically, whether they are doctors or pure scientists. There are these two threats. Maybe between the two some kind of reasonable balance can be achieved. Don't you think that is a reasonable approach?

Dr. WARREN. Yes. I think that is a very reasonable approach. That is what I had in mind when I spoke of any reasonable program of atomic weapons testing.

Senator JACKSON. What was your last statement?

Dr. WARREN. I simply said that was the general idea I had in mind when I said "any reasonable program of atomic weapons testing."

Represenative HOLIFIELD. If there are no questions, I will ask Dr. Brues to proceed.

Chairman DURHAM. Before you proceed, Dr. Warren, on the second page of your statement I believe your statement concurs with the statement Dr. Libby made on yesterday in his recommendation of increasing the research program of the Division of Biology and Medicine.

Dr. WARREN. Yes, sir.

Chairman DURHAM. You say "not at the expense of the present research program," but still you recommend an increase in it. Could

you put that on a percentage basis as to exactly what you mean by that type of statement?

Dr. WARREN. What I would think is this, sir. One is that there are a number of other very important problems that are being worked on by the Division of Biology and Medicine than those having to do with fallout. We should not interfere with those. There should be an expansion of the program which I think could perhaps be on the order of—it is hard to say—of an expansion that went up, say, 50 percent in 3 years. Maybe that much again in another 3 years would be 1 that could be handled in very orderly fashion. It would not be wasteful. There would be adequate scientific personnel to see it through.

Chairman DURHAM. In some sections of the whole program, you would place more emphasis on this?

Dr. WARREN. Yes. I think, for example, that we must not lose emphasis on the portions of the program that have to do with how to cure atomic innjuries once they have occurred. We must not lose emphasis on the good things that can be done in the field of medicine and biology with atomic energy. I think it is important that in the development of this whole atomic-energy field that the developments in biology and medicine be advanced on a broad foundation. As an illustration, for example, we began our studies on radioactive strontium long ago before we knew how important radioactive strontium could be. I am sure there are other things in the program that do not look very important at the present time that we will be very glad in 1965 we had started earlier.

Chairman DURHAM. You mean that many new things are showing up on a daily or monthly basis that funnel off some of your research in that direction.

Dr. WARREN. That is right.

Chairman DURHAM. You do feel that the impetus should be on an increased expansion.

Dr. WARREN. I feel this very strongly, sir.

Representative HOLIFIELD. Dr. Brues, did you have any comment to make on the testimony which has been given?

STATEMENT OF DR. AUSTIN BRUES, ARGONNE NATIONAL LABORATORY

Dr. BRUES. Mr. Chairman, I have not prepared a statement, but there might be 2 or 3 things I might say, particularly with reference to the programs as they are carried out at the national laboratories which constitute a considerable portion of the work.

As Dr. Dunham mentioned, the national laboratories are run by contract with various agencies. Ours is operated by the University of Chicago. Yet they are large enough to operate essentially as independent institutions, and they are financed by the Atomic Energy Commission. The fact that there are several national laboratories was established in the original Atomic Energy Act, and in a sense grew out of the fact that there had been several sites at which research was carried out before the Commission came into being. I have felt personally that the diversification of the large research centers into several rather than bringing them together has been a good thing from a number of standpoints. I would say particularly be-

cause it allows a certain flexibility. There is no embarrassment, for instance, if an investigator in one laboratory holds a different view from an investigator in another laboratory; just as in the universities they get together and these things are reconciled by further work.

The programs are defined very much by the individuals who carry out the programs. The amount of masterminding, I think, has been kept at a reasonable and proper minimum. What people in the laboratories are mostly interested in is knowledge of what are the fields which are not being adequately tackled, and what, therefore, may be needed in the way of both basic and practical research. In that sense, of course, it is very useful to keep in contact with one another and with the authorities in the Commission.

The selection of personnel is done entirely at the level of the laboratories themselves, with only the restriction, which actually occurs very rarely, that is placed upon us by the security provisions of the act.

Chairman DURHAM. I agree with you, Doctor. I think the program has been well planned on the basis of the distribution in the universities of the entire country. I think if we built one huge laboratory we would not be as far along as we are today.

Dr. BRUES. The type of work is divided in our national laboratories and I think in most of them, and rather equally, between work which is straight programatic investigation kind of work where we are collecting numbers of figures, work which has more or less immediate practical objective, such as looking for treatments of radiation illness and so forth, and work which some people might consider useless, but as Dr. Warren mentioned turns out very often to be quite useful.

We have on occasion been asked if we could not perhaps think it would be important to gain some information in a certain area. Often, actually this information has been at that time fairly well gained by investigators who perhaps through an intuitive process or their curiosity have been working on it. This certainly was true of strontium. We have dogs in the laboratory which received some of the earliest radio strontium 10 years ago. This is not part of the program that would have been given us at the time, but obviously animals of this sort are very important, at least in determining the specifications for other experiments which may be started at this time.

Senator ANDERSON. When Dr. Dunham presented his paper, he said on strontium toxicity:

> In 10 to 15 years we will have experimental data in dogs which will firm up the maximum permissible body burden of strontium 90 for all ages, and will have determined whether strontium 90 is ever leukemogenic in dogs and presumably man, and will have gone a long way to settling whether or not the bone-tumor effects of strontium 90 have a threshold.

Are we to understand that to mean that in 15 years or so we will find out whether it is dangerous to let strontium 90 to be lying around, and if so, who much of it we can have?

Dr. BRUES. I think we are sharpening our knowledge on that all the time. I think it will take another generation of long-lived animals probably before we have the sharpest figure that we can reasonably ever expect to get.

Senator ANDERSON. But by that time if we can believe the upward sweep of testing, with other countries getting into the act, and everybody coming along with their own weapon testing, we could possibly

have done heavy damage to ourselves, could we not? Is there any way of speeding this up? We build a bomb by a process of speeding up. We have had estimates of how long it would take to develop a bomb.

Dr. BRUES. I think, sir, that the way in which this may be and probably will be speeded up is by the expansion not only of these predictable programs, but of the type of work which is going on in what I term the useless areas. In other words, investigation of how cancer is produced by radiation, and indeed how cancer is produced by other agents in comparison with this. I think that we may therefore arrive at basic knowledge in this way which will resolve some of the questions that we are also asking in case that should not pay off in the area of carrying out practical experiments.

I might say these animals which we treated 10 years ago, which have never shown signs of damage, received about 700,000 sunshine units of strontium. The only trouble with this is that there were only three of these dogs. As you have heard, you have to have large statistical numbers in order to sharpen things up.

Senator Anderson. Do you think it will be 10 to 15 years before we know whether testing at the present levels were continued would present any perils to people from strontium 90? I am confining it strictly to strontium 90.

Dr. BRUES. To answer that I would have to go into some questions that were discussed yesterday.

Senator ANDERSON. I do not mean to prolong this.

Dr. BRUES. I believe that it is very likely that we will get at some very fundamental truths about cancer sooner than that period which will tell us this, but we must at the same time be carrying on experiments which will give us the figures, so to speak.

Senator ANDERSON. I say that because the third paragraph of Dr. Dunham's paper says that it will be 10 or 15 years before we get sufficient data, and the fourth paragraph said that the true doubling dose for the mutation rate would take 8 years, if ever, and everything suggests an awfully long time. A tolerable mutation rate would take many years and never be exact. It is a very discouraging picture. The probabilities are that we will never know anything about anything. Is that your feeling?

Dr. BRUES. I would feel that perhaps I am entitled to be more optimistic than Dr. Dunham, because it would not be so easy to come back at me and chastise me for making a prediction that was not sufficiently conservative. I am more optimistic than is indicated there.

Senator ANDERSON. I was not trying to pick a quarrel with Dr. Dunham on this. I was hoping there would be a little more optimism than this "if ever, and we will never be exact."

Representative HOLIFIELD. We are looking for more sunshine.

Senator JACKSON. I was going to comment, is there such a thing as an exact science?

Senator ANDERSON. We hope we get some answers that we regard as pretty specific.

Representative HOLIFIELD. Would there be any other comments? Did you have something else, Dr. Brues?

Dr. BRUES. I would like to say one other thing very briefly, sir, and that is to underline Dr. Warren's remark that there has not been any

interference in my experience or the experience of anyone I know with the dissemination of results, no matter what they showed. I have actually some documentation on that. There was a statement made by one of the witnesses the other day that indicated that the principle of life shortening had been kept under cover for some reason not known to the speaker. I will, if you wish, produce some documentation that this was not only not kept under cover, but that these things were given to the public in a publication which came out of the first national radiobiological meeting which was attended by many of those that were present here, and in fact made the scientific public aware of all of the work which had been done in the field of life shortening, raised the questions of cell mutations, and many other things of that sort.

Representative HOLIFIELD. You will concede, however, that speeches before limited number of scientists does not take the place of information prepared which is readily understood by the public and widely disseminated. I am blaming no one for this, but there is a difference in the expression of opinion to a limited group of scientists and actually getting the story to the people.

Dr. BRUES. That is true, of course. That is the general problem which we also have, that of disseminating the knowledge. I think that this meeting served the important purpose of getting many people working in this field. It was quite speculative at the time, but it has resulted in a wide interest. For example, the brilliant work Dr. Hardin Jones told you about the other day was assisted by the fact that this subject was discussed at that meeting which was aided by the Commission.

Representative HOLIFIELD. At this time the Chair is going to interrupt the proceedings. There is a rollcall which the House members have to attend. We will proceed at 2 o'clock this afternoon in this room as we have some important statements which have to be made. We will resume with this part as soon as we return.

Senator ANDERSON. May I ask one question of Dr. Warren. I started out on strontium 90 a minute ago. When you were head of the AEC's Division of Biology and Medicine, was there a proposal put up to you for a study of the chronic effects of strontium 90, and was there some decision taken on it on the basis that it would be a ten-year and very costly study? Do you recall?

Dr. WARREN. My memory is—and it is a little vague—that there was a proposal made of a 10-year study using dogs that would have cost about $5 million a year, as I remember. It is something of that sort. I believe that strontium 90 was included along with radium, mesothorium, and other substances.

(A letter clarifying Dr. Warren's testimony follows:)

CANCER RESEARCH INSTITUTE,
NEW ENGLAND DEACONESS HOSPITAL,
Boston, Mass., August 6, 1957.

Mr. HAL HOLLISTER,
Joint Committee on Atomic Energy,
Capitol Building, United States Senate Post Office,
Washington 25, D. C.

DEAR MR. HOLLISTER: On July 5, 1957, as I was leaving on vacation, I sent a suggested addition to my testimony on the final day of the hearings. In reading this over I am not sure that it entirely answers the question raised by Senator Anderson. If it is not too late to make a change in the proof, the following might be a better way of expressing it.

"Another project that might be thought of in this connection was one made by the Los Alamos Laboratory, as I recall it, but the proposal for which was never formalized. This project had to do with the effects of the inhalation of particulate radioactive substances by dogs. After a good deal of preliminary study it was felt that certain phases of the proposal were very useful and significant. Since suitable inhalation chambers were already available at the atomic energy project at the University of Rochester and only additional animal quarters were required, this experiment was undertaken at the University of Rochester on the completion of the necessary changes in animal quarters."

Sincerely yours,

SHIELDS WARREN.

Senator ANDERSON. Did you not recommend pretty firmly against it?

Dr. WARREN. I recommended against that particular study but I recommended in favor of continuing studies in this field and got together a group of all the competent and interested people to establish at the University of Utah a study on dogs which started, as I remember it—I will have to ask Dr. Dunham as to the year—was approved and started in 1950, Dr. Dunham tells me. There had to be some construction which slowed it down for some months. That study is the one to which reference has been made now, and useful data are coming from this.

I think that one has to keep in mind this point, Mr. Anderson. One always has to weigh the types of research undertaken in relation to the funds and personnel available for that research at the time. When this research was proposed in its original form, it seemed impractical and too costly for the data that hopefully would be obtained. It looked as though this University of Utah study would be a sounder way of approaching the problem, and one within our means, both of available scientists and available funds. I still hold that opinion.

Senator ANDERSON. I do not question you on the available scientists at all, but I do believe that on funds for items of this nature since I have been a member of the Joint Committee, I have had to go to the Appropriations Committee many times and urge them to be somewhat liberal with the Atomic Energy, and I have always found them so, and I only hope that we do get some sort of expanded program in research that may give us a little quicker answers on some of these questions that are very disturbing to a great many people.

Dr. WARREN. I strongly agree with you, Mr. Anderson.

Senator ANDERSON. We will meet at 2 o'clock.

(Thereupon, at 12:15 p. m., a recess was taken until 2 p. m., the same day.)

AFTERNOON SESSION

Representative HOLIFIELD. The committee will be in order. I want to announce that the Chair will have to leave the room at 5 minutes to 3, and, for fear that I may not get back before the witnesses are concluded, I would like at this time, on behalf of the committee, to thank all the witnesses who have appeared during this group of hearings.

The purpose of the committee, as stated in my opening statement, has been adhered to. It was in a limited field. There are other fields

that are of interest to the committee, and undoubtedly the committee will want to go into some of those fields laer. There has also been a great deal of information filed, either in oral form or in written form, with the committee which the members have not had a chance to thoroughly study, nor has the staff had a chance to analyze. So it has been thought best by the subcommittee that we adjourn our meetings today for the purpose of studying these documents and receiving information which has been requested and has not yet been given to the committee. The committee will make a later determination as to whether there will be additional hearings to fill any gaps that we may find in our testimony on this particular specific subject, and also make the determination of going further into fields which are indirectly related to the subject of weapon fallout in the field of radiation. I speak of the fields of industrial hazards, which will encompass large-size reactors, and also the fields of weapon detection, which has a great deal to do with our ability to know when these explosions occur, and, also, our ability to evaluate accurately the amount of fission products which are thrown into the air. Of course, this has tremendous importance in evaluating the overall burden of radiation which would be finally deposited on the earth and its people.

We also have not had time to analyze the weapons effects handbook which is now in print by the military liaison group of the Defense Department and the Atomic Energy Commission, and we may want to go into some of the matters contained in there.

Another very important area in which the committee is interested—which may, in view of certain industrial developments, have to be considered—is the problem of waste disposal from reactors, the methods that are now used to dispose of this waste material, and the safeguards that are in existence at this time and plans for the future in order to protect the people from radiation from waste materials.

We will want to know the quantities, in tons or gallons, of this material which are now in existence, and which are being produced every day and every year, and we will want to have an estimate of the problem that will be involved in the disposal of waste when we have a hundred of these large-size reactors of 100,000 to 200,000, or 300,000 electrical kilowatts of capacity.

We will want to know how the nations abroad are disposing of this waste; whether they are placing them in the ocean, where the contamination might become global; and where the United States is disposing and the methods it uses.

So there are fields which have not been covered, but they are fields which the comittee will get to in the future, no doubt.

The committee reserves the right, of course, to accept and incorporate in the record additional scientific material which has been requested by the members or referred to by witnesses. We also, of course, reserve the right, as I mentioned at the beginning of our hearings, to include answers to specific letters of inquiry to scientists and witnesses which have been sent out and will be sent out for the purpose of clarifying or completing certain areas of inquiry.

Representative COLE. Mr. Chairman, before you do that, since you have indicated that in all likelihood you will not be here at the con-

clusion of the hearings this afternoon, I feel I should now express what I otherwise would have withheld until that time, and that is to join you in the compliment to the witnesses who appeared, and, more particularly, to pay a compliment to yourself for the fine manner in which these hearings have been conducted and the tenor that has attached to the hearings, for which much credit is due yourself. I compliment you and Mr. Hollister, as well, who has prepared this agenda, for the total accomplishments of these hearings which, I am confident, in the course of time will prove to be most valuable.

Representative HOLIFIELD. I thank the gentleman from New York very much.

Representative VAN ZANDT. Mr. Chairman, I would like to join my colleague from New York in that statement.

Representative HOLIFIELD. I am sure, without the help of every member of the committee and the hard work of the staff night and day, both before and during the hearings, we could not have had as successful a conclusion as I think we are going to have in these hearings.

Senator JACKSON. Mr. Chairman, I, too, would like to associate myself with the remarks that have been made with reference to these hearings. I think the chairman and Mr. Hollister and the staff have done a fine job.

I believe that these hearings cannot help but have a favorable impact in the long run on the American people and world opinion. I think the most important thing of all is that we have had the opportunity of having different points of view. Certainly, in large part, the views that have been expressed have been honest scientific opinion, even though the scientists and experts in the field have not always agreed.

Representative HOLIFIELD. Thank you very much. It would be remiss on my part if I did not thank all the witnesses who have appeared before us, and especially do I wish to thank the witnesses from the AEC, and from projects which are under the auspices of the AEC, for their cooperation in preparing their statements and presenting them to us in the complete way in which they have.

Dr. DUNHAM. Mr. Chairman, is it in order that I make a statement on behalf of the AEC, which has appeared before you in several forms, to thank you for the extremely courteous and interested attitude of the committee. I want to assure you that any further information you may require we will endeavor to provide for you.

Representative HOLIFIELD. Thank you very much.

By mutual agreement we are going to reverse the order of the witnesses who have prepared statements, and we will ask Dr. Waterman, Dr. Alexander, Dr. Bach, and Dr. Shelton, and any other scientists who may be present and wish to join, to comment on their statements at their conclusion. I think it may be an orderly way to take the three statements which we have, which I understand are not too long, and have them in order, and then have the roundtable discussion.

At this time we would like to have Dr. Schulert of Lamont Laboratory to be our first witness.

STATEMENT OF DR. ARTHUR R. SCHULERT, LAMONT GEOLOGICAL OBSERVATORY, COLUMBIA UNIVERSITY [1]

Dr. SCHULERT. I have a statement which I prepared for the record. However, I do not believe it is necessary to read the statement in its entirety, since it would involve the repetition of many things that have transpired this morning. The statement involves my personal opinion as to what we should do along the research line in view of the present state of affairs. This, as you know, was quite comprehensively reviewed by Dr. Dunham of the AEC this morning.

I would like to just mention a few of the highlights of this statement.

(The statement referred to follows:)

REMARKS PREPARED FOR PRESENTATION BEFORE THE JOINT COMMITTEE ON ATOMIC ENERGY, UNDER SECTION XII—THE IMPACT OF THE PRESENT STATE OF AFFAIRS: WHAT SHOULD THE RESEARCH PROGRAM IN THE PHYSICAL, GEOLOGICAL, BIOLOGICAL, AND MEDICAL SCIENCES BE?

Arthur R. Schulert, Ph. D., Research Associate, Lamont Geological Observatory, Columbia University, Palisades, N. Y.

This presentation is given as a personal opinion of what the future research program should be in view of the present state of affairs with regard to fallout as discussed at these hearings.

The various testimony which has been presented to date, in spite of conflict in detail, has agreed on 2 basic points: (1) Present fallout represents a definite hazard in that it produces deleterious effects in man and (2) these effects are very small compared to the effect of background radiation which has always been with us. As has been brought out in previous testimony the problem of what our national policy should be in view of this small hazard is not a scientific problem, but a sociological, or a moral one. I think from these considerations, most scientists, and in particular those of us who have appeared before this committee agree that weapons testing should be limited insofar as it is possible and consistent with national security, and indeed, if it were possible, that such testing should be completely eliminated.

However, I would like to reiterate a point brought out by Dr. Dunham on the first day of these hearings—namely, that if we are to proceed into the atomic age with the accelerating tempo of development of nuclear energy, we are faced with the inevitable consequence of a continuous world burden of fission product debris, even without bomb tests, and although we can control this debris, from, e. g., a reactor, to a large extent, a certain irreducible minimum is bound to find its way into the soil and food chain and into man. What I am pointing out is that, bombs or no, we are inevitably going to live with man-made radiation in our bodies. Accepting the opinion that any radiation is dangerous or at least undesirable, it seems to me that a major research effort should be directed at keeping the hazard of fission debris at a minimum. Since internally deposited Sr–90 is generally agreed to represent the greatest long-term hazard, a major component of this problem is to keep the human burden of Sr–90 at a minimum for any given amount of fission. Or, in other words, how can we keep bone deposition of Sr–90 as small as possible and how can we keep the relative body elimination of Sr–90 as great as possible?

May I reiterate that this study is basic no matter what direction the atomic age may take if we accept the philosophy that all radiation is harmful.

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Before discussing in some detail what I feel should be done in this area, may I note that, of course, another important problem in which study should be continued and probably expanded is the problem of more precisely defining the physiological hazard per unit of radiation or per sunshine unit of Sr-90 with regard to such things as the genetic effects, leukemia, bone cancer, and life shortening, so that the degree of danger may be properly evaluated. Since the need in this area was brought out in testimony 2 days ago, and since it is outside of my sphere of activity, I will not discuss it further, except to say, that whatever the effects due to the presence of Sr-90 are, we can presume that they are proportioned to the Sr-90 concentration or approximately so, so that the basic practical problem remains one of reducing the human Sr-90 burden.

Now, if we are going to study the reduction of the body burden of Sr-90, and how this may be achieved, it is of initial interest to define what the present level is. This was dealt with by Dr. Kulp last week and I think the situation on this point is quite well in hand. We have been measuring Sr-90 in human bone samples for almost 4 years, and we now have a worldwide sampling network of over 25 stations sending us samples. We know what the average value for adult man is. We know that the young child is about five times this value. We have been able to follow the increment in Sr-90 in human bones from year to year. There are a few soft spots here that should be filled in. The sampling program should be expanded, particularly in primitive areas where because of low soil concentration of Ca and an exclusively local diet we may find higher values. Another problem is distribution and variation within a skeleton. We are currently attacking this problem through the extensive sampling and whole body determinations of 200 cadavers obtained through the cooperation of the New York City medical schools. We urgently need more information on children's skeletons, but such is difficult to obtain for obvious reasons.

Besides measurements in the bones it is of interest and importance to know the concentrations along the early stages of the biological chain—namely the foodstuff, and the soil. I feel here that the program might be stepped up somewhat particularly with regard to the food monitoring. Although milk has been followed quite adequately, other foodstuffs, to my knowledge have not. It seems to me that these food supplies should also be carefully monitored. Soil data should be extended, particularly in foreign locations.

Now to come back to what I initially stated to be, in my opinion, the key practical problem. How might we reduce the human skeleton deposition of Sr-90 for a given soil concentration of the isotope? We look for ways to block the Sr-90 transmission along the various steps of the biological chain. For completeness I'll just mention two possibilities which I haven't studied, but which have been considered, and which might be considered in the future. One is to bind the Sr-90 in the soil so that it cannot enter the plant. The other is to remove the Sr-90 selectively from the food before human consumption, which seems practically impossible, except perhaps from milk. Let us then consider the problem of minimizing the bone deposition for a given concentration of Sr-90 in the food. Considerable experimental work of this nature has been conducted by Dr. Laszlo in which he studied tracer doses of the short lived isotope Sr-85 instead of Sr-90.

Let me briefly explain the use of Sr-85. Sr-85 is another radioactive isotope of the element of strontium. It differs in that it has a relatively short half life of 65 days compared to the 28 years of Sr-90. We expect the Sr-85 to behave chemically and biologically in a manner identical to Sr-90—such a principle being well established and indeed the basis of a tracer work.

What Dr. Laszlo does is to conduct what we call a balance study; i. e., he carefully measures the amount of Sr-85 which the person ingests, and the amount which is excreted over a period of time in order to determine the net amount retained. He has found that certain agents, specifically, Ca gluconate, some chelating agents, and ammonium chloride reduce the net amount of strontium retained in the body.

From a practical standpoint this work has suffered from two difficulties which I think we can now correct: (1) The number of patients who are candidates for Sr-85 administration is strictly limited and (2) the Sr-85, being given over a short interval of time, is not distributed within the bone in the same manner as the Sr-90, which has been ingested continuously in small quantities for years, so that the results are not completely comparable.

We can now obviate the limitation and get at the precise problem since the level of Sr-90 in the food which man eats and the excreta which he eliminates

is sufficient to be adequately measured. Thus we can now study Sr-90 balance directly in humans and, having determined what the net retention is under normal conditions, we can determine to what extent the net retention may be reduced by control of diet and by various prophylactic and therapeutic measures.

May I add that although it is possible to conduct such experiments on ourselves, and we at Lamont have just completed the dietary phase of such an experiment, it is most practical for the most part to conduct such a study in a hospital metabolic ward such as Dr. Laszlo maintains. Such an extended study is contemplated for the near future.

In conclusion I would like to add that in my mind the extensive study of fallout points up a related problem with which we should all be concerned. If we agree, as we do, that it would be desirable to reduce the present fallout burden from Sr-90, then if we are to keep our proper perspective it seems that we should be even more concerned with other much greater sources of radiation around us that might be reduced. Since it is true that living in a brick house versus a wooden house may give a person an additional radiation equal to 20 times that currently received from Sr-90, should we not be concerned with investigating how one might reduce the background from these particular building materials, or the use of adequate substitutes with less radiation to take their place? If a chest X-ray may give 50 times the radiation currently received from Sr-90 in a year and a fluoroscopic examination will give over a thousand times this amount, should not these things be more rigidly controlled, the practitioner and the layman alike better informed as to their hazards, and basic research conducted so that these necessary medical tools may be used with a minimum of radiation? I realize that in the case of the X-ray, some such work is being done, and I'm not saying that the Atomic Energy Commission should get into this area. In fact they probably should not, but I am merely attempting to again put the fallout problem in perspective by noting that these are still much greater risks even in the field of radiation. Of course the fallout hazard still has the peculiar moral problem in that a person may choose whether or not he'll have an X-ray or live in a brick house, whereas in the case of fallout, there is no place to hide.

Dr. Schulert. First of all in the statement I have given the point of view, which is my point of view, that although as has been mentioned there is a considerable area of disagreement which has been evident during these hearings, yet there are some basic general areas of agreement, and we ought to emphasize what these areas of agreement are, and what should be done on the basis of what we do know.

I think we all agree that first of all—this is axiomatic at this point—a definite hazard does exist.

Point No. 2 is that this hazard, though it does exist, is very small compared to the effects of the natural radiation which has always been with us.

I might also interject here, in view of what has been said, that this hazard is small in my estimation and even if we stop bomb testing, we will still have the hazard to some degree due to reactors and so forth. In the industrial use of energy one can control the fission debris largely; yet there is a small irreducible minimum which is bound to get away and get into the human body.

If we take the philosophy that any radiation is hazardous and should be reduced, if possible, then this problem is with us, and the basic research which I will mention today is of importance. Of course, if tests continue it is more important; if nuclear war ensues it is very vital to the life of many people and possibly the Nation as a whole.

Then it seems the basic problem is twofold. Again I am being very axiomatic. First of all to define the hazard, and second of all, to reduce this as much as possible.

In the area of defining the hazard, the first thing might be to determine how much radiation exists. I think again we agree that

the principal hazard is the internal deposition of strontium 90. So the most important phase is to determine just how much strontium 90 is in the human body.

I think on this score we are in fairly good shape. This may sound like boasting since our Lamont Laboratory has been engaged in this phase. I should for the record also indicate that this was underway long before I came to Lamont and I am here in the absence of Dr. Kulp to take his place, and it is more of his show than mine, actually.

We do know pretty well—we have talked about the various uncertainties today—as to how good we know the data and how exact they are. We do know the average concentration of strontium 90 in humans. We know it in adults. I would say we know it to a factor of at least 1.5. We do know that the concentration in children is about 5 times this number from zero to 4 years old. We have been able to follow the increment from year to year. We have gotten some information—perhaps not enough yet—on the geographical variation in this concentration.

Representative COLE. Doctor, would you mind my interrupting?

Dr. SCHULERT. No, go right ahead.

Representative COLE. When you say you do know the concentration in children up to 4 years is 5 times this amount, what amount do you mean?

Dr. SCHULERT. The amount that is present in adults from 20 and above.

There are a few soft spots which I might fill in here. I think we need to expand the program particularly in primitive areas where, because of low calcium in the soil, and a local diet, we might find people with somewhat higher strontium 90 concentrations than the average. We are striving very much to get more data on the bones of children because this is the most important factor, since they are the higher values. As you might imagine, this is the hardest thing to do. We realize it is a rather grim problem to get bones at autopsy from children. The data is more limited here than we would like.

There also remains a little more work to be done on the correlation of the bone data with the data from the diet. I was made aware of this yesterday. We have bone samples from Chile, and we have learned at these hearings that the fallout comes down with the rain. Therefore, Chile, having very low rainfall would be expected to have a low amount of strontium 90 fallout, and it does. Dr. Alexander of the United States Department of Agriculture points out that Chile gets its milk from Wisconsin—dried milk—and this is their main source of calcium (and also strontium 90), and thus their bone concentration of strontium 90 is not as low as one might expect from their soil data.

Representative COLE. Doctor, on the question of milk, you have indicated the principal element of hazard is the strontium 90 and the method of being exposed to that hazard is ingestion. I think you will agree that the consumption of milk is probably the one single item of diet which contains the greatest concentration of this element.

Dr. SCHULERT. Yes.

Representative COLE. I am curious to know, and perhaps you cannot answer, whether there is any body of medical opinion whatever which feels that this degree of hazard of strontium 90 in milk is of

such size as to warrant discouragement of the consumption of milk by growing children?

Dr. SCHULERT. This raises an interesting point. It is true that even the average adult—and we have gotten the figures from many countries on this; do not ask me how many because I do not know offhand—in most places a good deal more than half of your calcium and therefore the strontium 90, we presume, comes from the milk. However, it is also true that the concentration of strontium 90 per gram of calcium is lowest in the milk. That is due to the fact that the cow very fortunately fractionates against the strontium 90. So that the milk that a cow produces has only about one-seventh the strontium 90 per gram of calcium that the cow's diet consists of.

We feel that the important number is the strontium 90 per gram of calcium due to the fact that they go together in the body. Therefore, I would say that the best place to get your calcium (and your strontium 90) is in the milk.

Representative COLE. You still have not answered my particular question. I say maybe you may not know whether there are any doctors of any considerable number or slight number who feel that this content of strontium 90 in the milk is of such amount as to warrant parents to find some substitute for milk.

Dr. SCHULERT. I do not know of any. As I tried to indicate, this would be bad advice. It is better to get your calcium from milk than other food sources, unless you are thinking of synthetic sources.

Representative COLE. I do not know what the alternative sources would be.

Dr. SCHULERT. From any natural source the milk would be the best source of calcium and it has the lowest amount of strontium 90 per gram of calcium.

Representative COLE. Of all the foodstuffs?

Dr. SCHULERT. Of all the foodstuffs we measured.

Representative HOLIFIELD. But it does have more calcium than any other foodstuffs.

Dr. SCHULERT. Yes.

Representative HOLIFIELD. And, therefore, the effect is that although it has less per gram of calcium, it has more in its totality than other foods.

Dr. SCHULERT. That is right.

Representative HOLIFIELD. Therefore, the child should have the milk to get the calcium for his bones and in getting that he must take in an unusual amount of strontium 90, more than he would if he used some other foods?

Dr. SCHULERT. No, you have to have a certain amount of calcium in your diet. I forget the figure. Maybe it is 10 grams. I don't know. If you have to have 10 grams of calcium, you better get it from milk because you will have less strontium 90 for this 10 grams of calcium than from other food.

Senator ANDERSON. I hope we do not leave the impression that milk is not still one of the finest foods for a child.

Dr. SCHULERT. That is right.

Senator ANDERSON. With or without regard to the dangers of strontium 90, there is no finer food in the world for children.

Senator JACKSON. Let us not limit it to children.

Senator ANDERSON. I know what Congressman Cole was trying to get to. In these discussions of how much strontium 90 there might be in milk, somebody might read it and say "I better be careful how much milk my child can absorb." I am completely out of the dairy business. I have sold all my dairy cows, so I am speaking as a disinterested witness. Surely there is nothing in the world that will do the child as much good in the formation of the good solid bone as the constant drinking of sufficient quantities of rich milk. I am very happy that Mr. Cole brought up this question so that before this hearing adjourns we could insert in the record firmly the fact that a child can drink all the milk he wants to all day long and not suffer from strontium poisoning so far as anybody now knows.

Representative COLE. I am very happy that the Senator has emphasized this point. I no doubt was derelict in accepting the doctor's answer with the simple word "no." He said he knew of no doctor who was concerned about the strontium 90 in the milk.

Dr. SCHULERT. You said concerned or was the suggestion that we avoid milk?

Representative COLE. Concerned.

Dr. SCHULERT. Concerned, certainly.

Representative COLE. I meant concerned to the point where they would recommend discontinuance or lessening of consumption.

Dr. SCHULERT. To correct the record, the calcium requirement for a day is 1 gram and not 10.

Senator JACKSON. Do you feel that you can make any kind of prediction now as to what will happen to people as far as strontium 90 is concerned if testing continues at the same current rate?

Dr. SCHULERT. In my prepared statement, and I can say it here——

Senator JACKSON. This is a factor that is not in the natural background. That is why I am asking this question. This is a new element.

Dr. SCHULERT. That is right.

Senator JACKSON. Could you tell us what in your best scientific judgment is apt to occur assuming that testing continues at the same rate, and that the strontium 90 continues at approximately the same rate, as a hypothetical matter?

Dr. SCHULERT. This has been discussed in the hearings before.

Representative HOLIFIELD. The Chair was going to say that this subject was treated exhaustively yesterday in the conference. I have no objection to the question.

Senator JACKSON. I was asking his opinion.

Dr. SCHULERT. It is my feeling that we can compare strontium 90 radiation at least to cosmic radiation, because the energy is roughly the same. To the other natural background it perhaps makes some difference. But the strontium 90 radiation today is about 2 percent of normal background. If testing continues at the present rate, it is calculated that this will go up by a factor of about 8 ultimately. So that this would make it 16 percent of the normal radiation.

Senator JACKSON. What about bone cancer? What is going to be the pattern?

Dr. SCHULERT. Whatever the effect of natural background is today, this would be increased by 16 percent. You can make certain assumptions as to how much bone cancer is due to natural radiation, and this

is uncertain, but whatever the effect is, it would be 16 percent greater. That is the way I look at it.

Senator ANDERSON. I have one question I have been wanting to ask with reference to this. The chart shows that 98 percent of the total radiation is background, and 2 percent may be accumulation from testing and other things. Is there a possibility that the 98 percent may not be as destructive as the 2 percent? By that I mean, is there a possibility that we may have developed through thousands of years a tolerance to cosmic rays that we do not have to strontium?

I see some shaking of heads, but nonetheless I recognize that when we first put out DDT, it was very effective for certain types of insects. Gradually they have developed quite a tolerance to it. We can go through a whole list of remedies and new devices that we have taken, such as penicillin in the human, and the human develops the situation after a while where his diseases do not respond to penicillin, because a tolerance has been developed to it. Could it be that over thousands and maybe millions of years of existence humans have become accustomed to cosmic rays, and therefore are not damaged by the natural background radiation to the same degree that they are bothered by this wholly new substance, strontium 90, that we never knew until 1945?

Dr. SCHULERT. I think in science in general, and particularly in biological science, it is difficult to state anything with absolute certainty. A scientist thinks in terms of probabilities. My own opinion is that if the energy of the cosmic radiation is very close to that which we get from strontium 90, if we have accustomed ourselves to radiation from cosmic ray, we have also accustomed ourselves to the radiation from strontium 90, there would be no added factor.

How sure am I? I can only guess, and I can say I am 99 plus percent sure of this. Since the energies are not exactly identical, it is possible that this precise energy of strontium 90 could hit a vital enzyme in a very special way to break a very critical bond and be much more hazardous. I say the chance is one in a thousand or less. This is the best I can do.

Senator ANDERSON. I realize that nothing is provable, perhaps, with reference to this, but it does seem strange that for a long, long time we have stressed the benefit of sunshine. I recall when I first went to the Southwest suffering with tuberculosis the very first thing the doctor stressed to me was that sunshine was going to be my great curative power. We have had testimony that all types of certain rays—I am not talking about cosmic rays, but strontium 90 and others—may be harmful. I am wondering if natural background radiation is also harmful because we have been talking about living in the sun. If you look at the population statistics, there is a tendency in this country to move into the more sunny States. I just wondered if it is not possible that the human being has not developed a tolerance to sunshine that perhaps makes it beneficial to him. It is a matter that probably is not of importance. I wondered why we worried about strontium 90, if we are worried, and I am, if only a minute particle of radiation we are now receiving and such a large part is already natural background.

Dr. SCHULERT. I share your reaction to the fact that so much is made of the fallout of strontium 90 when the natural background we have

about causes much more radiation. In fact, in the conclusion of my remarks, I do bring out the fact that I think this discussion of fallout should remind us of more important radiation hazards. If it is true that living in a brick house today on the average will give you 20 times the radiation that our present load of strontium 90 will give us, is it not important to try to change our technology to either get this radiation out of the bricks or find substitutes for the brick? If it is true that the average chest X-ray will give you 50 times the radiation that you get from strontium 90 in a year today, and if a fluoroscopy gives you better than a thousand times, should not these be more rigidly controlled? Shouldn't both the practitioner and the layman be educated, and shouldn't research be done so that these things can be reduced to a minimum, still consistent with their need in medical practice?

I think this is true and the people get frightened about fallout and forget the fact that they have had six X-rays in the last 3 years, or had a fluoroscopy and don't worry at all about it.

Representative HOLIFIELD. You may proceed.

Dr. SCHULERT. The other area of defining the hazard, if we confine it to strontium 90, which is the principal hazard, after we know this level, what will the physiological effects be. This has been dealt with in earlier testimony this week. This is an important problem. Of course, research should be continued so we can define as well as possible what the physiological hazard is.

From what I gathered earlier in the week, these things are known to a degree. The degree is not good. Perhaps it is a factor of three. This is much better than knowing nothing about it. We know it to a degree, but it is important to define the degree more precisely. Then going on to trying to reduce the hazard, there are a number of aspects we can consider along this line. Principally again if we stick to strontium 90, how do we reduce the amount of strontium 90 that remains or finds its way to the bone for a given amount of strontium 90 which is released or finds its way into the soil.

If we consider the strontium 90 in the soil, there may be ways to reduce its uptake by plants. This has been considered and it is a tough problem. Once it gets into the food there may be ways to get it out of the food in some instances, but the only example I can think of, is the case of milk. It is possible to take the strontium and calcium out of milk and replace your calcium so that you have milk with no strontium 90.

This is, I am sure, very expensive and technologically it would be very difficult to do on a routine basis.

I would like to confine my remarks primarily to the fact that given a certain amount of strontium 90 in the food, how can we reduce the amount that lodges in the bone. There has been some study on this. Dr. Laszo, the chief of the division of neoplastic diseases at Montefiore Hospital, has studied this problem. He has studied the problem in patients who have received for medical purposes strontium 85. Strontium 85 is another isotope of the element strontium. It differs by virtue of the fact that it has a physical half life of only 65 days whereas strontium 90 has a half life of 28 years. We would expect the strontium 85 to behave chemically and biologically in the manner identical to strontium 90. In fact, this is the basis of all tracer work,

and has been well established, I think. Therefore, we can assume that this is true, and use strontium 85 in our experiments.

What Dr. Laszlo has done is to run what we call a balance study in patients, and the balance study involves the careful measurement of the ingestion of the material, strontium 85, and then a careful measurement of its elimination. Thus if you take the amount ingested, subtract the amount which is eliminated, you have the net uptake by the body. So our problem, then, resolves itself into how can we make this net uptake as low as possible.

Dr. Laszlo and others have found that you can do this to some extent by administering certain therapeutic agents, such as calcium gluconate, chelating agents, ammonium chloride, and others. There are two difficulties, I should say, with the experiments.

One is that these are in hospital patients, and the number of hospital patients who are candidates for strontium 85 administration are strictly limited. So it is a very limited study.

Secondly, when we test this removal we test the removal of strontium 85 which is administered over a short period of time, and this is not the same as testing the removal of strontium 90 which has been ingested in small quantities for a number of years. So the results are not directly comparable.

I think we can now obviate these difficulties because at the present level of strontium 90, it is now possible to measure directly the strontium 90 intake of an individual and his excretion, so that we can do this on you or I or anybody, and having found the net retention under today's conditions, then try various agents to reduce this net retention.

I should add, however, that although you can do it on yourself, and I can do it on myself—in fact, our Lamont group has just completed a dietary phase of such a study, although we don't have the numbers as yet—for the long haul it is much more convenient, shall I say, to do this on hospital patients in a metabolic ward. Shall I say it is socially inconvenient to carry around a suitcase.

So we plan to expand our studies in this direction, not only getting the net retention under present conditions and studying them with various prophylactic and therapeutic agents, but this will give us direct results on this discrimination factor which has been bandied back and forth, another factor which should be known with more precision.

If we can actually do the discrimination under normal conditions with strontium 90 and common calcium, this should give us the answer in this area.

I believe that concludes my remarks.

Representative HOLIFIELD. Thank you very much, Dr. Schulert.

Senator JACKSON. Thank you. You have presented a very good statement.

Representative HOLIFIELD. Are there any comments on Dr. Schulert's statement that anyone would care to make at this time?

Dr. DUNHAM. I am only glad to say that I am happy he was given the opportunity to tell you of the painstaking work involved in these discrimination factors.

Representative HOLIFIELD. Then we will have the Public Health Service witnesses on next. We have Dr. LeRoy E. Burney and Dr. James A. Shannon. Before they present their statements I will insert

in the record at this point a statement from Dr. Henry Blair, director, University of Rochester Atomic Energy Project and his associates.

(The matter referred to is as follows:)

THE UNIVERSITY OF ROCHESTER,
SCHOOL OF MEDICINE AND DENTISTRY,
Rochester, N. Y., June 10, 1957.

A SCIENTIFIC STATEMENT TO THE JOINT CONGRESSIONAL COMMITTEE ON ATOMIC ENERGY

(Atomic energy project—Administered by the department of radiation biology under contract with the United States Atomic Energy Commission)

Mr. Chairman, the following statement represents the considered scientific judgment of the undersigned relative to one of the directions in which we believe the research program in biological and medical sciences dealing with radiation effects should be oriented for the next 10 to 20 years.

Increased emphasis and support should be afforded to long-term studies of radiation effects in longer lived species of animals such as dogs and monkeys whose general physiology and life span more closely approximate those of man.

Much of the present discussion and difference of opinion concerning the biological hazard of atomic fallout and the hazard of exposure to ionizing radiations result from the fact that data from which one can confidently draw conclusions which potentially affect the future of man and of our biosphere are sparse. Many of the available data are based upon painstaking attempts to reconstruct the conditions under which humans were accidentally exposed to toxic materials as long as a quarter of a century ago. There are obvious limitations to the reliability of such data. Other data are based upon careful experimentation with laboratory rats and mice. As useful as the information obtained in studies of such short-lived animals has proven to be, one must be extremely cautious in extrapolating such information to problems affecting man.

By way of perspective we might note that a life-span study using mice requires 3 to 4 years from inception to completion. Similar studies in the laboratory rat may require 4 to 5 years to complete. Studies in longer lived species such as the dog and monkey, whose physiology and life-span characteristics make them more nearly comparable to man, may require from 7 to 15 years for completion.

It is only when long-term studies, some of which are now in progress, have been completed and the information analyzed will we be able to assess with confidence the influence of atomic energy upon man's biological future.

Scientists are somewhat hesitant to undertake such studies for two very important reasons:

1. Such chronic experiments are extremely difficult to perform and sometimes may appear to be impossible to complete. Undertaking these experiments may involve an investment of 5 to 10 years of a man's time before significant scientific contributions appear. Unfortunately a scientist is judged frequently by his immediate scientific productivity and not necessarily by what he might contribute many years hence.
2. The scientific hazards of long-term investigations are of such a nature that even after many years of intense and careful work the experiments may "blow up" as a result of disease or epidemics or accidents in the experimental colony.

Thus the scientist who undertakes work of this nature takes two deliberate and calculated risks—the probable decrease in immediate scientific productivity upon which advancement depends; and the possibility that the experiment may never answer the questions it is designed to answer.

Laboratory program directors and responsible scientific administrators support long-term programs with some trepidation, because such programs are extremely expensive to support and tend to reduce the flexibility of a laboratory in pursuing important shorter term objectives. The nature of fiscal support of research is such that a minor fluctuation in the level of support afforded a laboratory which has long-term research commitments can wipe out much worthwhile short-term research, whereas a substantial budgetary fluctuation may wipe out partially completed work involving many years of scientific effort.

We stress the need for balance and continuity in the support of fundamental and applied research of a short-term nature and research of a long-term nature.

To permit one type of research to prosper at the expense of the other is to jeopardize scientific progress.

Some of the problems resulting from exposure to radiation which are subject to systematic attack by long-term investigations are the following:

1. The relationship of radiation exposure to somatic and genetic mutation.
2. The factors influencing development of cancer and the experimental therapy of cancers.
3. The aging process, whether aging results from exposure to ionizing radiation, exposure to toxic chemical agents, or to natural causes.
4. Fertility and sterility.

The above concepts and opinions are based upon many years of personal experience and observation of both the physical details of performing scientific research and in administrative responsibility for broad programs.

We believe that a statement by the members of the Joint Committee on Atomic Energy affirming its belief in the necessity and desirability of performing long-term animal experiments in the broad fields of radiation effects, carcinogenesis, and cancer therapy, of the aging process, and of genetics would have a widespread and beneficial effect upon both the scientists who must eventually perform this research and upon the public as a whole, whose concern about these problems is already evident.

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Chief, Radiation Toxicology Section, and Assistant Professor of Radiation Biology.
GEORGE W. CASARETT, Ph. D.,
Assistant Professor of Radiation Biology.

Representative HOLIFIELD. Dr. Burney, we are happy to have you before us again. Would you like to proceed with your statement?

STATEMENT OF DR. LEROY E. BURNEY,[2] SURGEON GENERAL, ACCOMPANIED BY JAMES G. TERRILL, JR., CHIEF, RADIOLOGICAL HEALTH PROGRAM, PUBLIC HEALTH SERVICE, DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE

Dr. BURNEY. Thank you, sir.

Mr. Chairman and members of the committee, it is a privilege to appear before this committee, and I do appreciate, on behalf of the

[2] Date and place of birth: December 31, 1906, Burney, Ind. Education: Butler University, Indianapolis, Ind., 1924–26; Indiana University, bachelor of science, 1928; doctor of medicine, 1930. Passed Indiana State Board of Medical Examiners, 1930. Rockefeller fellowship in Johns Hopkins University School of Hygiene and Public Health, 1931–32, doctor of medicine in public health. Work history: Interned at U. S. Marine Hospital, Chicago, Ill., 1930–31; commissioned in Regular Corps of the Public Health Service, 1932; established first mobile venereal-disease clinic service in Brunswick, Ga., 1937–39; Assistant Chief, Division of States Relations, Public Health Service, Washington, D. C., 1943–44; detailed to Navy for 5 months in 1944 and sent overseas by War Shipping Administration to investigate and determine effective measures for diminishing amount of communicable diseases in various Mediterranean ports, especially venereal diseases; director of district No. 4, Public Health Service, New Orleans, La., 1945; secretary and State health commissioner, Indiana State Board of Health, on detail from the Public Health Service, July 1, 1945, to August 1954; Assistant Surgeon General and Deputy Chief, Bureau of State Services, Public Health Service, August 1954 to 1956. Recess appointment as Surgeon General, Public Health Service, August 1956. Membership: American Medical Association, Indiana State Medical Association, Indianapolis Medical Society, fellow of American College of Physicians, fellow of American Public Health Association, past president of State and Territorial Health Officers, conference of State and Provincial Health Officers, Indiana Public Health Association, founders group and trustee, American Board of Preventive Medicine, regent for region 4, American College of Preventive Medicine, Commission of Chronic Illness. (Submitted by Department of Health, Education, and Welfare.)

Public Health Service, an opportunity to discuss briefly the deep interest which the Public Health Service has in the subject of radiation and radioactive fallout, and the activites in which we are engaged.

As the principal health agency of the Federal Government, the Public Health Service is concerned with all those factors in our society which affect the health of people. We in the Service believe—as do our colleagues in public-health work throughout the country—that the emerging and challenging problem of radiation deserves the most careful study and attention. I believe two essential points have been brought into focus:

The first is the great increase in sources of radiation as a result of the accelerating use of nuclear energy for all purposes—military, industrial, and scientific. This will require an acceleration in radiological health research and in the development and application of public health control measures.

The second point is that the varying views that have been expressed by scientists illustrate the lack of definite, generally accepted knowledge about the effects of radiation on human health. It is clear that we do not yet know the full consequences of chronic low level exposure. Geneticists have raised the question of the genetic effect of radiation on future generations.

What has been the role of the Public Health Service with respect to the problem of radiation? What can be done to protect people from its possible hazards and at the same time promote its beneficial uses? What is needed to help State and local agencies which are responsible for health preservation of the public?

In considering these questions, we must keep clearly in mind that health agencies must deal with the total problem of radiation, of which fallout, of course, is a part. For example, the efforts of the Public Health Service in this problem go back over several decades.

In the 1930's, the Public Health Service industrial health personnel shared in the investigation of radium poisoning among painters of clock dials, and later helped solve the problem of safe disposal of radioluminous instrument dials. In 1940 the Public Health Service conducted basic work on radium and X-ray health hazards in hospitals.

Dr. Leonard Scheele, the former Surgeon General, participated in this work, along with Dr. Lorenz, who was mentioned this morning.

Research conducted by the Service staff members contributed significantly to the establishment of human tolerance for radiation and performed other staff work at the Manhattan District project. As a result of these studies, Dr. Egon Lorenz served as a consultant in radiation protection at the Manhattan District. Early in the development of mass X-ray programs, the Service developed techniques for safeguarding the health of the operators of X-ray machines. In 1948 the Service set up a branch to conduct training and investigations in the growing field of radiological health. That branch has been active in providing services to the States in this field.

In addition, the Public Health Service has carried out for the Atomic Energy Commission and Joint Task Force 7 the monitoring of levels of radioactivity at the Nevada and Pacific Proving Grounds. We have presented a technical paper on this activity for your records. (See statement of James G. Terrill, Chief, Radiological Health Program, USPHS, p. 328.) In this activity, we have concentrated on

measuring the radiation received by people in the offsite test areas and on studying the means to reduce exposure. We have also studied reactor waste treatment and discharge problems in cooperation with the Atomic Energy Commission.

In sum, therefore, our concern has been with the total radiation exposure of people, whether the source is natural background, medical equipment, nuclear reactor plants, or weapons testing programs. We have undertaken to discover how much and what kinds of radiation emanate from the various sources and how each affects human beings.

The problem is complex. Ionizing radiation, for example, is an important tool in medical diagnosis and treatment, and each year helps conserve the health of thousands of people. Yet it can obviously cause undesirable and sometimes dangerous effects and, if not used with utmost care, can produce, for example, the cancer it is employed to cure. Thus public health agencies are faced with the problem of promoting the maximum use of this valuable tool and at the same time of developing protective measures against the potentially harmful effects. The deceptive latent period between exposure and effect, the cumulative nature of the action, and the difficulties in measuring radiation have commanded the attention of health workers throughout the Nation.

The National Academy of Sciences has said that the healing arts probably constitute the largest single manmade source of external radiation at the present time. The fact that exposure from other sources is on the increase makes it necessary to reexamine and reassess the use of radiation for research and for healing purposes. Exposure levels from a given source which might have been acceptable in the past may now—or in the future—become hazardous when added to the exposure from other sources.

Turning now to the specific problem of fallout, there seems to be general agreement that the principal long range hazard from weapons testing is through the production and assimilation of strontium 90. The most significant question in the strontium picture is the maximum permissible concentration for human beings. This aspect of the problem is further complicated by the fact that children probably assimilate strontium to a greater degree than adults because their bones are growing, and the rapidly growing tissues of the child are more susceptible per unit of radioactivity. The current concern with this radioactive element must not preclude, however, the continuing study of other radioactive elements, either alone or in combination.

Along with many other agencies we in the Public Health Service recognize the potentialities of the proportional or linear relationship of radiation effects to dosage, particularly with regard to genetic, cancerogenic and aging effects. Pending further evidence, we believe that the recommendations of the National Committee on Radiation Protection represent a reasonable balance between radiation effects and exposure. There is a great need, however, to measure existing radiation levels accurately and to resolve in a more definitive way the biological relationships involved.

The question of biological relationships will be discussed in greater detail by the Director of the Public Health Service's National Institutes of Health. But I do wish to point out now that we are vitally interested in obtaining better data on the fundamental relationships

beween radiation and man. We do not expect to obtain conclusive data in this area in a short period of time. Satisfactory answers will require careful and extensive studies.

Much of our current information on the effects of radiation, particularly the delayed effects of low-level radiation on human beings, is extrapolated from studies with animals. These studies indicate that the damage produced from such exposures is generally similar to diseases and other abnormal conditions already present in the population from causes other than radiation. It follows, therefore, that statistically valid analyses of changes in the incidence of such conditions in the population are needed in order to assess the impact of radiation on man.

A sound scientific basis for collection of data of this type is through epidemiological followup studies of relatively large numbers of people who are incidentally exposed to known amounts of radiation. Studies can be made with groups in certain industries and the healing arts in which the exposure history can be obtained or measured. Epidemiological studies with these groups are fundamental to the problem of obtaining data on which to base realistic, maximum permissible exposure limits, as well as to calculate the risks incidental to any given radiation exposure.

One such study which the Public Health Service is conducting is a long-term followup of the health status of uranium miners on the Colorado Plateau. Several years ago work was initiated to determine the extent of radioactive contamination of the atmosphere in uranium mines and to study and put into effect appropriate control measures. The environmental aspect of this study is continuing, and periodic clinical examinations are being made on more than 1,100 uranium miners. One such clinical examination is now being made on this group of 1,100 miners. Additional information on this study has been submitted to your committee for the record.

Senator ANDERSON. Does that study show anything thus far?

Dr. BURNEY. No, sir.

Senator ANDERSON. They have been mining on the plateau for many years. Have you see anything yet?

Dr. BURNEY. No, sir. We have not found anything in this period of time, but we rather doubted that we would, although some of these people go back to around 10 years of having worked in this area. A large number of them worked in this atmosphere only for 2 or 3 years. With the long latent period we are not surprised not to have found anything.

Senator ANDERSON. Is there any difference in the big Indian area of Utah as compared to the sort of open pit mining that has taken place in the grants area?

Dr. BURNEY. May I refer that to Mr. Terrill here, who has been doing that work?

Senator ANDERSON. I do not care who you refer it to. Is there a difference in the development of exposure inside a mine like the Mivida mine as contrasted with the open mining that Anaconda is doing in the Ambrosia Lake area?

Dr. BURNEY. I would suspect that there is, but I would want Mr. Terrill to answer that question.

Mr. TERRILL. The principal method utilized to lower radiation exposure in the mines is related to ventilation. So the actual concentrations to which the miners are exposed are a function of the ventilation in those mines, as well as the concentration of the uranium ores.

Senator ANDERSON. That is interesting, but does it answer my question? There is no ventilation in the Anaconda open-pit mine. Therefore, I am trying to find out is there any difference in the way a miner is exposed inside a mine of the Mivida nature as contrasted to the open-pit mining in the Anaconda mine?

Mr. TERRILL. Sir, I got the location of the Anaconda mine, but what was the other place?

Senator ANDERSON. The Mivida mine in the Big Indian country in Utah. Take any one. I do not care which one you take.

Mr. TERRILL. They have been relatively small mines.

Senator ANDERSON. They are ventilated?

Mr. TERRILL. Yes.

Senator ANDERSON. Does a miner working in the shaft of that mine with a relatively high uranium content ore, contrasted with work outdoors with the low-grade ore of the Anaconda Co. pick up more harmful radiation? Seventy percent of all known uranium reserves of this country are located where the open-pit method can be used.

Mr. TERRILL. Generally a person who is mining in an open pit would receive less radiation exposure than one who was working in a mine where you are dependent on artificial ventilation. On the basis of field tests the natural ventilation prevents the accumulation of radon and its decay products and thus reduces exposure very significantly.

Senator ANDERSON. Do we have any study which proves that?

Mr. TERRILL. We have a detailed study on underground mines, sir, and I think we have submitted it for the record. We have not studied open pits so intensively because the gross measurements have been so low. All of the details and all the mines that we have investigated have been written up and published in an article by Holiday and others working out of our Salt Lake City field station.

Senator ANDERSON. Does the examination of these 1,100 miners show that they get a relatively larger exposure than those individuals not working in mines, and do we therefore conclude that miners working in the unanium mines need to be rotated more than miners working in a lead, copper, or coal mine?

Mr. TERRILL. Generally they receive more exposure to radioactive materials than other miners.

Senator ANDERSON. I am sure they receive more exposure if they are in a uranium mine than if they are in a coal mine, but what does it do to them?

Mr. TERRILL. That is what the clinical studies are intended to determine over a period of time.

Senator ANDERSON. Then is the answer, We do not yet know?

Dr. BURNEY. The answer is we do not know. In this study, as in similar environmental health programs, we have made use of the cooperative relationships built up through the years with the State and local health agencies. In assessing the health problem, these agencies must consider exposure from all sources of ionizing radia-

tion—weapons, reactors, industry, medicine, and natural background. If the total exposure begins to approach maximum permissible concentrations, it is important that public health agencies seek ways to assess the significance and to apply techniques to maintain exposure at the lowest practicable level.

Senator ANDERSON. In your statement you say the most significant question in the strontium picture is the maximum permissible concentration for human beings, and here you have said something about the maximum concentration.

Dr. BURNEY. We have been using the recommendations of the National Committee on Radiation Protection.

Senator ANDERSON. Do you subscribe to that?

Dr. BURNEY. Yes, sir.

Senator ANDERSON. Did you make an independent investigation of it?

Dr. BURNEY. I can't answer that. We do work with Dr. Taylor and his group on this.

Mr. TERRILL. The Public Health Service is represented on the National Committee on Radiation Protection and all of the data that we collected through our National Institutes of Health or other studies are routinely submitted to the National Committee, and considered by it and our membership on it; yes, sir.

Dr. BURNEY. In other words, control procedures to minimize individual hazards should accompany any anticipated increase in radiological exposure.

Through measurement and epidemiological techniques, we should be able to evaluate in the field many of the sources and to obtain reliable estimates of exposure. This should lead to specific and practicable suggestions on ways in which radiation exposure can be reduced. For example, our studies of water decontamination indicate that the most practical method of reducing radiation exposure from water supplies is through adequate waste treatment and through monitoring and bypassing that portion of the water supply which may temporarily contain undesirable amounts of radioactivity. In order to do this, it will be necessary to do more frequent sampling of water supplies to keep track of any increases over normal background radiation.

Senator ANDERSON. When you refer to waste treatment, is there some established pattern of treating wastes?

Dr. BURNEY. We are investigating, again in some instances, with the Atomic Energy Commission and some on our own at the Robert A. Taft Sanitary Engineering Center at Cincinnati, the problem of treatment of industrial wastes or other radioactive waste. As far as I know, there is no economical and satisfactory method at the present time for major sources of nuclear waste.

Senator ANDERSON. I read your sentence again. For example:

> our studies of water decontamination indicate that the most practical method of reducing radiation exposure from water supplies is through adequate waste treatment.

In order to find that out, you must have know what adequate waste treatment was. What was it?

Dr. BURNEY. This would depend on the elements that were present in this treatment. We do know that normal waste treatment, includ-

ing filtration and aeration and even chlorine treatment, does not remove all of the radioactive material. But it does help to a certain extent.

Senator ANDERSON. To what percentage would you estimate aeration would remove the waste?

Dr. BURNEY. I will have to ask Mr. Terrill for the answer.

Mr. TERRILL. Mr. Anderson, we have found that normal types of treatment remove radioactivity only in terms of percentages comparable with other similar chemical substances. In most instances in the radioactive waste treatment we are interested in removing substances by several orders of magnitude. Those orders of magnitude, of course, vary anywhere from your high-level wastes that you have to treat at AEC processing installations on down to relatively low levels of waste that you get either industrially or from medical uses. Special waste treatment methods, such as distillation and ion exchange remove radioactivity by many factors of 10, but these methods are comparatively expensive and don't solve the disposal problems.

Dr. BURNEY. His question was what percentage of removal can we expect from waste treatment.

Senator ANDERSON. That is right. You have a reactor operating at Hanford. It has radioactive wastes. What percentage of the radioactive waste in there could be handled by aeration, as you have suggested? Practically none?

Mr. TERRILL. Practically none.

Senator ANDERSON. You could say that for the other accepted fashions?

Mr. TERRILL. Yes; for normal waste-treatment methods.

Senator ANDERSON. We do have a problem of waste disposal, do we not, that has not yet been solved?

Dr. BURNEY. That is correct.

What is needed, generally speaking, is a program that will be alert to any changes in our radiation environment and that will be ready to institute control measures which may be called for.

In the field of medicine, advances are being made in radiological techniques which result in lower exposures to clinical personnel as well as to patients. A concerted effort on the part of the health professions is needed to control harmful amounts of radiation in connection with diagnositic procedures—and such an effort is already underway.

These are but a few of the areas in which action can be taken. Their effectiveness, however, depends to a considerable extent on the participation by official health agencies and educational and scientific institutions, as well as on the close cooperation of the civilian and military agencies of the Federal Government.

In order to assume their proper roles, States and local agencies—in whom basic responsibility for protecting the health of their citizens rests—need to develop more technical competency in the field of radiological health. The Public Health Service has been able to provide short orientation and special training courses, primarily to States and local health personnel. We have also used our commissioned officers in monitoring radioactivity levels for the Atomic

Energy Commission in Nevada, thus providing firsthand field experience to personnel from health agencies.

To meet the long-range needs of health agencies, we believe it will be necessary for the professional and scientific schools of the country, which provide the basic training for our health workers, to strengthen their programs. Some Federal assistance is being provided through our general public-health traineeship program and through the research grants and fellowship programs administered by the Public Health Service's National Institutes of Health. It is apparent, however, that more specific emphasis will be necessary to provide the number of trained persons required.

In summary, we believe that radiation is a health problem of growing significance. Health agencies are, of course, looked to for leadership and counsel wherever a health problem exists and they must face the problem of radiation as it relates to health with vigor and determination. To do the job will require cooperation among all concerned at all levels of government. The Public Health Service pledges itself to this ideal.

Senator ANDERSON (presiding). Are there any questions?

Chairman DURHAM. Doctor, I am sorry I was not here at the beginning of your statement. What kind of a setup have you got in your department for this type of operation?

Dr. BURNEY. Basically we have two groups that are concerned with this problem. One is at the National Institutes of Health, where research and training in the broad field of physical biology which includes radiobiology has been done for many years, in which research is done within the various institutes—the National Cancer Institute, the Heart Institute, the Arthritis and Metabolic Institute—and also in which grants are made to outside scientific institutions for research in the field of radiobiology.

We have at this present year, I believe, about $2 million worth of grants going to scientific institutions for research in radiobiology itself. Then training programs for training research workers in the various sciences related to radiobiology—biophysicists, biochemists, and similar groups.

Chairman DURHAM. Is that entirely related to the field of radiation?

Dr. BURNEY. Yes, sir. It includes the effects of radiation in production of cancer. It is not entirely on the matter of radiation exposure as we are talking about it here.

Chairman DURHAM. Is all of that information made available to the different State public-health agencies?

Dr. BURNEY. Yes, sir, it is. Then in addition to that, in our field activities we have a very effective relationship with the Atomic Energy Commission and have had for a number of years in working with them on field studies. We have one or more men working at the Oak Ridge Laboratory, on the industrial waste problem that Senator Anderson mentioned, to try to solve that. We are also doing work at our sanitary engineering center in Cincinnati, in trying to evolve methods of disposing of radioactive wastes, and measuring radiological effects.

We are concerned in our air-pollution program as well as our water pollution with developing better sampling techniques, both in air and

water. Of course, radioactivity detection is an important part of that.

Chairman DURHAM. You have full cooperation from the AEC in all matters pertaining to this?

Dr. BURNEY. Yes, sir; we work very closely with them.

Chairman DURHAM. Can you give the committee the amount of money that your agency is spending on this program?

Dr. BURNEY. Excluding the National Institutes of Health—as I say, $2 million of those funds is for extramural research grants—Dr. Shannon may be able to tell you exactly how much we spend on our intramural programs. He tells me $250,000 on direct research.

Chairman DURHAM. Do you submit a separate budget for this, Doctor, identified as radiation health hazard?

Dr. BURNEY. No, sir, it is not; it is part of the National Cancer Institute budget, part of the arthritis and metabolic-disease budget. We do have a special Radiation-Study Section which reviews all of the requests.

Chairman DURHAM. This is a growing matter of concern. Have you given any thought to treating this as a separate item?

Dr. BURNEY. We do have, other than our research program at the National Institutes of Health, a separate item for radiological health. That at the present time amounts to $347,000. We requested quite a sizable increase in 1958. The House allowance was not quite what we asked for.

Chairman DURHAM. Would that apply on waste hazards?

Dr. BURNEY. Yes, sir. Research in waste treatment. It applies also to the training of State and local personnel in this area. It applies to the epidemiological studies we are trying to do, and it applies to the technical help we give the States. For example, one State wanted to develop a radiological health program. They had no experts.

They asked us to lend them an expert in the field for 2 years until they could develop their own people. At the end of that 2 years' time, they had their own people. We brought our person back and he is working with us again.

So that is the kind of technical assistance that we give to the States.

Senator ANDERSON. Thank you, Dr. Burney.

(A supplementary statement by Dr. Burney follows:)

STATEMENT TO SUPPLEMENT PRESENTATION BY THE SURGEON GENERAL, PUBLIC HEALTH SERVICE, DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE, BEFORE THE JOINT COMMITTEE ON ATOMIC ENERGY HEARINGS ON THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

An example of the long-term epidemiological and statistical study needed to better define the effects of ionizing radiation on humans is the continuing evaluation of the health status of uranium miners on the Colorado Plateau. Several years ago work was initiated to determine the extent of radioactive contamination of the atmosphere in uranium mines and to study and put into effect appropriate control measures where needed. The environmental aspect of this study is continuing and the data were reviewed in a recent publication by the Public Health Service.[1]

[1] Control of Radon and Daughters in Uranium Mines and Calculations on Biologic Effects: U. S. Department of Health, Education, and Welfare, Public Health Service Publication No. 494.

Various aspects of the degree of environmental contamination and measures for its control have also been reviewed in other publications.[2 3 4 5 6 7 8]

Armed with knowledge regarding the levels of radiation exposure, and following pilot work during previous years, a full-scale epidemiological followup study was initiated in 1954. At that time, physical examinations were conducted on 1,124 uranium miners in the field, to serve as a baseline for future observations. At yearly intervals since that time, a census has been conducted to define the current status of each individual in the study and this month a series of followup physical examinations will begin. To date, the followup has been quite successful. Although the group under study presents technical problems in followup that are somewhat more complicated than might be true in other groups, primarily because of its somewhat transient nature, approximately 95 percent of this original group have been successfully contacted since their original examination. It is anticipated that by the use of more sophisticated methods which are available to organizations with competencies in the field of epidemiology, this followup rate may approach 100 percent.

To date, it is much too soon to derive any definitive conclusions from this study. Among the reasons is the fact that previous experience,[9 10] has indicated that the damage, if any, might be delayed for many years after the beginning of exposure. The following table indicates the percentages of individuals under study falling in each of several categories with regard to the number of years of exposure to date:

Percentage distribution of miners by years exposure

Years	Percent	Years	Percent
Less than 1	38. 2	4 to 6	14. 4
1 to 2	20. 2	7 to 9	4. 2
2 to 3	11. 6	10 to 12	1. 0
3 to 4	8. 6	13 and over	1. 8

Although this study illustrates some of the problems which one encounters in conducting a long-term epidemiological and statistical study, we believe it also demonstrates the fact that epidemiological studies in this field can be successfully prosecuted.

Dr. Shannon, will you come forward please?

STATEMENT OF DR. JAMES A. SHANNON, DIRECTOR, NATIONAL INSTITUTES OF HEALTH, PUBLIC HEALTH SERVICE [10a]

Dr. Shannon. Mr. Chairman and members of the committee, the Surgeon General of the Public Health Service has emphasized the

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[2] Kusnetz, H. L. Radon daughters in mine atmospheres: A field method for determining concentrations. Amer. Ind. Hyg. Assoc. Quarterly, 17: 85, 1956.

[3] Tsivoglou, E. C., and H. E. Ayer. Emanation of radon in uranium mines and control by ventilation. Arch. Ind. Hyg., 8: 125, 1953.

[4] Idem. Ventilation of uranium mines. Arch. Ind. Hyg., 10: 363, 1954.

[5] Ayer, H. E. Control of Radon and its Daughters in Mines by Ventilation. United States Atomic Energy Commission AECU–2858. The Commission, Washington, D. C., 1954.

[6] Coleman, R. D., H. L. Kusnetz, P. F. Woolrich, and D. A. Holaday. Investigations on the supplemental control of radon and radon daughter hazards in mine atmospheres. Amer. Ind. Hyg. Assoc. Quarterly, 17: 405, 1956.

[7] Holaday, Duncan A. The Radon Problems in Deep-Level Mining. A. M. A. Archives of Ind. Health, 12: 163–166, 1955.

[8] Tsivoglou, E. C., H. E. Ayer, and D. A. Holaday. Occurrence of nonequilibrium atmospheric mixtures of radon and its daughters. Nucleonics, 11 (9): 40, 1953.

[9] Peller. Lung Cancer Among Mine Workers in Joachimsthal, Human Biol., 11: 130–143 (1939).

[10] Sikl, H. The Present Status of Knowledge About the Jachymov Disease (cancer of the lungs in the miners of the radium mines). Acta. Unio Intern. contra Cancrum, 6: 1366–1375 (1950).

[10a] Date and place of birth: August 9, 1904, Hollis, New York City. Education: Bachelor of arts, College of the Holy Cross, 1925; doctor of medicine, New York University, 1929; doctor of philosophy, New York University, 1935. Work history: Instructor in physiology, College of Medicine, New York University, 1932–35; assistant professor of physiology, New York University College of Medicine, 1935–41; guest investigator, physiological laboratory, Cambridge University, England, 1936; assistant professor of medicine, New York University College of Medicine, 1941–42; associate professor of medicine, New York University College of Medicine, 1942–46; director, Squibb Institute for Medical Research, New Brunswick, N. J., 1946–49; special consultant to the Surgeon General, Public Health Service, 1946–49; associate director in charge of research, National Heart Institute, National Institutes of Health, 1942–52; associate director, National Institutes of Health, in charge of intramural affairs, 1952–55; director, National Institutes of Health, 1955–. (Submitted by Department of Health, Education and Welfare.)

need for further research on the problems of radiation. It is in this area, and in the area of scientific training, that the activities of the National Institutes of Health relate to radiation.

The National Institutes of Health, as you know, is a research and training organization within the Public Health Service. We support medical and related research in universities and medical schools through grants which currently total about $90 million per year. We administer a training program which helps support the training of about 5,000 scientific personnel annually. We also carry on basic and clinical research in our own laboratories.

Our contribution to the problems of radiation lies primarily in further exploration, in our own laboratories and through research grants, of the manner in which radiation affects human beings. As the Surgeon General has pointed out, a more thorough understanding of the manner in which radiation affects biological systems is necessary.

The basic research inquiry into the effects of radiation as it relates to health, I believe, can best be approached in the context of physical biology, broadly defined. Physical biology includes the study of the means by which all physical phenomena act within biological systems.

It includes the effect of heat, cold, pressure, light, sound, vibration, acceleration and deceleration, electrical properties of molecules, the movement of material across membranes and ionizing and nonionizing radiation. As a field of study it depends heavily upon modern mathematics and physics and scientific instrumentation.

We share the view of those who believe that physical biology as a discipline will, in all probability, exert a dominant force upon medical and biological research over the coming decades.

Research in physical biology has for some time been a part of the total program of the laboratories of the Public Health Service's National Institutes of Health. We have, in fact, a Laboratory of Physical Biology in which research in some of the areas noted above is now being carried on. This laboratory, for example, has been under contract by the Armed Forces special weapons project in studies dealing with post radiation infections. This collaborative project is still continuing.

Another illustration, in a different field, is found in the contribution of the late Dr. Egon Lorenz of the National Cancer Institute to which Dr. Burney has already referred. His work in the general physiology of radiation effects, including protection against radiation damage, stands as a significant contribution in this field.

The National Cancer Institute conducts a substantial research program in clinical radiology, together with a basic program relating to the biological effects of ionizing radiation.

At the National Institutes of Health, we hope to place the highly specialized research in radiobiology within the context of the broad area of physical biology. For example, we are establishing a panel on mathematics attached to the research operation in physical biology. This panel will work on the applications of advanced mathematical techniques to biological problems, including the use of a large electronic computer in the solution of these problems.

Organizationally, we hope to place research in radiobiology in a single organization—the existing Laboratory of Physical Biology.

We intend to expand this laboratory at a moderate rate, primarily because of problems relating to research facilities and the shortage of scientific manpower in this field.

Research scientists themselves initiate the studies which are supported by NIH grants. Hence there are limits in the degree to which it is possible or advisable to change emphasis in fields of study. Experience has shown, however, that when an area of study is promising, investigators can be persuaded to enter or intensify their efforts. This is done primarily through persuasion by colleagues and a change of research emphasis by free choice. About $4.5 million per year in research grants now supports work in physical biology; of this amount, $1.2 million is for work in radiation biology.

Scientific manpower is in extremely short supply in the entire field of physical biology, including radiobiology. Because this is a relatively new field, fully satisfying career opportunities are few in number. A sound program for strengthening the field of physical biology should therefore encompass the establishment of a number of academic posts with adequate status, stability and income.

Attraction of additional students is, of course, also important, as is the content and vigor of their graduate instruction. There are waves of advance in fields of science which attract people as new and exciting fields open up. Nuclear physics has been one such field. Its immense popularity is shown by the fact that in 1951, 36 percent of all physicists in the age group 25 to 29 were nuclear physicists. Among those in the 50- to 54-age group, only 12 percent were nuclear physicists.

While the distribution of advanced students by field is fixed to a substantial degree by the stage of evolution of a science, this distribution can be affected—without producing accompanying harmful effects—by a carefully considered program.

The Institutes are establishing such a program as part of a total effort to increase the pool of research manpower for medicine and related scientists. Last year, for example, a senior research fellowship program was established. The fundamental objective of this program is to provide stable support for the most promising young investigators in the 5 to 10 years after they complete work for the doctor of philosophy in sciences basic to medicine. This is the critical period when too many younger investigators drift out of research. This obviously results in a loss of scientific manpower. In addition, the medical schools find it difficult to train people of high caliber in the teaching posts available for the sciences basic to medicine.

In fiscal year 1937, about 1,000 postdoctoral and 1,000 predoctoral fellows were preparing for research careers with the aid of fellowship funds of the NIH. In addition, almost 3,000 individuals were in training with the aid of training grants.

Over the past few years, the Institutes have emphasized the significance of training to the future of research in medicine and the related sciences. We are referring here to training not at the highly specialized technical level, but at a high level of scientific competence.

The total training activities of NIH have expanded rapidly. In fiscal year 1956, the appropriation for training activities approximated $17 million, while $33 million was provided for fiscal year 1957.

With this increase, substantial funds will be available to support advanced students in physical biology. Gradually, emphasis will shift

to the problem of recruitment and the problem of the content of training. The problem of motivation of students to study science—a problem which is fundamental to our national scientific effort—is shared by many groups ,and principally the National Science Foundation.

On the other hand, we believe that NIH can help universities set up courses in physical biology which will take students to the frontiers of knowledge. We also believe that NIH can help outstanding men in the field to find outstanding students who have already acquired a sound university grounding in science. Drs. Schmitt and Bolt of the Massachusetts Institute of Technology are working, on this problem in biophysics with the aid of a Public Health Service grant.

In conclusion, I should like to stress our conviction that a soundly conceived and operated research training program is a prerequisite to the successful solution of applied problems of radiobiology—of which the problem of radiation is one. In the field of manpower training we believe that the Public Health Service can make a most significant contribution to resolution of the problems related to radiological health.

Senator ANDERSON. Thank you, Doctor. I am glad to hear of the development of this work. Are there any questions?

Representative COLE. Mr. Chairman, I want to comment just a little bit on the doctor's statement, comparing the number of nuclear physicists in 1951 of the 25- to 29-age group as against those at that time of the 50- to 54-age group. The younger group is 36 percent of the total, and the older-age group is 12 percent of the total.

Frankly, I am not so much impressed by the fact that 36 percent of the total in 1951 were nuclear physicists, although that is encouraging, but I am very much impressed by the 12 percent in the upper-age region. That would lead me to conclude that as long ago as 25 or 30 years ago scientists became interested in nuclear physics as a profession. Is that a fair conclusion?

Dr. SHANNON. No, sir. This field was really opened up fairly widely in the 1930's. I think of the 12 percent of the older physicists who are in nuclear physics now, a sizable proportion of them are in there as a result of what one might call a retread, with the acquisition of new skills. In other words, I do not believe, despite the fact of the upswing of nuclear physics in the 1930's, that a sizable proportion of them entered this field initially. A 50-year-old physicist would have completed his degree in the 1920's. This is far too high a proportion to believe that they originated as nuclear physicists.

Representative COLE. I suspected there might be some such explanation.

Dr. SHANNON. The reason for quoting the figures, sir, is to indicate how one can in a carefully planned course, providing it is an intelligently planned course and makes sense, divert scientific talent without at the same time interfering with individual decisions.

Senator ANDERSON. Are there any additional questions? If not, this concludes what we are doing, with four exceptions. We want to make sure that if there are any closing and final statements that anyone wants to give, that we will not miss them. I particularly want to call on four people.

Dr. Waterman.

Representative VAN ZANDT. Mr. Chairman, may I ask Dr. Burney a question before he leaves the stand?

Senator ANDERSON. Yes.

Representative VAN ZANDT. Dr. Burney, you conclude your statement by saying, "To do the job will require cooperation among all concerned at all levels of government." That poses this question: How active are the several States in setting up some kind of control of radiation and the use of radiation?

Dr. BURNEY. That has been spotty, Mr. Van Zandt. Let me say first that there are several States, New York, California—I don't want to leave out any States, but I am naming those as examples—who have developed some good personnel and have what we would term a good radiological health program. But within the last 12 months there are a large number of other States which have recognized the importance of this problem, which have asked us for help in assigning personnel, or have sent personnel to our courses for training in order that they can go back and set up a program of their own. I would say it has been spotty, but it is growing now in State and local health departments.

Representative VAN ZANDT. With the development of knowledge and its dissemination, are you satisfied with the progress that is being made by the several States in setting up the necessary controls?

Dr. BURNEY. No, sir, but I would like to say that I am not satisfied with what the Public Health Service has done, either. I would not be too critical.

Representative COLE. I have a question of Dr. Shannon. I had to hurry over your statement because I had to answer a quorum call. I happen to be a member of another subcommittee of this Joint Committee that has to do with the problem of shortage of physicists, and so forth. Testimony that we took last year led us to believe that there is a terrific shortage. I am just wondering how long is this shortage going to exist on the effort that is being made today?

Dr. SHANNON. I would say, to pick a figure in years, certainly it will be here with us for at least 5 years. That figure is taken from an appreciation that a graduate degree takes a minimum of 3 years, and even with very active prosecution of programs, a 5-year period is much too short a time to figure that the shortage will have been remedied.

One of the important things is that wholly apart from the importance of physicists to programs in radiobiology, physicists now have a tremendous amount to contribute to the total field of biology and medicine. They are just coming into their own. You will have competition for physicists, biophysicists, physical chemists, and the like, from wholly new areas that were not available to compete for them as short a time ago as 5 years.

It is really an appreciation of this acute shortage which we feel will grow that led us a year and a half ago to mke our first positive effort to provide training opportunities for these university people.

Representative VAN ZANDT. Dr. Shannon, with your knowledge of the overall field, is it proper to say that there is a tremendous amount of enthusiasm?

Dr. SHANNON. Yes, sir.

Representative VAN ZANDT. And the field is attracting a lot of young capable people?

Dr. SHANNON. I will take the "yes" back until I am sure I know precisely what you have reference to. There is a tremendous amount of enthusiasm for entering training as physicists. There is not a proportionate amount of enthusiasm for entering the more restricted field of radiobiologists. There is essentially little in the way of professional training programs. We in the Cancer Institute have been attempting to develop professional personnel along these lines in a very positive way for a 5-year period. We have been wholly unsuccessful. This has led us in the past year to redefine our interest as being much broader than radiobiology, to redefine it in terms of physical biology, where we feel that the career opportunities of sufficient breadth will open up to attract people to them. But at the present time there are not sufficient university posts in the field of physical biology to really attract the brilliant youngster. We feel that the field has to be defined broadly and the research opportunity given which will automatically attract people into the field.

Representative VAN ZANDT. Thank you.

Representative COLE. How long does it take to make a nuclear physicist after he has finished high school?

Dr. SHANNON. Roughly 7 or 8 years. This would be the conventional college course with a 3- or 4-year graduate course superimposed.

Representative COLE. That would lead to a doctorate?

Dr. SHANNON. Yes, sir.

Senator ANDERSON. Dr. Waterman of the National Science Foundation.

STATEMENT OF DR. ALAN T. WATERMAN, NATIONAL SCIENCE FOUNDATION [11]

Dr. WATERMAN. I have no prepared statement, Mr. Chairman. I can, however, make some general comments.

First of all, with regard to the interest of the National Science Foundation in this program, we are, as you know, concerned primarily with basic research and training in the sciences and in that program we do not ourselves conduct research; but we provide support for research by means of grants to institutions for the support of work by scientists on projects selected from these that come to us. In that program we do have in the biological sciences a number of grants that are concerned with research in genetics and related problems, which are basic to the general topic of this committee. When it comes to work which is more directly of the applied nature, then this is not a province of the Foundation. We would leave this to the National Institutes of Health, the Public Health Service, and other agencies.

[11] Date and place of birth: June 4, 1892, Cornwall-on-Hudson, N. Y. Education: Bachelor of arts, Princeton University, 1913; doctor of philosophy in physics, Princeton University, 1916. Work history: Instructor in physics, University of Cincinnati, 1916–18; 2 years military service with Science and Research Division of Army Signal Corps in World War I; faculty member of Yale University in department of physics until 1948, with leave of absence during 1927–28 on a national research fellowship to King's College, London; to Massachusetts Institute of Technology in 1937, and to Office of Scientific Research and Development from 1942 to 1946; appointed Director of the National Science Foundation April 6, 1951, and reappointed April 6, 1957; serves as a member of the Defense Science Board and the Advisory Panel on General Sciences of the Department of Defense, and of the Science Advisory Committee and the Committee on Specialized Personnel of the Office of Defense Mobilization. He is also a member of the President's Advisory Committee on Weather Control; board of directors of the Center for Advanced Study in the Behavioral Sciences; board of trustees of Atoms for Peace Awards and the board of directors of the American Association for the Advancement of Science. (Submitted by Witness.)

That is to say, we are not concerned with research which is aimed at the diagnosis, treatment, and cure of disease; but, rather, a better understanding of the fundamental processes which occur in the body or in nature which have to deal with these subjects.

We have a fellowship program which provides about 800 predoctoral fellowships in all the sciences each year. It is a national program. We also have about 100 postdoctoral fellowships, and we have certain other postdoctoral fellowships for really senior research investigators, and faculty science fellowships primarily for teachers.

In our assistance to training in science, we have 96 summer institutes who select the field of radiobiology, but they are not large in number.

In our assistance to training in science, we have summer institutes which we are financing this summer, whereby teachers of science can get together in their individual sciences for 6 or 8 weeks, for the study of teaching methods and study of research material for teaching use. Six of those summer institutes are conducted and sponsored jointly by ourselves and the Atomic Energy Commission in the field of radiobiology. It is largely to have the teachers informed with regard to this subject.

In addition, we have provided funds to the Atomic Energy Commission for a program at Oak Ridge which will train high-school teachers in instructional methods and send them around the country so that the schools of the country and educational institutions can learn directly about nuclear energy and radiobiology. Those are the activities in our program which bear directly on this program.

The research and training aspects in this whole field are regarded as very important indeed.

With respect to the general subject, I cannot qualify as a research expert in particular fields. However, I am a physicist by profession and for the past 15 years or so have been concerned with the broad features of various aspects of this whole subject, such things as nuclear weapons, nuclear power, fallout, radiobiology, and similar problems.

In the middle of so much discussion, it seems to me, it pays to keep attention on some of the main features and not lose sight of them. This problem that we are dealing with is puzzling for a very fundamental reason, because the two alternatives are not really comparable. On the one hand, one has the national issue of the danger of war and the need to have superiority in the latest weapons. If it were clear that anything we do could establish a clear superiority in a weapon of any kind, and by that act prevent war, I am sure the problem would be much clearer, and the country would do whatever is necessary. Unfortunately, the evidence in these matters is not easy to come by, and the conclusion is very puzzling to make. But this is a national question which involves primarily our own country and the countries of the free world, and is of the gravest importance.

On the other hand, we have the question of fallout and the dangers of radiation. This is much more a personal and individual matter. There are individual reactions to this. Also it applies to all mankind and not only our country.

Furthermore, it involves a type of consideration which in the history of mankind has always been a very serious thing to face: the danger of the unknown. When one realizes a danger he has no

control over, and knows nothing about, to him it is a big danger, much more of a danger than a car running down the street which he can see and thinks he can avoid.

The next thing is that the consequences pointed out by scientists are frankly ugly and repulsive to him, for example, such matters as the effect on future generations. So this becomes a problem which to the individual is much more serious in his mind than many of the ordinary, more dangerous problems we face. That has to be reckoned with.

It has been said before, but bears repeating, that what we are talking about in this fallout danger and other radiation danger is not a new danger. It is a danger that has already existed in the lifetime of mankind. The level of that is quite considerably greater than any new dangers we are talking about. So we are merely talking about adding to the present danger. This then becomes a matter of probabilities, and probabilities are not an easy thing to get clear.

For example, if it were established that carrying a watch with a radioactive dial ran 1 chance in 100 million of proving fatal to the carrier, I suspect that many people would continue carrying watches. There would certainly be a few who would stop at once. On the other hand, if the risk were 1 in 10, probably nobody would carry radioactive dial watches.

The matter of estimation of the probability in this case is a hard thing to do and it reacts differently on different people. To take an illustration which is closer to this problem, we know, for example, that cosmic rays come in to the earth in greater quantities at the Poles than they do at the Equator, due to the magnetic field of the earth. This means that the danger from radiation from cosmic rays is greater at the Poles than at the Equator. Because that is true, does that means that the population immediately should move to the Equator? I think that poses the kind of question we are up against. It is probably true they are safer there, but is it worth it? So one runs into that type of consideration in dealing with the whole question.

I have just one more comment to make and then I would be very glad to answer any questions. While the difficulties in this are great, the promise of future valuable results and data bearing on the problem is also very great. By the evidence you have heard today, there are a number of fields of research that are now being prosecuted vigorously and in capable hands. We can confidently expect that these will produce results. It will be everyone's hope that these results can be speeded up in every possible way. Probably as research goes on, more different methods will occur to us. The power of this kind of research is very great. From our limited point of view at the present time we can see certain areas that are bound to be promising. We know we are going to get more precise information on the basis of which to make decisions and in certain other directions we can find that certain constructive things can be done to forestall the dangers, such as some treatment by which the body can deal with these menaces more efficiently. However, at any given time there are things that are unforeseen that will be discovered that will be most important.

My personal view about this is that we should lose no opportunity to prosecute this research in competent hands, and also that we lose no opportunity to see to it that those who are attracted and have

ability along the lines of this type of research are persuaded to go into it. Research cannot be done without competent people, and we should therefore see to it that we train these competent people. Only by the systematic study in competent hands will we really reach a good conclusion. In the meantime it seems to me that today's hearings and the other hearings which have been held are pretty convincing testimony that by the interest and thoroughness with which this committee has approached these problems and by the competence and thoroughness with which the Atomic Energy Commission and the Institutes of Health and others have been working along these lines, we have every reason to believe that we are on the right track. If anything else is needed—and I think it is—we have the two committees that have been mentioned—the National Committee for Protection Against Radiation and the committee of the National Academy of Sciences that can be counted upon to monitor this from the standpoint of desirable standards of tolerance levels, and so on, and also from the standpoint of review of the research which is going on. If that is done, and continued actively, it seems to me that we have the situation as well in hand as we could possibly hope.

Senator ANDERSON. Thank you very much. Are there any questions?

Chairman DURHAM. I have one statement. I do not believe I ever heard a finer summary of the whole problem in so few words.

Dr. WATERMAN. Thank you.

Representative COLE. I was going to say exactly the same thing, except I may have used different words. I think it is most appropriate, Doctor, that you are the last witness. I am not sure but it presently appears you are the last witness to make this résumé, and a synopsis of the subject matter that is of concern to this committee and the cause of the hearing. Like Mr. Durham, I compliment you on your capacity to pierce through the maze of all this information that has been given and heard in the last 2 weeks and lay out in the ledger for us and the public to evaluate the different factors that are to be taken into consideration in striking a balance on this problem.

Dr. WATERMAN. Thank you, sir.

Senator ANDERSON. Thank you very, very much, Dr. Waterman.

Dr. Alexander, do you have any short comment you wish to make before we are thraugh with this hearing?

Dr. ALEXANDER. I might say a few words on the part of the Department of Agriculture.

Senator ANDERSON. We certainly do not want that Department left out, we assure you.

Dr. ALEXANDER. I thought you would not. I always feel that Senator Anderson has an interest in the Department of Agriculture from some years back.

The program of the Soil Conservation Service of the Department of Agriculture is entirely one of support to the fallout program that has been presented here. We collect samples, select areas of work and help in interpretation of results in relation to food supplies for man and animals.

The Agricultural Research Service carries out basic research that contributes to our understanding of the behavior of fallout in soils, plants, and animals. We in the Department of Agriculture are glad to be a part of this program.

I might say for Mr. Cole and Mr. Anderson that my grandchildren are drinking copious quantities of milk, and I have no fear whatever for their future from the standpoint of the hazard of strontium 90.

Senator ANDERSON. From the size of you, you have drunk some yourself.

May we find out if either of the representatives of the Department of Defense have a word as we close?

Dr. SHELTON. I have no formal statement for you, but I do want to say a few words in order that the committee be informed of the fallout research being conducted in the Department of Defense, and principally in the Armed Forces special weapons project, a name which has occurred repeatedly in testimony to you.

I would like to repeat very briefly the pertinent portions of some of that effort.

In regard to Dr. Dunham's statements to you concerning the AEC research program in fallout and effects on man, several of the programs mentioned are jointly funded by the AEC and DOD. Dr. Dunham's organization and the organization which I represent work rather closely together on these common problems.

Representative COLE. At that point, Dr. Dunham indicated the total as I recall of $30 million. Does that indicate that the AEC's portion is $30 million, or does that $30 million include defense appropriations?

Dr. SHELTON. When we jointly fund a program, I would imagine he would include his contribution to that program and probably not include our contribution in the amount he has given.

Representative COLE. Are you going to tell us what your figure is?

Dr. SHELTON. I will indicate roughly those numbers; yes, sir. I wanted to indicate to you that the Department of Defense participation in documenting fallout and its effects on man are of a research nature, and we do have a broad interest. The AFSWP research directly related to fallout and its effects on man can be conveniently divided into three rather distinct areas. The first area in which the Department of Defense through the Armed Forces special weapons project has participated has been the documentation of fallout on the weapons test series. This activity has been principally concerned with the local fallout. Considerable effort has been expended and is now being expended in analyzing and reducing the data obtained to date. We anticipate our main effort in future will be along these lines.

It must be realized that millions of dollars and an extremely large effort has been expended in documenting fallout in order that from the standpoint of the Department of Defense the weapons can be properly employed, if necessary. We are not really complacent, however, regarding local fallout.

Senator ANDERSON. I saw you jump across a couple of pages.

(Discussion off the record.)

Dr. SHELTON. I do have a few more statements to make.

We are not complacent in the Department of Defense regarding local fallout and hence we are not complacent about that portion which is left over, which will manifest itself in the latitudinal and the worldwide fallout. As new weapons and brust conditions arise, we will certainly document those, as well as appears feasible.

There is a second large area in which we are participating and that is because of the importance of the worldwide fallout. The Depart-

ment of Defense does have a stratospheric program. The Department of Defense program has been coordinated with a similar project in the AEC. The Department of Defense will continue research on worldwide sampling and we do utilize the private organizations for analysis of those samples.

There is a final and third large area of research in the Department of Defense, and that concerns the laboratory programs on the effects of ionizing radiation on man. Lieutenant Colonel Hartgering of Walter Reed gave you the results of the uptake of various isotopes in man during testing.

Chairman DURHAM. How much of your work is classified and how much is declassified?

Dr. SHELTON. That changes from time to time. During a test series the work under progress at that time may be classified. Indeed, the subject just mentioned as performed by Colonel Hartgering is an example; we did not know during the periods of taking those samples on the Nevada operation, and finally the Pacific operation, just what would turn up. We did not want to reveal the number of shots. So that was classified. It was classified at one time, and we reviewed that subject and we were happy to present those results to you and they were unclassified. What is classified today may not be classified tomorrow.

Chairman DURHAM. I am speaking primarily of radiation and fallout.

Dr. SHELTON. Details of the fallout from a specific shot of a specific yield, having a specific burst condition, has been treated in a classified manner. It reveals specific information on that particular event. The AEC and we have periodically released fallout patterns of a generalized nature to indicate the areas and hazards involved. So we do the best we can in getting out as much information in an unclassified manner. We use classified information to develop the unclassified.

Representative VAN ZANDT. Dr. Shelton, how long does it generally take you to complete your study and then release to the public nonclassified information?

Dr. SHELTON. If something is of a startling nature, such as the obvious very large fallout patterns being involved with thermonuclear weapons, such as the March 1954 shot; we released the fallout patterns from that within a period of 4 to 5 or 6 weeks following the event. It was put in the best way we knew how not to divulge specifically the weapon, but to provide the necessary information to the people, and that came out jointly by the AEC and Department of Defense.

Representative VAN ZANDT. In other words, it takes about 4 or 5 or 6 weeks for you to delete the classified material from the information which you receive after a test.

Dr. SHELTON. Typically for us to really understand what we have and to put it out out and not have to retract and change it. We have consolidated a position of accuracy of the data by then.

We are continuing the human measurement program at Walter Reed, and we are extending that to other lines of approach. Dr. Langham mentioned the LASL whole body counter. We anticipate using a whole body counter in the Department of Defense. We would have available to us a large group of people whom we know where they

have been stationed, and where they will be stationed in the future. We will be checking some personnel and we hope to make a contribution in that area.

Concerning the effects of strontium 90 on man, we are jointly funding with the AEC two sizeable projects, one being the strontium 90 programs on dogs, and we have another one with them at Oak Ridge. At the present time we are spending in these two projects and others something over a quarter of a million dollars annually on research directly related to the biological effects of fallout on man. None of our medical programs are classified. That is, the results come right out as they are produced at those individual places.

So I have covered, then, the three broad areas in which the Department of Defense continually and has in the past expended quite an effort. We are well aware of the problems and we are making our contribution to solving the problems.

Senator ANDERSON. Thank you very much. I am privileged to check some of the work in one of the areas where the special weapons project is interested. It is a very active operation. I know how much you have done, and I want to compliment you on it.

This brings to a close the first part of these hearings. I do hope as a result of these hearings the various agencies which have testified, the various groups that have been represented, the scientific fraternity generally, will examine these hearings and give the committee such guidance as they may be able to for any subsequent hearings we may have.

We are going to regard these as adjourned, merely until additional material may be obtained and the committee, I am sure—and I am speaking for the chairman of the committee as well as the chairman of the subcommittee—thank all the witnesses very sincerely for their fine contributions.

Chairman DURHAM. I want to thank the scientific profession for the excellent hearings we have had. It is something that will be valuable for the future and to the country at large.

Senator ANDERSON. Thank you all.

(Thereupon at 4 p. m., Friday, June 7, 1957, the hearings were adjourned.)

APPENDIXES

APPENDIX 1

PRINCIPAL TECHNICAL SPEECHES AND PAPERS ON FALLOUT BY COMMISSIONER W. F. LIBBY

[Reprinted from Science, v. 122, July 8, 1955: 57–58]

DOSAGES FROM NATURAL RADIOACTIVITY AND COSMIC RAYS

W. F. Libby [1]

The radiation dosages that people receive from the natural radioactivities and cosmic rays have been calculated and are given in Tables 1, 2, 3, and 4. Some direct observations are given in Table 5.

Table 1 gives the dosages in milliroentgens per year for exposures at various altitudes directly over ordinary granite, typical sedimentary rock, and open oceans. Surface dosages decrease with height above the ground because of air absorption; 50-percent reduction occurs for every 370 ft.[2] For comparison purposes, it is interesting to note that in the United States, the average exposure *rate* from total fallout from atomic tests on 1 Jan. 1955, was about 1 mr/yr.[3] The total dose during 1954 probably averaged about 15 mr,[3] principally because of the Pacific tests in the spring.

The values listed in Table 1 were calculated on the following basis. The roentgen was taken to be 100 ergs of energy per gram of water. (Actually this definition is that of the rad, the internationally recognized unit of radiation dosage. For gamma rays it is nearly equal to the roentgen, which is 93 ergs/g.) The absorption coefficients of all radiations in tissue were taken as being equivalent to those of water. The dosages from the natural radioactivities in the earth were calculated on the approximation that the energy absorbed per gram by the human body on the surface of the earth is, to a sufficient approximation, equivalent to that absorbed per gram by the top layers of the rock of the earth's surface itself from the gamma radiation emitted by the rock.[4] In other words, the total gamma-ray energy produced in a gram of granite from the thorium, uranium, and potassium contained was taken to be equal to the energy absorbed per gram of human tissue in the human body on the surface, except that a factor of 2 was used to correct for the geometric loss. It was interesting to observe that this simple method of calculation gave results in good agreement with those based both on separate consideration of each of the complicated radiations emitted by thorium and uranium in the rocks and on the use of the individual absorption coefficients for these radiations in tissue, together with correction for the "buildup" factors as the radiation is scattered and diffuses out of the rock.[5]

The abundance of uranium, thorium, and potassium in granite were taken as 4×10^{-6} g/g (*1*), 13×10^{-6} g/g (*1*), and 0.03 g/g, respectively (*1*). In selecting these numbers, it was realized that these were only averages and that fluctuations around these values do occur, that uranium contents as high as 200 ppm have been found in granite, and that thorium has been found as abundant as 500 ppm in some granites.

[1] The author is a member of the U. S. Atomic Energy Commission.
[2] H. Faul, Nuclear Geology (Wiley, New York, 1954).
[3] M. Eisenbud and J. H. Harley, Science 121, 677 (1955).
[4] This calculation was kindly suggested by L. D. Marinelli of Argonne National Laboratory.
[5] U. Fano, Nucleonics 11, No. 8, 8 (1953); 11, No. 9, 55 (1953).

For sedimentary rocks, the general average figure of one-fourth of the values quoted for granites has been used. It is realized, however, that this is very approximate, because the amounts of the various radioactive minerals in the sedimentaries fluctuate widely. The abundances of uranium and potassium in sea water were taken, respectively, as 1.3×10^{-9}g/g[1] and 3.5×10^{-4}g/g[1]. The abundance of potassium in the human body was taken as 2×10^{-3}g/g,[6] and the abundance of carbon in the human body was taken as 18 percent. For the calculation of the dosage from radium assimilated in drinking waters throughout the normal lifetime, the bone weight was taken as 10 percent for the adult man. But for relatively brief periods of assimilation when the radium would be expected to be concentrated in the small volumes of the bone most metabolically active, the figure of 1 percent was used. All these numbers of the human body were taken as being equivalent to those of the "standard man".[7]

The dosages resulting from cosmic radiation were calculated from the ionization chamber data of Millikan et al.[8] From these data the dosages were calculated at altitudes up to 20,000 ft. and at the latitude of 55° N (geomagnetic) as well as at the geomagnetic equator. The results are given in Table 2. It should be mentioned that the biological effects per unit energy may be larger for cosmic radiation, because it consists of high-energy particles rather than gamma radiation.

The natural radioactivity in the human body contributes the dosages given in Table 3. Of the 19-mr/yr dosage from potassium, 17 mr/yr is from the beta rays of the potassium itself. These were taken to be of a mean energy 40 percent of the maximum energy of 1.36 Mev. The specific activity of natural potassium was taken as 1800 beta rays per gram, per minute and 180 gamma rays of 1.45-Mev energy per gram, per minute.[9] The gamma rays that contribute the remaining two units of the dosage of potassium were calculated on the basis of the assumption that only half of the gamma-ray energy is actually absorbed in the body. This leads to the result that in a packed crowd the radioactivity from the potassium in one's neighbors' bodies contributes an additional dosage of 2 mr/yr.

TABLE 1.—*Total radiation dosages from normal background radiation (mr/yr)*

Altitude of ground surface (feet)	Ordinary granite		Typical sedimentary rock		Open ocean	
	Equator	55° N	Equator	55° N	Equator	55° N
Sea level	143	147	76	80	53	57
5,000	150	170	83	103	----------	----------
10,000	190	230	123	163	----------	----------
15,000	270	350	203	283	----------	----------
20,000	414	560	347	493	----------	----------

The dosage from carbon was calculated on the basis of the assumptions that the body is 18-percent carbon; the specific radioactivity of carbon is 15 disintegrations/g, per minute;[10] and that the mean energy of the beta radiation is 40 percent of the maximum energy of 167 kev.[9]

In Table 4, various ordinary but somewhat unusual circumstances are used to illustrate the types of exposure that can occur in normal living. A wristwatch worn 24 hr/day that has a luminous dial assumed to have 1 μc of radium per watch—a figure perhaps slightly larger than the average—would give the central body, including the sex organs, a dosage of about 40 mr/yr. An airplane pilot flying 24 hr/day with an instrument panel consisting of 100 dials with 3 μc of radium each would receive, at an average distance of 1 yd, a dosage of 1300 mr/yer.

In order to check whether the dosages calculated here and given in Tables 1 to 4 are essentially correct, some direct measurements reported by various

[6] L. D. Marinelli, private communication.
[7] "Recommendations of the International Commission on Radiological Protection," NBS Handbook No. 47 (1950), p. 16.
[8] R. A. Millikan and H. V. Neher, Phys. Rev. 50, 15 (1936); H. V. Neher and W. H. Pickering, ibid. 61, 407 (1942).
[9] "Nuclear Data," Natl. Bur. Standards U. S. Circ. 499 (1950), suppl. 1, 2, and 3.
[10] W. F. Libby, Radiocarbon Dating (Univ. of Chicago Press, Chicago, 1952).

observers are given in Table 5. They agree reasonably well with the external component of the total dosages given for sea level in Table I—the residues after subtracting 20.5 mr/yr, the dosage from body radioactivities given in Table 3.

It is interesting that the variations in natural dosage are large and under certain conditions the natural dosage may be nearly 100 times higher than the minimum—the dosage of seafarers. The fallout dosage rate in the United States on 1 Jan. 1955—1 mr/yr—was only 2 percent of this lowest natural dosage rate. Of course, during a test period when bombs are fired, the fallout dosage rates may approach, or somewhat exceed, the natural dosage rate for a few days before decay and weathering processes reduce them in a few weeks to rates that are small percentages of the natural background.

TABLE 2.—*Cosmic ray dosages*

Altitude (ft.):	*Dosages (mr/yr)*
Sea level	33 to 37
5,000	40 to 60
10,000	80 to 120
15,000	160 to 240
20,000	300 to 450

TABLE 3.—*Radiation dosages from the natural radioactivity of the human body*

Source of radioactivity:	*Dosage (mr/yr)*
Potassium	19
Carbon	1.5
Radium (bones only), uniform distribution	6.7
Radium (bones only), nonuniform distribution	[1] 67

[1] The radium content of the human body is based on data of A. F. Stehney of Argonne National Laboratory.

TABLE 4.—*Radiation dosages in various ordinary circumstances*

Radiation source	Location	Dosage (mr/yr)
Wrist watch (1 μc of Ra per watch)	Central body, including sex organs, at average distance of 1 ft.	40.
Luminous dials in airplane cabin (100 dials with 3 μc of Ra each).	Pilot is taken to be at an average distance of 1 yd from the dials.	1300.
X-rays [1]	Lumbar spine, anterior-posterior	1500 each.
	Lumbar spine, lateral	5700 each.
	Pregnancy, anterior-posterior	3600 each.
	Pregnancy, lateral	9000 each.
Uranium ore (0.1 percent—the minimum accepted by the AEC for purchase).	Flat surface ground	2800.
	Mine with all walls of ore	5600. (neglecting radon)
Phosphate rock (commercial fertilizer 0.01 to 0.025 percent U).	Flat surface ground	280–700.
People	Packed in crowd	2.

[1] A. H. Sturtevant, "Genetic effects of high-energy irradiation of human population," address given 11 Jan. 1955 at California Institute of Technology.

TABLE 5.—*Experimental data for hard background radiation (mr/yr)*[1]

Observer	Cosmic rays	Gamma rays: From air	Gamma rays: From ground	Cosmic and gamma rays (total)	Location
Sievert and Hultquist	44	--------	--------	121–150	Streets of Stockholm
				104–182	Over igneous rocks, Sweden
				94	Clay soil [2]
				104	Wood houses (average center of room)
				145	Brick and concrete houses (types 1, 2) [2]
				296 (max., 520)	Brick and concrete houses (type 3) [2]
Cowan	--------	--------	--------	98	Outdoors, Brookhaven, N. Y., measured [3]
Hess and Vancour	34	2	53	90	Outdoors, Fordham Univ. campus, N. Y., 1 m above ground [4]
Burch	31–34	--------	62	94–96	Leeds, England [5]

[1] Kindly collected by L. D. Marinelli of Argonne National Laboratory.
[2] R. M. Sievert and B. Hultquist, "Variation in natural gamma radiation in Sweden," Acta Radiol. 37, 388, 399 (1952).
[3] F. P. Cowan, "Everyday radiation," Phys. Today 5, No. 10, 10 (1952); also AECU-1138.
[4] V. F. Hess and R. P. Vancour, "Ionization balance of the atmosphere," J. Atm. and Terrest. Phys. 1, 13 (1950).
[5] P. R. J. Burch and F. W. Spiers, "Radioactivity of the human being," Science 120, 719 (1954); P. R. J. Burch, "Cosmic radiation," Proc. Phys. Soc. London A67, 421 (1954).

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RADIOACTIVE FALLOUT AND RADIOACTIVE STRONTIUM

W. F. Libby[1]

The radioactivity that falls out of the atmosphere after the explosion of a nuclear weapon is called the radioactive fallout. In the ordinary atomic bomb, for example, for each 20,000 tons of TNT equivalent of explosive energy, about 2 pounds of radioactive materials are produced. In these 2 pounds are some 90 different radioactive species varying in lifetime from a fraction of a second to many years. This mixture of radioactivity decreases in radioactivity in such a way that for every sevenfold increase in age, the total radioactivity is decreased tenfold. Thus the radioactivity by 7 hours after the explosion has decreased to one-tenth the radioactivity of 1 hour, and in 49 hours to 1/100, in 2 weeks to 1/1000, in 3 months to 1/10,000, and so forth.

The conditions of fallout are largely determined by the amount and type of material vaporized into the fireball of the bomb itself. A bomb fired in the air contributes such a relatively small amount of matter to the cloud that the particles formed after dissipation of the enormous energy released are of necessity very tiny and therefore very slow in settling. The result is that most of the radioactivities are expended in the air and the area over which the fallout occurs is rendered very large indeed, extending to the ends of the earth in minute although detectable amounts.

A bomb fired on the surface of the earth, however, may have an appreciable portion of its radioactivity reprecipitated within relatively short distances, while bombs fired beneath the surface of the earth may place essentially no fallout radioactivity in the atmosphere. Therefore, the question of the area of contamination to be expected from nuclear weapons cannot be answered categorically without specifying the degree of contact of the fireball with the surface of the earth and probably also specifying the characteristics of this surface. Obviously water would differ considerably from soil in its ability to precipitate radioactive fallout. The coral in the southern Pacific islands that are used for the larger United States weapons tests will under the great heat decompose to form calcium oxide, which will then rehydrate to form calcium hydroxide, which in turn will absorb carbon dioxide to form a crust of calcium carbonate. Obviously such a complicated series of chemical reactions will make the fallout particles from the great tests at Eniwetok differ from what would be observed if

[1] The author is a member of the U. S. Atomic Energy Commission.
[1] The author is a member of the U. S. Atomic Energy Commission. This article is based on a speech given at Northwestern University, Evanston, Ill., Jan. 19, 1956.

the same weapons had been fired over ordinary sand or granite. We cannot imagine all of the details in which the nature of the soil will affect the local fallout, but it is clear that the effects will be substantial.

In the weapon test operations, great care is taken to insure that no danger results from fallout. Criteria are used that are meant to insure that this is so. However, it is well to note that it is from the test operations that we have learned what we do know about the problem of civilian defense against fallout. We must speak of test experience, for it is the only source of experimental information about the phenomena of radioactive fallout.

The radioactivities resulting from the burst of a nuclear weapon can be classified as follows: (i) radioactivities induced in the environment and (ii) products dependent directly on the nature of the weapon. The environment can be made radioactive only by neutrons, but all nuclear weapons involve large numbers of neutrons, some of which are certain to escape into the surroundings.

RADIOACTIVITIES INDUCED IN THE ENVIRONMENT

Taking air bursts first, our problem is: What do neutrons do to air? The answer is simple. They make radioactive carbon, C^{14} which has a half-life of 5600 years. Fortunately, this radioactivity is essentially safe because of its long lifetime and the enormous amount of diluting carbon dioxide in the atmosphere. The cosmic rays themselves make neutrons, which, of course, make radiocarbon. In fact, the earth has on its surface a total of 80 tons of radiocarbon from the cosmic radiation. Now, since each neutron forms one C^{14} atom of mass 14 times the neutron's mass, this corresponds to 5.2 tons of neutrons, and we see that this enormous number of neutrons would have to be produced and escape in order that nuclear weapons would just double the feeble natural radioactivity of living matter due to radiocarbon. Such an increase would have no significance from the standpoint of health. The atmosphere itself contains only 1.5 percent of the total carbon with which the cosmic-ray-produced radiocarbon is mixed, the main part being dissolved in the sea, so we expect that nuclear weapons could produce a short-range rise in the radiocarbon content of the carbon dioxide in the atmosphere, which later would decrease as the atmospheric carbon dioxide mixed with the sea. Therefore, only 1.5 percent as many neutrons would be required to double the natural radiocarbon content of atmospheric carbon dioxide for this time before mixing with the sea could occur, or about 78 kilograms or 170 pounds of neutrons. To orient ourselves, the 20,000-tons-of-TNT-equivalent atomic weapon involves the fission of 1 kilogram of uranium or plutonium and the liberation of about 10 grams of neutrons. If all these neutrons escaped into the atmosphere, it would obviously require 7800 such weapons to double the radiocarbon content of the atmospheric carbon dioxide even with no mixing with the sea, and about 520,000 with complete mixing. These correspond to explosive energies of 156 and 10,400 megatons of TNT, respectively, if all neutrons formed escaped. A reasonable escape figure might be 15 percent, so we can expect that nearly 1000 megatons of fission would be necessary just to double the atmospheric radiocarbon content, and that about 66,000 megatons would be necessary for the same effect on a long-term basis.

The interchange between the atmosphere and the sea water, which is constantly taking place, would deplete and remove the excess radioactive carbon dioxide. Now it is known from measurements of the radioactive hydrogen, tritium—which is also made in the atmosphere by the cosmic rays—that this interchange is slow. In fact, we learn that the radioactive water is formed by the burning of the tritium made by the cosmic rays is not diluted by more than the top 100 meters or so of sea water in its lifetime of about 18 years. The carbon dioxide dissolved in the water is about equal to the total in the air. In other words, a dilution by more than the twofold that corresponds to the dissolved carbon dioxide in the top 100 meters of ocean water would take longer than 18 years. However, the dilution by this factor of 2 would occur essentially immediately within a matter of weeks or months. Therefore, we would have to double our estimates for even the short time scale activation of the atmosphere to reach the enormous figure of 2000 megatons of fission required. Thermonuclear weapons, of course, also involve neutrons. For a given energy release, they produce somewhat more neutrons than fission weapons; however, the order of magnitude of atmospheric activation would not be greatly different. So our estimates apply to all nuclear weapons. The essential point is that the atmosphere is difficult to activate and the activities produced are safe. In addition to

carbon-14, there are a few others produced in low yield; they include tritium and very short-lived products, but none is produced in sufficient amounts to be hazardous.

For weapons fired on the surface, the activation of the surface materials is a possibility, but in general it appears that most of the neutrons form stable isotopes and that the amount of radioactivity produced, at least with ordinary surface materials, is relatively small. The principal radioactivities produced by nuclear weapons are produced in the weapons themselves, and not in the environment.

RADIOACTIVITIES PRODUCED IN WEAPONS

Turning now to the radioactivities naturally produced in nuclear weapons themselves, probably the most important is radioactive strontium, which has a half-life of 28 years. The first reason this is so important is that strontium is chemically similar to calcium, which is one of the main mineral constituents of the body. Bone consists principally of calcium phosphate, and for this reason radiostrontium, like calcium, is deposited in the bone. The amounts of ordinary nonradioactive strontium naturally present are so small that the radioactive strontium will follow ordinary calcium into the body. The second reason that radioactive strontium, Sr^{90}, is an important fallout radioactivity is that it has a long but finite lifetime—28 years half-life, 40 years average life—and thus has a persistent effect. Third, because of its bone seeking property, it stays in the body a long time. Fourth, the probabilities of body ingestion can be high. Finally, the fifth reason for its importance is that strontium-90 is produced in high yield in the fission reaction—about 4 or 5 percent of all fissioning atoms yield this isotope.

In order to orient ourselves about this, let us consider the maximum permissible concentration recommended by the National Committee on Radiation Protection for AEC workers for radiostrontium—1 microcurie for the standard man, whose body is taken to contain 1000 grams of calcium in total. The maximum permissible concentration is of course well below any level at which one would expect any damaging effects to appear. On the basis of experiments with animals, statistically observable increases in the number of bone tumors *should not* be expected to appear at *less than* 10 times this level.. As we go above this figure, the chance for bone tumors occurring increases rapidly so that the likelihood of bone cancer with 30 to 40 times that figure is appreciable.

INTAKE OF STRONTIUM-90

Let us consider in some detail the mechanism by which this most important fallout radioactivity produced in nuclear weapons might be expected to enter the human body. The first point is that from the point of view of fallout there are essentially two classes of nuclear weapons—the high-yield megaton weapons and the lower-yield kiloton weapons. All nuclear weapons produce atomic clouds that rise to heights dependent on the energy released, and the clouds from the megaton class of weapons rise rapidly up through the tropopause and pass into the top layer of the atmosphere, which we know as the stratosphere. This part of the atmosphere is essentially isolated from the lower layer in which we live, the troposphere, and where all of our normal winds, storms, and so forth, occur. Therefore, radioactivity produced in megaton weapons is placed largely immediately in the stratosphere, while the smaller kiloton weapons produce clouds that in general do not reach into the stratosphere, but stop near the tropopause—the imaginary boundary between the stratosphere and troposphere—and have the bulk of their radioactivity left in the troposphere.

In the troposphere where rain occurs, any particulate matter will be washed down in a period of days or weeks. It is easy to show, for example, that 0.1 inch of ordinary rainfall will probably remove essentially completely all particulate matter except for that which is so small as to be almost of molecular dimensions. In other words, for 0.1 inch of rainfall one can be quite certain that the air between the layer in which the rain originates and the ground is washed clean of fallout activity, except for the minute fraction that may be so small that it moves with the air out of the way of the falling raindrops as they make their way toward the earth; and even this tiniest fallout material is likely to be precipitated also, for it will migrate rapidly by molecular-type motion and in this manner is likely to absorb itself on other particulate material and so be rained out. For these reasons, tropospheric radioactive fallout does not stay in the atmosphere for more

than a matter of weeks. It may make two or three trips around the earth in a given latitude before being entirely removed, but its lifetime in the atmosphere will be a matter of weeks.

This is in very sharp contrast to the material that is placed in the stratosphere by megaton weapons; this material appears to stay there for a matter of years. Perhaps 10 years is a good average, at least for the weapons fired to date. It is well to bear in mind that this conclusion may be dependent on the nature of the material carried up in the cloud, but our present experience indicates that the fallout from megaton weapons that does not occur essentially in the first few hours or days, and is therefore deposited mainly locally, is deposited only at a very slow rate corresponding to an average time in the stratosphere of about 10 years. As a result of this long residence time in the highest layers of the atmosphere, the winds mix and distribute the radioactive material broadly over the earth and one finds, when the fallout does finally find its way down into the troposphere where the rain and snow wash it out, that the rates of recipitation are relatively uniform over the entire earth's surface.

Returning now to radiostrontium—at the rate of 1 kilogram of fission for 20 kilotons of TNT equivalent, 2 megatons of fission energy would be equivalent to very nearly 1 millicurie of strontium-90 per square mile of the earth's surface, or about 79 disintegrations per minute, per square foot of the earth's surface. The average soil of the earth has about 20 grams of calcium that is in a form available for plant metabolism in the top 2.5 inches for each square foot of area. Now, recalling the maximum permissible concentration level of 1 microcurie per standard man and noting, as will be shown later, that in order that this concentration not be exceeded, the topsoil of the earth should not contain any more radiostrontium than would correspond to 10 times the concentration in the human body that is just permissible—that is, 1 microcurie per 1000 grams of calcium, or 2200 disintegrations per minute, per gram of calcium—we find that 11,000 megatons of fission energy would produce this average level of radioactivity. Actually, as I will indicate, there can be a concentration of strontium-90 in the soil about 10 times greater than the recommended maximum permissible concentration before one would expect a man living in such an environment to accumulate a maximum permission concentration. The afore-mentioned 11,000 megatons of fission energy would yield a strontium-90 content in human beings just equal to the maximum permissible concentration (MPC); at less than 10 times this value, or below 110,000 megatons energy equivalent, statistically observable incidence of bone tumor should not appear; but at 30 to 40 times the MPC, or 330,000 to 440,000 megatons, the likelihood of untoward effects would be appreciable. Even the lowest of these figures is very far in excess of the total energy released to date.

KINDS OF FALLOUT

High-yield weapons fired near the surface have a portion of their activity deposited in and on particles large enough to fall out in the first few hours or days. Thus we have three kinds of fallout from high-yield weapons.

1) The first, or local, is due mainly to large-sized particles. This may cover a considerable area depending mainly on winds. In the 15 February 1955 release of the Atomic Energy Commission that described the experience in the Marshall Islands in the Castle test series in the spring of 1954, some 7000 square miles were described as being contaminated by this type of fallout.

2) The second fallout from the high-yield weapons is that portion which resides on the small particles, but which never reaches the stratosphere and thus stays in the troposphere until it is carried down by rainfall or settles out. There is thus a band of fallout in the same general latitude as the test site; the material may circle the earth two or three times before it is precipitated, but it does fall out within the first few weeks.

3) However, a large part—half or more depending on firing conditions—of the radioactive yield from high-yield weapons resides in the third category, which is the fallout that occurs from the stratosphere itself. Of course, some of the large local fallout may form particles which were lifted into the stratosphere, but which were so large and so bulky that they fell out rapidly anyhow. The finely divided material that reaches the stratosphere apparently stays there for years in the main. A slow leakage through the tropopause into the troposphere occurs—apparently something like 10 percent per year descends. Measurements of the strontium-90 content of soils, rain and snow, and biological materials on a worldwide basis have all shown that strontium-90 fallout occurs all over the

world at rates that are not very dissimilar from one another, except that there is a tendency in the middle latitudes in which the tests are conducted for an extra fallout, presumably of the aforementioned tropospheric variety. Since the completion of the Castle series of tests two years ago, this worldwide rate of fallout has approximated 1.5 millicuries of radiostrontium per square mile, per year. We thus see that radioactive fallout from the stratosphere is a very slow process. This is very fortunate indeed, since the high-yield weapons thus have a major part of their radioactivity dissipated in the atmosphere in a harmless way if they are fired in the air or on the surface.

The fallout apparently occurs in the final step by a washing down of the tropospheric air by rain together with direct falling. The radiostrontium descends from the stratosphere into the troposphere by the processes of diffusion and falling, and is then caught up by the tropospheric weather and in a matter of a few days is deposited. Reasonable estimates for the middle latitudes indicate that the average life in the troposphere is about 1 week.

DEPOSITION OF STRONTIUM-90

The radiostrontium comes down mainly in raindrops although fine morning mists and fogs may be particularly effective in this regard also, as well as surface contact and direct falling. It descends on the foliage and on the soil. That fraction of it which falls on plant leaves has a good chance of being absorbed directly into the plant—much in the way that most modern leaf fertilizers operate. The Eniwetok tests were conducted on coral islands and as a result their fallout may be largely water-soluble. In any case, direct measurements of the radiostrontium content of alfalfa and other crops showed them to be appreciably higher in radioactivity than the soils on which they grew, strongly indicating that a leaf assimilation mechanism is important. The rain falls and carries radioactivity, but when it runs off to the rivers and the seas it is nearly pure because of the action of the soil in absorbing the fallout, so that rivers are essentially free from radiostrontium. Lakes and reservoirs have a content that corresponds approximately to their surface areas only. The radiostrontium is absorbed in the top 2 or 3 inches of soil and held there very tenaciously. Plowing, of course, buries it more deeply, but it appears that in unplowed soil the radiostrontium does not move in a matter of 2 or 3 years.

The researches on radiostrontium conducted by the Atomic Energy Commission have been extensive. The AEC has sampled soils on a world-wide basis and submitted the samples for analysis of radiostrontium content to the Health and Safety Laboratory of the New York Operations Office of the Atomic Energy Commission, the Lamont Geological Observatory of Columbia University, and the Enrico Fermi Institute for Nuclear Studies at the University of Chicago. Direct fallout collected on gummed papers, milk and cheese, alfalfa, animal meat and bone, and even human bodies has been extensively studied. On the basis of the information so obtained, it is possible to say unequivocally that nuclear weapons tests as carried out at the present time do not constitute a health hazard to the human population insofar as radiostrontium is concerned, and it is believed with good reason that radiostrontium is likely to be the most important of the radioactivities produced. It is well to note that since radiostrontium is assimilated in the bones it constitutes essentially no genetic hazard, for its radiations do not reach the reproductive organs.

The milk and cheese radiostrontium content is not as high, relative to that of the grass which the cows eat, as one might expect. There appears to be a discrimination against the fallout material such that the calcium in milk and cheese is roughly one-fifth to one-tenth as radioactive with radiostrontium as the grass that the animals eat. There are various possible physiological explanations of this, and the conclusion itself may not be completely certain, but the data available to date indicate this to be true. In addition, the plant uptake of radiostrontium from soil does discriminate somewhat against radiostrontium as compared with calcium. The calcium taken up from the soil into the plant has in general about one-half the radiostrontium content that the soil calcium has. These two results protect the human population against ingestion of radiostrontium, since milk and cheese are the principal sources of calcium in the human diet. We find, therefore, that the radiostrontium content of human bodies is the lowest of all animals measured and is lower than the average soil and the average foliage by tenfold. The Sr^{90}-to-calcium ratio in young people—

whose bones are still forming—corresponds to about 1/1000 of the maximum permissible concentration recommended for adults—1 microcurie per standard man containing 1000 grams of calcium. The average soil in the United States contains about 10 times more, whereas abroad the radiostrontium content in other areas of the world not subject to the local test fallout is about one-third of that for the United States.

The surface air itself contains radiostrontium due to the fallout from the stratosphere and corresponding to the average time between rainstorms in which it can collect. Filtration of air at sea level discloses radiostrontium on filters if the filters are fine enough, even in periods when bombs are not being tested; thus the only fallout is from the stratosphere reservoir from the high-yield weapons. Measurements in the antarctic on snow samples collected there show that the fallout rate in January and February 1955 was comparable with that observed in the middle latitudes.

CONCLUSION

Finally, although the main part of the radioactivity from high-yield weapons fortunately dissipates in the stratosphere, the small but very significant part that falls out within a few hundred miles of the site of the explosion for weapons fired on the surface constitutes a very real hazard and nothing I have said should be interpreted otherwise. The weapons tests are conducted with great attention to this and the other dangers and every effort made to protect against misadventure. What we have learned from the studies I have described—which by the way have been conducted under the name Project Sunshine—is that these local precautions should be entirely adequate and the worldwide health hazards from the present rate of testing are insignificant.

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RADIOACTIVE STRONTIUM FALLOUT

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COMMISSIONER, UNITED STATES ATOMIC ENERGY COMMISSION

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CONVERSION FACTORS

"Average soil"	= 20 gm Ca/ft^2 in top 2.5 inches
1 MPC unit in any medium	= 1 μc Sr^{90}/kg Ca
	= 2,200 dpm Sr^{90}/gm Ca
1 megaton fission distributed uniformly over entire earth	= 0.0009 MPC unit
	= 0.5 mc Sr^{90}/mi^2

I. EXPERIMENTAL MEASUREMENTS

Strontium 90 is of particular importance among the fission products because of chemical and physical characteristics which result in comparatively high retention in the skeleton. These are chemical similarity to Ca, an element essential to both plants and animals; an average life of about 40 years; and a low rate of elimination from the skeleton. On the basis of studies of the comparative effects of Sr^{90} and Ra^{226} in experimental animals and of the effects of Ra in humans, the generally accepted maximum permissible body burden (skeletal content) of Sr^{90} in adult humans is 1 microcurie. Since the body of the average adult contains about 1,000 gm. of Ca, this is equivalent to saying that the maximum permissible average concentration of Sr^{90} in the adult skeleton is 1 μc/1,000 gm. of Ca. For purposes

of this discussion, this ratio of Sr^{90} to Ca, in whatever medium it may occur, is designated an "MPC unit." One MPC unit of Sr^{90} in the human body is considered to be safe—a significant risk occurring only at much higher dosages.[1] The majority of analyses for Sr^{90} encountered in this work were of the order of a few thousandths of 1 MPC unit. For purposes of orientation, it is helpful to remember that 0.001 MPC unit corresponds to 1/1,000 μc of Sr^{90}/kg of Ca, or 2.2 dpm of Sr^{90}/gm of Ca. The small weights of Sr^{90} involved in both the radiostrontium and normal strontium being considered, and the similarities of the element to Ca, justify the assumption that its distribution in the body will follow that of Ca in a general way.

Two megatons of fission will produce 1 millicurie (mc.) of Sr^{90}/mi², if the fission products are uniformly distributed over the earth's surface. If this amount of radioactivity is mixed with the available Ca in the soil, an average of about 20 gm/ft² in the top 2.5 inches of soil, the specific radioactivity produced is 0.0018 MPC unit. It is observed that most of the Sr^{90} fallout is concentrated in the top 1 or 2 inches of soil. For example, in Tables 1 and 2, which show the Sr^{90} burden in the fall of 1953 in the soil of twelve farms in the Wisconsin-Illinois area as well as in the alfalfa and the milk of the cows fed thereon, we note that the top inch of soil contains about 56 per cent and the next 5 inches contain the remaining 44 per cent of the total Sr^{90}. Recently some evidence has been discovered that the radiostrontium finds its way to greater depths.[2] In Table 2 data are given for Iowa soil collected in 1937 which, as expected, shows no Sr^{90}. The average available Ca content of the domestic soils was 8 ± 1 gm Ca/ft²/in., the average fraction of total Ca exchangeable was 68 ± 3 per cent, and the average Sr^{90} content was 4.7 ± 0.4 mc/mi².

TABLE 1

BIOSPHERE Sr^{90} ASSAYS* (WISCONSIN MILKSHED—PRE-CASTLE, OCTOBER, 1953)
(Values Are Given in Terms of 0.001 MPC Units, Except where Noted)

Farm†	Soil 0"–1"	Soil 1"–6"	Total Sr^{90} (Mc/Mi²)	Alfalfa	Milk
Grabow, Wisconsin	26.2	6.7	4.5	12.8	1.7
Oliver Swain, Wisconsin	7.4	2.2	3.1	5.3	1.3
Swanson, Illinois	15.8	2.5	9.2	7.1	1.2
Holcomb, Wisconsin	8.7	1.8	5.1	8.3	1.6
Lewke, Wisconsin	10.2	2.9	3.5	20.9	2.3
Premo, Wisconsin	13.1	2.5	3.8	4.1	0.7
Kurpeski, Illinois	16.3	5.6	4.0	7.4	1.3
Austin, Illinois	22.4	4.7	4.7	5.0	1.8
McKee, Illinois	8.1	0.9	6.3	14.8	1.4
Blomberg, Illinois	1.7	<0.3	4	9.5	1.2
Van Winkle, Illinois	13.8	7.9	3.8	5.0	...
Carver, Illinois	42.1	5.6	3.3	2.3	...
Average	16.8	3.9	...	8.9	1.4
Average Sr^{90} (mc/mi²)	2.6	2.1	4.7 ± 0.4		
Total Sr^{90} (mc/mi²)		4.7 ± 0.4			

* E. A. Martell and W. F. Libby, Project Sunshine Bull. No. 10, January 10, 1955; E. A. Martell, *ibid.*, Suppl. 3, September 1, 1955.
† Samples collected by Dr. Lyle T. Alexander, Chief, Soil Survey Laboratory, Plant Industry Station, Beltsville, Maryland.

As might be expected because of the similarity of the Sr chemistry to that of Ca, milk and cheese show radiostrontium without exception. Figures 1*a*, 1*b*, 1*c*, and 1*d* show the data for both foreign and domestic samples.

TABLE 2*

1953 DOMESTIC PRE-CASTLE SOIL SAMPLES

Location	Lab. No.	Date Sample Taken	Area of Sample (Ft.²)	Depth of Sample (Inches)	Ca Extracted (1 N NH_4Ac Reflux) (Gm.)	Weight Sample Extracted (Pounds)	Exchangeable Ca, Analytical Method (Me/100 Gm)	Total Weight Sample Taken (Pounds)	Calc. Exch. Ca in Sample (Gm.)	Calc. Ca. (Gm/Ft²)	Per Cent Ca. Extracted (10th Col.)	Milli-MPC Units	Total Sr^{90} (Mc/Mi²)	
Rock Co., Wis., Site No. 1, Knox fine sandy loam, Grabow Farm	531665	9/28/53	1.5	0–1	3.2	10.0	4.8	11.0	4.8	3.2	72.7	26.2	2.3	
Rock Co. Wis. Site No. 1, Knox fine sandy loam	531665	9/28/53	1.5	0–1	1.0	10	4.8	11.0	4.8	3.2	(6 N HCl after NH_4Ac extract)	24.6	...	4.5
Rock Co., Wis. Site No. 1, Knox fine sandy loam	531666	9/28/53	0.3	1–6	3.0	10	3.5	11.0	3.5	11.7	93.8	6.7	2.2	
Rock Co., Wis., Swain Farm, Site No. 2, Knox fine sandy loam	531667	9/28/53	1.4	0–1	10.8	9.5	8.5	10.5	8.1	5.8	147	7.4	1.2	
Rock Co., Wis., Swain Farm, Site No. 2, Knox fine sandy loam	531668	9/29/53	0.3	1–6	7.4	9.5	9.8	10.5	9.3	31.0	88.1	2.2	1.9	3.1
Winnebago Co., Illinois, Swanson Farm, Site No. 3, Carrington lib. sil	531669	9/29/53	1.2	0–2	9.5	6.0	13.9	7.0	8.8	7.3	125	15.8	3.2	
Winnebago Co., Illinois, Swanson Farm, Site No. 3, Carrington lib. sil	531670	9/29/53	0.24	1–6	10.0	6.5	13.5	7.5	9.2	38.3	125	2.5	2.7	5.9
Rock Co., Wis., Holcomb Farm, Site No. 4, Carrington sil	531671	9/29/53	1.33	0–1	9.4	6.5	14.9	7.5	10.1	7.6	107	8.7	1.8	
Rock Co., Wis., Holcomb Farm, Site No. 4, Carrington sil	531672	9/29/53	0.36	1–6	11.6	8	14.3	9.0	11.7	32.5	111	1.8	1.6	3.4
Dave Co., Wis., Lewke Farm, Miami sil, Site No. 4	531673	9/30/53	1.2	0–1	5.7	7	7.7	8.0	5.6	4.7	116	10.2	1.3	
Dave Co., Wis., Lewke Farm, Miami sil, Site No. 4	531674	9/30/53	0.28	0–6	7.0	8.5	8.8	9.5	7.6	27.1	103	2.9	2.2	3.5
Columbia Co., Wis., Premo Farm, Miami sil, Site No. 6	531675	9/30/53	1.2	0–1	5.0	7.5	10.2	8.5	7.9	6.6	72.4	13.1	2.4	
Columbia Co., Wis., Premo Farm, Miami sil, Site No. 6	531676	9/30/53	1.2	0–1	2.0	7.5	10.2	8.5	7.9	6.6	...	12.5	...	3.8
Columbia Co., Wis., Premo Farm, Miami sil, Site No. 6	531676	9/30/53	0.30	1–6	8.2	9.0	6.7	10.0	6.1	20.3	149	2.5	1.4	
McHenry Co., Illinois, Miami sil, Kurpeski Farm, Site No. 7	531677	9/30/53	1.16	0–1	5.0	7.0	6.4	8.0	4.6	4.0	122	16.3	1.8	
McHenry Co., Illinois, Miami sil, Kurpeski Farm, Site No. 7	531678	9/30/53	0.30	1–6	5.0	9.0	4.7	10.0	4.3	14.3	132	5.6	2.2	4.0
McHenry Co., Illinois, Miami sil, Austin Farm, Site No. 8	531679	10/1/53	1.3	0–1	4.2	8.0	6.8	9.0	5.6	4.3	85.7	22.4	2.7	
McHenry Co., Illinois, Miami sil, Austin Farm, Site No. 8	531680	10/1/53	0.31	1–6	3.9	9.5	5.1	10.5	4.9	15.8	88.6	4.7	2.0	4.7

TABLE 2—*Continued*

Location	Lab. No.	Date Sample Taken	Area of Sample (Ft.²)	Depth of Sample (Inches)	Ca Extracted (1 N NH_4Ac Reflux) (Gm.)	Weight Sample Extracted (Pounds)	Exchangeable Ca, Analytical Method (Me/100 Gm)	Total Weight Sample Taken (Pounds)	Calc. Exch. Ca in Sample (Gm.)	Calc. Ca. (Gm/Ft²)	Per Cent Ca Extracted (10th Col.)	Milli-MPC Units	Tota Sr^{90} (Mc/Mi²)	
McHenry Co., Illinois, McKee Bros. Farm, Site No. 9, Drummer rich	531681	10/1/53	1.14	0–1	16.0	7.0	25.9	8.0	18.8	16.5	97.0	8.1	3.7	6.3
McHenry Co., Illinois, McKee Bros. Farm, Site No. 9, Drummer rich	531681	10/1/53	1.14	0–1	10.0	7.0	25.9	8.0	18.8	16.5	(6 N HCl after NH_4Ac extract)	4.0	...	
McHenry Co., Illinois, McKee Bros. Farm, Site No. 9, Drummer rich	531682	10/1/53	0.24	1–6	23.5	9.0	27.4	10.0	24.9	103.8	105	0.9	2.6	
McHenry Co., Illinois, Blomberg Farm, Site No. 10, Drummer rich	531683	10/1/53	0.85	0–1	8.0	5.0	25.6	6.0	13.9	16.4	69.0	1.65	(0.8)	
McHenry Co., Illinois, Blomberg Farm, Site No. 10, Drummer rich	531683	10/1/53	0.85	0–1	5.0	5.0	25.6	6.0	13.9	16.4	(6 N HCl extraction following NH_4Ac)	4.4	2.0	
McHenry Co., Illinois, Blomberg Farm, Site No. 10, Drummer rich	531684	10/1/53	0.22	1–6	17.5	8.0	25.4	9.0	20.8	94.5	95.1	0.3	...	
Will Co., Illinois, Van Winkle Farm, Plainfield sand, Site No. 11	531685	10/2/53	1.23	0–1	3.1	9.0	3.5	10.0	3.2	2.6	107	13.8	1.0	3.8
Will Co., Illinois, Van Winkle Farm, Plainfield sand, Site No. 11	531686	10/2/53	0.26	1–6	3.8	10.5	3.2	11.5	3.3	12.7	127	7.9	2.8	
Will Co., Illinois, Carver Farm, Site No. 12, Plainfield sand	531687	10/2/53	1.38	0–1	2.1	9.0	2.8	10	2.5	1.8	91.3	42.1	2.1	3.3
Will Co., Illinois, Carver Farm, Site No. 12, Plainfield sand	531688	10/2/53	0.35	1–6	2.2	11.0	2.4	12	2.6	7.5	91.7	5.6	1.2	
Utah, College Pasture	531689	10/53	1.45	0–1	11.2	7.5	28.0	7.5	19.1	13.2	58.6	1.38	0.51	0.98
Utah, College Pasture	531690	10/53	0.265	1–6	13.2	8.5	28.8	8.5	22.2	83.4	59.4	0.20	0.47	

Average Ca content (exchangeable): 8.0 ± 1 gm Ca/ft²/in = 8.6 ± 1 mg Ca/cm²/in.

Average fraction of total Ca exchangeable = 68 ± 3 per cent (assuming 6 N HCl to extract all Ca).

$$\frac{Sr^{90}\text{ concentration, 1–6 inches}}{Sr^{90}\text{ concentration, 0–1 inches}} = 0.23 \pm 0.027.$$

Effective depth at top 1 inch concentration = 2.15 inches = 19 mg Ca/cm² = 18 gm Ca/ft².

Pre-atomic samples:

Location	Lab. No.	Date Sample Taken	Depth of Sample (Inches)	
Iowa, Carrington loam	C-2916	1937	0–3	0 ± 0.00005 MPC units
Iowa, Carrington loam	C-2917	1937	3–13	0 ± 0.00005 MPC units

* All radiostrontium measurements made in Chicago Sunshine Laboratory (see E. A. Martell and W. F. Libby, Project Sunshine Bull. No. 10, January 10, 1955; E. A. Martell, *ibid* Suppl. 1, March 1, 1955; Suppl. 2, June 1, 1955; Suppl. 3, September 1, 1955; and E. A. Martell. Project Sunshine Bulletin 11, December 1, 1955).

The amount of radiostrontium found in humans is shown in Figures 2*a* and 2*b*. The data show that the present Sr^{90} content probably averages somewhat less than 0.001 MPC unit in young people. Apparently a number of barriers protect the human skeleton from this fallout radioactivity.

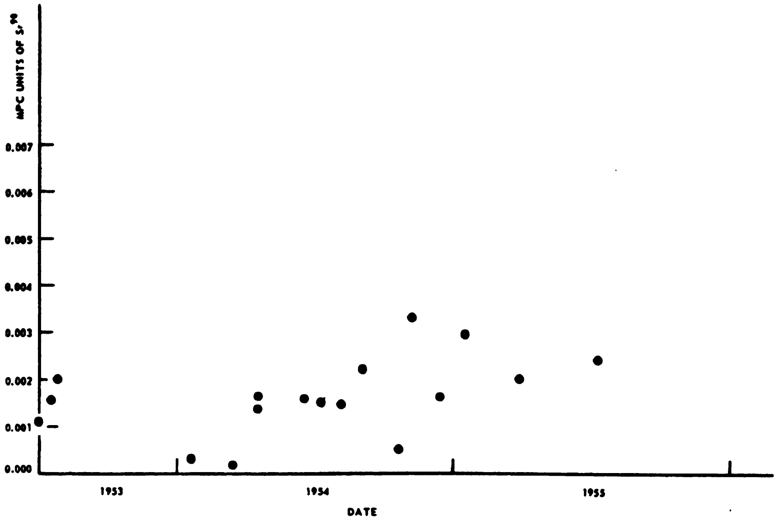

Fig. 1*a*.—Wisconsin cheese—Sr^{90} content

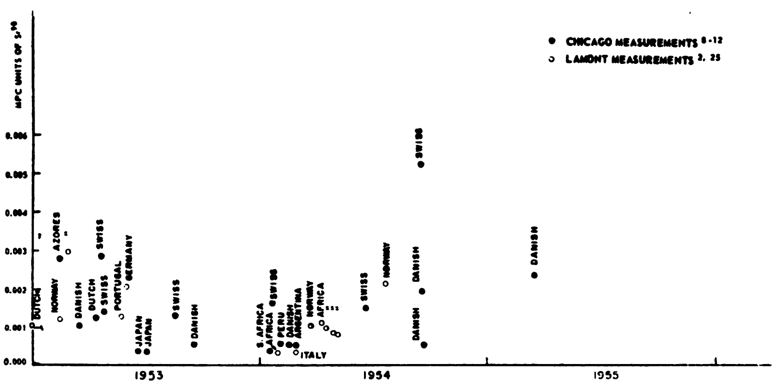

Fig. 1*b*.—Foreign cheese—Sr^{90} content

Measurements have been made on animals, principally cattle and sheep. These data are given in Figures 3*a* and 3*b*. We see here that the contents are much higher than those for milk and human samples, apparently due to selective deposition of Sr in the animal bones, which protects the milk and thus human bone.

Data for foreign soil samples collected just before the Castle test series are presented in Table 3 and Figures 4*a* and 4*b*. From these data we deduce that the band around the earth bounded by the latitudes 60° N. and 10° S. shows a deposi-

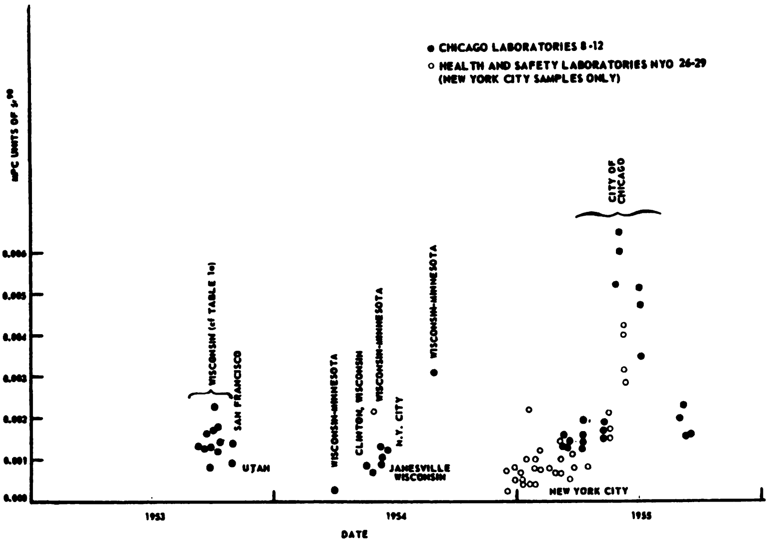

Fig. 1*c*.—U.S. milks—Sr^{90} content

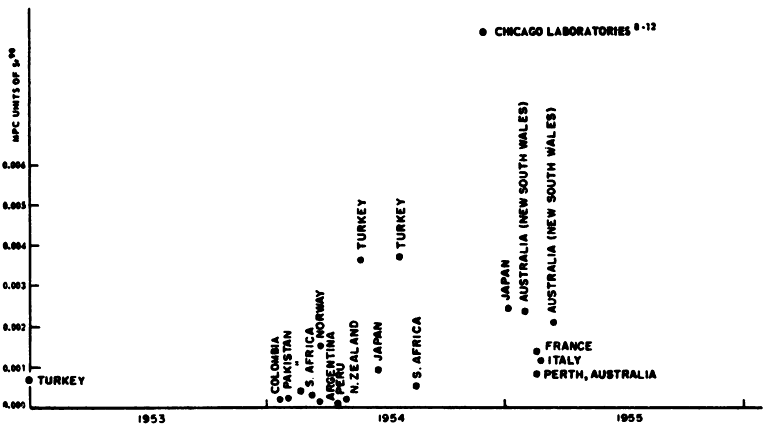

Fig. 1*d*.—Foreign milks—Sr^{90} content

tion of 0.8 megatons (MT) equivalent of Sr^{90}, in addition to some 0.4 MT which appears to be nearly uniformly deposited, as would have been expected from a slow deposition from a large stratospheric reservoir. No evidence for longitudinal variation is apparent in Figure 4*b*.

TABLE 3*

Sr^{90} CONTENT AND OTHER PERTINENT DATA ON FOREIGN SOIL SAMPLES COLLECTED BEFORE CASTLE

Lat./Long.	Location	Lab. No.	Date Sample Taken	Area of Sample (Ft^2)	Depth of Sample (Inches)	Ca Extracted (1 N NH_4Ac Extraction (Gm.)	Weight Sample Extracted (Pounds)	Exchangeable Ca, Analytical Method (Me/100 Gm.)	Total Weight Sample Taken (Pounds)	Calc. Exch. Ca in Sample (Gm.)	Calc. Exch. Ca (Gm/Ft^2)	(Gm/Ft^2/In.)	Per Cent Ca Extracted (7th Col.)	Sr^{90} Content (Milli-MPC Units)	Sr^{90} Fallout (Mc/Mi^2)
35° S./60° W.	Buenos Aires, Arg. (8 miles apart)	54569	3/15/54	2	0–4	8.7	8	15.0	45.7	62.2	31.1	7.8	80	0.45 ± 0.03	0.39
35° S./60° W.	Buenos Aires, Arg.	54570	3/15/54	2	0–4	6.6	8	10.2	45.7	42.3	21.2	5.3	89	0.44 ± 0.03	0.26
2° N./104° E.	Bin Tong Park, Sing.	54571	3/4/54	2	0–4	0.01	8	0.4	40.6	1.5	0.73	0.13	3	≤ 50	≤1.0
2° N./104° E.	Tengah Air Base, Sing.	54572	3/4/54	2	0–4	0.3	8	0.4	42.5	1.54	0.77	0.19	100	22 ± 2	0.5
30° N./75° E.	Molir Village, Pakistan	54671	2/25/54	2	0–4	4.3	4	18.4	60.5	101	50.5	12.6	64	0.27 ± 0.03	0.38
30° N./75° E.	Hub River, Pakistan	54672	2/25/54	2	0–4	4.0	4	16.6	65.3	98.4	49.2	12.3	66	0.35 ± 0.04	0.48
52° N./0°	Rothamstead, Eng.	54675	4/?/54	7 (est.)	0–3	4.5	4	13.5	95.8	117.4	16.8	5.6	92	1.31 ± 0.07	0.61
5° N./75° W.	Bogotá, Colombia (plowed)	54715	3/?/54	2	0–4	4.3	4	13.8	29.9	37.5	18.7	4.7	86	0.67 ± 0.04	0.35
5° N./75° W.	Bogotá, Colombia (grass)	54716	3/?/54	2	0–4	4.8	4	13.9	51.0	64.4	32.2	8.1	95	0.91 ± 0.08	0.82
5° S./30° E.	Leopoldville, Belgian Congo	54717	3/?/54	2	0–4	2.4	4	4.6	51.1	21.3	10.7	2.7	All	0.92 ± 0.08	0.27
5° S./30° E.	Leopoldville, Belgian Congo	54718	3/?/54	2	0–4	4.5	4	13.2	69.4	83.2	41.6	10.4	94	0.21 ± 0.04	0.24
12° N./45° E.	Aden Protectorate, SW Arabia	54421	3/?/54	2	0–4	9.6	8	23.7	32.0	68.9	34.4	8.6	56	0.51 ± 0.03	0.49
12° N./45° E.	Aden Protectorate, SW Arabia	54422	3/?/54	2	0–4	9.4	8	19.7	30.0	53.7	26.8	6.7	66	1.05 ± 0.10	0.78
60° N./10° E.	Oslo, Norway (Walsh)	54412	4/27/54	2.21	0–2	9.8	8	17.0	16.2	25.0	11.3	5.7	79	1.51 ± 0.05	0.48
60° N./10° E.	Oslo, Norway, No. 1	54410	4/27/54	2.21	0–2	11.0	8	18.0	16.0	26.2	11.9	5.9	84	1.44 ± 0.06	0.48
32° N./36° E.	Beka'a Valley, Lebanon	54293	2/25/54	1.5	0–3	20.2	7	41.5	17.0	64.1	42.7	14.2	77	0.86 ± 0.05	1.0
35° N./2° E.	Boghari, Algeria, No. 3	54359	2/22/54	2.21	0–2	13.2	8	29.8	19.0	51.4	23.3	11.6	61	3.4 ± 0.08	2.2
35° N./2° E.	Boghari, Algeria, No. 1	54360	2/22/54	2.76	0–2	13.7	8	34.3	19.4	60.4	21.9	10.9	55	3.8 ± 0.1	2.3
35° N./2° E.	Boghari, Algeria, No. 2	54361	2/22/54	2.21	0–2	10.0	8	25.8	18.3	42.5	19.4	9.7	53	3.46 ± 0.08	1.9
35° N./40° E.	Village 41 km. SE. of Damascus, Syria	54295	2/26/54	2.21	0–2	12.4	7	27.1	14.4	35.4	15.6	7.8	72	1.9 ± 0.08	0.82
35° N./40° E.	Tel Muskan, Syria	54296	2/26/54	2.21	0–2	18.6	7	29.6	20.0	53.8	24.3	12.1	99	1.1 ± 0.10	0.75
52° N./0°	Ffostill, Breton, Wales	54415	4/?/54	2.78	0–2	8.0	8	11.4	11.4	11.8	4.27	2.1	97	3.3 ± 0.12	0.39
52° N./0°	Montgomery, B.I., Wales	54416	4/?/54	2.78	0–2	9.6	8	11.6	8.3	8.7	3.1	1.6	All	...	...
52° N./5° W.	Cardigan, B.I., Wales	54417	4/?/54	2.78	0–2	0.3	5	1.3	10.2	1.2	0.4	0.2	50	97 ± 9.1	1.1
52° N./0°	Suffolk, Eng., No. 4	54418	4/?/54	1.74	0–2	15.2	7	37.2	16.6	56.1	32.2	16.1	64	1.37 ± 0.06	1.2
52° N./0°	Suffolk, Eng., Pudding Crnr.	54419	4/?/54	1.74	0–2	18.1	8	38.0	16.8	58.0	33.3	16.7	66	0.89 ± 0.05	0.83
52° N./0°	Suffolk, Eng., Old Orchard	54420	4/?/54	1.74	0–2	16.6	8	36.2	17.9	58.8	33.8	16.9	63	0.94 ± 0.09	0.89
45° S./170° E.	New Zealand, Hutt Co.	531804	11/?/53	5 (est.)	0–3	7.9	8.5	13.0	44.0	51.9	10.4	3.5	79	≤ 0.21	≤0.06
45° S./170° E.	New Zealand, S. Canterbury	5485	1/18/54	5 (est.)	0–3	6.5	9	10.8	47.2	46.3	9.3	3.1	74	≤ 0.18	≤0.05
45° S./170° E.	New Zealand, Whangarei Co.	5471	1/18/54	5 (est.)	0–3	0.9	8	1.7	37.7	5.8	1.20	0.4	75	2.51 ± 0.09	0.084
20° S./45° W.	Belo Horizonte, Brazil (3 km. apart)	54288	3/?/54	2	0–4	0.2	10	0.4	42.0	1.5	0.8	0.2	50	13.5 ± 1.25	0.30
20° S./45° W.	Belo Horizonte, Brazil	54289	3/?/54	2	0–4	0.6	8	0.7	26.5	1.7	0.9	0.2	All	4.17 ± 0.3	0.10
35° S./75° W.	Santiago, Chile	5472	12/?/53	30	0–1	10.2	8	27.2	50.4	124.5	4.15	4.1	51.5	≤ 0.33	≤0.04
30° S./30° E.	Natal, S. Africa	54399	2/?/54	2	0–4	2.6	8	4.2	29.5	11.2	5.6	1.4	86.7	0.49 ± 0.08	0.076
30° S./30° E.	Natal, S. Africa, 3 mi. SE.	54400	2/?/54	2	0–4	1.7	8	1.2	47.0	5.1	2.55	0.6	...	9.80 ± 0.71	0.70
15° N./120° E.	Philippine Islands	54401	2/26/54	2	0–4	14.8	8	18.6	48.4	81.7	40.8	10.2	All	1.47 ± 0.21	1.67
15° N./120° E.	Philippine Islands	54402	2/26/54	2	0–4	3.5	8	5.6	51.7	26.3	13.1	3.3	85.4	20.1 ± 2.3	7.3

Average: 8.1 ± 0.8 gm Ca/ft^2/in = 8.8 ± 0.9 mg Ca/cm^2/in.

* See note to Table 2.

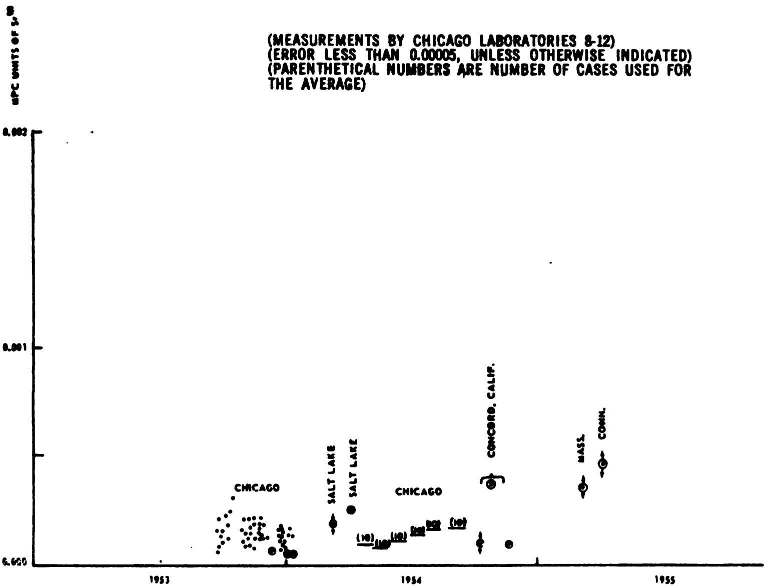

FIG. 2a.—Human stillborns

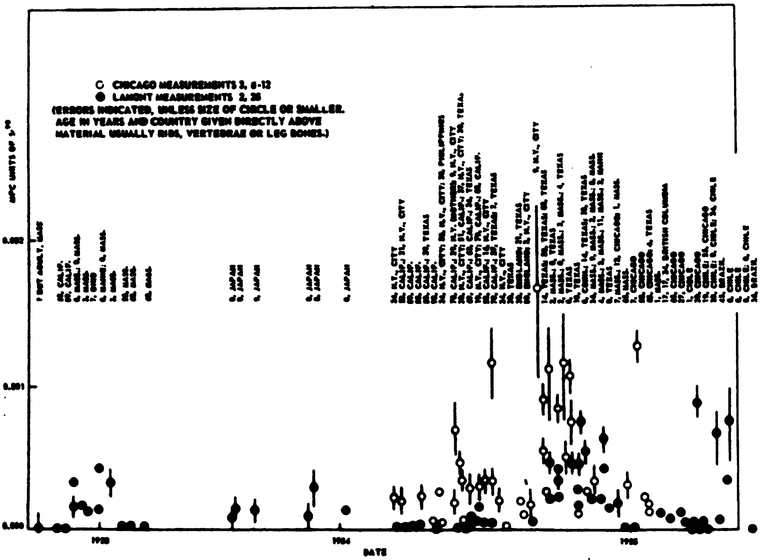

FIG. 2b.—Human bone—Sr^{90} content

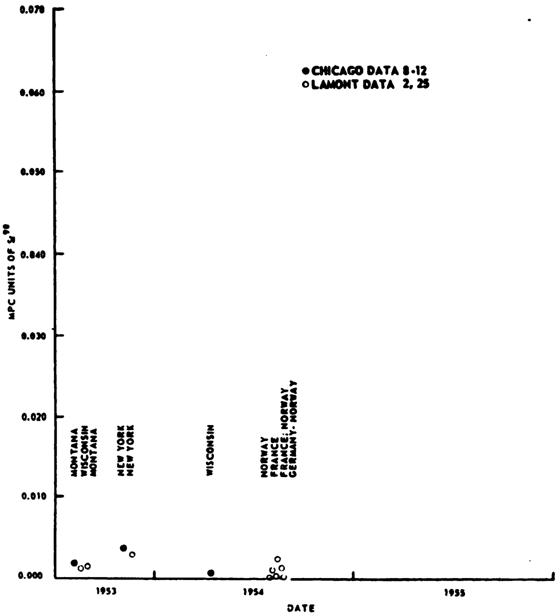

FIG. 3a.—Calf bones

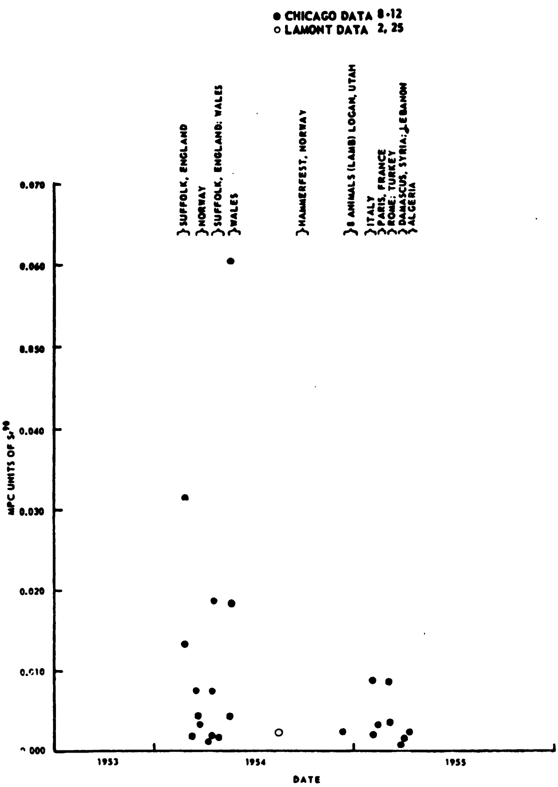

FIG. 3b.—Sheep bones

The actual heights of rise of the bomb clouds are the basis for the assumption that all distant fallout from megaton weapons occurs from a stratospheric reservoir, while that from those of lower yield occurs from the troposphere. Actually, the height of the tropopause varies with the season, so that the season must be con-

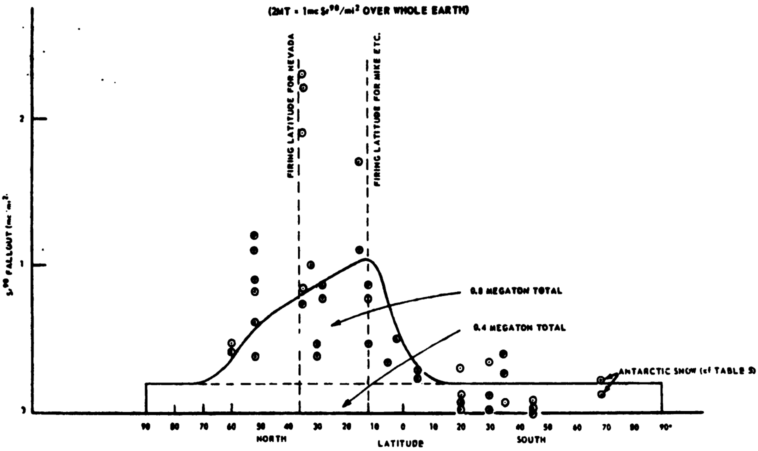

Fig. 4a.—Latitudinal distribution of foreign pre-Castle Sr^{90} fallout (soil assays)

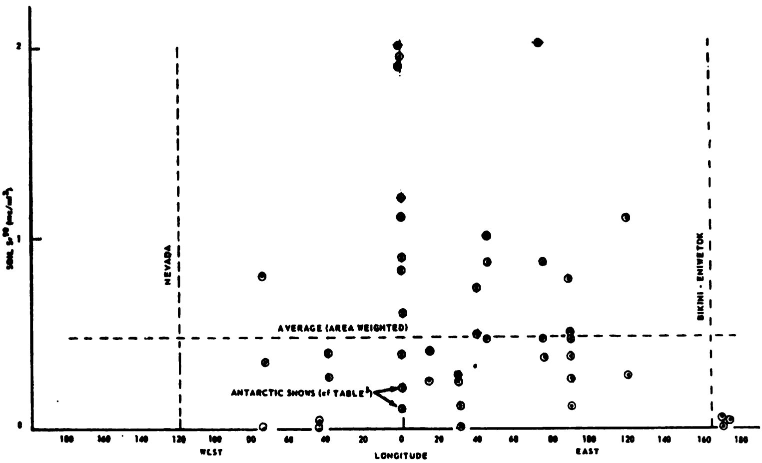

Fig. 4b.—Longitudinal distribution of pre-Castle Sr^{90} soil data from foreign samples.

sidered in the assignment. During the Pacific tests it has been near 55,000 ft.; hence our classification has validity in this respect.

Two of the most important data in Figures 4a and 4b are those from the antarctic series. The samples were snow cores, collected for the Chicago Sunshine

Laboratory and for cosmic-ray tritium analysis,[3–5] by Mr. Paul Humphrey of the United States Weather Bureau in January and February, 1955, at Admiral Byrd Bay (69°34′ S.; 00°41′ W.), at Atka Bay (70°35′ S.; 08°06′ W.), and at Little America III. The data are given in Tables 4 and 5. Table 4, Part A, gives the Sr^{90} and T contents of surface snow at four locations. From the T concentrations and the expected T production rate in this region[3–5] (T produced in the Castle test itself was precipitated out in a few weeks and never entered the Southern Hemisphere appreciably, because of the large amount of water taken into the cloud with which it became mixed[6]), we determined the annual precipitation to be 7.8 ± 2 inches of water. This calculation, together with that for the January–February,

TABLE 4

POST-CASTLE Sr^{90} FALLOUT IN ANTARCTICA

A. SURFACE SNOW

Date	Location	Sr^{90} (dpm/Liter)	T (Atoms/10^{18} H's)
January 15, 1955	Near Quonset, Little America III	3.2 ± 0.3	14.1 ± 0.6
January 17, 1955	One-half mile east, Little America III	3.1 ± 0.7	7.5 ± 0.6
February, 1955	Atka Bay, 6 miles inland on shelf (70°35′ S.; 08°06′ W.)	5.3 ± 0.5	19.2 ± 0.8
February 19, 1955	Admiral Byrd Bay (69°34′ S.; 00°41′ W.)	2.0 ± 0.2	24 ± 5
	Average	3.4 ± 0.5	14 ± 3

B. Sr^{90} PRECIPITATION RATE IN JANUARY AND FEBRUARY, 1955

Annual snow precipitation rate from T assay* and 0.59 T's/cm²/sec as the expected antarctic cosmic-ray T production.

$$\rho = \frac{4.7 \times 0.59}{14} \text{ meters/yr} = 7.8 \pm 2 \text{ inches of water.}$$

Sr^{90} rate of precipitation:

3.4 ± 0.5 dpm/liter = 62 dpm/ft²/yr for 7.8 inches of water/yr = 0.8 ± 0.2 mc/mi²/yr.

* H. von Buttlar and W. F. Libby, "Natural Distribution of Cosmic Ray Produced Tritium. II," *J. Inorg. and Nuclear Chem.*, 1, 75, 1955; L. A. Currie, W. F. Libby, and R. Wolfgang, *Phys. Rev.*, 101, 1557, 1956.

TABLE 5

PRE- AND POST-CASTLE Sr^{90} FALLOUT AT ADMIRAL BYRD BAY (69°34′ S.; 00°41′ W.)
(Collected 2/19/55)

SNOW CORE

Age (Years) (7.8 Inches Water/Yr)	Depth (Ft.)	Density	Sr^{90} (dpm/Liter)	T (Atoms/10^{18} H's)	Sr^{90} Rate* (Mc/Mi²/Yr)
0–0.54	0–1	0.35	1.95 ± 0.20	24 ± 5	0.46
0.54–1.04	1–2	0.32	1.7 ± 0.2	12.5 ± 0.8	0.40
1.04–1.52	2–3	0.30	0.48 ± 0.04	13.5 ± 0.7	0.11
1.52–2.16	3–4	0.41	0.90 ± 0.06	...	0.21

* Assumed annual precipitation, 7.8 inches water/yr, on the basis of T contents of surface waters (cf. Table 4).

1955, Sr^{90} precipitation rate of 0.8 mc/mi²/yr, are given in Part B of Table 4. The result for the annual precipitation agrees well with direct observation by the Atka Expedition and by Mr. Humphrey personally.[7] At Little America IV, direct observation showed that the floor of the tent projecting from the ice front was beneath only 7 or 8 feet of snow after roughly seven years. Since the density of the snow was about 0.35, this would correspond to about 5 inches of annual precipitation. Other observations[7] checked this general magnitude. In Table 5 a core taken at Admiral Byrd Bay was measured for both Sr^{90} and T. From these data we observe the pre-Castle fallout rates of 0.11 and 0.21 Sr^{90} mc/mi²/yr. The surface rate at this site is 0.43, which is less than the general average in the area for January and February, 1955, of 0.8 mc/mi²/yr—as shown in Table 4—hence it

may be that the pre-Castle values at this site are low also and should be increased by the ratio 0.8/0.43, or by 90 per cent, to 0.2 and 0.4, respectively.

The average Sr^{90} content of rain and snow in the Chicago area since the fall of 1952 was calculated by weighting each datum by the total rainfall observed in the particular storm. The data on the Sr^{90} content of Chicago rain are given in Figure 5.[8-12] It is clear that large fluctuations can occur in individual storms. However, these extremes were, in general, of low total rainfall; hence the effect on the average is small. It is interesting that the antarctic snows have about the same Sr^{90} content as the average Chicago rain of Figure 5. We recall that pre-

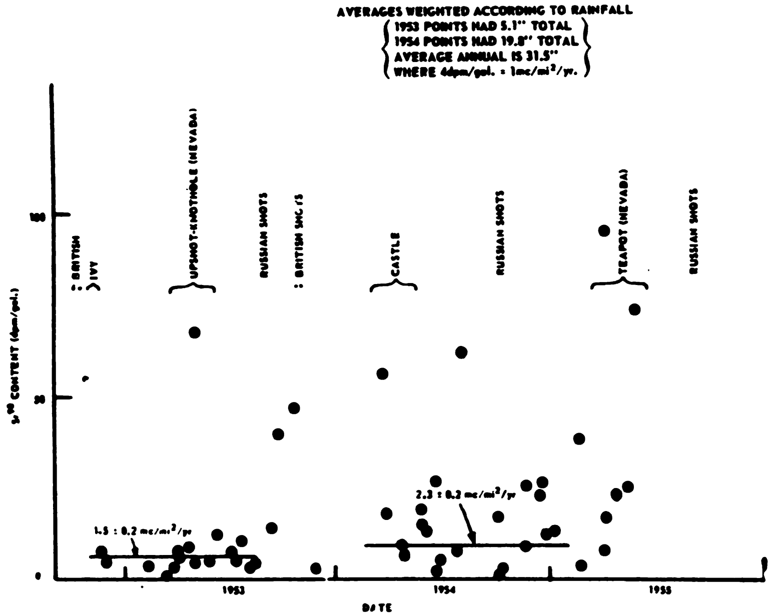

FIG. 5.—Sr^{90} content of Chicago rains

cipitation there is only one-fourth to one-fifth that in Chicago. According to the mechanism espoused in this paper, these fluctuations are to be expected, because the fallout from the stratosphere is thought to be steady and continuing, and the washing-out by rain is expected to carry down the fallout accumulated since the last precipitation from the particular air mass involved. Many of the samples given in Figure 5 and in Project Sunshine Bulletins Nos. 10 and 11[8-12] have been measured for T as well;[2-6] hence further correlations of the type described above for antarctic snow (Tables 4 and 5) can be made.

In Tables 1 and 2 the Sr^{90} contents of soils in the midwest region of the United States were shown to have an assay of 4.7 mc/mi² in October, 1953. The total from rains in the preceding year was only 1.5 mc/mi², according to Figure 5; hence we have to expect about 3 mc/mi² to have been deposited prior to Operation Ivy by direct fallout, most reasonably from tropospheric debris. The total fired in

Nevada prior to this time which would not have fallen out in the immediate vicinity of the test site was approximately 200 KT for the Operations Tumbler-Snapper and Buster-Jangle together. If this were all deposited in the United States, it would amount to about 7.0 mc/mi². The test series Upshot-Knothole, with approximately 220–230 KT of distant fallout, and Teapot, with approximately 160–180 KT, should have added correspondingly to the 4.7 mc/mi² in October, 1953, and to the 3.0 mc/mi² of stratospheric fallout for the intervening period, for a total of perhaps 12 mc/mi² expected in the spring of 1956 in the United States.

The efficiency with which rain removes fallout from the air through which it passes is probably high. One knows, on simple physical grounds, that as little as 0.1 inch of rain will traverse at least 90 per cent of the air volume lying below the layer in which the rain originates, so that 90 per cent of all particles which can be swept up by a falling raindrop will be carried down by such quantities of rain. On the point as to whether fallout is likely to be deposited by rain, we note that G. H. Wilkening[13] showed that the decay products of the radioactive gas Rn which in themselves are isotopes of nongaseous elements are found affixed to particles of diameters between 20 and 800 angstrom units (0.002–0.080 μ)—a submicroscopic range not at all unlikely for the radioactive fallout stored in the stratosphere.[1] The velocity of fall for such particles would be very small and in this respect quite compatible with the long stratospheric storage times indicated by the Project Sunshine data. Blifford, Lockhart, and Rosenstock[14] studied the concentration of the Rn decay products in rainfall in the Washington, D.C., area and concluded that rain was the mechanism by which the particles containing these products were precipitated and that the average time the decay products spent in the air before being precipitated was only 15 days, approximately. O. Haxel and G. Schumann,[15] found this time in Heidelberg, Germany, to be about 4 days, and Damon and Kuroda[16] concluded that Blifford *et al.*, were correct in attributing the precipitation of the aerosol carrying the Rn decay products to rain, their conclusion being based in part on additional data that they had taken. The average time spent by water in the air was found by von Buttlar and Libby[3] to be between 5 and 14 days.

For these reasons it seems very likely that rainfall or snowfall carries down a major part of the fallout which comes from the stratosphere and probably is a very important mechanism for that part of the tropospheric fallout material which does not fall out in the first few hours or day or two after the detonations. Of course, the whole question can be settled by direct experiment in which a correlation between rainfall and total fallout is sought. The present data seem to favor the hypothesis. This conclusion and prediction seem to be borne out by Table 6, which presents the total Sr^{90} content of the top 2 inches of typical United States soils collected in October, 1955, and leached at room temperature for 30 minutes with 6 N HCl.

Table 7 shows data obtained in the Chicago laboratories on the Sr^{90} contents of rivers and lakes. It is clear that these are much lower than those of the rain from which they are derived. For example, from Figure 5 we should estimate that the average rain in the Chicago area in the summer of 1953 had a Sr^{90} content of about 7 dpm/gal. From Table 1 for the domestic pre-Castle soil contents and from Figures 4*a* and 4*b* for foreign pre-Castle soil contents, we estimate that the European

rains averaged about 3 dpm/gal. The four rivers, Mississippi, Mosel, Seine, and Danube, show less than 5 per cent of this; hence we conclude that the Sr^{90} in rain is removed by the soil before the water runs off to the rivers and lakes. This fact agrees, of course, with the sharp localization of the Sr^{90} in the top 2 inches of soil (cf. Tables 1, 2, and 3).

TABLE 6*

Sr^{90} FALLOUT ACCUMULATION IN TOP SOIL (0–2 INCHES) IN U.S. IN 1955
(Sampled September 23–October 20, 1955)

Station	Measured in Soil (dpm/Ft²)
La Guardia	310†
	350 ± 21
	550 ± 16
Binghamton	710 ± 16
Philadelphia	450 ± 19
Rochester	550 ± 20
Jacksonville	470 ± 20
Atlanta	530 ± 12
Detroit	640 ± 21
New Orleans	470 ± 14
Memphis	900 ± 20
Des Moines	540 ± 13
Rapid City	1070 ± 21
Seattle	400 ± 15
Boise	1160 ± 23
Albuquerque	270 ± 14
	290 ± 20
Grand Junction	280 ± 14
Salt Lake City	860 ± 18
Los Angeles	120 ± 12
Average	578
or	7.3 mc/mi²

(Probable additional Sr^{90} in lower layers and to be released by additional leaching probably will raise this about twofold.)

* Data by E. P. Hardy and R. S. Morse, of the Health and Safety Laboratory, New York Operations Office, personal communication.
† This datum was obtained by Dr. J. L. Kulp, Lamont Geological Observatory, Columbia University. The procedure was different from that of the New York Operations Office (personal communication).

TABLE 7

Sr^{90} CONTENT OF RIVER AND LAKE WATERS*

Location	Sr^{90} Content (dpm/Gal)	Location	Sr^{90} Content (dpm/Gal)
Lake Michigan, October 27, 1953	0.39 ± 0.08	Mosel River, Metz, September 7, 1953	0 ± 0.05
Mississippi River, Memphis, February 4, 1953	1.13 ± 0.16	Seine River, Nogent, September 8, 1953	0 ± 0.09
Mississippi River, St. Louis, April 17, 1953	<0.77 ± 0.18	Danube River, Ulm, September 12, 1953	0 ± 0.07

* See note to Table 2.

Examination of the data in Tables 2 and 3 on the Sr^{90} content of the exchangeable calcium in soils shows that there is a strong tendency for the lowest activity of Sr^{90} per unit weight of exchangeable Ca to occur in Ca-rich soils and vice versa, as would be expected, of course, if the fallout rate were uniform. This situation is displayed graphically for the pre-Castle soil data in Figure 6.

For years the Health and Safety Laboratory of the New York Operations Office of the Atomic Energy Commission (Mr. Merril Eisenbud, manager), with the co-

operation of the United States Weather Bureau, has been collecting fallout data[17] by use of gummed papers of 1-ft.^{2} area which are laid flat for a certain time out in the open away from buildings. After the exposure, the paper is folded and mailed to the New York Operations Office in an ordinary envelope. Samples thus can be collected cheaply, easily, and quickly from any populated area anywhere the postal service reaches. Most of the data so obtained have dealt with total fallout rather than with Sr^{90} specifically, but many analyses for Sr^{90} have been made since Operation Castle. These are presented later.

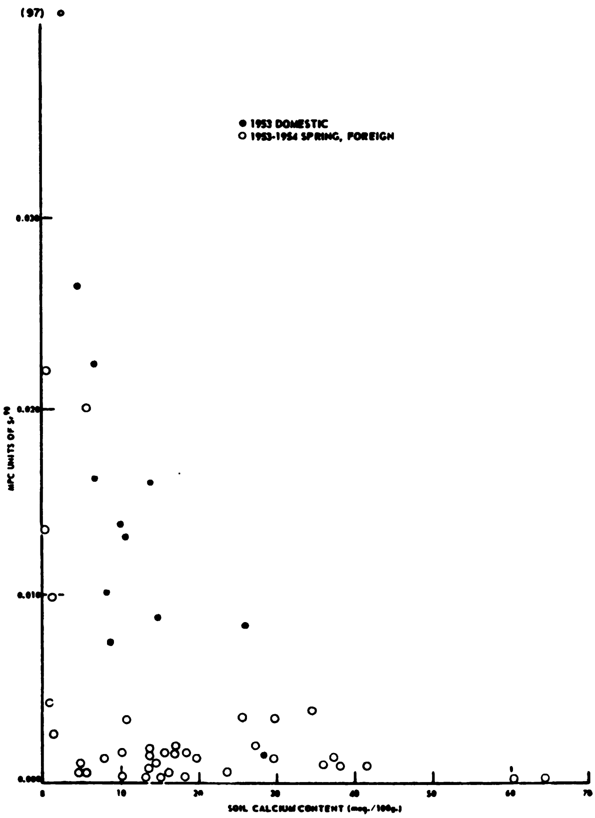

FIG. 6.—Top soil Sr^{90} concentration versus calcium content

The main question about the gummed-paper technique is its over-all efficiency of collection. In order to determine this, the Health and Safety Laboratory has conducted an extensive series of comparisons of the amounts of fallout by gummed paper and a 12-gallon pot with an 18-inch vertical cylindrical wall placed immediately beside the paper. Some of the data thus obtained are given in Table 8. From them we deduce a collection efficiency of 69 ± 9 per cent (but we use 63 per cent, since Mr. Eisenbud recommends this on the basis of more data and a better statistical treatment).

The data thus obtained for the post-Castle Sr^{90} fallout rate in the United States and South America are given in Table 9. These are combined with those for other areas, to give the world Sr^{90} fallout rates for September, October, November, and

TABLE 8

GUMMED PAPER COLLECTION EFFICIENCY
(Relative to 12-Gallon Pot [18-Inch Vertical Wall, 12-Inch Diameter; Cylindrical])

Month	Gummed Paper (dpm/Ft²/Mo)	Pot (dpm/Ft²/Mo)	Efficiency (Per Cent)	Reference
Mar., 1954	11.6	14	84	*
Apr., 1954	15.2	31	49	*
May, 1954	21.6	34	63	*
June, 1954	10.7	9.2	116	*
July, 1954	17.6	25	70	*
Aug., 1954	13.5	7.7	176	*
Sept., 1954	20.7	92	22	*
Oct., 1954	3.1	11	29	*
Nov., 1954	5.7	32	18	*
Jan., 1955	9.0	9.9	92	†
Feb., 1955	29.8	50.6	58	‡
Mar., 1955	210	150	140	‡
Apr., 1955	44.9	79.5	57	§
May, 1955	18.6	56.4	33	‖
June, 1955	12.4	51.7	24	‖
		Average	69 ± 9#	

* Interim Sunshine Report, NYOO Report NYO-4620, January 17, 1955.
† Sunshine Report for January and February, NYOO Report NYO-4643, April 21, 1955.
‡ Sunshine Report for March and April, NYOO Report NYO-4646, May 30, 1955.
§ Sunshine Report for May and June, NYOO Report NYO-4653, July 5, 1955.
‖ NYOO Report NYO-4623, January 18, 1955.
The figure of 63 per cent will be used for consistency. Mr. Merril Eisenbud (NYOO) recommends this on the basis of more data and a better statistical treatment.

TABLE 9

POST-CASTLE FALLOUT IN U.S. FROM GUMMED PAPERS
(Taken at 63 Per Cent Efficiency [cf. Table 8])

Month	Location	Sr^{90} Fallout Rate (Mc/Mi²/Yr)	Reference
Sept., 1954	Eastern U.S. (10 stations)	2.5 ± 0.2	*
Oct., 1954	Eastern U.S. (10 stations)	2.0 ± 0.4	*
Nov., 1954	Eastern U.S. (10 stations)	2.0 ± 0.4	*
Dec., 1954	Eastern U.S. (10 stations)	1.4 ± 0.2	†
Jan., 1955	Eastern U.S. (9 stations)	1.3 ± 0.2	†
Sept., 1954	Western U.S. (20 stations)	0.9 ± 0.2	‡
Sept., 1954	U.S. (38 stations)	0.92 ± 0.2	§
Oct., 1954	U.S. (38 stations)	0.79 ± 0.2	§
Nov., 1954	U.S. (38 stations)	0.95 ± 0.2	§
Dec., 1954	U.S. (37 stations)	0.71 ± 0.2	‖
Sept., 1954	South America (12 stations)	2.1 ± 0.2	‡
Oct., 1954	South America (12 stations)	1.6 ± 0.2	‡
Nov., 1954	South America (12 stations)	2.4 ± 0.2	‡

* Sunshine Report for March and April, NYOO Report NYO-4646, May 30, 1955.
† Sunshine Report for January and February, NYOO Report NYO-4643, April 21, 1955.
‡ Sunshine Report for May and June, NYOO Report NYO-4653, July 5, 1955.
§ Sunshine Report, August 1954, NYOO, HASL-S-2 (NYO-4656), November 12, 1954.
‖ Sunshine Report for July and August, NYOO Report NYO-4661, September 16, 1955.

December, 1954, presented in Table 10 and Figure 7. From these data we obtained these extremely important conclusions:

1. A Sr^{90} fallout probably derived from megaton weapons and nearly uniform over the world, except for local effects due to rainfall variations and to fallout from

submegaton weapons, seems clearly established. The fallout from the kiloton weapons lasts only a few weeks at most, since they involve only tropospheric

TABLE 10

WORLD-WIDE Sr^{90} FALLOUT RATE FROM GUMMED PAPERS*

(Taken at 63 Per Cent Efficiency [cf. Table 8]; $Mc/Mi^2/Yr$)

Area	Per Cent Total Earth's Area	No. Stations (Sept.)	Sept., 1954	Oct., 1954	Nov., 1954	Dec., 1954	Average
Arctic	6.5	5	1.2 ± 0.4	3.0 ± 0.4	1.4 ± 0.4	0.9 ± 0.4	1.6 ± 0.2
North Temperate	10.9	14	1.7 ± 0.24	0.68 ± 0.24	1.4 ± 0.24	0.6 ± 0.24	1.1 ± 0.12
Pacific	8.0	2	0 ± 0.6	0.6 ± 0.6	1.9 ± 0.6	1.2 ± 0.6	0.9 ± 0.3
U.S.	1.5	39	1.3 ± 1.4	0.9 ± 0.14	1.4 ± 0.14	0.9 ± 0.14	1.1 ± 0.07
North Tropic	18.5	8	0.7 ± 0.3	1.5 ± 0.3	0.7 ± 0.3	0.4 ± 0.3	0.8 ± 0.16
South Tropic	25.5	9	3.2 ± 0.3	2.4 ± 0.3	2.5 ± 0.3	0.6 ± 0.3	2.1 ± 0.15
					Average (area weighted)		1.5 ± 0.1

Area	Jan., 1955	Feb., 1955	Mar., 1955	Apr., 1955	May, 1955	June, 1955	July, 1955	Aug., 1955	Average
Arctic	0.32	1.2	0.52	0.62	2.0	1.5	1.6	0.82	1.07
North Temperate	1.0	0.55	1.1	1.1	2.9	1.9	2.5	1.4	1.57
Pacific (21 stations)	0.53	0.58	2.2	1.1	1.6	1.3	1.2	1.0	1.19
U.S.	0.86	0.86	1.4	2.3	4.0	2.8	2.9	1.4	2.07
North Tropic	0.71	0.70	1.2	0.78	1.5	0.80	0.87	0.50	0.88
South Tropic	1.5	1.3	0.83	0.40	1.0	1.5	0.60	0.72	0.98
South Temperate (4 stations)	1.1	0.46	0.9	0.40	0.38	1.9	1.1	1.4	0.74
Average (area weighted, except U.S. and North Temperate omitted because of Teapot)									0.95 ± 0.1

* Sunshine Report for July and August, NYOO Report NYO-4661, September 16, 1955, and personal communication from Dr. E. C. Plesset, Rand Corporation.

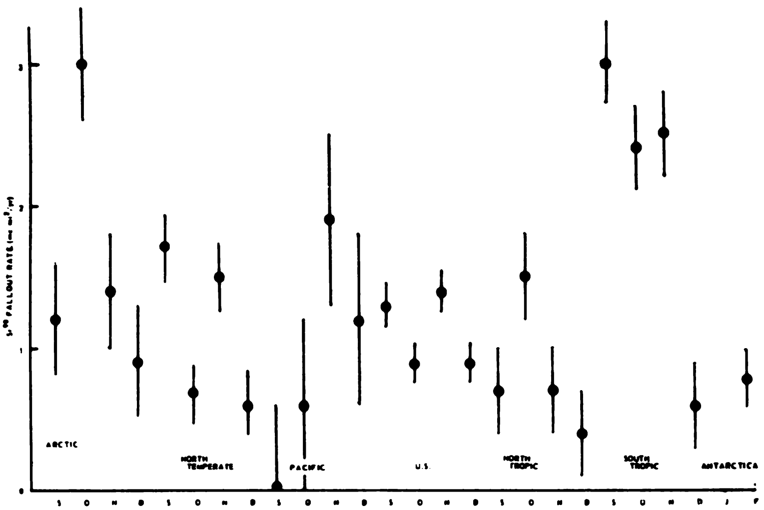

FIG. 7.—World-wide Sr^{90} fallout rates, September–December, 1954, (gummed paper at 63 per cent efficiency except antarctic value, which was snow).

storage, but widespread fallout is found to occur at least 1.7 years after a megaton test series.

2. This average world-wide Sr^{90} fallout rate in the fall of 1954 and the spring and summer of 1955 was 1.2 mc/mi²/yr (see Table 10).

Additional information available on the Sr^{90} distribution has been obtained by air filters operated at sea level, principally by the Naval Research Laboratory, measured in the Chicago Sunshine Laboratory, and by the Health and Safety Laboratory.[18] The surface samples collected at Washington, D.C., by the Naval Research Laboratory and measured at Chicago[8–12] are given in Table 11 and

TABLE 11

AIR-FILTER DATA, WASHINGTON, D.C.

(Army Chemical Corps Type 5 Paper, 99 Per Cent Efficient Down to Few Tenths Micron; 75 Per Cent Efficient Down to 0.01 Micron)

Washington, D.C.	Sr^{90} (dpm/10^6 Ft³)	Equivalent* Fallout (Mc/Mi²/Yr)
April 5–8, 1953	18.6 ± 0.7	0.14
October 2–6, 1953	41.1 ± 3.0	0.3
October 6–9, 1953	30.5 ± 1.1	0.2
October 12–15, 1953	70.4 ± 12	0.5
April 3–5, 1954	91.0 ± 7	0.7
April 8–10, 1954	6.4 ± 0.2	0.05
April 10–12, 1954	258 ± 6	1.9
April 12–14, 1954	65.5 ± 4.6	0.5
April 15–17, 1954	11 ± 0.5	0.08
April 17–19, 1954	21 ± 0.6	0.16
April 29–May 1, 1954	32.2 ± 2.6	0.2
May 11–13, 1954	31.3 ± 2.2	0.2
May 24–26, 1954	216 ± 11	1.6
June 1–3, 1954	68.3 ± 4	0.5
July 16–17, 1954	73.5 ± 5.2	0.5
July 26–29, 1954	48 ± 3.9	0.36
November 1–3, 1954	120 ± 7	0.9
December 1–2, 1954	103 ± 4	0.8
January 3–4, 1955	281 ± 6	2.1
February 5–6, 1955	127 ± 5	0.9
February 10–12, 1955	241 ± 10	1.8
February 22–23, 1955	202 ± 11	1.5
March 3–4, 1955	270 ± 13	2.0
March 7–8, 1955	394 ± 20	2.9
March 13–14, 1955	267 ± 16	2.0
March 16–17, 1955	310 ± 15	2.3
March 22–23, 1955	393 ± 20	2.9

* 134 dpm/10^6 ft³ = 1 mc/mi²/yr (cf. text); (28300 ft³/ft² = 10^6 ft³/35.5 ft³).

Figure 8. In order to correlate these data with the fallout rate, we recall that, as remarked previously, any rain of 0.1 inch or more probably will thoroughly wash down the fallout in the air below the layer at which the rain originates. Examination of the weather data for the Washington, D.C., area in the period when the samples were taken shows that the average interval between rains was 6 ± 3 days. Therefore, the Sr^{90} content of surface air should correspond to fallout for this time on the average, and we would expect a fallout rate R (mc/mi²/yr) to correspond to a surface-air content of $R \times \frac{6}{365} \times 79 \times 41 \times 2.5$ dpm/10^6 ft³, since 79 dpm/ft² is equivalent to 1 mc/mi² and there are 41 ft²/10^6 ft³ below the tropopause on the average at Washington, D.C., and 2.5 is the average ratio of height of tropopause to rain-bearing layers. The resulting rate of 0.70 ± 0.2

mc/mi²/yr is definitely low, compared to the rain result of 2.3 ± 0.2 mc/mi²/yr for Chicago (Fig. 5) and of 1.5 ± 0.1 mc/mi²/yr for data involving gummed paper (Fig. 7 and Table 9). The uncertainty in the factor of 2.5 for the ratio of rain-bearing layer height is probably the principal uncertainty in the calculation of the fallout rate from air-filter data, though it may be that the perfect vertical mixing of the lower 40 per cent of the troposphere implicit in the calculation is an incorrect assumption, in that air right at the surface is cleaned to a considerable extent by surface contact with vegetation and water and soil and therefore has less fallout than the average for the lower troposphere.

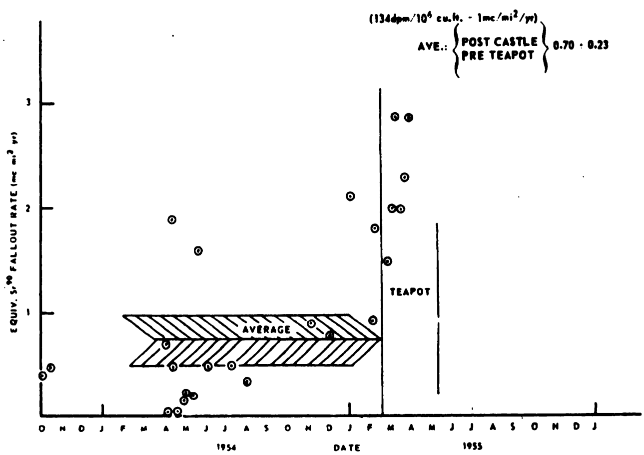

Fig. 8.—Sr^{90} in air, Washington, D.C.

From the data given on rates of fallout, we calculate the average stratospheric residence time, $\tau = 10 \pm 5$ years. The high-altitude data show a definite rise above the tropopause. This is strong confirmation for the stratospheric storage and dissemination mechanism.

II. DISCUSSION

A. Predicted Sr^{90} Fallout

The stratosphere reservoir of Sr^{90} immediately after Operation Castle had been completed was about 12 mc/mi². The fallout rate of Sr^{90} corresponds to an average storage time of 10 ± 5 years and essentially uniform world-wide dissemination. The radioactive half-life of Sr^{90} is 28 years, corresponding to an average life of 40 years. Therefore, the Sr^{90} fallout rate from tests up to and through the Castle series should be given by

$$R = \left(\frac{1}{10}\right) 12e^{-t(1/10+1/40)} \text{ mc/mi}^2\text{/yr}, \tag{1}$$

where t is the time elapsed, in years, since May, 1954. This relation is given in Figure 9. From it, we can predict the tropospheric air content to be

$$A = \frac{RT \times 2.5 \times 79 \times 41}{365} \text{ dpm}/10^6 \text{ ft}^3, \tag{2}$$

where a period of T days elapses between the rains washing out the air mass concerned. Taking T to be 6 days on the average, the surface air should contain 134 R dpm/10^6 ft^3 in the middle latitudes.

The rainfall content will be 4 R dpm/gal for regions with an annual rainfall of 31.5 inches. The concentrations of Sr^{90} per unit volume for other annual precipitations are to be derived by inverse proportionately, e.g., in Antarctica, where the annual precipitation is about one-fourth of 31.5 inches, the Sr^{90} content of snow should be given by 16 R, or

$$A' = 16 \times 1.20e^{-t(1/10+1/40)} \text{ dpm/gal}. \tag{3}$$

In this connection, it is very important to note that regions of frequent rainfall very probably will receive more Sr^{90} fallout than will more arid regions.

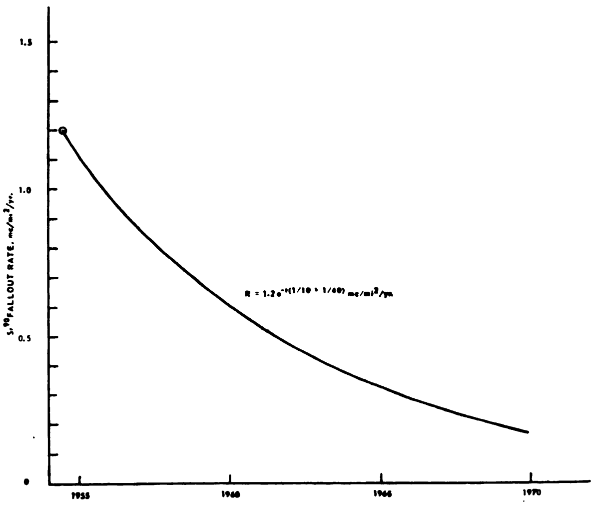

Fig. 9.—Predicted fallout rate for Sr^{90} formed prior to and during operation castle.

Of course, some fallout will be deposited by the surface winds blowing over the leaves of trees and grass. For example, the Naval Research Laboratory[10] has mounted an uncharged platinum screen vertically and held normal to the surface winds by a large vane. The deposition is by impact. Two weeks' total collections were made, and these gave up to 20 times as much as for gummed papers of the same area exposed for the same time in the same place. The screen was 80 mesh and probably passed about 0.5 × 10^6 ft.^{3} in the 2 weeks' exposure. From this it is clear that surface contact with fallout-laden tropospheric air must result in dep-

osition. Further evidence for this is seen in Table 1, where there is seen to be essentially no correlation between the Sr^{90} contents of soils and the crops of alfalfa grown on them. There obviously must have been very considerable direct dep osition on the surface of the plants. Also, the relatively low values for the Sr^{90} content of surface air found by the Naval Research Laboratory (Table 11), calculated with respect to the observed rain content, may very well be due to surface deposition by direct contact on tree leaves and grass.

The soil content will be the total of all fallout radioactivity, *less any natural weathering processes which serve to remove the fallout Sr^{90} from the chemically available form in which plants can assimilate it.* Neglecting this latter effect, although, as we shall see, there is reason to believe that such effects are operative in an important way, and taking the average for the exchangeable Ca content of soils over the world (cf. Tables 2 and 3), which is 8 gm/ft²/in. we calculate that the top 2.5 inches of soil, which in general holds nearly all the Sr^{90} (Tables 2 and 3), has some 20 gm of exchangeable Ca. Therefore, we predict that in the absence of curative weathering effects acting to remove Sr^{90} from contact with the biosphere, the average Sr^{90} concentration of exchangeable Ca, in MPC units, should be

$$S = \frac{0.0792}{2.2 \times 20}\left[P + \int_0^t R\,dt\right], \tag{4}$$

where P is the pre-Castle deposition of 0.8 mc/mi² in the middle latitudes and 0.2 mc/mi² world wide. Thus in the middle latitudes the Sr^{90} concentration of exchangeable soil Ca should be given in MPC units by

$$S = \frac{0.0792}{2.2 \times 20}\left[0.8 + 1.2\int_0^t e^{-t/10}\,dt\right]e^{-t/40} \tag{5}$$

or,

$$S = [0.0015 + 0.0213(1 - e^{-t/10})]e^{-t/40}. \tag{5'}$$

This result in terms of mc/mi² and MPC units is presented graphically in Figure 10. The maximum post-Castle Sr^{90} soil activity will be expected in about 1970. The present average should be about 0.005 MPC unit for soil with 20 gm. exchangeable Ca/ft². Those soils of low Ca content can, of course, have a much higher Sr^{90} content for unit amount of exchangeable Ca. Consider, for example, certain areas in Wales near Cardigan (cf. Table 3, Sample No. 54417), where the available Ca/ft² amounted to only 0.4 gm. and the specific Sr^{90} activity was found to be 0.097 MPC. For this area our analysis would predict a forty fold higher content than that given in Figure 10. This, of course, is reflected in higher contents for the bones of grazing animals.

The weathering processes which may operate to fix the fallout Sr^{90} and make it unavailable to the biosphere, such as the fixation in massive Ca deposits, are worthy of consideration. Only further investigation will reveal how important such processes are. The present Project Sunshine sampling program includes repeated sampling of given regions. The data so obtained should disclose any such trends. Some data already in hand seem to indicate such effects, but further confirmation is necessary.

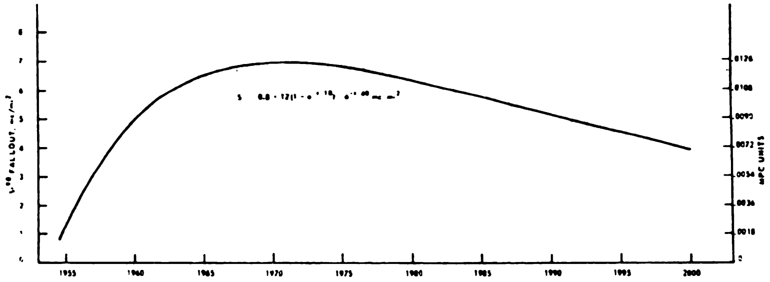

FIG. 10.—Predicted Sr^{90} in soil following Castle (Assumed Ca = 20 gms./ft²/2.5")

In addition, palliative measures may prove effective. For example, Nervik, Kalkstein, and Libby[19] have shown that milk can be purified for radiostrontium by a treatment which may well prove to be quite practical and inexpensive.

B. BIOSPHERE CONTENT OF Sr^{90}

It seems clear that there will be discriminatory barriers of some magnitude operating at each of the stages of transfer from the soil into the biosphere. Taking these stages in general to be

(1) Soil → Plants,
(2) Plants → Animals,
(3) Animals (milk) → Humans,

we consider the three corresponding barriers.

1. *Soil-to-Plant Transfers.*—In Table 12 the Sr^{90} contents of plants are compared with those for the soils on which they grew at a variety of localities just before Operation Castle. It seems clear that, on the average, plants have about twice the specific Sr^{90} content relative to Ca that the supporting soils do. It seems

TABLE 12

PRE-CASTLE PLANT Sr^{90} CONTENTS
(Values Are Given in Terms of 1/1,000 MPC Units)

Location	Plant	Date	Reference	Plant Content	Content of Soil Which Grew Plant
U.S.	Alfalfa	(Average of Table 1)		8.9	4.7 ± 0.4
Turkey	Alfalfa	10–53	*	2.16 ± 0.18	1.2 ± 0.1
Cuba	Tobacco	4–54	*	1.7 ± 0.2	(1.6)†
New Zealand	Forage	11–53	‡	1.17 ± 0.28	0.21
New Zealand	Forage	1–54	‡	0.84 ± 0.10	0.18
Chile	Forage	11–53	§	0.84 ± 0.02	0.33*

* E. A. Martell and W. F. Libby, Project Sunshine Bull. No. 10, January 10, 1955.
† Calculated from average pre-Castle deposition (Fig. 4a) for latitude of Cuba and assumed average Ca content of 20 gm/ft² in top 2.5 inches.
‡ E. A. Martell, Project Sunshine Bull. No. 10, Suppl. 1, March 1, 1955.
§ E. A. Martell, *ibid.*, Suppl. 2, June 1, 1955.

very likely that this is due in part to fallout occurring directly on the external plant surfaces. This is further borne out by the lack of detailed correlation in Table 1 between soil and alfalfa results for eleven middle western United States farms. From this result, we may calculate that about half the total Sr^{90} content of alfalfa is due to direct fallout, while for general forage it is probably an even

higher percentage. This comparison is not quite accurate, because, although almost all the Sr^{90} is contained in the top 2.5 inches of soil, the Ca, on the other hand, is available to the entire depth of the root system. It is also recognized that the vertical distribution of the Ca may be nonuniform. There is no reason to expect preferential assimilation of Sr from the soil relative to Ca; hence the only other explanation for the data in Table 12 is direct fallout. As remarked in Section I in the paragraphs on rain and air-filter data, there are two mechanisms for direct fallout of the ultra-finely divided particulate matter carrying the Sr^{90} in the stratospheric reservoir—rainout and contact deposition after the fallout has entered the troposphere. Rainout appears to be the principal mechanism, though it has been demonstrated by the Naval Research Laboratory,[20] as mentioned earlier, that an 80-mesh screen mounted vertically to prevailing surface winds can gather more fallout than falls out directly on the average by all mechanisms. It is not clear, however, to what degree foliage acts in this way, but the probability seems high that the effect is relatively minor and that rainout, followed by drying of raindrops, is the main way in which the foliage surfaces gather fallout.

On this basis we conclude that total fallout in arid regions should be appreciably lower than in areas with normal rainfall. This effect seems to be borne out by the data available now, though more definitive experiments are needed. It probably follows also that regions subject to seasonal rainfall rather than relatively uniform precipitation all year should show less fallout for the same total annual rainfall. Also, regions subject to frequent morning fogs may be particularly high in total fallout. The Sr^{90} probably will enter the troposphere at relatively uniform rates, but the chance of precipitation will depend strongly on the local weather.

Of course, rainfall is necessary to plant growths, so that plants are certain to gather some fallout. However, for regions of low rainfall where irrigation is used—such as the Imperial Valley in California—the fallout content of the crops should be particularly low, for, as shown in Table 7, rivers are nearly free of fallout, since the soil purifies the runoff water before it reaches the rivers. Similarly, reservoirs and lakes will be low relative to rain because of dilution and the importance of runoff water from surrounding watersheds in replacing evaporative and withdrawal losses. It is also well to note that the ordinary water purification processes are effective in removing an appreciable fraction of the radiostrontium.

The by-passing of soil entirely, which occurs in the direct fallout on plant surfaces, of course means that the retarding effects of high Ca contents in soil are inoperative, and cattle grazing on such foliage may show little correlation in the Sr^{90} contents with the soil Sr^{90} activity for this reason. This appears, from the data in Table 1, to be true in the United States Midwest.

It is clear, however, that washing may reduce the level of fallout externally carried by plants, though direct leaf absorption will be expected to occur rather rapidly for water-soluble fallout. For the megaton weapons fired in the Pacific, the bulk of the fallout resides on particles of CaO or $Ca(OH)_2$ or mixtures of CaO, $Ca(OH)_2$, and $CaCO_3$ made by the great heat of the fireball acting on the coral of the islands and sea floor in the firing areas.[21] A large amount also is carried on NaCl particles. This material, therefore, should be quite water-soluble and should

be rapidly absorbed into the leaves. Washing therefore probably will not be particularly effective for the world-wide fallout, which derives from the Pacific tests. From weapons fired in the air, the particles probably will consist of less soluble oxides and therefore are more likely to wash off of plant surfaces before being absorbed.

Menzel,[22] of the United States Department of Agriculture, grew cowpeas on 42 American soils to which equal amounts of bomb debris had been added. Available Ca ranged from 0.7 to 48 milliequivalents of Ca/100 gm of soil. The Sr^{90}/Ca ratio of the plants was approximately inversely proportional to the available Ca in the soil over the full range of Ca availability. In another set of experiments on a particular type of soil (Evesboro) to which known amounts of Sr^{90} had been added at two carrier levels, the results listed in Table 13 were found. The distribution

TABLE 13

PLANT ASSIMILATION OF Sr^{90} FROM SOIL

Crop	(Sr/Ca) Soil (By Equivalents)	k_{Sr}	k_{Ba}
Barley	0.017	0.45	0.020
	0.0017	0.39	0.022
Buckwheat	0.017	0.49	0.023
	0.0017	0.43	0.028
Cowpeas	0.017	0.53	0.057
	0.0017	0.37	0.053

factor, k_{Sr}, defined as (Sr/Ca)plant/(Sr/Ca)soil, indicates the discrimination which the plant makes between Sr and Ca uptake. Similar tests were made for Ba.

By combining these data, Menzel concluded, as shown in Table 13, that the average Sr uptake from American soils was best fitted by a distribution factor of $k_{Sr} = 0.36$. This average probably will apply world-wide about as well.

2. *Plant-to-Animal Transfer.*—The Sr^{90} contained in grass and foliage eaten by grazing animals will be retained to an extent dependent on the metabolism of the animal. For example, for a 1-year-old steer[23] 30 per cent of the Sr^{90} fed orally was retained with essentially no discrimination relative to Ca. There appears to be a higher retention, approaching 90 per cent, for intravenous injection of young rats.[23] High Ca diets reduce the Sr^{90} uptake for rats, adult rats take up about 16 per cent of the ingested Sr^{90} on the same low Ca diet for which the young rats took up 73 per cent.[23]

Comar[24] has performed experiments on cows in which the Sr/Ca ratio in feed, blood, and milk was measured under equilibrium conditions. Typical relative values were 1.0, 0.37, and 0.13, respectively, thus indicating a relative lowering of the Sr^{90} content of the milk relative to the feed. This is borne out by direct observation on the fallout, as seen in Table 1, where for the Chicago milkshed the milk averaged 0.0014 MPC, while the alfalfa fed averaged 0.0089 MPC.

3. *Milk-to-Human Transfer.*—This ability of the cow to reduce the Sr^{90} in the milk relative to the feed is important as a barrier to human ingestion of this fallout radioactivity. With it in mind, we can expect that human Sr^{90} burdens should be 20 per cent or less (for older people) of the plant contents and about equal to the milk and cheese levels if the entire Ca in the body were assimilated at a given Sr^{90} content of the milk.

Since the Sr^{90} content of the whole biosphere is continually rising, about as shown in Figure 10, the average Sr^{90} content of milk should rise in a similar manner. Therefore, the intake of Sr^{90} by humans is steadily increasing, as shown in Figures 1*a*, 1*b*, 1*c*, and 1*d*. The data in Table 3 and Figures 1*c* and 1*d* show that the milk level at the time of Operation Castle averaged about 0.001 MPC, peaking in the middle latitudes, as did the soil assays (cf. Fig. 4*a*). This value shows a ratio of about 0.7 for the milk level to the average soil level for the top 2.5 inches of soil with 20 gm. of contained available Ca/ft^2. This high value probably is due to leaf pickup of fallout. Taking this as a general result, we predict the world values for milk and cheese at about 70 per cent of the soil values given in Figure 10, plus any local fallout. This would mean that we would expect average foreign milk and cheese samples to show about 0.004 MPC at the present time.

The human bone, of course, is formed during the growing process, so that the Sr^{90} content of children should be higher than for adults. The data[2, 8–12, 25] verify this prediction; however, for newborn babies there is less Sr^{90} than corresponds to the milk, showing the retarding effect of the mother's older Ca pool. Children seem to carry Sr^{90} approximately equal to the average level of the milk during the period of their lives. Adults decrease in Sr^{90} concentration with age as expected. It seems that the adults of the future will have Sr^{90} levels corresponding to the milk levels during their lifetime weighted according to their rate of growth. For example, the years from 10 to 18 will be most important for men and from 6 to 12 for women. Foreign children born now, according to Figure 10, should develop about 0.011 MPC unit during their lives. Children born now in the United States will develop a somewhat higher level, due to somewhat higher milk levels.

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CURRENT RESEARCH FINDINGS ON RADIOACTIVE FALLOUT

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I. INTRODUCTION

The radioactivity produced by the fission reaction, being due to a mixture of many different fission products, changes its characteristics continuously and rapidly following release by the bomb detonation. Thus the conditions of firing are of extreme importance in determining the fallout effects. The intensity of radiation is enormously greater soon after the detonations, decreasing about tenfold for every seven fold increase in age. Since the time required for ingestion into the body is long, ingestion is unlikely for the shorter-lived fission products, and therefore the principal hazards for close-in fallout are radiation exposures by gamma radiation of the whole body and by beta radiation on the skin.

In the longer times, weeks and months after the explosion, the ingestive hazards begin to become important. The most serious of these is the high-yield fission product, radioactive strontium (Sr^{90}), which because of its own radiation and those of its short-lived daughter, yttrium 90 (Y^{90}), and because of its chemical similarity to the bone-building element, calcium, finds itself deposited in bone structure. Other radioactivities produced would be as bad if they spent as long a time in the body or if their radioactive lifetimes were long enough or if they were produced in high yield. Strontium has all these characteristics. Hence for the fission products which have survived the first weeks, the most important fallout constituent and the one most seriously to be considered is Sr^{90}. Neither radiostrontium nor its yttrium daughter emits gamma radiation, but only beta radiation. After the first-year cesium 137 (Cs^{137}), with half-life of thirty-three years, is the principal source of the residual gamma radiation, and any gamma radiation exposures due to fission products which are more than one year old are due very largely to radioactive cesium. In fact, old fallout can be thought of as a mixture of roughly equal radiation intensities in millicuries of radioactive Sr^{90} and radioactive Cs^{137}. The other isotopes either constitute no ingestive hazard or fail to emit gamma radiation in appreciable intensity. Thus, the hazards of world-wide fallout reduce themselves largely to the ingestive hazard of radioactive Sr^{90} and the external exposure from radioactive Cs^{137}.

The mechanism by which atomic-weapon debris is disseminated leads to three kinds of fallout. First, there is the strictly *local fallout*, which is due to the return to earth of the larger particles in the fireball. These may have their origin in the dirt, the soil, or tower structures which are taken into the fireball and either wholly or partially vaporized. The fraction of the total which falls out locally depends very much upon the conditions of firing. The most serious factor is the degree of contact of the fireball with the surface; another is the nature of the surface. For example, soil appears to be much more effective than water in producing local fallout.

Experience has shown that an atomic device exploded on the surface distributes about 80 per cent of its fission products on the ground within a few hundred miles of the burst point. A somewhat larger percentage takes part in the close-in fallout from an underground burst, and a smaller percentage will be scavenged from a near-surface burst or tower shot.

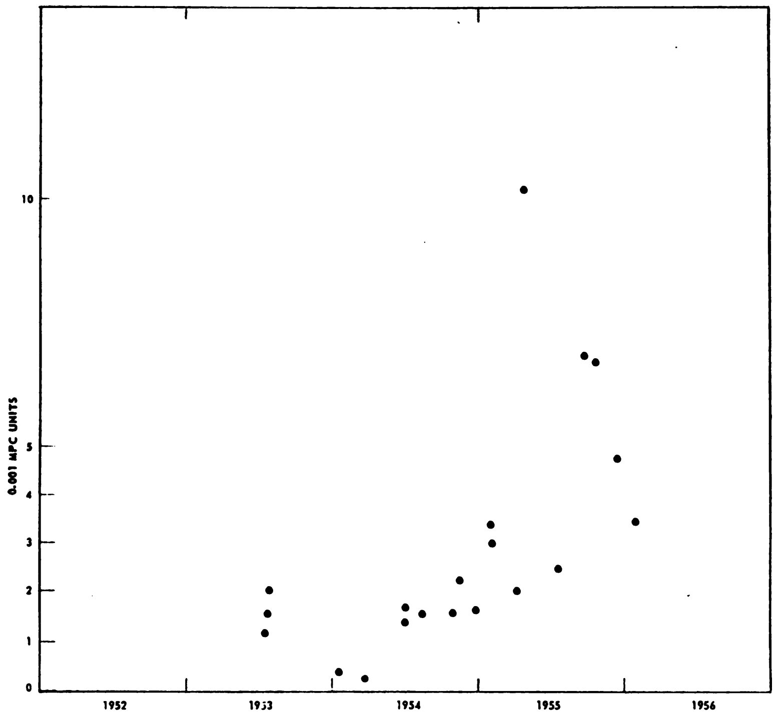

FIG. 1*a*.—Wisconsin cheese, Sr^{90} content

The tower shot is, in a sense, a special case of a surface burst, since the material of the tower itself is mixed with the fission products in the fireball to a greater or lesser degree, depending on the yield. Experience with tower shots indicates that even in cases where the fireball does not touch the ground a few per cent of the radioactive fission products come down as close-in fallout.

The fraction which takes part in the close-in fallout from a surface burst over deep ocean water appears to be somewhere between 20 and 50 per cent. This is less than the fraction of close-in fallout occurring from a corresponding surface burst over land, due to the evaporation of many of the drops before they reach the ground. Presumably this fraction is also affected by the prevailing humidity and temperature structure of the atmosphere through which the drops must fall. As the depth of the water is decreased, the point is reached where the fireball extends downward to the bottom and picks up bottom material. In such shallow water one would expect a higher percentage of close-in fallout than in deep water. Ex-

perience in the Pacific indicates that such is indeed the case and that in fact there would be very little difference in the fallout between a large-yield device in very shallow water and a true surface shot.[1]

The second type of fallout is the material which, though not coarse enough to fall of its own weight in the first few hours, is, nevertheless, left in the lower layer of the atmosphere, known as the *troposphere*, where ordinary weather phenomena occur. It is now well established that fallout particles are removed from this lower layer of the atmosphere in the first month or so; therefore, the tropospheric

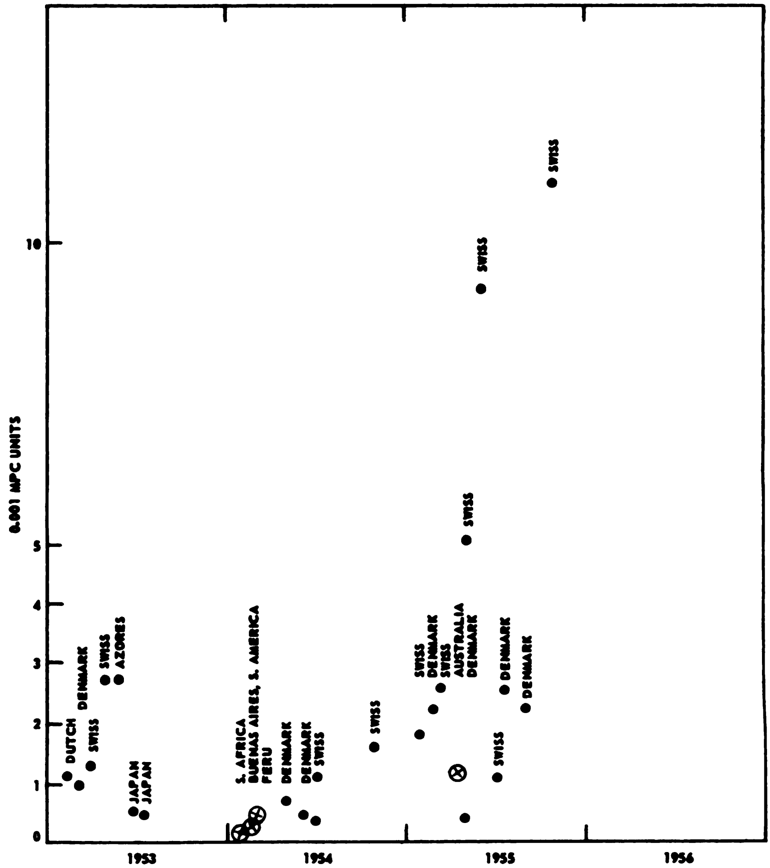

Fig. 1b.—Foreign cheese, Sr^{90} content (*crosses:* Southern Hemisphere)

fallout stays airborne at most a matter of a month or two before being deposited. Of course, in this time, it will in general move great distances, possibly even all the way around the earth; but, in general, it stays in the general latitude in which the explosion occurred. So the second type of fallout, the tropospheric world-wide fallout, produces a band of radioactivity in the general latitude of the firing site.

The fraction of the fallout in this category depends mainly on the bomb yield and the conditions of firing. A bomb which is fired on the ground produces a maximum of *local fallout*, naturally leaving less for the world-wide fallout of either the tropospheric or stratospheric variety. The bomb yield determines the division of the world-wide fallout between the two kinds of world-wide fallout. A general rough rule is that a 1-megaton bomb will produce clouds which push into the higher layer of the atmosphere, the stratosphere, before disseminating and that the clouds from bombs of less than 1 megaton will tend to stay mainly in the troposphere. Thus we see that a 500-kiloton weapon fired so its fireball did not touch the ground would

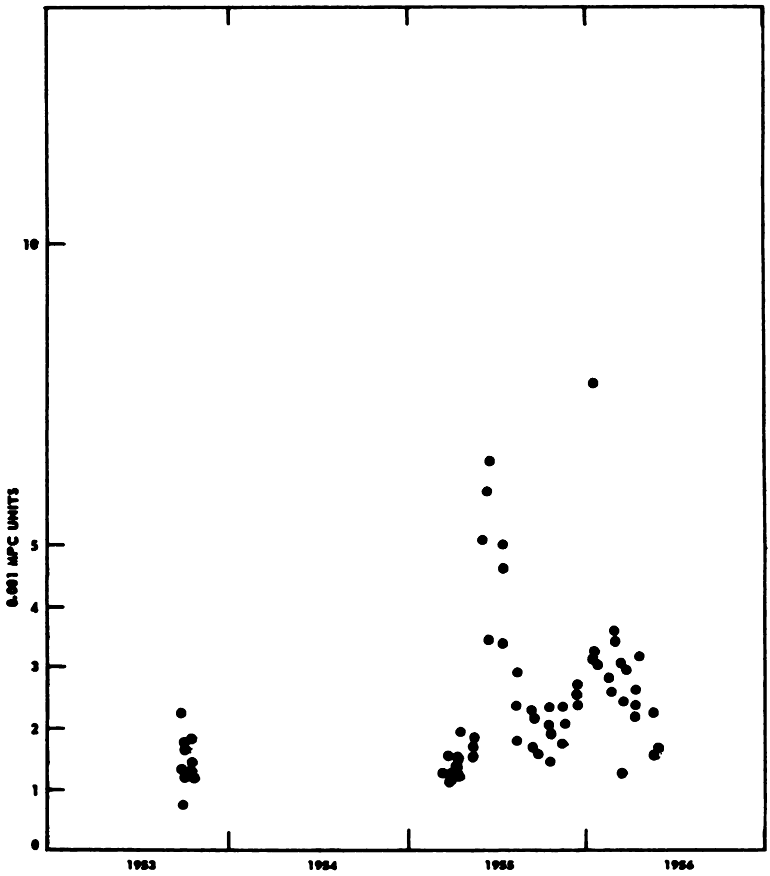

FIG. 1c.—Chicago Milk, Sr^{90} content

be expected to put the major part of its radioactivity in a band stretching around the world in the general latitude of the firing site. The distribution of the activity would be world wide and would be completed within the first month or two. Similarly, the same bomb fired in contact with the earth with ordinary soil would have a

large fraction—something like 80 per cent—of its fallout deposited within the first few hours within a few hundred miles of the test site, and the rest of the material would be spread in the tropospheric world-wide fallout pattern in a band around the earth in the same general latitude.

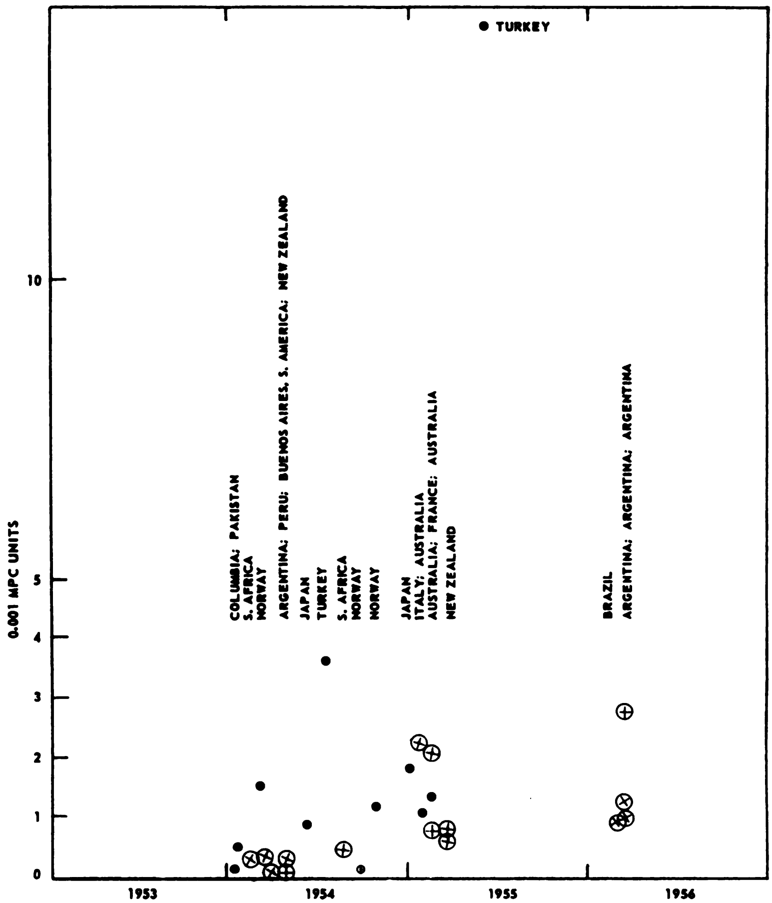

FIG. 1*d*.—Foreign milk, Sr^{90} content (*crosses:* Southern Hemisphere)

The third type of fallout is the *stratospheric world-wide fallout.* Weapons of yields of 1 megaton and greater thrust their radioactive clouds into the stratosphere, and the material which does not fall of its own weight within the first few hours is then very largely borne in the stratosphere for great lengths of time. An average time seems to be about ten years or somewhat less. A small amount of tropospheric world fallout is also produced, presumably due to a small fraction of particulate matter which is of just the right size to descend in a matter of weeks. The division into the two types, the *local* and the *stratospheric world-wide,* is very sharp and

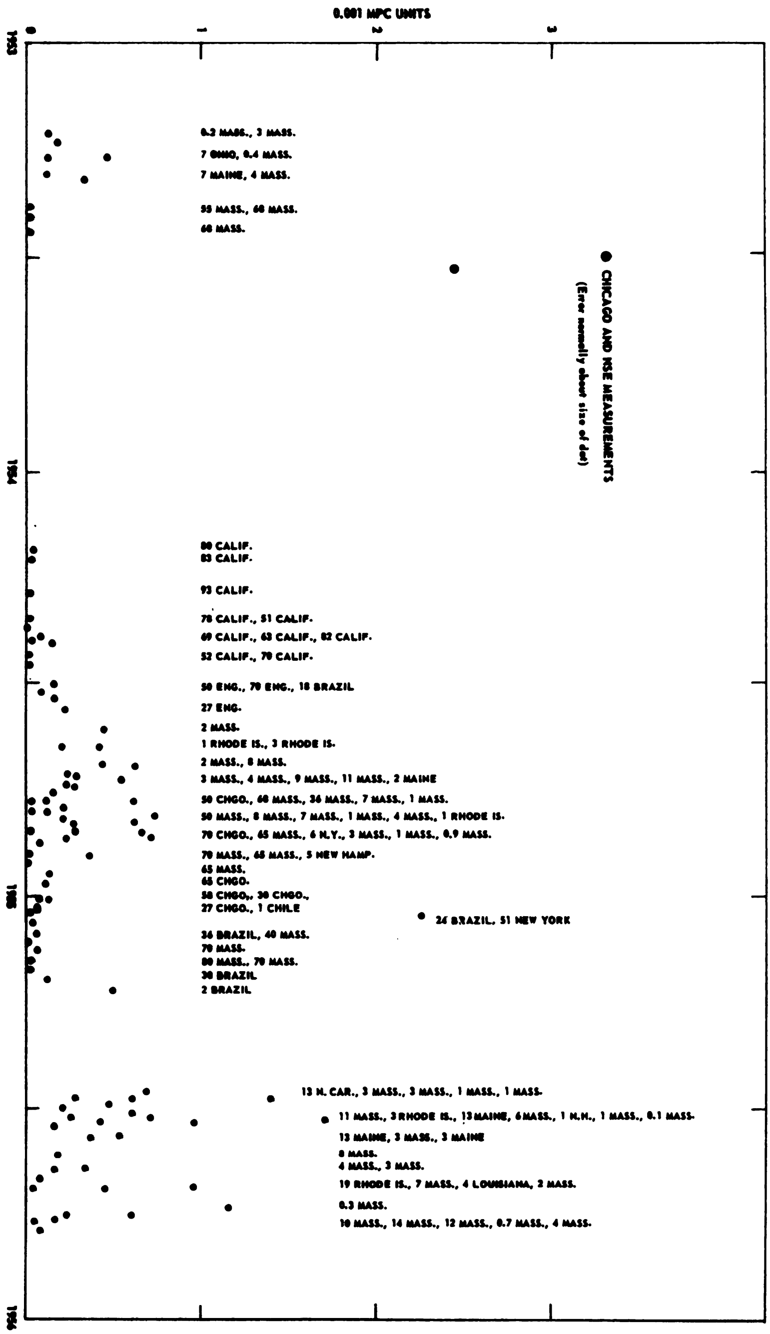

FIG. 2a.—Human bone, Sr^{90} content

marked, however, and to a very considerable approximation one can say that the megaton weapons yield the bulk of their fallout in these two categories. A weapon involving 1 megaton of fission would, if fired in the air, place most of its radioactivity in the stratosphere, and this, in contrast to the tropospheric fallout, appears to be

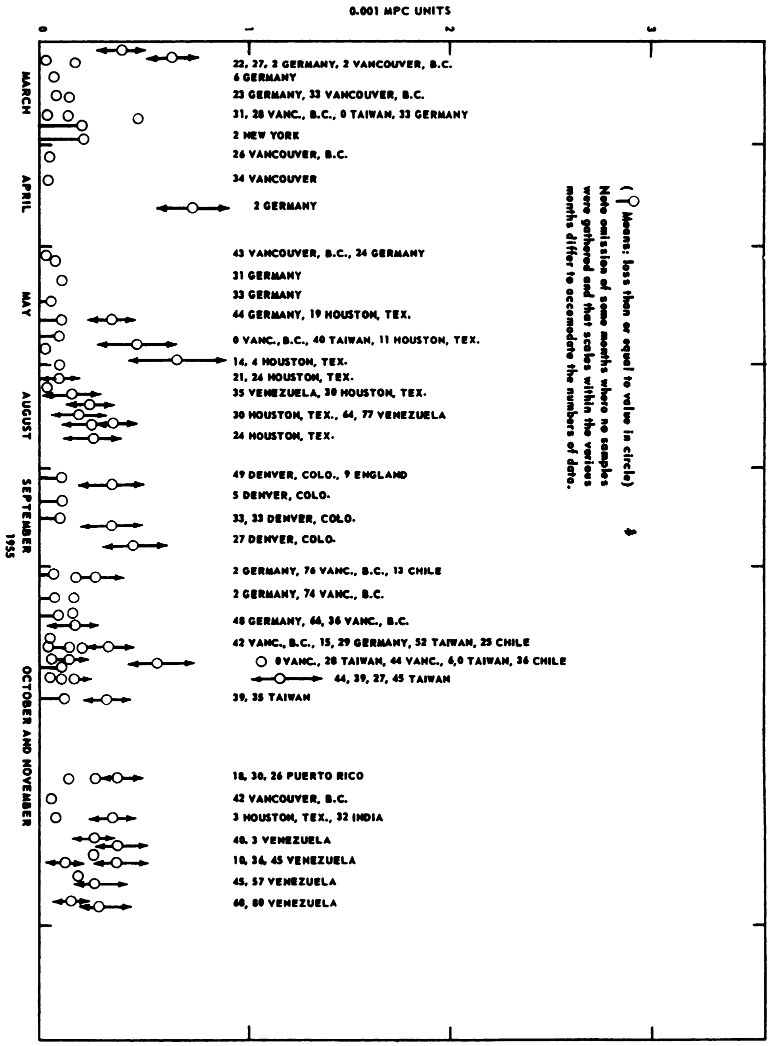

FIG. 2b.—Human bone, Sr^{90} content, Lamont measurements

widely distributed latitudinally and does not descend through the tropopause into the troposphere until months and years—an average time of about ten years—have elapsed. The resultant pattern of fallout appears to be essentially in approximately uniform world-wide distribution. The long time spent in the stratosphere is proba-

bly due to the absence of such scavenging agents as rain or snow, so the particles either must fall out of their own weight or diffuse downward by slight eddy motion, either of these processes being of their very nature slow. After passing through the tropopause into the troposphere, they will be rained out rather quickly in a matter of days or weeks. Because of the long residence time in the air, this type of fallout is particularly harmless as a gamma-ray hazard, since only the Cs^{137} is left. The amount of Cs^{137} is about the same in millicuries as the Sr^{90}; thus 1 megaton of fission thus distributed throughout the stratosphere would yield about 1/2 mc/mi^2 of either Sr^{90} or Cs^{137}. Just as in the case of Sr^{90}, the rate of deposition of about 10 per cent of the reservoir per year corresponds to a stratospheric fallout rate of Cs^{137} of 0.05 mc/mi^2/yr in the beginning. This rate decreases to half, or 0.025 mc/mi^2/yr, at about seven years, as the stratospheric reservoir becomes depleted.

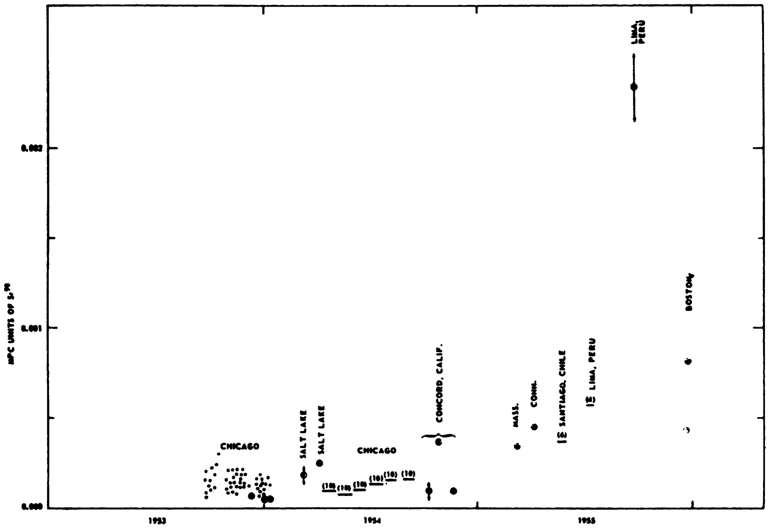

FIG. 2c.—Human stillborns (measurements by Chicago laboratories) (error less than 0.00005, unless otherwise indicated; parenthetical numbers are number of cases used for the average)

After the Castle test series was completed, there were about 24 megatons of fission in the stratosphere, corresponding to about 12 mc/mi^2 of Sr^{90} on the average and about the same amount of Cs^{137}. The subsequent stratospheric world-wide fallout rate appears to have been a little over 1 mc/mi^2/yr all over the world. In addition, of course, local fallout and tropospheric latitudinally localized world-wide fallout have occurred from subsequent weapons tests by the Russians and the Redwing series completed last summer at the Eniwetok Proving Grounds.

II. RESEARCH FINDINGS

A. Amount of Fallout and Expected Sr^{90} Body Burden from Weapons Fired to Date.—Since our last report,[2] further data on the actual magnitude of fallout in various places in the world and in various selected spots in the food chain have be-

come available. We shall summarize the results. Some human bone now contains radioactive strontium at levels of about one-thousandth of maximum permissible concentration (0.001 MPC; the MPC is 1 microcurie of Sr^{90} for the standard man and proportionally less for children, and is the maximum permissible concentration) in the northern latitudes where the bombs have been fired and the world-wide tropospheric fallout has occurred. There is evidence in the data on human material that age is a factor, e.g., older people who have had their calcium deposited

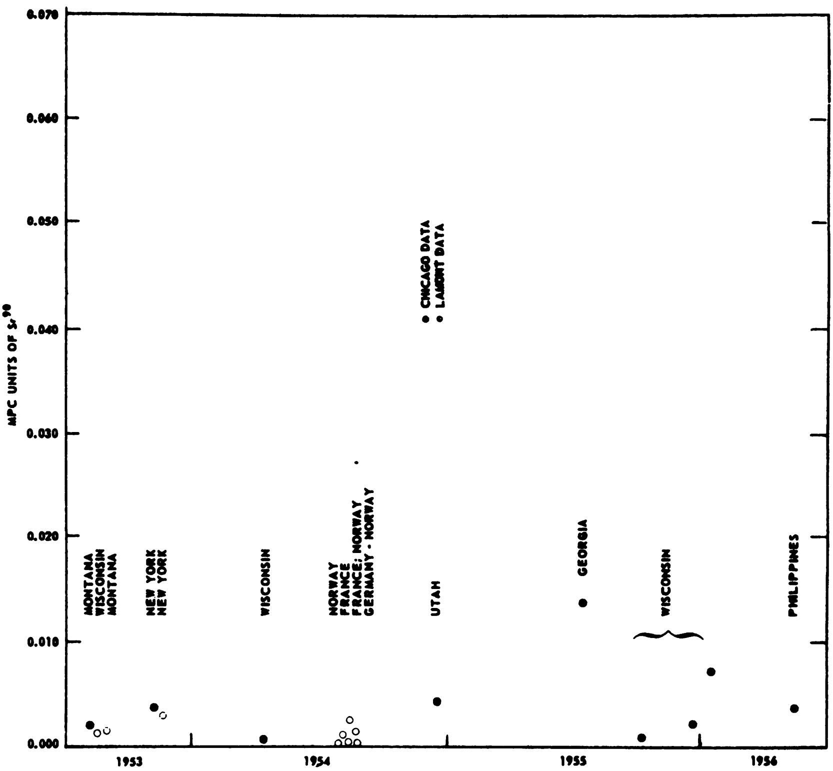

Fig. 3a.—Calf bones

prior to the weapons tests show lower concentrations, though in some instances exceptions to this rule are to be found. Lower levels are found in the Southern Hemisphere, where the major contamination is due solely to the world-wide stratospheric fallout, which in the northern latitudes of 10°–50° N. is generally less than one-half or one-quarter of the total fallout. The deposition in the human body seems roughly to parallel the levels of the total fallout. More data are necessary to validate this point fully.

At the end of 1955 the total deposition in the upper Midwest of the United States was some 13 mc/mi² of Sr^{90}. In the spring of 1956 this total rose to about 16 mc/mi².

Between May 5 and mid-July of this year, Operation Redwing was conducted at the Eniwetok Proving Grounds in the Pacific. Particular attention was paid to the fallout problem in this operation, and a major effect was made to produce a megaton-range weapon with an inherently smaller amount of fallout for a given energy release. This effort was successful. In addition, considerable attention

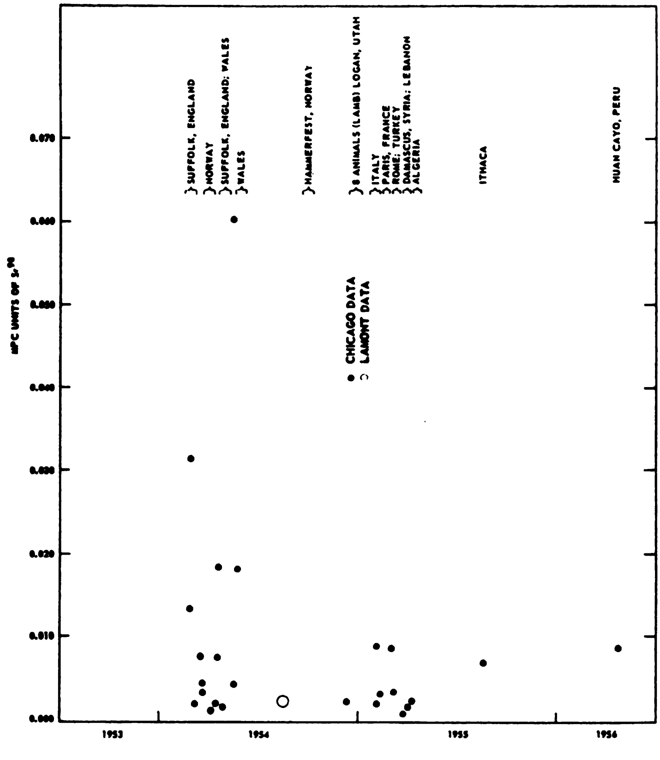

FIG. 3b.—Sheep bones

was paid to operational factors which would minimize world-wide fallout. Thus the total deposition in the stratosphere during this operation was held to a figure very considerably less than that present in the stratosphere before the operation. In fact, we estimate at the present time that the total stratospheric reservoir, counting all sources, is about the same as it was two years ago, i.e., 12 mc/mi² or Sr^{90}, or the equivalent of 24 megatons of fission, calculated as a uniform world-wide distribution. During the last two years the additional depositions in the stratosphere have amounted to about 6 megatons equivalent of fission products total, or 3 mc/mi² of Sr^{90} or Cs^{137}. This appears to have compensated approximately for the 10 per

cent per year of fallout and the 2.5 per cent per year of radioactive decay. In other words, the testing by all countries seems to have restored the stratospheric reservoir to approximately the 24-megaton value of two years ago.

The latitudinal tropospheric world-wide fallout, which is maximized by weapons of high yield which do not puncture into the stratosphere, is increasing. Several such weapons have been air-fired abroad in the last months. This material, for the reasons explained above, descends rather rapidly but all the way around the world in the same general latitude as the firing site. Thus, though it is difficult to estimate, it appears that this amounts to perhaps 5 additional mc/mi^2 of Sr^{90} and Cs^{137} in the United States. Adding to this about 1/2 mc/mi^2 for the world-wide tropospheric fallout from Operation Redwing and 1 mc/mi^2 for stratospheric fallout, we would estimate at present that a total of about 22 mc/mi^2 or Sr^{90} is to be found in the soils of the midwestern United States and that perhaps 15–17 mc/mi^2 is the total to be expected for similar latitudes elsewhere in the world, the difference being due to our proximity to our own weapons testing site in Nevada. These 22 mc/mi^2 of Sr^{90} in the soil of the United States amount to about 0.040 MPC units in the top two inches of soil, where most of the fallout is absorbed.

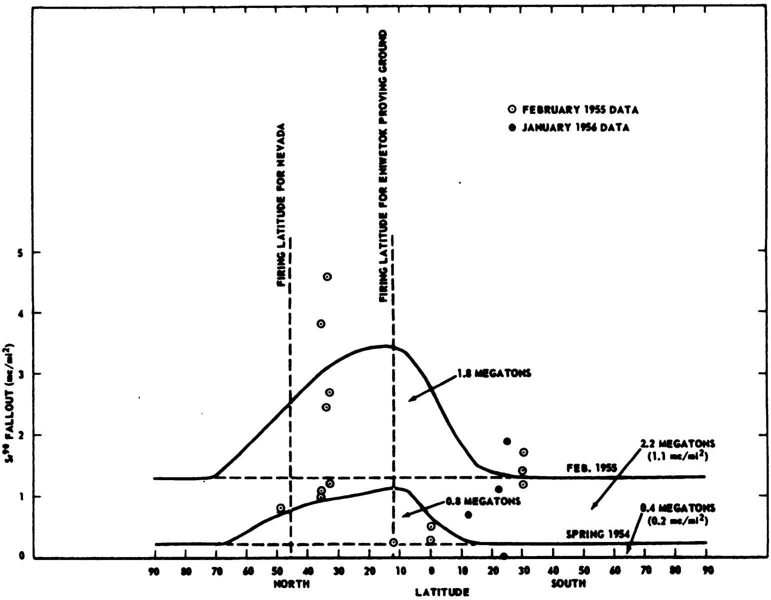

FIG. 4.—World-wide fallout, Sr^{90}, spring, 1955. Latitudinal distribution of foreign post-Castle Sr^{90} fallout (soil assays).

As of the present time, considering the latest human-bone and milkshed data, both domestic and foreign, together with the total fallout figures for corresponding periods, we find that the level of somewhat less than 0.001 MPC units now found in the bones of young children is to be compared with a total Sr^{90} fallout in the soil of about 12 times higher concentration. Additionally, laboratory data have shown that there is a threefold discrimination against strontium as compared to calcium in the assimilation by plants from the soil and that a further factor exists of about

eightfold discrimination against strontium relative to calcium in the excretion of strontium in milk as compared to the cow feed. Earlier,[2] it seemed reasonable to conclude that the human-body burden of Sr^{90} might well be as high as 70 per cent of the concentration in the top soil on which people live. The further evidence just

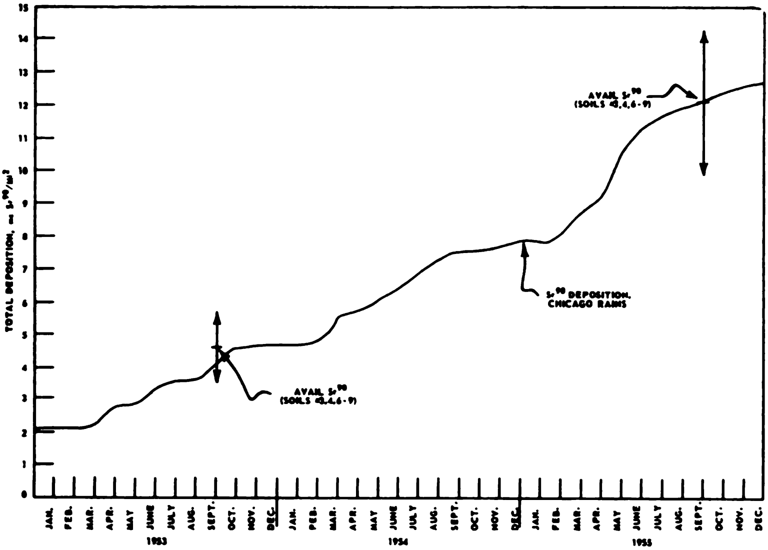

FIG. 5.—Sr^{90} fallout history, Chicago milkshed area

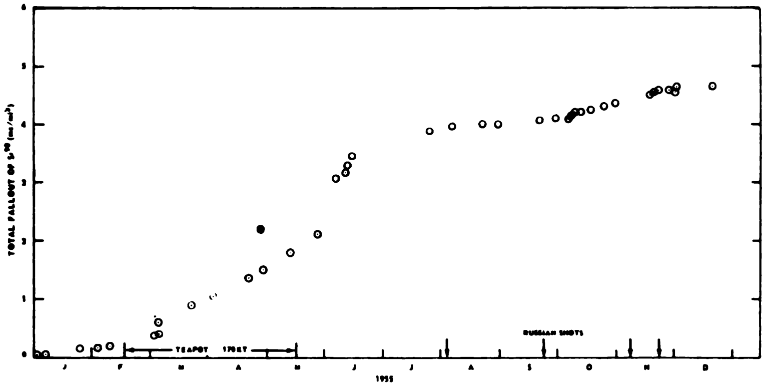

FIG. 6.—Total Sr^{90} fallout in Chicago in 1955 (Pot collection)

cited seems to indicate that this figure is much too high and possibly should be reduced to about 10 per cent. A strict application of the two discrimination factors described would give 4 per cent. Leaf retention of fallout which bypasses the soil causes the figure to be higher. Therefore, at the moment, we would expect that the body burden for children born now in America eventually would amount to be-

tween 0.004 MPC units, corresponding to 10 per cent of the top-soil concentration, and possibly a figure two or three times higher. The stratospheric deposition would be expected to continue at the expected rate, which at the present is about 1.2 mc/yr, so that some fifteen years from now, in the early 1970's a maximum additional total stratospheric fallout of about 6 mc/mi^2 will have occurred. In the meantime, the present 22 mc/mi^2 would have been reduced to 15 by radioactive decay, just about compensating for the stratospheric deposition. Thus the conclusion that the body burden in the United States from weapons fired to date would be about 0.004 MPC units, or possibly as high as 0.010 MPC units, seems justified. This level probably will not be exceeded in other countries unless particular factors of environment intervene, since the United States probably has the highest total fallout in the world. It seems very unlikely, however, that environmental factors could increase the level over the United States by more than a factor of 2 or 3.

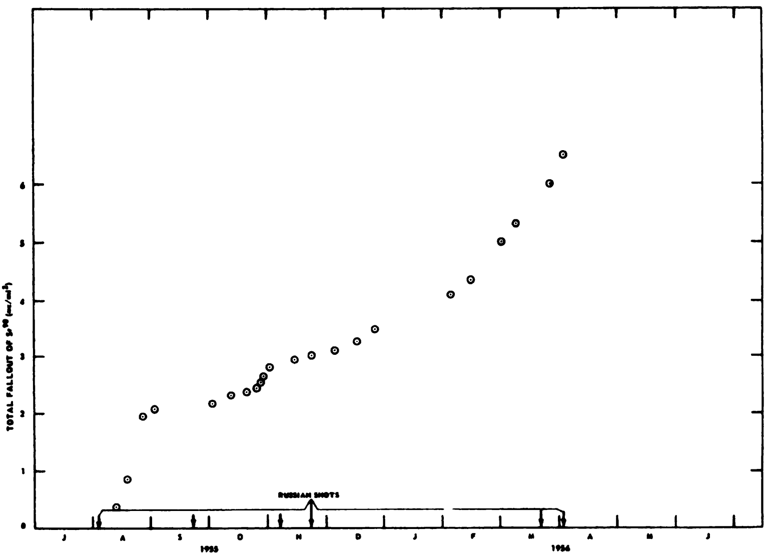

FIG. 7.—Total Sr^{90} fallout in Pittsburgh in 1955–1956 (Pot collection)

B. *Effect of Rainfall.*—As mentioned earlier, there is excellent reason to suppose that the deposition from the troposphere on the earth's surface is best accomplished by rain. By rain is meant not heavy rain but anything which involves the settling of water droplets. This might include fog or mist. The suggestion has been made[8] that the small size of the stratospheric fallout particles gives them a very high mobility due to molecular motion, since, in fact, they probably are almost molecular in dimensions. This high mobility of the particles makes it probable that direct contact of the fallout particles with water droplets will occur. One imagines on this theory that the tiny particles pass through the tropopause from the stratosphere and then meet water droplets in a cloud or mist or rain in the course of their rapid random motion due to collisions with the air molecules. Thus,

rather than the classical Langmuir mechanism of the rain sweeping out the air through which it falls by colliding with the particles themselves, the particles probably collide with the water droplets either before or during the rainstorm, probably most importantly before. It is clear from this mechanism that fog and mist may well be very effective and that a cloud probably gathers a considerable fraction of the fallout from the air in its bulk.

In any case, some experimental evidence has been found for the effect of rainfall on fallout by studying three particularly arid regions: the Imperial Valley in California at the town of Brawley and the western coast of South America at Antofagasta, Chile, and Lima, Peru. The soil in Brawley was sampled in January of this year and found to contain less than 0.6 mc/mi^2 of Sr^{90}. In order to realize the significance of this number, one should recall that we would have expected about 13 mc/mi^2 as an average figure for the United States. It is true that a considerable part of this is from the Nevada tests—the depositions of which occur mainly in an easterly direction and might well miss southern California—but it certainly seems that at least 8 mc/mi^2 would have been expected in the Imperial Valley under normal conditions such as prevail elsewhere in the United States and in Europe and Asia. Thus the observed fallout is not over a few per cent of that expected normally.

TABLE 1

BIOSPHERE Sr^{90} ASSAY, WISCONSIN MILKSHED, SEPTEMBER 30, 1955
(0.001 MPC Units unless Otherwise Stated)

Farm	Soil 0"–2"	Soil 2"–6"	Total Sr^{90} (Mc/Mi^2) 1955 Value	Total Sr^{90} (Mc/Mi^2) 1953 Value	Alfalfa 1955 Value	Alfalfa 1953 Value
Holcomb, Wisconsin	26.7 ± 1.0	3.8 ± 0.14	14.8	5.1	19.2 ± 1.0	8.3
Premo, Wisconsin*	15.0 ± 0.5 (0"–6")		10.6	3.8	25.5 ± 1.3	4.1
Kurpeski, Illinois*	12.2 ± 0.4 (0"–6.5")		10.4	4.0	7.05 ± 0.33	7.4
Austin, Illinois	49.9 ± 1.3	9.6 ± 0.4	16.5	4.7	38.0 ± 2.0	5.0
McKee, Illinois	9.8 ± 0.4	0.99 ± 0.04	10.6	6.3	30.5 ± 1.7	14.8
Van Winkle, Illinois	65.1 ± 2.6	10.0 ± 0.4	9.4	3.8	5.76 ± 0.29	5.0
Carver, Illinois	64.5 ± 1.3	14.0 ± 0.7	8.9	3.3	2.73 ± 0.18	2.3
	Average Sr^{90} (mc/mi^2)		12.0	4.5		

* Had been plowed to 6" depth.

In addition to the soil samples, tests were made on the vegetation grown at Brawley, California, as well. As expected, it was found that the level was lower. Lettuce samples collected from this region at the same time as the soil samples showed 0.0004 MPC; broccoli, 0.00025 MPC; green peas, 0.00134 MPC; alfalfa, 0.0021 MPC. These values all are much lower than those for the midwestern United States (shown in Table 2).

The rainfall data for Brawley are as follows: In 1955 the rainfall totaled 1.70 inches, 1.3 inches having occurred in January of that year and 9 months having had no registered rainfall at all. In 1953 the annual total rainfall was only a trace, this trace having occurred in February.

At Antofagasta, Chile, where it has never been known to rain, except possibly on one occasion, we find 0.02 mc/mi^2 of Sr^{90} in January, 1956, when the general deposition for this latitude was apparently a little over 2 mc/mi.^{2} In other words

about 1 per cent of the fallout expected was found, and 0.02 is hardly larger than the experimental error of measurement.

TABLE 2

1955 Domestic Soil Samples

Location	Lab No.	Date Sample Taken	Depth of Sample (Inches)	Ca Extracted (Electrodialysis or NH_4Ac)	Calc. Ca (Gm/Ft^2)	Sr^{90} Content (0.001 MPC)	Total Sr^{90} (Mc/Mi^2)	
Winnebago Co., Ill., Swanson Farm, Site No. 3, Carrington silt loam	551503	9/30/55	0–8	NH_4Ac	82.8	6.83 ± 0.08	9.8	
Rock Co., Wis., Holcomb Farm, Site No. 4, Carrington silt loam	551500	9/30/55	0–2	NH_4Ac	15.0	26.7 ± 1.0	11.4	14.8
Rock Co., Wis., Holcomb Farm, site No. 4, Carrington silt loam	551501	9/30/55	2–6	NH_4Ac	31.8	3.81 ± 0.14	3.4	
Columbia Co., Wis. Premo Farm, Site No. 6, Miami silt loam	551502	9/30/55	0–6	NH_4Ac	25.5	15.0 ± 0.5	10.6	
McHenry Co., Ill., Kurpeski Farm, Site, No. 7	551496	9/30/55	0–6.5	NH_4Ac	30.9	12.2 ± 0.4	10.4	
McHenry Co., Ill., Austin Farm, Site No. 8, Miami silt loam	551504	9/30/55	0–2	NH_4Ac	6.98	49.9 ± 1.3	12.0	16.5
McHenry Co., Ill., Austin Farm, Site No. 8, Miami silt loam	551505	9/30/55	2–6	NH_4Ac	7.8	9.6 ± 0.4	4.5	
McHenry Co., Ill., McKee Farm, Site No. 9, Drummer silt-clay loam	551448	9/30/55	0–2	NH_4Ac	31.2	9.8 ± 0.4	8.5	10.6
McHenry Co., Ill., McKee Farm, Site No. 9, Drummer silt-clay loam	551499	9/30/55	2–6	NH_4Ac	78.4	0.99 ± 0.04	2.1	
Will Co., Ill., Van Winkle Farm, Site No. 11, Plainfield sand	551508	9/30/55	0–2	NH_4Ac	4.4	65.1 ± 2.6	8.0	9.4
Will Co., Ill., Van Winkle Farm, Site No. 11, Plainfield sand	551509	9/30/55	2–6	NH_4Ac	5.1	10.0 ± 0.4	1.4	
Will Co., Ill., Carver Farm, Site No. 12, Plainfield sand	551510	9/30/55	0–2	NH_4Ac	3.7	64.5 ± 1.3	6.7	8.9
Will Co., Ill., Carver Farm, Site No. 12, Plainfield sand	551511	9/30/55	2–6	NH_4Ac	5.6	14.0 ± 0.7	2.2	
Brawley, Calif. (in Imperial Valley)	56316	1/5/56	0–6	Electrodialysis	54.9	≤0.4	≤0.6	

Annual rainfall = 2.57 inches

In Lima, Peru, the total fallout of Sr^{90} in January, 1956, was 0.7 mc/mi^2. The annual precipitation in Lima averages only 1.89 inches, though there is a considerable amount of ground fog and mist.

It seems clear from these results and the reasonableness of the mechanism of deposition advanced by Mr. Greenfield that there is valid reason to believe that world-wide fallout is small in the absence of precipitation. Also, it is clear from this mechanism that the fallout should not be strictly proportional to total rainfall. Frequent light rains or mists would be expected to be more efficient than occasional heavy rains. The importance of rain is only to be revealed by a study of desert areas and a careful investigation of the scavenging mechanism itself. This work may well prove to be of considerable importance in meteorology, as well as in fallout studies. One should note that the local fallout due to larger particles which descend in the first hours probably does not need rain to precipitate it and occurs in the absence of the precipitation of moisture, although rain may well be able to increase even this fallout.

TABLE 3

Sr^{90} CONTENT OF FOREIGN SOILS AFTER CASTLE

Lat./Long.	Location	Lab. No.	Date Sample Taken	Depth of Sample (Inches)	Sunshine Units	Calc. Exch. Ca (gm/Ft²) (Ammonium Acetate)	Calc. Exch. Ca (gm/Ft²) (Electrodialysis)	Total Sr^{90} (Mc/Mi²)
33° N/36° E.	Damascus, Syria	55590	2/55	0–4	1.40 ± 0.10	62.8	31.7	1.2
49° N/2° E.	Paris, France	55614	2/55	0–4	0.69 ± 0.05	66.2	41.6	0.8
36° N/139° E.	Tokyo, Japan	55644	2/55	0–4	5.86 ± 0.29	18.9	6.1	1.0
0°/27° W.	Dakar, French W. Africa	55645	2/55	0–4	3.71 ± 0.14	4.4	4.5	0.5
0°/27° W.	Dakar, French W. Africa	55646	2/55	0–4	9.31 ± 0.74	1.3	1.1	0.3
36° N/2° E.	Algiers, Algeria	55647	2/55	0–4	1.20 ± 0.08	60.0	32.6	1.1
36° N/2° E.	Algiers, Algeria	55648	2/55	0–4	2.90 ± 0.20	53.8	47.1	3.8
25° S/57° W.	Asunción, Paraguay	56450	1/56	0–6	11.3 ± 0.75	6.1	6.2	2.0
22° S/46° W.	São Paulo, Brazil	56448	1/56	0–6	3.04 ± 0.27	21.0	14.2	1.2
30° S/30° E.	Durban, Natal, S. Africa	55777	2/55	0–4	4.43 ± 0.19	12.6	13.7	1.7
30° S/30° E.	Durban, Natal, S. Africa	55778	2/55	0–4	15.0 ± 0.08	3.3	3.4	1.4
12° N/45° E.	Aden, Saudi Arabia*	55787	2/55	0–4	0.69 ± 0.09	107.2	12.5	0.24
33° N/40° E.	Ankara, Turkey	55878	2/55	0–4	1.9	96.6	51.6	2.7
34° N/36° E.	Beirut, Lebanon	55591	2/55	0–4	3.2	63.9	52.6	4.6
34° N/36° E.	Terbol, Lebanon	55592	2/55	0–4	1.8	110.7	46.9	2.4
24° S/70° W.	Antofagasta, Chile	56447	1/56	0–½	0.44 ± 0.04	5.2	1.7	0.02
12° S/80° W.	Lima, Peru	56456	1/56	0–6	0.60 ± 0.04	63.9	42.7	0.7
30° S/115° E.	Perth, Australia	55839	2/55	0–4	14.7 ± 1.1	2.8	3.0	1.2

* Exchangeable Ca by isotopic exchange method for this sample was 19.0 gm/ft².

The importance of precipitation as a scavenging mechanism raises the possibility that different regions will be subjected to varying intensities of fallout, depending upon the weather conditions. It will be important to test whether this is so and whether it is a major effect in populated areas. We have evidence showing that extreme aridity greatly reduces the long-range or world-wide fallout, as explained above. The evidence to date does not indicate that it is a major effect for normal climates, in the sense that it does not appear to amount to more than a factor of 2. Regions in which people live normally have enough precipitation so that differences in precipitation appear not to affect the fallout by more than such a factor. Careful study of the data appended and the previously released data[2, 4, 5]

have failed to reveal any more serious deviation. To summarize, desert regions with little or no precipitation, or with only very minimum precipitation, apparently have minimum long-range, world-wide fallout; but other regions do not show that the fallout is proportional to the total precipitation, nor should it be expected to be so; however, detailed conditions related to frequency of precipitation might well be important. The data to date do not reveal deviations from the general average by more than about a factor of 2, and, in fact, they seem to indicate a smaller deviation than this. There is some evidence that certain areas have had more fallout than one might expect on the model described above and on previous occasions. In particular, there are reports that certain areas in England show higher levels, but the deviations appear to be considerably less than twofold.

More important, probably, than the variations in total fallout due to weather conditions is the effect of calcium in the soil in reducing the rate of assimilation of radioactive strontium by plants. The plants assimilate strontium because it is chemically similar to calcium and, since their appetite for calcium is limited, a larger calcium content in the soil dilutes the strontium so that a smaller fraction is assimilated. This effect might amount to a factor of 5 in the human-body assimilation of radiostrontium in regions with very low calcium content in the top soil.

C. *Direct Measurement of the Stratospheric Fallout Content.*—Direct measurement by means of high-flying balloons has shown that the stratosphere does indeed have about the fallout anticipated in it.[6] In addition, it has been found in these measurements that the radiocesium, Cs^{137}, occurs at about the same level as Sr^{90} in millicurie units, indicating that there has been no serious fractionation of the two fission products by the fallout mechanism. It further points out that the sampling of the stratosphere is a practical matter and that measurements can be made of the stratospheric content of radioactive fallout. Such data should greatly assist the whole study of fallout.

D. *Radiocesium Assays for the Biosphere.*—Recently a technique for measuring Cs^{137} in biosphere samples, particularly in human bodies, has been developed by Mr. L. D. Marinelli, of the Argonne National Laboratory.[7] It has been found that about four-millionths of one millicurie is present in an average adult. This corresponds rather well to the expected amount, considering the short residence time of radiocesium in the body, which is about 3 months, and the expected precipitation rate, which is taken to be equal, as a first approximation, to that of radiostrontium. The radiocesium, of course, constitutes no hazard, amounting in radiation dosage to a small fraction of the amount present in the blood in the form of that received from the ordinary potassium present in the body. Potassium is naturally radioactive. It is interesting further evidence, however, that the general model of fallout set forth is consistent with the data and essentially correct.

III. PLANS

It is clear that the peoples of the world are extremely interested in radioactive fallout because of the bearing that the new phenomenology of the nuclear age has on everyone's life. For this reason we must understand radioactive fallout in all its intricacies. It is to be hoped that the study will be a co-operative, international one. The United Nations Scientific Committee on Effects of Atomic Radiation offers an ideal forum for the discussion and consideration of the problem. From

these deliberations will come further suggestions, ideas, appraisals, and statement of the problem. The methods developed in this country for measurement and all the data collected are available to everyone. It is our hope and intention that this problem, like others of the atomic age, will come to be generally understood.

Fallout is normally considered an aspect of atomic warfare and nuclear armament. There is some similarity, however, between the weapons fallout and the hazard from a reactor accident, in which radioactive products would be disseminated over a limited area but would never reach the stratosphere or undergo anything like the world-wide tropospheric dissemination. As it has so often been observed in the past, so it is again true in this instance, that a new fact of nature is likely to have its beneficent as well as its somber and frightening aspects. As we learn about the way the world-wide fallout particle, probably as tiny as a virus molecule, wends its way from the stratosphere through the tropopause into the troposphere and, within a few weeks, collides with a water droplet and thus is brought to the earth's surface by rain, we shall learn more about the circulation of the atmosphere, about the way in which rain is formed, and about the questions which will naturally arise more and more frequently as the world's population increases world-wide pollution of the atmosphere not only with fission products but with the other by-products of our new technological age.

[1] W. W. Kellogg, R. R. Rapp, and S. M. Greenfield, "Close-in Fallout," P-822-AEC, March 12, 1956.

[2] W. F. Libby, "Radioactive Strontium Fallout," these PROCEEDINGS, 42, 365, 1956.

[3] Stanley Greenfield, Rand Corporation.

[4] John H. Harley, Edward P. Hardy, Jr., George A. Welford, Ira B. Whitney, and Merril Eisenbud, "Summary of Analytical Results from the HASL Strontium Program to June 1956," NYO-4751, August 31, 1956.

[5] Project Sunshine Bulletins Nos. 11 and 12, Enrico Fermi Institute for Nuclear Studies University of Chicago, December 1, 1955, and August 1, 1956.

[6] A. P. Hardy and S. Tarras, "General Mills High Altitude Balloon Filter Samples," Memorandum, New York Operations Office, July 2, 1956.

[7] "Gamma-Ray Activity of Contemporary Man," *Science*, 124, 122, July 20, 1956.

ISOTOPES IN METEOROLOGY

Remarks Prepared by Dr. Willard F. Libby, Commissioner United States Atomic Energy Commission, for delivery before the American Meteorological Society Chicago, Ill., Wednesday, March 20, 1957

A. STRATOSPHERIC MIXING

The existence of winds in the stratosphere together with the long residence times for fine particulates revealed by the fallout studies, make it seem possible that worldwide mixing may occur in the stratosphere in contrast to the troposphere. To establish or disprove this conclusion definitely and to measure the mixing rates would be possible with various radioactive isotopic tracers. The most immediately available are the bomb fallout products, in particular Sr90 and Cs137. This program is underway as Project Sunshine principally, with fallout collections being made by soil sampling and open pots at various places in both hemispheres as well as by means of a program of direct stratospheric sampling with air filters. From this work our present indications are that stratospheric mixing occurs in one or two years and that the over-all storage times for both Sr90 and Cs137 are both about ten years.

These conclusions will either become firmer or else be replaced by a more complicated mechanism corresponding to incomplete stratospheric mixing as Project Sunshine proceeds into its fifth year, and we probably will not be too optimistic if we expect that this study will definitely establish the mixing rate (and possibly the pattern), the storage time, and the general applicability of the single residence time to all stratospheric components which are not subject to commanding and dominating gravity fall. In other words, Sunshine may well teach us whether the whole concept of rapid stratospheric mixing relative to rates of removal into the troposphere is valid and whether these removal rates are the same for gases and particulate matter too fine to fall at any important rate, i. e., below 1 micron. This program will be prosecuted because of its commanding importance for fallout and meteorologists can expect that the help they may get may be appreciable.

It probably would be well to anticipate certain of these results so as to begin planning future experiments on this basis. For example, certain samples can be collected during the International Geophysical Year which would be important if the tentative conclusions that the stratospheric storage time is about ten years and the stratospheric mixing is essentially complete in two years, and that both apply generally to non-falling constituents, all are correct.

There are various types of particulate substances which may originate in the stratosphere and which should, therefore, be constantly fed into rainfall by the slow stratospheric feeding mechanism, whatever it be. Among these is meteoric dust. The fine meteoric matter which is too small to fall by gravity, i. e., O.1 micron or smaller, should blow about in the stratosphere establishing a more or less uniform concentration and finally descend through the tropopause and be precipitated by rain just as in the case of radioactive fallout. It might be wise to take filter samples of stratospheric air to establish the concentration of this material and if some technique for distinguishing this dust from ordinary dust can be devised, it would be interesting to establish the concentration in ordinary rain. It seems likely that the amount is adequate for detection in view of the quantities of meteoric material found in deep sea sediments.

Another type of material which may be descending from the stratosphere is the dust thrown into the stratosphere by megaton sized nuclear explosions. This material probably can be distinguished from meteoric dust by its chemical properties. For example, material from the South Pacific is mainly either calcium carbonate, calcium hydroxide, or sodium chloride, the various solid substances other than ice and, of course, the bomb materials themselves which are derivable from the surface material which could contribute to the fireballs.

It has been suggested recently by Dr. Philip Abelson that the type of mechanism described by Professor Urey and Dr. Stanley Miller might generate amino acids in the atmosphere by the photochemical action of the ultraviolet light from the sun in generating free radicals which could react to form these all important molecules. If this be true, it may be that rain contains amino acids from this source. It, of course, will be difficult to distinguish these molecules from earth dust and the amino acids from the ordinary living material. It may be possible to do this by using the fact that the ordinary amino acids are optically active whereas the postulated stratospheric materials presumably

would not be and thus could be distinguished in principal, at least. Of course, direct stratospheric samples should reveal these materials in much less contaminated form so if search in rain samples fails to reveal them stratospheric air samples may.

Natural tritium produced by the cosmic rays, or, as recently suggested by Dr. Arnold and Dr. Feld, possibly assimilated directly from the sun, is naturally produced or acquired at the highest rates in the highest layers of the stratosphere. The cosmic ray primary radiations interact with a mean free path of about 1/10 of the atmosphere, so although there is appreciable probability of production below the tropopause, the main part of the cosmic ray interactions with the aid occurs in the stratosphere. Since it is extremely likely that tritium, being an isotope of hydrogen, will burn to water, we essentially have a source of radioactive water in the stratosphere. The recent results from fallout suggesting that the stratospheric storage time for fine particulate matter is of the order of ten years, make it appear likely that a considerable fraction of all the natural tritium resides in the stratosphere, and that, in fact, it may spend a considerable part of its average radioactive life of 18 years there and so disintegrate there in considerable degree to form He^3 before entering the troposphere and the earth's water system. The ratio of the moisture content of the stratosphere to that of the troposphere is so small that if taken together with the stratospheric storage factor mentioned, it leads one to expect that the tritium concentration of stratospheric moisture may well be as much as 100,000-fold above that for ordinary surface water. This ratio of the stratospheric concentration to the tropospheric concentration will, in itself, give a more or less direct measure of the storage time. The sampling of the stratosphere for moisture must, of course, be done with balloons, aircraft or rockets, and air filters which will collect the moisture. It is not necessary apparently to know the volume of the air which is sampled, so the problem is one of the simplest of the stratospheric sampling problems, and it seems that it should be relatively simple to do. It would be important to sample at various latitudes to determine whether the expected uniformity with respect to latitude actually exists. It also would be very valuable to have data versus altitude.

Perhaps a word should be said about the short residence time of the bomb tritium in the stratosphere. Why is it possible that natural tritium have a residence time of perhaps ten years in the stratosphere whereas at least a major part of the bomb tritium, which certainly was pushed into the stratosphere with the bomb cloud, was observed by Dr. Begemann and myself to spend only something like 40 days in the atmosphere? The reason probably is that the bomb tritium is contained in relatively large ice crystals. The amount of water in the fireball is so large that when the fireball is pushed into the stratophere and cooled, relatively large ice crystals form. The proof of this lies in the appearance of the cloud, for the cloud as actually seen in its white mushroom outline in the stratosphere is visible mainly because of ice crystals or supercooled water droplets. In any case, the material which is pushed into the stratosphere is of sufficient mass so that gravity fall will cause it to leave the stratosphere relatively quickly. It is the material which does not settle by gravity that we expect has the ten year residence time. Probably any bomb fired in the troposphere will have enough moisture from the air so that it will form large enough ice crystals to cause the tritium to fall out relatively quickly. The stratospheric water samples need not amount to more than a few grams for the concentration should be high enough so isotopic enrichment would not be required. I am sure that Dr. Begemann would be pleased to measure these samples if anyone can obtain them. We will make efforts on our own part, but it is our hope that flights being made for other purposes can be used to obtain these moisture samples.

B. TROPOSPHERIC MIXING

In contrast to the stratosphere, the tropospheric residence times for particulate matter seem to be relatively short. Work with radon decay products done by Haxel in Germany and by Blifford and Damen and other people in this country, all agree that whatever the fate of the non-volatile radioactive decay products of the four day half-life radioactive noble gas radon emitted by uranium, the materials stay in the atmosphere only a fwe days before being precipitated out, probably mainly with water droplets. This is the same story as for the fallout particles. All the evidence known apparently indicates that there is some type of mechanism by which troposheric air is periodically cleansed in a matter of a few days, or at most a month or so.

It has been known for a long time that the rain and snow are good scavengers for atmospheric contaminants, and the clearing of the atmosphere following precipitation is recognized by everyone as a fact. It is less well known that this holds true even for the finest material. The new evidence for this is that the fine material which resides in the stratosphere for periods of years is, in fact, carried down by water droplets when it enters the troposphere in a relatively short time. This agrees with the radon decay products evidence for it was shown by Werkman and his coworkers that the particles which collect the radon decay products are in general microscopic, probably of the order of a few hundred angstroms in diameter. What can this mechanism be?

It is very well known that a falling raindrop can hardly pick up any particle so small that its inertia will not prevent its flowing around the raindrop as the air molecules do. Therefore, it has been something of a mystery as to how the fine particulate matter which carries the world-wide fallout in the stratosphere could be precipitated in the form of rain, as was shown rather conclusively to be the case by the fact that fallout is observed to be minimal in desert regions (though it is by no means proportional to total rainfall in other areas). The answer seems to have been given by Dr. Stanley Greenfield, who pointed out that the Brownian motion, that is, the violent random motion due to collisions with the air colecules, gives these tiny particles a considerable probability of colliding with the droplets in clouds. Therefore, he suggests that any cloud, mist or dew is an excellent medium for scavenging fine particulate matter and placing it, of course, in the rain which is formed subsequently from clouds. From this theory, which seems to be so explanatory of much of the information on moisture scavenging of fine particulate matter, we would expect that the efficiency with which fine particles entering the troposphere from the stratosphere are carried down will depend not so much on the total rainfall or total moisture precipitated as on the average lifetime of a particle as determined by the probability of its colliding with a smaller water droplet in a cloud, mist or fog. In other words, a foggy climate, or one given to morning dews, or one with frequent light rains, may well be more effective in this precipitation mechanism than one with infrequent heavy rains. The fallout evidence seems to agree with this, but further work with the Greenfield theory in mind ought to establish the matter clearly.

Of considerable more importance for meteorology, it seems likely that nucleation nuclei will in themselves have the same fate that these fine particles we speak of suffer, and that the fallout pattern should be the pattern for the cleansing of the air of stratospheric nucleation nuclei which are so important in the formation of ice crystals. The study of this problem of the history of fine particulate matter in the troposphere ought to be of real value to meteorology.

Thus, we have a model for the complete removal of particulate matter from the troposphere by water drops since the mixing up to the tropopause seems to occur in a matter of weeks, and this residence time for the particulate matter appears to be about the same as the residence time for the average water molecule in the troposphere. It appears that the air in the bottom rain-bearing layer of the troposphere is cleared in about 1 week while the troposphere as a whole requires 3 weeks or a month on the average.

Of course, these results are incomplete and it is necessary that many of the features be tested by further experimentation. For example, we should find whether it is really true that the air in a cloud is clear of fallout, and that the air in a fog is free of condensing nuclei and of virus molecules and all other forms of particulate matter which are fine enough for the Greenfield mechanism to operate. Some of these experiments could well be conducted in the course of the International Geophysical Year.

In contrast to the stratosphere, it seems that the short residence time in the troposphere makes latitudinal mixing essentially impossible. Thus, we find that fallout which never reaches the stratosphere or which falls out of the stratosphere quickly does not spread itself across the equator latitudinally. Most of the fallout from the testing in the Northern Hemisphere being tropospheric in character has occurred in the same general ranges of latitudes as the test sites occupy, and to a rough approximation it is true that all of the fallout in the Southern Hemisphere is due to stratospheric material disseminated by the stratospheric mixing mechanism. Therefore, it should consist of radioactivities which are necessarily old and, therefore, should not contain short-lived radioactive fission products because of the long stratospheric residence time. This point needs experimental testing. It is not a difficult measurement and the results should be available soon.

Insofar as these conclusions and predictions are borne out by further observation, we can expect that particulate contamination of the tropospheric air by industrial sources and others will restrict itself largely to the general region of the heavily populated areas in the Northern Hemisphere, and that any effects of such sources of particulate matter on the local weather should not pass themselves across the equator. It would be extremely interesting to observe whether the density of nucleation nuclei for ice crystal formation is appreciably lower in the Southern Hemisphere, as it might be according to the above notions.

C. MIXING OF THE STRATOSPHERE WITH THE TROPOSPHERE

The mechanism by which stratospheric material eventually finds its way into the troposphere can be elucidated in part by the use of isotopes. For example, if the stratospheric residence time for fine particulate matter and gaseous materials should prove to be identical there would be no doubt any longer that the mixing is one of air masses rather than the falling out of particles or the removal of fine material by some other mechanism. We might for a moment consider a specific mechanism for the removal of matter from the stratosphere into the troposphere and vice versa—a mechanism which consists of a mixing caused by or associated with the seasonal change in the height of the tropopause. This rise and fall of the dividing layer between the troposphere and stratosphere will constitute a type of pumping action. The shifting of the boundary occurs periodically and while the tropopause is near its higher levels, the characteristic rapid mixing of the troposphere takes the air which was formerly in the lower layers of the stratosphere and mixes with what was formerly in the troposphere; and during the subsequent time when the tropopause is at its lowest level, tropospheric air will be mixed similarly with the stratosphere. Now if the oscillation is assumed to occur annually and perhaps one-tenth or one-twentieth of the atmosphere lies between the two extreme locations of the tropopause, it would appear that ten to twenty years would be the expected average time for mixing. This is so reasonable in terms of fallout observation that it may just be that this relatively simple mechanism is the correct one. It was first mentioned to me by Professor James Arnold of Princeton University, but I am sure that meteorologists have thought about it for a long time. This model would predict that the mixing does not occur at exactly the same rate at all points on the earth's surface and the longer residence time in the stratosphere and the magnitude of the stratopheric winds would have to be depended upon to accomplish the latitudinal mixing.

Assuming this mechanism, one could say that the noble gases would establish a steady concentration in the atmosphere which in the case of radioactive materials would be determined by their radioactive half-life and the mixing time, since rain and water cannot scavenge and precipitate them. There is evidence for this already in the case of radon. The ratio of the abundance in the stratosphere to the abundance in the troposphere of the radioactive noble gas radon which has its origin at the earth's surface is known to be essentially zero. By the time the radon can be transported into the stratosphere there is litle if any left because of the four-day half-life for the radioactive decay. Air filters in the stratosphere show little radon decay products. This fact shows that the general model has some applicability to gases.

Gases generated in the stratosphere such as molecular hydrogen containing tritium should establish a concentration ratio between the stratosphere and troposphere which would be determined again by the stratospheric residence time and the radioactive half-life. If, for example, we assume that tritium is generated only in the stratosphere and that the molecular hydrogen has a negligible rate of combustion to water after being formed (an experimental fact in the laboratory unless a spark occurs); one calculates that the ratio of the tritium concentration in the form of molecular hydrogen in tropospheric air to that in the stratosphere should be slightly less than one-half, the half-life for the radiotactive decay of tritium being 12.26 years. This calculation also assumes that the escape of molecular hydrogen from the atmosphere into interplanetary space occurs at a rate which is negligible compared to the rates of combustion and the rates of mixing with the tropospheric air.

A check can be made on the assumption that the rate of combustion of molecular hydrogen in the stratosphere is small compared to the rate of mixing with the tropesphere by observing the ratio of the concentration of molecular hydrogen in the stratospheric air to that in the troposphere. This is so because

it is probably a valid assumption that molecular hydrogen is generated solely in the stratosphere by the action on water vapor of the ultraviolet radiations from the sun. If the combustion lifetime is long compared to the general stratospheric residence time for stratospheric matter, the compositions in the two layers will be the same. This observation would be important to make. It would require the collection of considerable amounts of stratospheric air in order that the hydrogen content could be determined. Similar analyses have been made on the noble gas content of air as a function of altitude.

D. SUMMARY

In conclusion, it seems likely that isotopes can contribute appreciably to meteorology both in the new techniques and data they can furnish as well as in interesting students of other disciplines in the problems of meteorology. Meteorology is such a broad subject that the possibilities for applications of specialized techniques and knowledge are large, ranging all the way from biology to pure astrophysics.

Isotopes have been observed many times to have a catalytic effect in encouraging the mixing of disciplines in science. Physicists and chemists have intermingled for years in the pursuit of nuclear science and the long history of biology is remarkable for the important contributions made by men from other disciplines. If isotopes can help catalyze this intermingling, this service alone will in itself constitute a considerable contribution to meteorology.

We have made only brief references this evening to the cosmic ray radioactivities. The reason is that these have been discussed previously in considerable detail by others, but we should not leave the subject without referring to the fact that in addition to radiocarbon and tritium, two beryllium isotopes and a chlorine, a sulfur, and two phosphorous isotopes all have been reported to be produced by the cosmic rays as they bombard the atmosphere. These give additional opportunities for meteorological observation similar in kind to those discussed in some detail tonight.

There are many other potential applications of isotopes. The possibilities of artificial labeling of air masses by the use of radioactive isotopes are important, and the maximum information has not yet been obtained from fallout material. So many other possibilities exist that meteorologists should earnestly consider the applications of nuclear techniques to their field. In many instances they will find the Atomic Energy Commission very interested in the research.

Remarks prepared by Dr. Willard F. Libby, Commissioner, United States Atomic Energy Commission, for delivery before the University of New Hampshire Distinguished Lecture Series, Durham, N. H., April 11, 1957

Radioactive Fallout From Nuclear Tests

FALLOUT FROM TESTS

There is a great deal we do not know about the precise effect of radiation on the human body, but we do know that the effect of radioactive fallout from nuclear tests is not, nor is it likely ever to be, the danger to the human race in this generation or in later generations which many people have been led to believe.

Long before nuclear weapons were even thought of, in fact, ever since people have lived on this planet, they have been subject to radiation from cosmic rays and from the radioactive material in the crust of the earth. In recent years has been added radiation from the use of X-rays, from luminous devices, etc.

Let us compare radiation from test fallout with radiation from some of the other sources which have been with us through all times so that we may better evaluate fallout as an element of danger. Cosmic rays, which come from outer space, have their radiation effect progressively diluted as they pass through the atmosphere surrounding the earth. Thus, the person living in Denver, Colorado, at an altitude of about 5,000 feet receives a dosage of cosmic rays approaching double that of a person who lives at sea level.

At the present time, the radiation dosage to bone from the most worrysome part of radioactive fallout, which is Strontium-90, is about the same as what

the Denver resident would receive as additional radiation from cosmic rays if he moved from Denver proper about 200 feet up on the mountainside, or to the resident at sea level if he moved from the beach to the top of a hill 300 feet high.

More exactly, and using the measure commonly used for radiation exposure, the roentgen, which is an amount equal in the chest to six to 10 chest X-rays—at sea level in our latitudes we receive 37 thousandths of one roentgen each year from cosmic rays, while people living at Denver, Colorado, at an altitude of 5,000 feet receive 60 thousandths of a roentgen annually. Now, in order to compare fallout we must put all items in the same terms.

FALLOUT AND RADIOACTIVE STRONTIUM

At the present time, the radiation effect on human bones from radioactive strontium, the most worrisome part of radioactive fallout, is about 1.5 thousandths of a roentgen annually—about one-fifteenth of the difference between the cosmic ray dosages at sea level, and at 5,000 feet elevation, or equal to the extra cosmic rays at the extra height of about three hundred feet.

Radioactive strontium does not occur naturally but is produced by nuclear reactions. It also has a natural tendency to stay in our bones and thus to irradiate them. How much does it amount to? As just stated, the most recent data show that children in the United States have about 1.5 thousandths of a roentgen annually of extra bone exposure from the radiation from radioactive strontium. Adults have much less because their bones grow more slowly. This country has somewhat higher rates than elsewhere because of our tests in Nevada.

The effects of irradiation of bone are bone tumor and leukemia. How much hazard does the bomb test radioactive strontium constitute? This is a difficult question to answer with complete certainty for the amounts which cause these effects are not well known, but we do know that the dosage is much less than the extra cosmic ray dosage at 5,000 feet altitude as compared to sea level. We have examined the vital statistics on these dread illnesses to see whether there is a higher rate for populations living at 5,000 feet than for those living at sea level in order to get from these data some direct notion about the magnitude of the effect. There is no evidence of any observable effect of the extra radiation in these numbers. The average annual incidence of bone cancer at both levels was about 2.8 per 100,000 people in 1947, and for leukemia, the average was about 7. The actual numbers were:

Occurrence of bone cancer and leukemia

[New cases per year per 100,000 population]

	Bone cancer	Leukemia
Denver	2.4	6.4
New Orleans	2.8	6.9
San Francisco	2.9	10.3

Figures obtained from National Institutes of Health, Department of Health, Education, and Welfare.

Since excessive doses of radioactive strontium are known with certainty to cause both bone cancer and leukemia in animals, we must not deprecate or casually dismiss the possible results of a widespread, but low intensity, effect in causing these disabilities. However, by using normal experience insofar as it is applicable, we can orient ourselves with respect to this new factor in our environment—much as we do for other results of our modern way of life. We cannot see any effect of a radiation dosage fifteen times the present test fallout dosage in large populations, but the number of cases of bone cancer and leukemia each year, even in the large cities, is small so our data may not be too significant. However, these figures do point to a definite tangible evidence of a margin of safety.

Perhaps more pertinent, though less direct, is the result of animal experimentation and the rare human experience on the effects of radiation in causing bone tumors and leukemia. The official tolerance limit for people working in atomic energy plants and with radioactivity professionally is 2,000 times the present bone content for children in the United States, and about 10,000 times that in

adults. However, it has been recommended by high scientific authorities that the tolerable amounts of radioactive strontium for the population in general should be only one-tenth of those acceptable for workers who of course expose themselves voluntarily. On this basis the present level in children is 1/200 and, in adults, about 1/1000 of this maximum permissible concentration for large groups of people. Perhaps a word of explanation of these tolerance or maximum permissible concentration limits would be helpful. When scientists speak of "risk" or "hazard," they do not use the words in the same sense that most laymen regard them. Scientists try to be precise; they measure such things almost to the limits of the finite; therefore, "risk" means possible effects far beyond the range of the probable or detectable. The maximum permissible limits do not mean that above those limits one encounters trouble—but rather that perhaps only a ten-fold larger concentration would give effects which would be definitely detectable.

WHAT ABOUT THE FALLOUT STILL IN THE STRATOSPHERE?

If all the radioactive fallout which still is airborne should come down suddenly, there would be about one-third more total radioactive strontium on the ground than we now have here in the United States. It falls so slowly, however, that there is expected to be relatively little increase over the present amount deposited—the extra fallout just about compensating for the naural radioactive decay of the radiostrontium already deposited. (The radioactive decay occurs at the rate of 50 percent every 28 years.) The ground level will remain about constant for the next ten years and then drop off at the rate of about 2.5 percent per year due to radioactive decay uncompensated by further fallout from the upper atmosphere. Therefore, in the United States the present level is about as much as we shall ever have from tests already fired.

WHAT ABOUT POSSIBLE GENETIC EFFECTS?

Radiostrontium is not the only fallout product of nuclear bombs. In fact, there are dozens of others but, for one reason or another, these fail to accumulate inside the body as radiostrontium does. However, certain of these other fallout materials do emit penetrating radiation which can irradiate the body from the outside and thus possibly can have effects on the health and can produce genetic mutations. In fact, for early fresh fallout close to the site of a nuclear explosion, this radiation of the body from the outside is the principal hazard, and it is only later on that radiostrontium becomes important. There is one short-lived form of radioactive iodine which is assimilated into the thyroid gland so it is not quite correct to say that no internal irradiation occurs from early fallout, but the external radiation certainly is the main hazard with which the United States Government is concerned in planning protection against enemy nuclear bombs. Later on and far away from the test site, months and years afterwards, there still is some external radiation which does amount to a detectable total and it is this radiation which raises questions.

We are continually being bombarded by radiations not only from the cosmic rays from outer space—the cosmic rays mentioned above—but from the earth and even from our own bodies. Following our previous tactic of comparing these new fallout effects with normal experience insofar as it is justified, we now compare these external radiation exposures with those normally encountered. The external radiation doses from test fallout have averaged between 1 and 5 thousandths of one roentgen per year during the last three or four years. Now, this is to be compared with a normal dosage from ourselves and our environment of 150 thousandths of a roentgen per year. Therefore, we conclude that the radiation dose from test fallout is relatively very small, but the question remains: "Small as it is, does it have genetic and health effects?"

The direct experience which we have had on health effects is reassuring. We do not see that the much larger dosages received by people living in the higher altitudes or in localities which are particularly radioactive have had noticeably bad effects—the effects expected are cancer and shortening of life. However, it may be that small effects do occur which because of their smallness are difficult to see. The only definite thing we can be certain of is that test fallout is very small as compared to natural dosages and, therefore, we know that the effects must not be very far outside of normal experience. One environment can differ from another in natural radiation intensity by much more than the total fallout. For example, a brick or concrete house can easily have enough natural radioactive material in the walls to give up to 40 thousandths of a roentgen per year more exposure than a wooden house—this is between 8 and 40 times the

annual dosage from test fallout. The genetic effects are much more difficult to consider since they will show up only in later generations and, most importantly, only when both parents have been affected either directly or through affected parents. But by study of animals and plants we know that radiation does produce genetic changes—we even irradiate plant seeds in order to speed up the rate at which new forms appear so superior new plants can be produced by selection of the few desirable ones and cultivating them in the way Luther Burbank developed so many useful new plants using just those naturally occurring forms. However, most of the forms from the irradiated seeds have inferior properties and it is only a rare one that is a definite improvement on the original plant. Similar results are found with animals so we guess that human beings probably are subject to same type of effects. Therefore, we believe that there must be *some* genetic effects of test fallout radiation but, again, from our normal experience in which no effects of high altitudes versus low, or brick versus wooden houses, etc., have been observed, we know that the effect must be very small. The laboratory experiments on plants and animals agree with this, but do insist that there must be some very small effect, although it will be entirely undetectable from test fallout.

WHAT ABOUT FUTURE TESTS?

Continued testing at the same rate and in the same way as during the last five years will not increase the hazard on a straight additive basis since an equilibrium will be established between additions of radioactivity and radioactive decay. For Strontium-90, the maximum factor of increase possible is eight-fold and the external radiation exposure outside the immediate test area would behave similarly. In 1980 it would be four times the present and, in 2011, about six times, and so on until after a very long time it would approach the factor of eight.

The cause for real concern is not the deleterious effect of radiation resulting from weapons tests, but rather what would be the effect of the infinitely greater amount of radiation which would result from the massive use of nuclear weapons in warfare. Here we would be dealing with excessive radiation, not to all the people of the world as has been suggested, but quite probably to large numbers of people residing in areas of substantial contamination. With regard to the people so overexposed, there would be serious increases in the pathological effect of excess radiation, such as cancer and leukemia. There would, also, be the genetic effect which would manifest itself in the children and the children's children of such people.

In nature, there are mutations in plants, in animals, in human beings. Some mutations show superior characteristics and these make it possible for us to develop superior strains, but unfortunately most mutations are harmful rather than beneficial. For this reason and because their effect is not limited to one generation, the genetic effects of excessive radiation may be more important than the pathological effects.

If radiation—from weapons test fallout, from natural sources, from the normal use of X-rays—is all measured in quantities so minute as to have very small effects on either present or future generations, we should concentrate our concern on what would happen if the world should engage in a nuclear war, for therein lies the real danger to mankind.

It is not contended that there is no risk, however minute. But all life, and every minute of our day and night, is measured in terms of risk—40,000 highway deaths each year in this country, accidents in the home, etc. We make our choice: How much risk are we willing to take as payment for our pleasures (swimming at the seashore, for example), our comfort or our material progress? Here our choice seems much clearer. Are we willing to take this very small and rigidly controlled risk, or would we prefer to run the risk of annihilation which might result if we surrendered the weapons which are so essential to our freedom and our actual survival.

RADIOACTIVE FALLOUT

Remarks prepared by Dr. Willard F. Libby, Commissioner, United States Atomic Energy Commission, for delivery before the spring meeting of the American Physical Society, Washington, D. C., April 26, 1957

I. INTRODUCTION

The radioactivity produced by the fission reaction changes its characteristics continuously and rapidly following the explosion of an atomic weapon and the

conditions of firing are of extreme importance in determining the rate at which the radioactivity descends to earth. As a result there are in general three different kinds of radioactive fallout, the relative importance of which is determined by the nature of the weapon, principally its yield, and the conditions of firing. These three types are: First, *the local fallout*, which is insignificant unless the fireball touches or comes close to the ground, but which in case the fireball does touch the ground can amount to a major fraction, in some instances as much as 80 percent of the total debris. This type of fallout consists of radioactivity which is carried down by the larger particles. It consists largely of matter drawn up into the fireball from the surface which is either totally or partially vaporized. Under these conditions so much matter is vaporized by virtue of the fireball's touching the ground that the particle sizes formed in the freshly cooled vapor are large.

The second and third types of radioactive fallout are world-wide in nature and consist of finer material and are divided according to whether the material happens to lie in the lower part of the atmosphere, the troposphere, where rain and weather phenomena occur, or the higher part of the atmosphere, the stratosphere, which is free of such precipitating mechanisms. The *tropospheric fallout* occurs in a matter of two or three weeks or a month or so. It occurs largely as a result of rain and snow, and water precipitation in general, and falls in the general latitude of the test site. The *stratospheric fallout*, in contrast, takes years. We are not completely certain, but it appears that an average time of something like ten years, or perhaps somewhat less, is a reasonable figure, and during this time the distribution becomes nearly world-wide. When the stratospheric fallout manages finally to pass into the troposphere it is quickly removed by the same type of mechanism that brings down the world-wide tropospheric fallout, namely rain and moisture.

The precipitating mechanisms consist in general of the collision of the tiny particles with moisture droplets in clouds, together with the interception of particles by falling raindrops. The first mechanism was recently suggested by Dr. Greenfield in connection with Sunshine problems—the study of world-wide fallout is called Project Sunshine. In addition to the scavenging action of rains and fogs, there is definite evidence for a considerable probability of pick-up on direct contact of air with surfaces such as the leaves of grass and trees. Frequently, grasses are found to have higher strontium-90 content than would correspond to the soils in which they grow, and this is due undoubtedly to direct pick-up.

The dissemination of strontium-90 and all fallout is greatly dependent upon the firing conditions. There is every evidence that important factors include not only contact of the fireball with the surface, but the nature of the surface, whether it be land or water and the type of soil and the composition of the water, whether fresh or sea water. Also, the height to which the fireball rises is important, in particular the height relative to the tropopause, the dividing layer between the troposphere and the stratosphere. Yield is the main consideration here. A rough rule is that megaton weapons push through the tropopause into the stratosphere, and kiloton weapons stay below the tropopause in the troposphere.

Thus, we see immediately that kiloton weapons deposit their fission products much more quickly than do megaton weapons. Of course this is of less importance in so far as the long-lived fission products, such as strontium–90 and cesium–137, are concerned, but it is of more importance for the shorter-lived fission products. As a general rule, an air-fired kiloton weapon will deposit its radioactive fallout in a period of between two weeks and one month on the average after the detonation, whereas an air-fired megaton weapon will deposit its radioactive fallout over many years—on the average about ten years. Thus, the effects which are due to the short-lived fission products are larger for a given amount of fission energy release in kiloton weapons than they are for air-fired megaton weapons. Considering the average age of the kiloton fission products to be 1 month, the external gamma ray exposure from one megaton of fission fired as say 50 bombs of 20 kilotons each would be 30 times that for a single bomb giving one megaton of fission energy—if both were fired well up in the air. The fission products from the small bombs fired in Nevada would fall in the latitudes 10°N to 60°N in about one month, while the larger bomb

would give fallout over essentially the whole earth in about 10 years. For strontium-90 effects there is relatively little difference per unit fission yield since even to the residence time in the stratosphere is small compared to the 28 year half-life of radioactive strontium and the 27 year half-life of radioactive cesium, which is produced at slightly higher yield than strontium-90, and which appears to be disseminated in about the same way.

The content of radiostrontium and radiocesium in the stratosphere is by direct measurement shown to be roughly the same though the radiocesium is somewhat higher possibly due to the slightly higher fission yield. The content of radiocesium in rainwater is comparable to that of strontium-90. Also, the content of radiocesium in the human body as measured by Marinelli at Argonne and Anderson and Langham at Los Alamos agrees well with the fact that it has an average residence time in the human body of about five months as compared to many years for strontium-90. The radiocesium data are very interesting because of their bearing on the fallout dissemination mechanism and the confidence with which we can establish the probable future behavior of radioactive strontium. The data confirm previous suggestions as to the dissemination mechanism, that is, we find that radiocesium fallout except of the local variety is carried down very largely in the form of moisture droplets and that there is some direct pick-up by leaves and grass on surfaces. It is captured and held tightly by the top two inches of most soils, so the water which falls and runs off in the form of rivers is clean by the time it has drained a short distance through soil. All of this is very similar to the radiostrontium behavior.

The plants pick the strontium-90, and radiocesium to a lesser extent, out of the soil and also off of their leaves and take it into their systems. There appears to be a discrimination mechanism which operates in most plants so that the strontium-90 content of the plant is considerably less relative to its calicum content than in the case of the soil. On the average, the discrimination factor between the top soil and plants against strontium relative to calcium seems to be about 1.4. When the cows eat grass they further discriminate by about a factor of 7 in making milk so there is an overall protection factor for strontium-90 from the top soil to milk of about 1.4×7 or 10. Also, there is a further discrimination factor against strontium relative to calcium in the human body. This factor is not known too well, but is known definitely to be at least as large as 2 and is thought possibly to be as high as 8. Researches are now in progress to settle this. Therefore, there is a series of protective factors which makes the concentration of radiostrontium derived from milk relative to calcium in human bone not over 1/20 and possibly as little as 1/80 of that in the top soil. Of course, it should be pointed out that there is a considerable part of the fallout which is picked up directly on the leaves and to this the factor of 1.4 does not apply, so for this fraction of the fallout the protective factor may be reduced to 14. Since milk is the source of most of our calcium, this means that the actual ratio of radiostrontium concentration in new human bones relative to that in the top soil should approach these numbers.

It must be realized that though only a small part of the calicum is derived from vegetables and meat, a similar calculation must be made for this portion and the total average ratio obtained. It seems that the meat-vegetable overall discrimination factor is about 10 so if 20 percent of the calcium is derived from such sources on the average the average overall factor will be between 1/13 and 1/30. The experimental data on new human bone in children appear to give a smaller figure, 1/60, as mentioned later.

A matter of importance in connection with the amount of strontium-90 which one would expect to be deposited in human bone as a result of atomic weapon detonation is the calcium concentration in the top soil. Since calcium is so similar to strontium, it seems very likely, and the evidence confirms this, that high available calcium content of the soil will reduce the probability of strontium-90 being taken up into the plants. Of course this probably does not have nearly as great an effect on the uptake of the material which is picked up directly on the leaves. We might expect therefore that soils which are particularly low in calicum might show higher strontium-90 contents for the grasses grown on them. This is, in fact, so, and sheep and goats and cattle feeding on such pasture display a higher strontium-90 bone content.

How such calcium deficiencies in the soil should affect the strontium-90 uptake by the human population is a most important question. One sees immediately that food distribution systems are such that the food supply is derived from large areas, and that there is consequently a sharp reduction in the sensitivity of the

human population to calcium deficiencies in local soils. This is brought out particularly well by the data on the radium contents of human bones and their obvious lack of strong dependence on the radium contents of local waters. But for people who consistently drink milk from cows grazing on such ground there should be a definite effect on the amount of radio-strontium uptake and the effect should be proportional to the radiostrontium content of the milk. So the question resolves itself largely into "What are the strontium-90 contents of the foods people in such regions actually consume?" We find on inspection of the food eating habits and calculation of the strontium-90 intake relative to calcium, that the increase in average strontium-90 concentration of the food due to the low calcium content of the particular soils can hardly be more than five-fold for a soil-calcium deficiency of 50-fold. That is, whereas normal soil carried about 20 grams of available calcium in the top 2.5 inches, a region with soil of only .4 grams per square foot would produce a human body burden equilibrium of about five times that which the normal soil would produce.

In order to understand the hazard of radiostrontium, which is generally agreed to be the most hazardous of the long-lived fission products, we try to establish the maximum permissible concentration both for occupational workers and for the population in general. These numbers have been set at 1 microcurie and .1 microcurie for the standard man, respectively. That is, an occupational worker may carry 1 microcurie of strontium-90 in his body, whereas the general public should not have over .1 of a microcurie of strontium-90 in the average standard adult. This last figure corresponds to a concentration of 100 micromicrocuries per gram of body calcium or what we call 100 Sunshine Units, that is, 1 micromicrocurie of strontium-90 per gram of body calcium is defined as 1 Sunshine Unit.

Now, we must try to see in some other way how our normal experiences can be brought to bear on the question: "How dangerous is atomic weapons testing from the pont of view of radioactive fallout?" At the present time we have in our bodies about .1 or .2 of a Sunshine Unit and children have about one-half of a Sunshine Unit. In a few minutes I will speak about the question of the variation from these average values, but assuming at the moment that these are the values, what is the threat or the hazard from these quantities? Obviously, they are much smaller than the 100 Sunshine Unit tolerance figure mentioned above. To obtain a comparison with normal experience, let us consider the fact that we know in a general way the magnitude of the radiation levels to which we are normally subjected by the cosmic rays, potassium in our own bodies, and the uranium, thorium and potassium in the ground and in our surroundings. We know these qantities amount to something like 150 milliroentgens per year for an average person in this latitude. But we also know that there are considerable variations with conditions.

For example, a person living in a brick house may very well get 25 to 50 milliroentgens per year more than one living in a wooden house, because of the natural radioactivity of the bricks. It is also very well known that whereas at sea level in this latitude the cosmic ray dosage is 37 milliroentgens per year, at 5,000 feet altitude as in Denver, Colorado, the dosage from cosmic rays is 60 milliroentgens per year, or a difference of 23 milliroentgens per year. What is this in terms of strontium-90 body burden?

First, we must consider what part of the natural radiation, if any, is similar to the radiation of strontium-90 in biological effect so we can say without doubt and hesitancy that the physiological effects, whatever they are, will be the same for the same energy absorbed. Fortunately, the cosmic rays seem to fit this bill. In other words, we are at liberty to compare the cosmic ray radiation dosages with the dosages from radiostrontium in our bone structure. The reason this is permissible is that the ionization density along the tracks of the mu-mesons which are the principal cosmic ray components at sea level and at altitudes of 5,000 feet are nearly the same as those of the yttrium-90 beta rays, the principal radiation which radiostrontium emits; that is, radiostrontium has a radioactive daughter, yttrium-90, which emits a very energetic beta ray and the ionization density along the track of this radiation is very similar to that of the mu-mesons of the cosmic rays and their disintegration electrons, and it is generally accepted by health physicists and radiobiologists that radiations of the same ionization density have very similar, if not identical biological effects for the same energy absorbed. The high energy of the yttrium-90 gives it an average distance of penetration in tissue of 2 millimeters so any effect of local non-uniformity of deposition of strontium-90 in the bone is removed. The cosmic ray exposure is, of course, uniform throughout the bone structure. Therefore, we can equate cosmic ray dosage

with strontium-90 dosage and thus it is possible for us to say that the difference between one altitude and another is equal in effect, other effects being equal, to a certain number of Sunshine Units in bone. Now to follow this thought through, 1 Sunshine Unit is equal to 3 milliroentgens per year. Therefore, the difference in annual cosmic ray radiation dosage between Washington, D. C., or any place at sea level in this latitude, and Denver, Colorado, is equal to 8 Sunshine Units, that is, 16 times the present body burden of equilibrium bone or bone near equilibrium as we see it in young children who are growing now.

Therefore, we must examine whether anything in our experience indicates that these differences are significant in terms of the occurrence of the principal effects expected of radiostrontium, namely leukemia and bone cancer. Now of course when one looks for such vital statistics, one finds that they are very hard to acquire. However, the National Institutes of Health and the Department of Health, Education and Welfare, have given us statistics for the occurrence of leukemia and bone cancer for the year 1947 for the three cities, New Orleans, San Francisco and Denver. They are shown in Table I.

TABLE I.—*Occurrence of bone cancer and leukemia*

[New cases per year per 100,000 population]

	Bone cancer	Leukemia
Denver	2.4	6.4
New Orleans	2.8	6.9
San Francisco	2.9	10.3

It is clear from this table there is no obvious effect of altitude, and it is also clear that there are other factors which are noticeably more important than cosmic ray dosage. Of course there may still be a considerable effect of altitude hidden in large fluctuations caused by other factors, which presumably are largely unknown and we cannot say that this proves anything. It does, however, give us some assurance from normal experience that the effect of eight Sunshine Units will not cause a detectable increase in bone cancer or leukemia.

This fits well with the laboratory data on animals and the limited experience on humans with radium. That is, 1 microcurie being 1,000 Sunshine Units, is still considered to be pretty safe on the basis of the laboratory data. It is set as a tolerance for occupational workers and it is therefore reasonable that eight Sunshine Units should give an effect so small as to be very, very difficult to detect. It is, I think, helpful for us, however, to realize that the present body burden of strontium-90 in new bone from the weapons tests that have occurred in the past is equal to the increase in cosmic ray intensity that goes with an increase of some 400 feet in altitude, a very small fraction of the difference in cosmic radiation intensity between Denver and sea level. Therefore, at the same time that we consider the possible effects of strontium-90 from such concentrations, we may deduce from our everyday ordinary experience limits on the effects to be expected. None of the evidence on the occurrence of bone cancer or leukemia as a function of altitude has given us any reason to believe that the present tolerance limits are in any way in error. The present body burdens in new bones are small compared to these limits.

Separate from the strontium-90 effects are the effects of general gamma radiation, the radiation that is received mainly from outside the human body, and which comes mainly from the very young fission products in the local fallout area, but which can come in smallest part from radiocesium accumulating on the ground in the case of the stratospheric fallout, or more importantly, from the shorter-lived fission products deposited by the tropospheric fallout. Of course, weapons tests are so conducted as to avoid exposures to local fallout, so our present discussion of the effects of weapons will be restricted to the much smaller gamma ray doses from the offsite tropospheric and stratospherice types of fallout. In time of war, of course, it would be the local fallout which would be of more direct concern, next to blast and thermal effects, and it is to this aspect of fallout which FCDA addresses itself in the main. In regard to nuclear tests, we have to study the effects on human genetics and the possible effects of such doses of radiation on health. Let us again apply the criterion of normal human experience to this. Measurements have shown that the general average intensity of fallout gamma rays from tests is 1 to 5 milliroentgens per year. Now

the general magnitude of the effects to be expected from this can be compared with the natural radiation intensity. We find, as mentioned earlier, that such things as living in a brick house, instead of a wooden house can amount to as much as 25 to 50 milliroentgens extra dosage per year, that there are certain areas in the world where the average dose in this country of 150 milliroentgens per year is exceeded by ten-fold, that people living on granitic rock as compared to those living on sedimentary rock receive about 70 milliroentgens per year more dosage due to the higher content of uranium and thorium in these rocks and that people living at higher altitudes have a higher natural cosmic ray dosage. Also, of course, we know that medical uses of X-rays can be considerably larger than any of these fallout dosages.

We do have experience and valid evidence that the somatic effects other than cancer and leukemia, that is, the effects of radiation on ordinary human health, require dosages which are very much larger, of the order of 25 to 50 roentgen units in order to be observed as changes in the blood and 100 to 200 roentgens for injury symptoms; whereas the dosages we are speaking of from test fallout are about one hundred thousand fold smaller.

As for genetic effects, these are extremely difficult to evaluate, since there is so little known about human genetics. But judging from experience with plants, insects, animals, and lower organisms, there is every reason to expect some genetic effects of radiation. The question is how much radiation is required for a given level of effect. There are a certain number of mutations in every new human generation. Are these largely induced by natural radiation or are they mainly of chemical, or rather biochemical origin, or both? From a chemical point of view, it seems likely that not all the spontaneous mutations in the human or any other species are caused by radiation effects, because it seems likely that radiation acts in inducing mutations mainly via molecules which it generates in the human cell, and that the mutations are caused by these chemicals and therefore in a sense are chemical in nature. Now if this be so, and the radiation induced mutations are nearly always caused by chemicals which are produced in the first instance by radiation, then chemicals themselves which are not produced by radiation but have other origins, can cause mutations, so it seems likely that a major part of the natural or spontaneous mutations in any species is not radiation induced. This point is an important one to settle, for the reason that we have to compare the effects of fallout radiation with the fraction of the natural spontaneous mutations which is due to the radiation we are normally subjected to. In other words, if the normal mutations are all due to radiation, then the effects of the additional radiation from general test fallout, or from other sources of radiation such as atomic power, or the medical uses of isotopes and X-ray, will be larger. It seems likely, and many genetic authorities agree on genetic grounds with this conclusion, that a major portion of the spontaneous mutations of the human species is not due to radiation but due to other causes. Therefore, a fraction of the spontaneous mutations in the human species is taken as being due to irradiation. Now, what this fraction is, it is difficult to say, but Professor H. J. Muller has estimated that this might be 10 percent. Therefore, one estimates the 150 milliroentgens per year from natural radiation now causes about 10 percent of the spontaneous mutations, and therefore, that the test fallout if continued indefinitely will, at the present level of about 1 to 5 milliroentgens per year, cause an increase in the natural spontaneous mutation rate of something like $\frac{1}{50}$ of ten percent, or 0.2 of a percent of the spontaneous mutations. In the extreme, if it should prove that all of the spontaneous mutation rate is radiation induced despite the chemical arguments, the effect would be ten times as great, or two percent. Dr. Dunning of the Division of Biology and Medicine of the AEC estimated 1.4 percent in 1955 on similar assumptions. (The Scientific Monthly *81*, 265–December 1955.) This effect is one which is comparable to moving to a slightly different locality and is much less serious than changing from one house to another or doing any of a dozen things. The only important point is that genetic effects show only if large numbers of people are subjected to them. Therefore, we would expect that the effects of large populations changing their environment, such as living at a higher altitude, or living in a region of naturally higher radioactivity, should cause genetic effects, if test fallout does so. An examination of vital records should be made to test for such effects and the Atomic Energy Commission is doing so as best it can. The United Nations Scientific Committee on the Effects of Atomic Radiation has been comparing the data on natural background dosages, and it is hoped that this study will be continued and that the search will be made for observable effects of variations in the natural background dosage, for it is certain that any effects

due to gamma rays from fallout must be already present in much larger measure due to the natural dosage.

II. VARIATION IN INDIVIDUAL STRONTIUM-90 BURDENS

What is the likelihood that even though the average strontium-90 content be well within tolerance limits, that a few individuals should exceed tolerance limits, that a few individuals should exceed tolerance limits? Let us consider first the case which will ultimately hold, the situation of complete equilibrium with the environment in so far as the strontium-90 burden is concerned. The only way we can make judgments about the expected individual variations from the mean concentration is by direct experiment on human body composition, not only for strontium-90 but for other analogous constituents. Most of the recent data on the strontium-90 body burden are from odd bits of bone removed during surgical operations, but fortunately we have actual data for the strontium-90 content of the entire bodies of some several dozen stillborn children [1] in the city of Chicago in the year 1953. A strenuous effort is now being made on the Sunshine Project to continue this series and also to check the human bone data by analyses of complete skeletons. We present the distribution of the strontium-90 data for the stillborn children in Figure 1. Data for the occurrence of ordinary non-radioactive strontium in human bones also have been published.[2] These obviously refer to the full steady state condition and are obviously at least as nearly in equilibrium with the environment as the fallout radioactive strontium ever will be. These data are presented in Figure 2. The occurrence of radium in the human body also has been used since it is chemically similar to both calcium and strontium, and therefore is a bone seeker and because it is obviously also in steady state equilibrium. The data used were those by Palmer and Queen [3] in Figure 3. And, finally, we use the recent data on occurrence of normal potassium in human bodies as determined by Anderson and Langham [4] at the Los Alamos Scientific Laboratory as presented in Figure 4. All of these data show a normal frequency distribution as indicated by the theoretical curves. The respective widths of the curves (standard deviations) are 36 percent for radiostrontium, 40 percent for normal strontium, 40 percent for radium, and 18 percent for natural radiopotassium. It is completely clear from these data that they agree with one another in general shape and that the magnitude of the distribution of the strontium-90 contents of the Chicago stillborn babies was not in any way anomalous. Therefore, we shall take the distribution curve for radiostrontium to be the same as for the normal strontium data. The occurrence of non-radioactive normal ordinary strontium in the bones should certainly tell us what the equilibrium distribution will be for radioactive strontium, and from it we should be able to learn the points about distribution which we cannot yet learn in any detail from the radioactive strontium itself. Turekian and Kulp noted in their study of normal strontium in human bone that in a given region the deviation from the average was about 34 percent of the average, that is, for human bone from the regions Colorado, Texas, Cologne, Bonn, Venezuela, Chile, Vancouver, China, and India. In each instance the ratio of the standard deviation from the mean itself was taken and the average calculated to obtain 34 percent. Therefore, we take 34 percent as the expected standard deviation from the mean *for a given locality* for the eventual strontium-90 equilibrium burden in human bones.

With this result we can, assuming a normal error curve shape of the distribution of probabilities, answer the immediate question: What is the probability of an individual exceeding the tolerance even though the mean does not? On the basis of this analysis we find that at steady state and in equilibrium the variation from the mean will constitute an error curve with a shape corresponding to the standard deviation, being ⅓ of the mean. Therefore, at steady state among people living in a given locality, only one person in about 700 will have more than twice the average strontium-90 burden, and the chances of anyone having as much as three times the normal burden will be about one in twenty million.

Now what about the non-equilibrium distribution, when the strontium-90 is finding its way into the biological system? Obviously, the burden will be much

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[1] W. F. Libby, "Radioactive Strontium Fallout," Proc. Nat. Acad. Sci. *42*, No. 6, 365–390 (1956); University of Chicago, Project Sunshine Bulletin No. 12, August 1, 1956.
[2] K. K. Turekian and J. L. Kulp, Science *124*, 405 (1956).
[3] Hanford Report, HW–31242.
[4] E. C. Anderson, R. L. Schuch, W. R. Fisher, and W. Langham, "Potassium and Cesium Radioactivity in People and Foodstuffs." (In press.)

lower here, but the deviation from the mean will probably be much higher percentagewise, particularly in adults where most of the bone has been deposited before strontium-90 was produced. The present strontium-90 content of adults depends very much on the growth rate and the metabolic activity of the various bones in the given individual's body which happens to be sampled. However, the specific concentration of the strontium-90 deposited will not exceed that in new bone developed entirely in the present biological environment, i. e., the local concentration in adult bone will not exceed that for the whole bone in young children, whose total bodies are composed of the mixture of strontium-90 and calcium which now is present in food. Since the present ratio for children to adults is about four to one for average total strontium-90 content, the factor of concentration in adults' active bone regions may be as much as four-fold greater than the whole body average. Thus the apparent spread for random bone samples taken from adults should be very large compared to the true equilibrium spread for these reasons. As equilibrium is approached, however, the spread must decrease very, very markedly.

The data on human bones indicate a very wide scatter, but it seems extremely clear that the variation is a reflection of the fact that the main skeleton of adult individuals is not in equilibrium with the present food supply, and that the variations reflect the different rates at which the various bones in the bodies of individuals are coming into equilibrium with the food supply in the general biological environment. A study of whole skeletons taken from one given locality which is now under way as a part of Project Sunshine will clarify the point about the variations among individuals in their rate of coming into equilibrium with the general biological environment. This study is under way in Dr. Kulp's laboratory.

It should appear from these studies that the variation from the mean of adults will be larger than the factor of one-third which apparently is normal for the types of equilibrium distribution considered above. It is, of course, very important to establish the truth of this prediction clearly. However, the general agreement in shape of the distribution curves for such widely different materials as normal potassium in whole bodies, radium, and normal elementary strontium in fragmentary human bone, and actual fallout radioactive strontium in the whole bodies of stillborn children, give us good reason to believe that there is nothing extraordinary in the distribution of radiostrontium in human bone.

III. VARIATION OF THE STRONTIUM-90 BODY BURDEN WITH LOCALITY

Most important of the causes of variation of the strontium-90 content of individuals with locality is, of course, the amount of fallout in a given region. The general rules about the intensity of fallout have been described above. For air-fired megaton weapons our present indication is that the fallout is almost world-wide and for reasons of simplicity and in the absence of better information at the present time, we work on the model that this is a uniform distribution, over the entire world, of material that falls from the stratosphere. Further evidence and data on this point are rapidly being collected which will undoubtedly settle the stratospheric horizontal mixing question.

At the present itme, the general latitudes in the Northern Hemisphere which are betwen 10° and 60° North have the highest strontium-90 content. In the United States, which because of our proximity to the Nevada Test Site has unusually high fallout, there are at the present time about 25 millicuries per square mile of strontium-90. For average soil this means a concentration in the top soil of about 50 Sunshine Units. With the factors of discrimination mentioned above, this means that an equilibrium body burden between 1.7 Sunshine Units and 3.9 Sunshine Units is to be expected. Actually, the present body burden in young children indicates that the lower value is probably more realistic. The present body burden in children—about 0.5 Sunshine Units—probably was derived from an average strontium-90 content in the top soil of something like 15 millicuries per square mile, or about 25 to 30 Sunshine Units during the time the strontium-90 was being acquired. Thus we find that the experimental value for the ratio between the body burden of young children and the average concentration in the top soil is about 50 to 1; rather closer to the higher range of the laboratory results than to the lowest range.

Table II contains the latest data for the total strontium-90 fallout as measured in U. S. soils, and Figure 5 displays these data graphically.

The Northern part of the United States has about 20 to 30 millicuries of strontium-90 per square mile, the Southern States are somewhat lower. The low figure of 7 millicuries per square mile for Grand Junction, Colorado, is probably due to local climatic and sample site conditions.

TABLE II.—*Health and safety laboratory 1956 survey of United States soils for Strontium-90—Samples taken between Oct. 8 and 13, 1956—Strontium extracted with 6N HC1 at room temperature—Replicates represent individual soil aliquots taken after sampling and air drying—Each error term represents 1 standard deviation due to counting error*

Sampling site	Depth	d/m/gm soil	mc/mi²	mc/mi²	
				Ave.	Total
Albuquerque, N. Mex	0–2″	0.078±0.001	7.5±0.1	7.3	
		0.075±0.001	7.2±0.1		
	2–10½″	0.008±0.002	4.4±0.9	3.4	11
		0.005±0.002	2.4±0.8		
Atlanta, Ga	0–2″	0.35 ±0.007	14.0±0.3	15.0	
		0.42 ±0.009	16.0±0.4		
	2–6″	0.018±0.004	2.8±0.6	3.0	18
		0.021±0.003	3.3±0.5		
Binghamton, N. Y	0–2″	0.32 ±0.007	17 ±0.4	18.0	
		0.35 ±0.007	18 ±0.4		
	2–6″	0.019±0.003	4.4±0.8	4.8	23
		0.023±0.005	5.2±1.1		
Boise, Idaho	0–2″	0.23 ±0.006	20.0±0.6	22.0	
		0.26 ±0.006	23.0±0.6		
	2–6″	0.012±0.002	3.1±0.6	3.5	26
		0.015±0.002	4.0±0.6		
Des Moines, Iowa	0–2″	0.31 ±0.007	23.0±0.5	23.0	
		0.31 ±0.007	23.0±0.5		
	2–6″	0.028±0.002	7.6±0.7	7.1	30
		0.024±0.003	6.6±0.7		
Detroit, Mich	0–2″	0.26 ±0.006	20.0±0.5	20.0	
		0.27 ±0.006	20.0±0.5		
	2–6″	0.038±0.003	7.3±0.5	7.8	28
		0.044±0.003	8.4±0.6		
Grand Junction, Colo	0–2″	0.10 ±0.001	7.8±0.1	7.0	
		0.091±0.001	7.1±0.1		
		0.11 ±0.019	8.2±1.4		
		0.070±0.013	5.1±1.0		
	2–10½″	≤0.002	≤0.45	≤0.48	7
		≤0.002	≤0.51		
Jacksonville, Fla	0–2″	0.11 ±0.009	7.3±0.6	7.3	
	2–6″	0.013±0.004	2.7±0.9	3.4	11
		0.020±0.005	4.0±1.0		
Los Angeles, Calif	0–2″	0.12 ±0.008	6.9±0.5	7.5	
		0.14 ±0.009	8.0±0.5		
	2–7″	0.009±0.002	3.3±0.9	2.8	10
		0.006±0.002	2.2±0.7		
Memphis, Tenn	0–2″	0.27 ±0.006	15.0±0.4	15.0	
		0.26 ±0.006	15.0±0.4		
	2–6″	0.028±0.003	6.5±0.7	6.6	22
		0.029±0.003	6.6±0.7		
New Orleans, La	0–2″	0.24 ±0.006	8.8±0.2	8.6	
		0.22 ±0.006	8.3±0.2		
	2–6″	0.009±0.002	3.3±0.9	2.8	11
		0.006±0.002	2.2±0.7		
New York, N. Y	0–2″	0.21 ±0.006	10.0±0.3	12.0	
		0.29 ±0.007	14.0±0.3		
	2–6″	0.072±0.004	14.0±0.8	14.0	26
		0.068±0.004	14.0±0.8		
Philadelphia, Pa	0–2″	0.17 ±0.005	12.0±0.4	12.0	
		0.16 ±0.005	11.0±0.4		
	2–6″	0.029±0.003	7.3±0.8	6.8	19
		0.026±0.003	6.4±0.7		
Rapid City, S. Dak	0–2″	0.29 ±0.006	20.0±0.4	22.0	
		0.34 ±0.006	23.0±0.4		
	2–6″	0.053±0.004	12.0±1.0	11.0	33
		0.045±0.003	10.0±0.7		
Rochester, N. Y	0–2″	0.22 ±0.006	16.0±0.4	16.0	
	2–6″	0.013±0.004	2.5±0.4	2.5	19
		0.013±0.002	2.5±0.4		
Salt Lake City, Utah	0–2″	0.32 ±0.007	22.0±0.5	22.0	
		0.33 ±0.007	23.0±0.5		
		0.31 ±0.007	22.0±0.5		
	2–8″	0.016±0.002	5.7±0.7	5.8	28
		0.016±0.002	5.9±0.8		
Seattle, Wash	0–2″	0.46 ±0.011	17.0±0.4	17.0	
		0.44 ±0.010	16.0±0.4		
	2–6″	0.051±0.007	9.4±1.2	9.5	27
		0.052±0.004	9.6±0.7		

The differential rates at which the fallout has been occurring probably are best measured by the so-called "pot collection" method. A bucket with vertical walls of appreciable height is placed out in the open and allowed to collect the total fallout for a given period including the rain, snow, dust, etc. The bucket is left out whether it has rained or not and covers the total fallout for a given period. Figures 6 and 7 give the curves so obtained for New York and Pittsburgh areas together with the estimated errors of measurement. It is interesting to note the changes in slope and to correlate them with the occurrence of test activitis and the relatively short-lived troposheric fallout. The minimum slopes which appear during quiet periods when no one is testing are the stratospheric fallout of which we have spoken and these slopes when we have enough pots operating all over the world will, when taken together with the results of the measurements of the amounts of radiostrontium and radiocesium in the stratosphere, give an accurate value for the stratospheric residence time and settle the mixing question.

In addition to the intensity of fallout, the question of the fraction of the radiostrontium, and, for tropospheric fallout, the radioiodine of eight-day half-life, that is in assimilable form is an important one. So far most fallout strontium appears to be completely water soluble and therefore most assimilable, though continued tests on this point should be made. Direct leaf pick-up of course promotes assimilation of the strontium because the plant differentiation against strontium when it assimilates it from soil thus is avoided. Another factor is, of course, the concentration of available calcium in the soil. By available calcium we mean calcium which is available to plants and not the total calcium in the soil. It is known that soils which are high in available calcium produce plants of lower radioactive strontium content; that is, the radioactive strontium to calcium ratio in the plant is lower as a direct consequence of the lower concentration of radiostrontium in the available soil calcium. In addition, as mentioned previously, plants tend to prefer calcium to strontium with a discrimination factor of about 1.4. Sheep which grow in certain areas of Wales have shown concentrations in their bones approaching 150 Sunshine Units, while sheep and cattle growing in the U. S. have hardly ever exceeded one-fifth of this. The Welsh soil in certain areas is very low in calcium and as a result grows grasses of high radiostrontium content. Of course, it is clear that fertilization with calcium will immediately relieve this difficulty, but in the absence of such fertilization, the question is: How serious is the effect of calcium deficiency in promoting strontium-90 pick-up through the food chain?

As was remarked earlier, there is an averaging which occurs in food distribution systems and calcium deficient soils are naturally rather poor producers and as a consequence the weight of the food so produced is less than for a good well fertilized, well balanced soil. This factor reduces the flow into the general food system of material of exceptionally high strontium-90 content. It therefore will probably be sufficient to consider the radiostrontium of milk, since milk is the main source of calcium, in order to test for the radiostrontium content of the food in given areas. Direct measurements have shown that a factor of five encompasses the total variation due to all factors including calcium deficiencies in acid soils.

The general intake must depend on the food distribution pattern and the relatively small fluctuation in milk contents must reflect this. The number of individuals who rely totally on the food output of soil of very low calcium content is very small indeed, but it must be true that these individuals if they grew up on such a provincial, isolated farm would have as much as ten to 50 times the normal average strontium-90 content. The normal calcium concentration in soils in the United States is about 20 grams per square foot for the top 2.5 inches and about the poorest soil known has about 0.4 gram available calcium per square foot for the top 2.5 inches—a deficiency factor of 50.

It is clear from a detailed examination made by the author for people living in calcium deficient areas with normal food distribution patterns, that a factor of 5 is about as large an effect as can be expected from a fifty-fold deficiency of calcium in the soil. The food from outside of the calcium deficient area reduces by a factor of about ten the increase in strontium-90 pick-up rate which would be expected from the calcium deficiency in the soil if people lived entirely off the soil for their whole growing period of 20 years or so.

The food of lowest strontium-90 content is fish flesh, because of the great dilution the fallout receives by the hundred meters of sea water above the thermocline which rapidly mix with the fallout within a few hours or days. This means that

the specific concentration of radioactive strontium, or any other fallout constituent in sea water, is relatively very much lower than it would be in soil. For example, 100 meters of sea water has 370 grams of dissolved calcium per square foot as compared to the average of 20 grams per square foot for the top 2.5 inches of soil which absorbs and holds the fallout radiostrontium. Therefore, in principle sea food and fish are lowest among foods in content of radiostrontium fallout.

IV. EFFECTS OF CONTINUED TESTING AND GENERAL CONCLUSIONS

In summary, then, we see that the present body burden of strontium 90 from atomic weapons tests in the United States corresponds to the radiation dosage to the bones which would result from a few hundred feet increase in altitude, and the present vital statistics show no observable effect on the occurrence of bone cancer or leukemia of much larger changes in altitude. The tolerance figure of 100 Sunshine Units, or 0.1 of a microcurie for an average individual, or 100 micromicrocuries per gram of body calcium, that is recommended now is about two hundred times the present level for new bone in the U. S., and it will be exceeded by fallout from weapons tests in any foreseeable circumstances.

The distribution of strontium–90 burdens among individuals *for a given locality* will be a normal error curve with a standard deviation of about one-third of the average concentration. This means that about one individual in 300 will have more than twice the normal average value for a given locality, and that about 1 in several million will have three times the average value.

The effect of locality is more important, however, particularly in the effect of calcium deficiency in the soils. Careful consideration of this question indicates that there will be very few individuals who show a strontium–90 content which is strictly inversely proportional to the available calcium concentration of the soil in their region. This is due to the fact that food distribution systems automatically average over a wide area and people assimilate their calcium slowly. Most people drink milk and eat cheese and other calcium-bearing foods from a rather wide area, and this effect reduces by an estimated factor of ten the potential effect of calcium deficiency in the local soils.

On the basis of laboratory experiments the human body concentration of strontium–90 at equilibrium will be between 13 and 30 times less than that in the top soil. The present data indicate that the higher figure is closer to the truth, and so we will be conservative in taking the figure of 20 for this ratio. Therefore, the present burden of 50 Sunshine Units in the top soil of the United States may eventually lead to as much as 2.5 Sunshine Units in the human bones, but more likely will lead to about 1.7.

Of course, as testing continues and more fallout occurs, the levels will rise. The strontium-90 that still resides in the stratosphere at the present will fall out according to our expectations at a rate which just about compensates for the decay of the material already deposited, so that no great additional increase from this source is to be expected from weapons fired in the past. If the testing should continue at about the same rate as it has averaged over the last five years, then we should at equilibrium, after an infinite time, approach a level of 8 times the present rate, since the average life of strontium-90 is 40 years. This assumes that the future testing will be conducted so as to give in each future five-year period the same as the last five have. And so we would expect in the United States at that time an average human strontium-90 concentration of 20 Sunshine Units with the conservative factor of 20 between the top soil concentration and the concentration in human bone, or 5 Sunshine Units if the factor of 80 is used. In other words, in the United States something between 5 and 20 Sunshine Units would be the equilibrium concentration of human bones if testing continued indefinitely at the average rate of the past five years. This level would be approached only after a few decades. After 28 years the level would be half of this equilibrium value, and after another 28 years, 56 years total, from an arbitrary beginning which we have set as 1952, we would expect in the year 2008 three-fourths of the equilibrium figures. So somewhere between 4 and 15 Sunshine Units of strontium-90 in human bones in the United States might result from the present type of testing being continued for the next 50 years.

In those certain areas in the world where the soil is low in calcium, this level might go five-fold higher. At the present rate of testing we might indeed approach the figure of 100 Sunshine Units, the tolerance limit for large populations, at the beginning of the 21st century for these certain limited regions in

the world. The observed conditions in these regions could be relieved, however, by fertilization of the soil with calcium, either calcium nitrate or lime being used, as appropriate from other considerations.

The Sunshine Project continues to study the problems of world-wide fallout—the stratospheric inventory of radiostrontium and radiocesium, the occurrence of these isotopes in the soils and water and the biosphere all over the earth, the biological effects at certain levels of contamination with strontium-90, and to a lesser degree with cesium-137, and the possible genetic effects of the low gamma ray dosages associated with world-wide fallout from atomic tests. All of these are studied not only with the point in mind of devising methods of protection against atomic warfare, but also with the thought of possible application in the remote event of industrial accidents which may happen in connection with certain of the peaceful uses of atomic energy, particularly atomic power. Certainly an understanding of the basic principles of world-wide fallout is applicable to the control and safe-handling of isotopes. All of this is done in collaboration with the United Nations Scientific Committee on the Effects of Atomic Radiation, and it is to be hoped that as the data appear all of the countries in the world will join together in this international effort to understand better the effects of the great new fact in life, the nuclear atom.

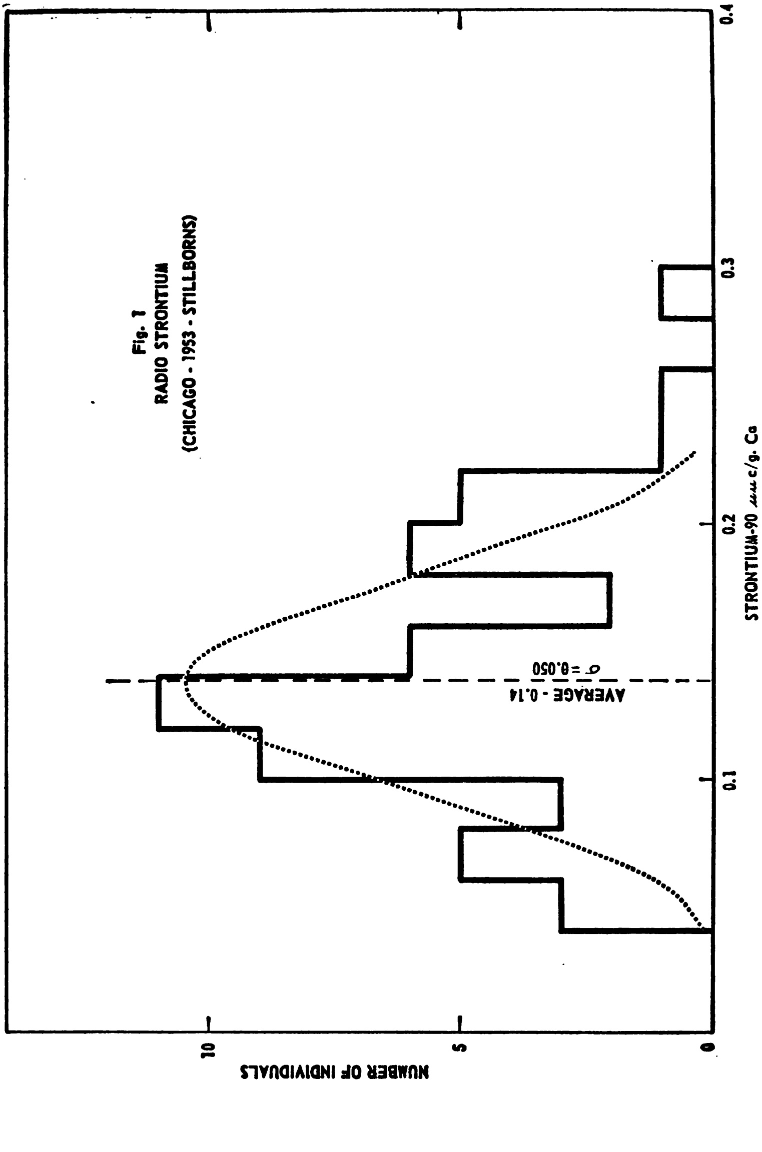


Fig. 1
RADIO STRONTIUM
(CHICAGO - 1953 - STILLBORNS)

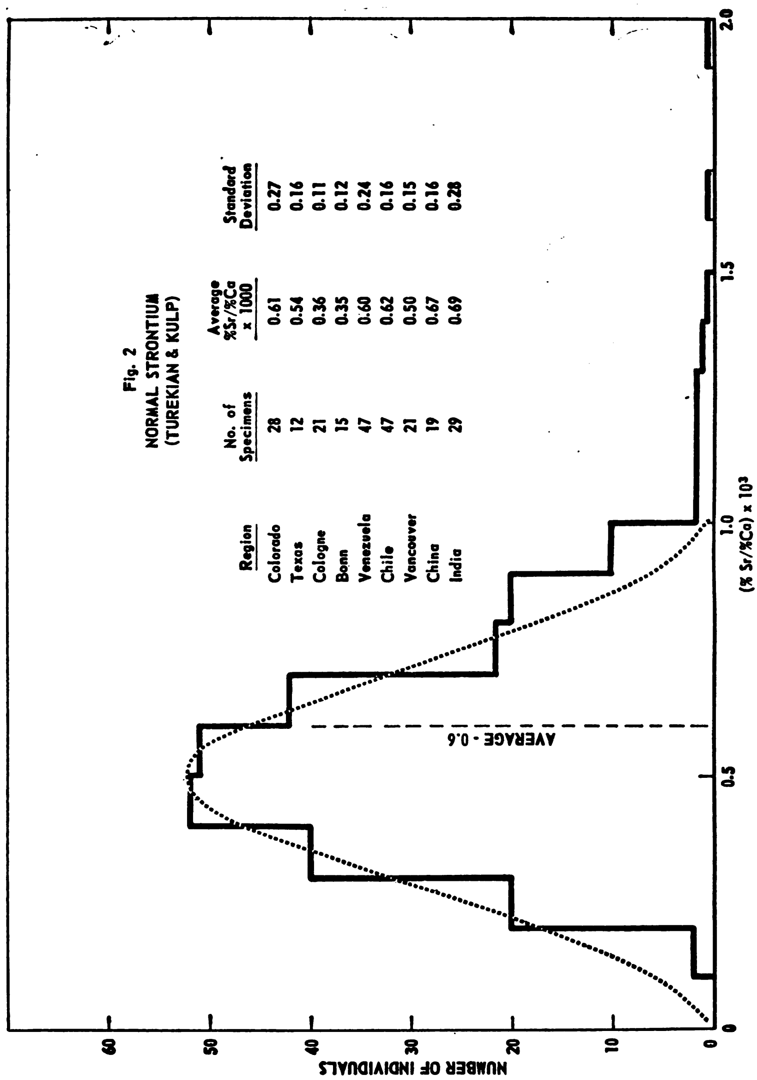


Fig. 2
NORMAL STRONTIUM
(TUREKIAN & KULP)
Region
No. of Specimens
Average %Sr/%Ca x 1000
Standard Deviation
Colorado 28 0.61 0.27
Texas 12 0.54 0.16
Cologne 21 0.36 0.11
Bonn 15 0.35 0.12
Venezuela 47 0.60 0.24
Chile 47 0.62 0.16
Vancouver 21 0.50 0.15
China 19 0.67 0.16
India 29 0.69 0.28
AVERAGE - 0.6
NUMBER OF INDIVIDUALS
0
10
20
30
40
50
60
0
0.5
1.0
1.5
2.0
(% Sr/%Ca) x 10³

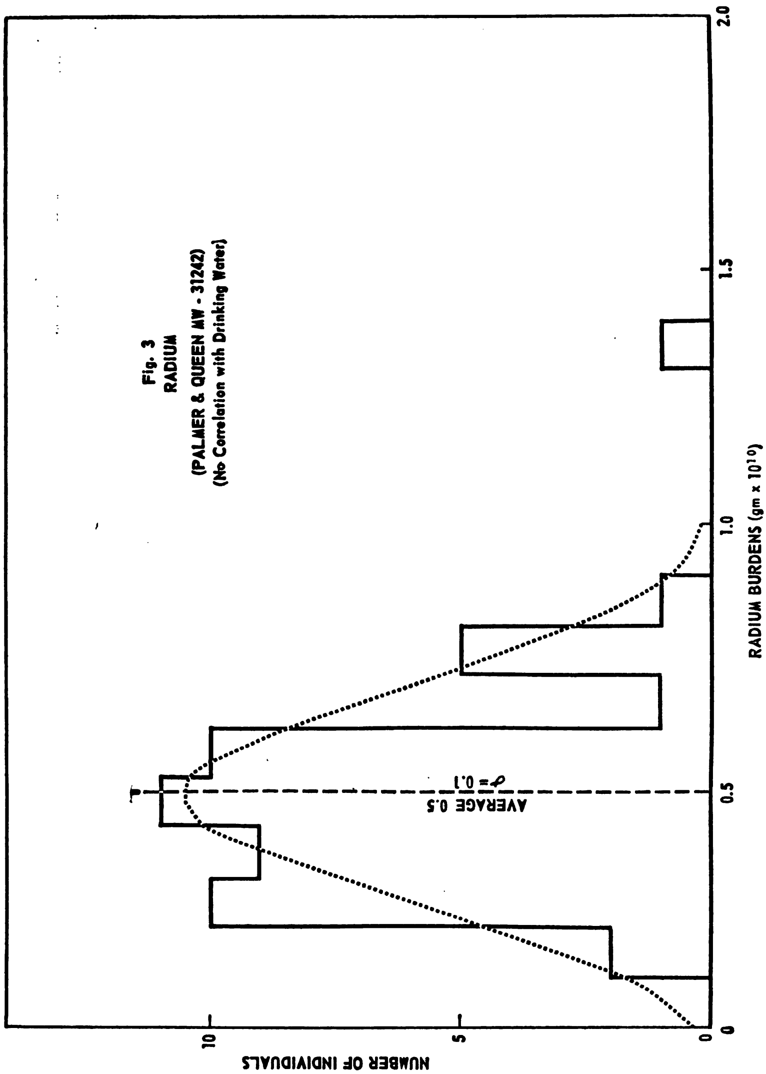


Fig. 3
RADIUM
(PALMER & QUEEN MW - 31242)
(No Correlation with Drinking Water)

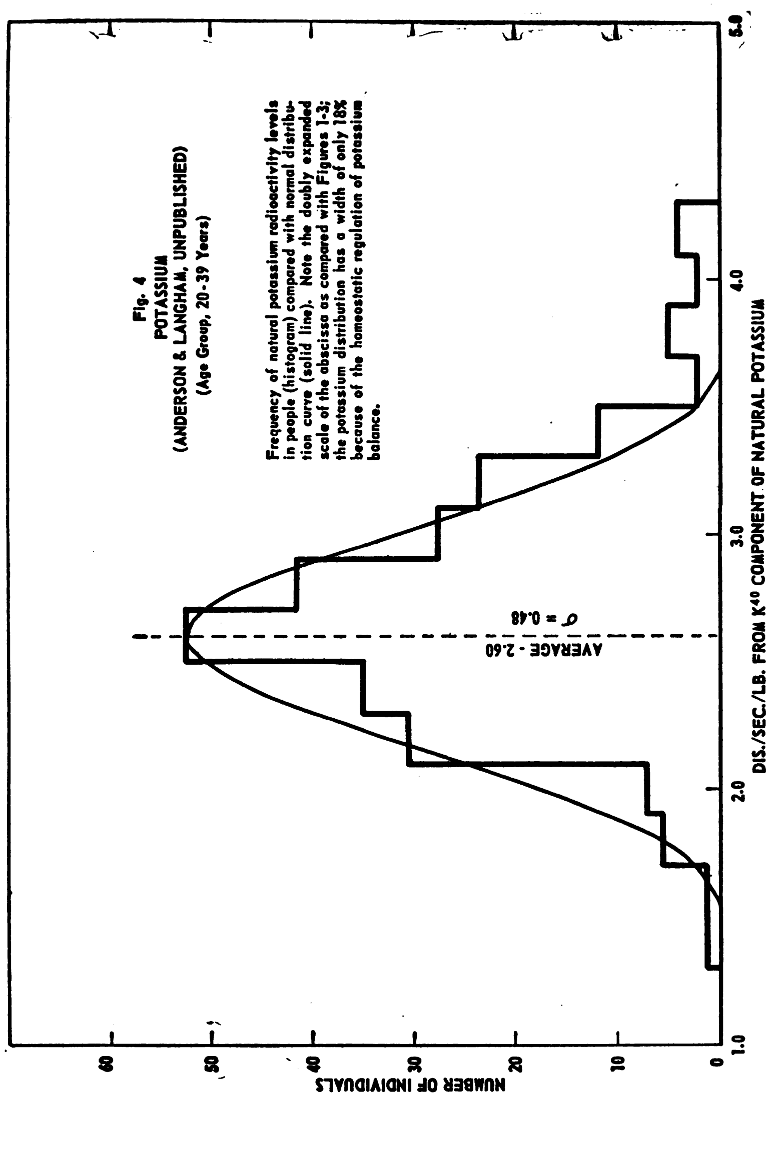
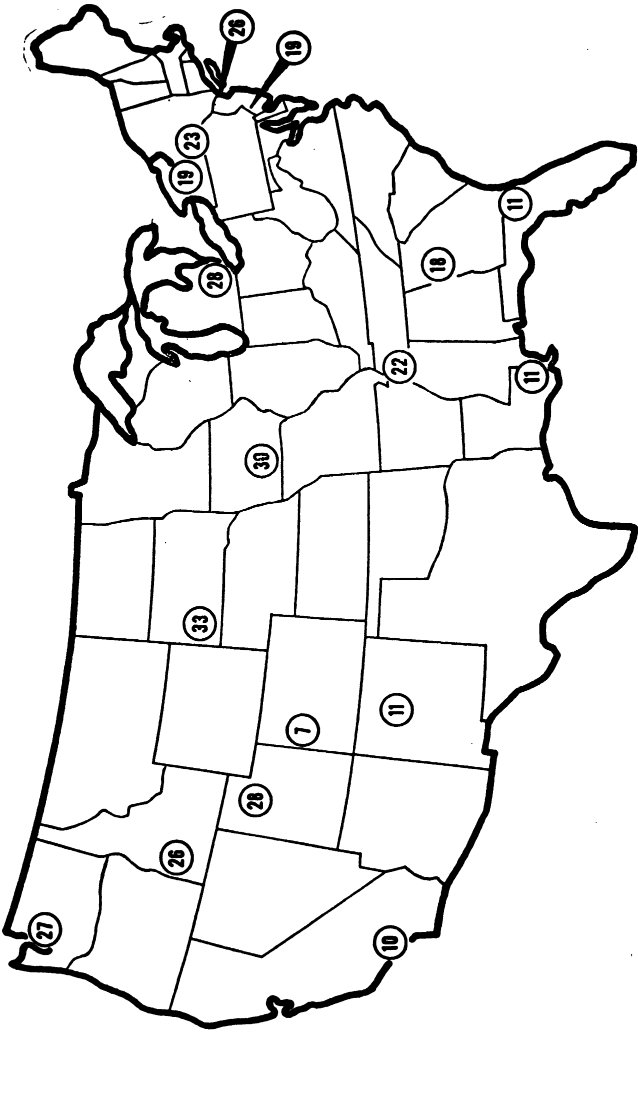


Fig. 4
POTASSIUM
(ANDERSON & LANGHAM, UNPUBLISHED)
(Age Group, 20-39 Years)

Frequency of natural potassium radioactivity levels in people (histogram) compared with normal distribution curve (solid line). Note the doubly expanded scale of the abscissa as compared with Figures 1-3; the potassium distribution has a width of only 18% because of the homeostatic regulation of potassium balance.

Fig. 5

Sr^{90} IN U. S. SOIL (HASL - OCT. 8, 1956) (HCl EXTRACTION METHOD)



Numbers are in mc/mi² at individual site.

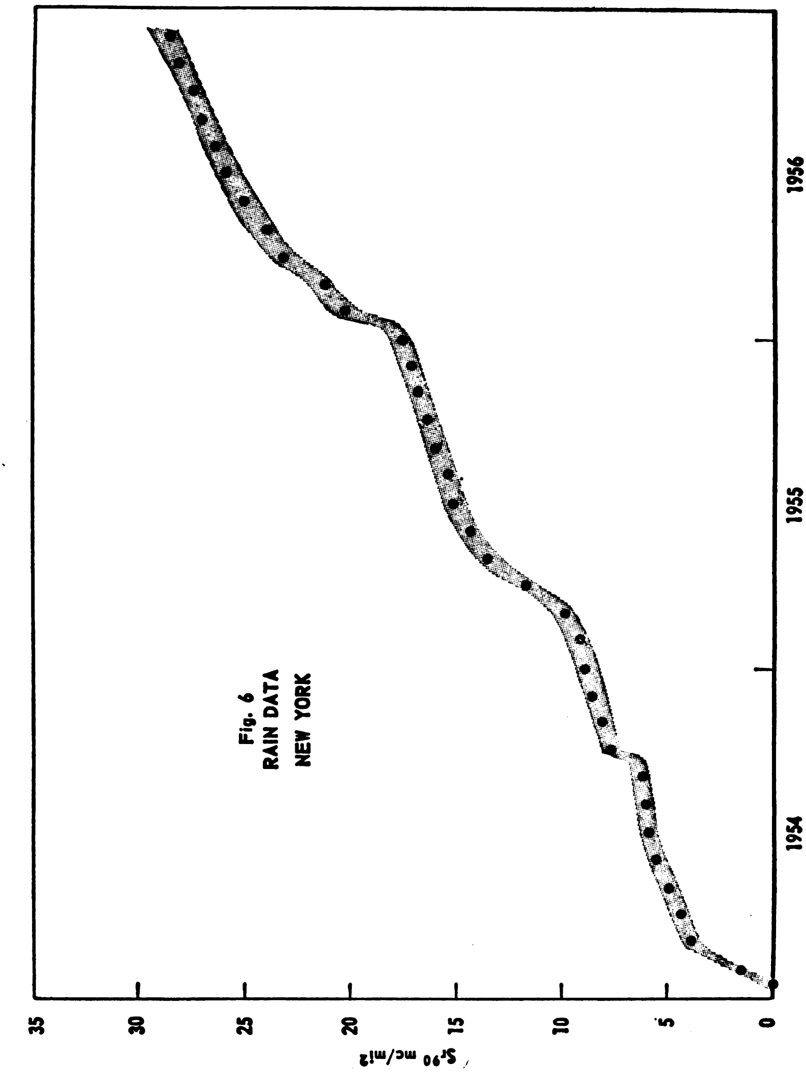


Fig. 6
RAIN DATA
NEW YORK

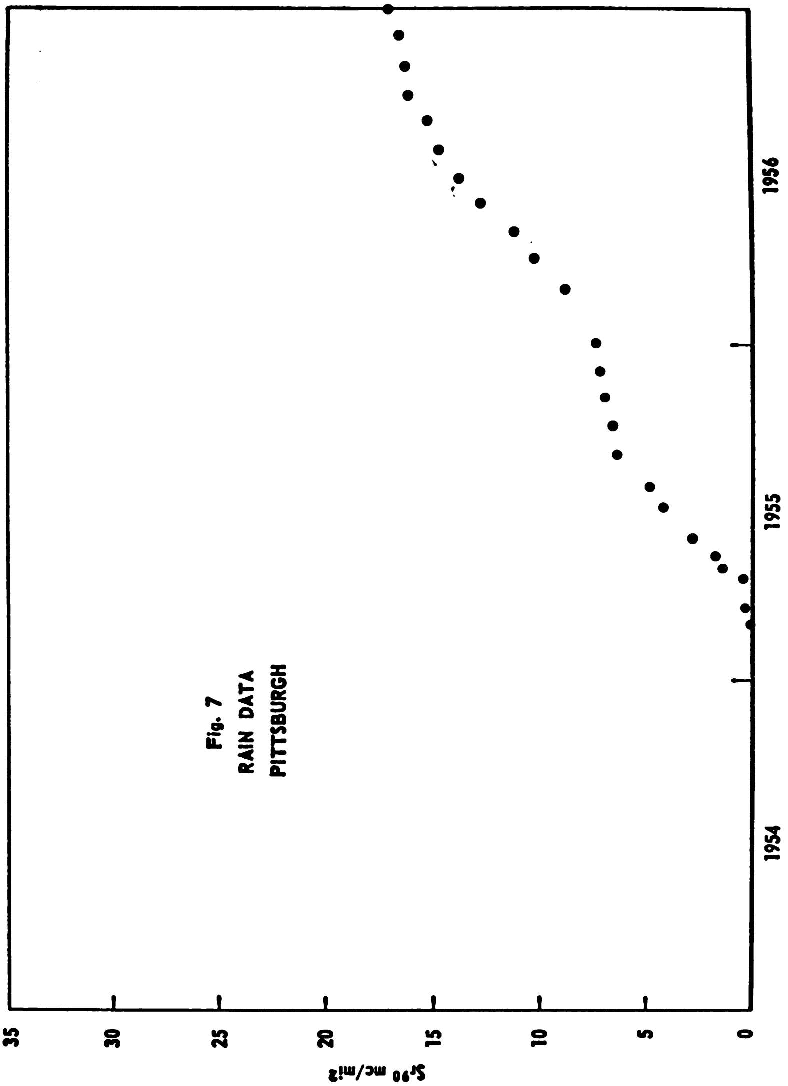


Fig. 7
RAIN DATA
PITTSBURGH

APPENDIX 2

BRITISH STATEMENTS, REPORTS, AND ARTICLES ON FALLOUT AND RADIATION EFFECTS

MEDICAL RESEARCH COUNCIL

THE HAZARDS TO MAN OF NUCLEAR AND ALLIED RADIATIONS

Presented by the Lord President of the Council to Parliament by Command of Her Majesty
June 1956

93299 O—57—pt. 2——35

To The Most Honourable
THE MARQUESS OF SALISBURY, K.G., LORD PRESIDENT OF THE COUNCIL

My Lord,

On the 29th March, 1955, the Prime Minister, through you, requested the Medical Research Council to appoint an independent committee to report on the medical aspects of nuclear radiation, including the genetic aspects. The Council accordingly appointed a committee, under the chairmanship of Sir Harold Himsworth, and this body has now reported.

The report has been accepted by the Medical Research Council, and I have been authorised to transmit it to you with a view to its presentation to Parliament. It is the wish of the Council that, in so doing, I should express their high appreciation of the care, thought and ability which all members of the Committee have devoted so freely to their most difficult and important task.

I have the honour to be, my Lord,
Your Lordship's obedient Servant,

LIMERICK,
Chairman,
Medical Research Council.

June, 1956.

THE HAZARDS TO MAN OF NUCLEAR AND ALLIED RADIATIONS

A REPORT TO THE MEDICAL RESEARCH COUNCIL

CHAPTER I

INTRODUCTION

1. In accordance with the undertaking of the Prime Minister given in the House of Commons on the 29th March, 1955, the Medical Research Council, in April of that year, appointed us members of a committee, under the chairmanship of Sir Harold Himsworth, to review the existing scientific evidence on the medical aspects of nuclear and allied radiations, and we have unanimously signed this report.

2. We held our first meeting on the 3rd May, 1955, and decided to carry out our enquiry for the most part through two panels, one of which undertook to consider the effects of radiation on the health of the individual, and the other the possible genetic consequences of radiation to the population as a whole as well as to the individual and his descendants. Sir Ernest Rock Carling, Professor A. Haddow, Professor A. Bradford Hill, Dr. J. F. Loutit, Professor W. V. Mayneord, Dr. F. G. Spear, Professor Sir Lionel Whitby and Professor B. W. Windeyer have served on the former panel, which has met nine times. Professor A. Bradford Hill, Professor K. Mather, Professor P. B. Medawar, Professor J. S. Mitchell, Professor L. S. Penrose, Sir Edward Salisbury and Professor C. H. Waddington have served on the latter, which has met eleven times. In addition, Sir John Cockcroft and Professor J. R. Squire have served on the main committee. Both panels have worked under the chairmanship of Sir Harold Himsworth and all papers have been circulated to every member of the committee. More than seventy specially prepared papers have been considered, some of them written by scientists not serving on the committee, and we have drawn widely on the relevant published material. Groups to consider special problems have been appointed as the need arose. We have also taken into account relevant work carried out under the auspices of the Medical Research Council's Committee on Protection against Ionizing Radiations, and the recommendations and discussions of the International Commission on Radiological Protection. On the completion of the work of the panels, we have met together in full committee to consider our conclusions and to draw up this report. We have held four meetings of the whole committee during the period of our enquiry.

3. Throughout the course of our work, and in the preparation of this report, we have been greatly helped by our two scientific secretaries, Dr. W. M. Court Brown and Dr. T. C. Carter, of the scientific staff of the Medical Research Council. We are also very much indebted to Dr. R. H. L. Cohen and to Dr. Joan Faulkner, of the Council's headquarters staff, who have been responsible for co-ordinating the work of the panels and the various special enquiries that we have initiated.

4. The immediate occasion for the Government's request to the Medical Research Council to set up this Committee was the widespread public concern

about the long-term effects of nuclear weapon testing. This is only one aspect, however, of the much larger problem arising from the increasing use of ionizing radiations. It is already apparent that the future development of our civilisation is closely bound up with the exploitation of nuclear energy. At present, the potential hazards from its possible military uses overshadow in many people's minds the vast potentialities for good of this new source of power. The hazards to health are qualitatively the same, however, whether they arise from nuclear weapons or from the use of ionizing radiation for peaceful purposes. The difference is one of degree and intensity only. As with other sources of energy that man has harnessed to his service, the use of ionizing radiation necessarily entails risk; but the risk is controllable within limits that he can accept. It is the purpose of this report to indicate the nature of the risks and the extent to which they can be controlled.

5. Our purpose has been to give as firm a guide as the evidence allows to informed opinion in the country as a whole, and more especially to those with whom lies the responsibility for practical decisions of policy. This has laid on us the duty of drawing more precise conclusions than we might wish to do on purely scientific grounds, and we feel bound to point out that in the course of our enquiry we have become increasingly aware of the impossibility, in the present state of knowledge, of coming to final conclusions on many questions of importance in the subject under study. Nevertheless, because of the many and urgent problems on which action cannot be delayed, we have felt it incumbent upon us to attempt to give guidance to the best of our ability. It is inevitable that, with the advance of knowledge, many of the views which we have expressed will come to require amendment, but we feel reasonably confident that the general picture which we have drawn is unlikely to be found grossly inaccurate in perspective or scope.

6. We wish to remind those who read this report that human populations have always been exposed to ionizing radiation, and that it is the scale and not the nature of the hazard which is new. Moreover, our remarks in many respects can be applied only to large populations living under conditions similar to those prevailing in this country. A technically advanced community, such as our own, is likely to be exposed to a greater risk from the industrial and medical uses of atomic energy. These risks have to be weighed against the established benefits derived from the use of ionizing radiation in industry and medicine, and against the benefits likely to be conferred in the future.

7. We have thought it helpful to the general reader to follow this introduction with a brief account of radiation and its mode of action on living cells (*Chapter II*). The types of radiation and the units in which they are measured are described, and the chapter concludes with an outline of the way in which radiation acts on living tissues.

8. In *Chapter III* consideration is given to the effects of radiation on the health of the exposed individual. A brief review of the available sources of information is followed by an account, first, of the clinical manifestations which may occur within a short time of exposure, and, secondly, of the effects which may appear a considerable time, perhaps many years, afterwards. The chapter includes a discussion of the evidence from a detailed study of the relationship between radiation dose and the incidence of leukaemia in patients suffering from a particular disease. This study was undertaken at our request by members of the Medical Research Council's staff, and we should like to thank Dr. W. M. Court Brown and Dr. R. Doll, who carried out the work, for the great help that they have given us. The full results of the enquiry are to be published separately and are summarised in Appendix B.

9. *Chapter IV* of the report opens with a short account of the biological processes controlling the hereditary constitution in human beings, and proceeds to a description of the way in which radiation might affect the genetic structure of human populations. An attempt is then made, in terms of the incidence of certain specific grossly harmful conditions, to assess the consequences for the individual and society of increasing the rate of mutation, and to define the levels of dose which might be expected to bring about such an increase.

10. In *Chapter V* the contributions made to the present level of radiation by naturally occurring radioactivity and by the medical, industrial and other uses of ionizing radiation are reviewed. Many new data have been obtained, and an investigation has been initiated at our request, and is still in progress, to establish more precisely the contribution from various diagnostic and therapeutic procedures (Appendix K). An assessment is then made of the results of contamination from the fall-out from atomic and thermonuclear test-explosions, and the chapter concludes with a brief description of the nature of nuclear warfare.

11. *Chapter VI* sets out our assessment of the hazards of ionizing radiation on the basis of the evidence put forward in the earlier chapters and proceeds to a discussion of the dangers from radiation in peace and war.

12. *Chapter VII* contains a summary of our report which is followed by a statement of our conclusions.

13. The highly technical nature of this report has made it necessary to devote some space in each chapter to a description of generally accepted scientific theory in terms comprehensible to the general reader. No attempt has been made to prepare a bibliography for these parts of the report. Where new material has been drawn upon or controversial topics are discussed, the evidence has been set out in greater detail in appendices, to which lists of selected references to published work have been attached.

14. It will be evident to any reader of this report that, at the present time, there are many large and serious gaps in our knowledge of the medical and biological effects of ionizing radiation. If the potentialities for good are to be exploited with confidence and safety, it is necessary that these gaps should be filled. Much research on many broad fronts will be required. Given the necessary facilities, there is no reason to doubt that the information can be obtained; and we attach the greatest importance to the recommendations for future work that we have been invited to submit for the consideration of the Medical Research Council.

CHAPTER II

THE NATURE OF RADIATION AND ITS ACTION ON LIVING CELLS

Introduction

Discovery of X-rays

15. The transference of energy from sun to earth by radiant heat and light was already well recognised when in 1895 the discovery of X-rays revealed something quite novel—namely rays which had the power of penetrating normally opaque objects. The power of penetration varied in relation to different tissues of the body, and this variation enabled shadow pictures of internal structures to be seen, and so laid the foundation of the first great use of the new discovery. Almost by accident it came to be recognised in the next few years that part of the radiation was absorbed in the tissues, with the production of physico-chemical changes which could lead to biological damage.

Discovery of natural radioactivity

16. The production of X-rays was soon followed by the discovery of natural radioactivity. It was found that compounds of certain heavy elements in the earth's crust, such as uranium and radium, spontaneously emitted rays which had similar properties to X-radiation, although they were of different penetrating power. Later, rays were identified which reached the earth from outer space and these were named 'cosmic rays' *.

Disintegration of radioactive elements

17. The radiation emitted from radioactive elements is due to spontaneous disintegration of their atoms, with the production of one or more types of radiation and of a new element lighter in weight than the original one. A radioactive element, such as radium, may be regarded as a population of atoms, each of which has a length of life ending in spontaneous break-up of the atomic nucleus with emission of radiation, partly in the form of a stream of particles and partly as wave-propagated radiant energy. In this way the amount of radium gradually diminishes. While the moment of disintegration of any particular atom is unpredictable, the rate of decay of the population of atoms as a whole follows a strictly constant rule. Thus, any group of radium atoms decays to half its original number in a period of about 1,600 years. This period, called the half-life, varies widely for different radioactive substances but for each it is constant and characteristic. Sometimes the new element formed by atomic disintegration is itself unstable and a cascade of successive disintegrations occurs, each with the emission of one or more types of radiation, until finally a stable non-radioactive substance is formed.

Radioactive isotopes

18. After much pioneer work, dating from Rutherford's experiments as early as 1919, it was found possible in the nineteen-thirties to turn many normally stable elements into unstable versions of their original form, by treating their nuclei with suitably energetic radiations, and so to produce artificial radioactive substances. At first it was only possible to make these

* For a description of cosmic rays see paragraphs 192–193.

artificial radioactive substances under very special conditions and on a very small scale. The expansion of nuclear research during the last war, however, led to the development of the nuclear reactor, by means of which it became possible to create radioactive substances in very large quantities either directly or as by-products of the fission* processes in the reactor itself. Both natural and artificial radioactive substances are now commonly called radio-isotopes.

Types of Radiation and Units of Measurement

19. Several different kinds of penetrating radiation are known, of which the common types are the following:

alpha particles: These are the nuclei of helium atoms and are swiftly moving particles of high energy, carrying a positive electric charge. They have little power of penetration, passing into soft tissues for only small fractions of a millimetre, and irradiation of the body from outside with alpha particles is consequently of little significance. They may, however, affect living tissues when they arise from radioactive materials actually within the body, and, in sufficient quantity, they are then biologically very destructive.

beta particles: These are fast-moving energy-carrying particles (electrons) of very small mass with a negative charge. The amount of energy that they carry may vary considerably and their power of penetration will vary accordingly. In general, beta particles are more penetrating than alpha particles and can traverse distances of up to about a centimetre in soft tissues. For this reason they are valuable therapeutically, and radioactive substances emitting beta radiations are used for the destruction of superficial tumours. For the same reason heavy doses from outside the body can damage the superficial tissues and, if beta-emitting substances are ingested, destructive effects within the body may be produced.

gamma rays: These are electro-magnetic radiations of high energy emitted by atomic nuclei. Like alpha and beta particles they are produced in the process of natural or artificially induced atomic disintegration. Gamma rays have great penetrating powers in comparison with alpha and beta particles and the more energetic gamma rays can traverse the whole body with relatively little absorption. As a result, almost the whole thickness of the body may be irradiated by gamma radiations and this is a deciding factor in producing the general illnesses which may follow this type of irradiation. The properties of gamma rays are essentially similar to those of X-radiations but in general gamma-rays have an energy and penetrating power corresponding to the more penetrating X-rays produced at such extremely high voltages as several million volts.

X-rays: These are similar wave-propagated radiations, which are usually produced artificially by electrical machines and which are widely used both diagnostically and therapeutically in medical radiology. They vary considerably in their penetrating power, according to the electrical energy used in their production. The biological effects of X-rays are brought about by high-energy electrons, which are liberated in the tissues during the passage of the rays, so that the biological action of X-rays and beta particles is essentially the same.

neutrons: These are normal constituents of atomic nuclei but may be liberated with considerable energy. They carry no electric charge and

***Fission:* The splitting of the nucleus of a heavy atom into two roughly equal parts with the release of a large amount of energy.

are therefore not repelled by the charged nuclei of atoms, but enter into them to build up unstable structures which often disintegrate with the production of artificial radioactivity. Fast neutrons act chiefly by collision with the hydrogen of the water and of the other compounds which the tissues contain, the resulting 'recoil hydrogen nuclei' somewhat resembling alpha particles in their action. The initially fast neutrons are gradually slowed down in the tissues and may then bring about biological effects by interaction particularly with nitrogen. They may also be captured by hydrogen, thereby releasing energetic gamma rays.

Ionization

20. These several types of radiation vary not only in their powers of penetration but also in relation to the number of electrically charged atoms and molecules, called ions, that they leave in their tracks as they pass through tissues. For this reason they are collectively known as ionizing radiations. It is the production of electrically charged particles, or ions, which is mainly responsible for initiating the physico-chemical changes in living tissue that lead ultimately to the production of overt radiation damage. The efficiency of a given dose of radiations in producing biological effects can be related to the numbers of ions produced per unit length of track.

Intensity of dose and length of exposure

21. The biological effects of radiation are closely related to the dose, or quantity, of radiation received. An analogy can be drawn with the effects of ultra-violet light on the skin in producing sunburn. It is well known that these effects depend on two main factors, the brightness of the sunshine and the length of the exposure to it. Similarly, the effects of radiations such as gamma rays, X-rays or beta rays are determined by the same two factors, the intensity of the radiation and the period of exposure. Radiation may be regarded as consisting of small units of energy called 'quanta', and the intensity of the beam of a given kind of X- or gamma rays is simply a measure of how many such quanta are striking a particular area in a given time. The dose of radiation might therefore be described as the energy which is absorbed in the small mass of tissue upon which the radiation impinges. Living tissues, however, are not inert. After some types of damage by radiation, repair processes take place and the rate at which the dose of radiation is given becomes an important factor in determining the observed biological effect. Thus, if a dose of radiation is spread out over a very long time, for example many years, the response may be very much smaller than or even quite different from that which would occur if the same amount of radiation were given in a very short time. On the other hand, with some well known forms of biological damage produced by ionizing radiations, recovery does not occur. The production of gene mutation is perhaps the most important example of this latter type of change.

Measurement of dose: the roentgen

22. The difficulty of making satisfactory measurements of the dose of radiation has been overcome by making use of the changes of electrical conductivity which are brought about in air when ionizing radiations pass through it. This particular method of measurement is used, not only because it is technically convenient but also because the atomic composition of air or water is in this respect essentially similar to that of the body, the

absorption of X-rays taking account only of the kind of atoms present and not of their particular chemical combinations. The unit of dose which has hitherto been adopted internationally is called the roentgen,* which is abbreviated to the letter 'r'; for very small quantities of radiation the milliroentgen (0·001 r) is often used as the unit. The intensity of radiation to which we are ordinarily exposed from our natural surroundings is about 0·1 r per year.

Measurement of radioactivity: the curie

23. It has been seen that the biological effects of radiation depend upon the amount of energy which is absorbed in the tissues. At each radioactive disintegration, an atom emits a certain amount of energy in the form of high-speed particles and gamma rays. The total rate at which the tissues are irradiated therefore depends on how many atoms disintegrate per second. In considering the effects of radioactive materials actually within the tissues, use is accordingly made of units of radioactivity which depend on the number of atomic disintegrations per second. Based originally upon the rate of disintegration of radium, the unit of radioactivity is called the curie and represents the amount of an element in which $3{\cdot}7 \times 10^{10}$ disintegrations occur per second. This is too large an amount of radioactivity for most biological work and it is customary to measure the amounts of radioactivity in the body in microcuries (millionths of a curie). For some radioactive materials the maximum amounts which can be allowed in the body are of the order of only one microcurie or less; but even this very small amount of material corresponds to many thousands of atomic disintegrations per second.

Relative biological efficiency

24. The destructiveness of the different types of radiation can also be expressed in relation to that of an equivalent amount, in terms of energy, of gamma rays. This measure is known as the relative biological efficiency (R.B.E.) and varies between different tissues.

Action on Living Tissues

25. The basis of the biological action of ionizing radiation is not fully understood. The consensus of opinion is that radiation acts primarily upon the cell and its constituents, and upon the complex chemical processes occurring in these, rather than upon the fluids in which the cell is bathed. It is thought that the processes associated with the formation of ions during the passage of radiation lead to changes in some of the highly organised molecular systems within the cell. These changes are probably brought about by highly reactive chemical intermediates liberated within the cell subsequent to the physical process of ionization.

Effect of radiation on organisms, tissues and cells

26. All living tissue is killed if exposed to large enough doses of radiation. Different types of organisms, tissues and cells, however, vary greatly in the amount of radiation which they can withstand. Among mammals the dose of X-rays to the whole body which will kill 50 per cent of an animal population varies from 200 to 1000 r—depending on the species; for man it is thought to be between 400 and 500 r. There is also a wide variation in sensitivity between different animal tissues. For instance, in man the most sensitive tissues include the lymphatic glands, the epithelium of the small

* The roentgen shall be the quantity of X- or gamma radiation such that the associated corpuscular emission per 0.001293 gramme of air produces, in air, ions carrying 1 electrostatic unit of quantity of electricity of either sign.

bowel, and the precursors of the blood cells which are situated in the bone marrow, whereas adult nerve and muscle tissue are comparatively insensitive. Variations in sensitivity also occur at different stages in the life cycle of a cell; for example, cells about to divide are often more sensitive than those in the resting state.

Repair processes

27. At dose levels below those which damage tissue irretrievably, the situation is modified by processes of repair; but a distinction must be drawn between true recovery, in which the damaged cells return to normal form and function, and the replacement of injured cells by those coming from outside the field of radiation. The latter is the more conspicuous form of repair after heavy radiation damage in the higher animals and leads to the original tissue being replaced by simpler unspecialised material or scar tissue. Repair processes within the individual cell are little understood and still largely a matter of speculation, but they must play an important part after low doses.

CHAPTER III

THE EFFECTS OF RADIATION ON THE HEALTH OF THE INDIVIDUAL

Introduction

28. Experience of the effects of ionizing radiations has been accumulating with increasing rapidity since the benefits which they may produce in the treatment of malignant disease first began to be appreciated. This experience has been limited, in the main, to the effects produced by the relatively large doses which it is often necessary to give to the area of the body under treatment. More recently, knowledge of the effects of very large doses to the whole body has been obtained as a result of the atomic bomb explosions in Japan. In this chapter it will be necessary to draw heavily on the information from these two sources in considering both the acute and the long-term effects of exposure to radiation, but the reader must bear in mind that such information is only indirectly relevant to the circumstances of ordinary civilian life, since doses of this magnitude would only be conceivable in the immediate vicinity of an accident in a nuclear reactor.

29. There is much less information about the possible effects of chronic exposure to very low doses of radiation, such as those to which special groups of workers may be exposed in the course of their occupations. At a time like the present, when nuclear energy is being intensively developed for civil use, the importance of obtaining such information cannot be exaggerated. The investigation which we have sponsored on leukaemia was undertaken in an attempt to obtain information on the relationship between the size of the dose of radiation and the incidence of the disease among patients with ankylosing spondylitis, so that conclusions might be drawn about the effects of lower doses. The investigation must be regarded, however, as only the first step towards this goal.

Sources of Information

30. Information about the effects of radiation on man has been derived from four main sources: radiotherapeutic experience; occupational experience, including that from accidents; experience from atomic bomb explosions; and animal studies.

31. *Radiotherapeutic experience.* Both X-rays and the gamma rays of radium have been used for many years in the treatment of disease, mainly in the treatment of cancer. Observation of patients receiving radiotherapy has yielded information on the general effects of radiation and on the effects produced in different tissues; and the therapeutic use of radioactive isotopes has provided data on the effects of radioactivity within the body.

32. *Occupational experience.* Information on the occupational hazards of radiation has been obtained from studies of three groups of workers: medical radiological workers, painters of luminous dials for watches and clocks, and miners working radioactive ores in the Schneeberg mines in Saxony and in Joachimsthal. The experience of these three groups serves to illustrate three different forms of radiation hazard. The radiological workers were exposed mainly to external irradiation by X- and gamma rays, and some developed

skin damage leading to skin cancer, or bone-marrow damage leading to severe diseases of the blood. The luminous-dial painters ingested paint containing the naturally-occurring radioactive elements radium, mesothorium and radiothorium, which are retained within the skeleton, and some developed bone tumours. The miners of Joachimsthal and Schneeberg worked in an atmosphere containing high concentrations of the radioactive gas, radon, and many developed lung cancer. The study of these three different hazards has contributed greatly to our knowledge of the harmful effects of radiation, and has provided data for the formulation of safety standards.

33. *Atomic bomb experience.* The atomic bomb explosions over Hiroshima and Nagasaki brought widespread destruction to these cities. Blast and fire caused most of the casualties, but about 15 to 20 per cent were caused by the gamma and neutron radiations emitted during the explosions. In 1946 the United States established in Japan the Atomic Bomb Casualty Commission, which has studied the immediate and the long-term effects of radiation from the bombs on the populations of both cities; the findings have been of great value in expanding knowledge on this subject.

34. *Analogous effects produced in animals.* The discovery that X-rays could produce changes in human tissues led investigators to study the effects of radiation on animals. As a result, it was established that radiation produces effects in animals similar to those observed in man and it thus became possible to make an experimental approach to the problem of radiation hazards. The knowledge thus gained has been drawn on freely in this report.

Factors Affecting the Severity of Radiation Injury

35. The harmful effects of radiation can be divided into those developing within a few weeks of exposure and those developing some considerable time, perhaps many years, afterwards. Illnesses which develop within a few weeks are sudden in onset and run an acute course, whereas those occurring some years after exposure develop insidiously.

36. The severity of radiation injury in any particular instance is determined by the interplay of several factors: the type and dose of radiation received, the duration of the period of exposure, the extent and part of the body which has been irradiated, and also the age of the person exposed.

The dose of radiation received

37. If the dose of radiation is a large one and is received by the whole body in the space of a few minutes, a severe and possibly fatal illness is likely to develop within a few hours, and certainly within a few weeks, of exposure. Some of those who survive this early illness may die several years later from one of the delayed effects of radiation, such as anaemia or leukaemia. Exposure of the whole body to smaller doses of radiation, over a period of months or years, will not cause the early illness, but there may still be a slightly increased risk of death from the delayed effects in later years.

The extent of the body irradiated

38. On the other hand, if only a fraction of the whole body is irradiated, as in radiotherapy, immediate general effects are rare, although some patients may develop a mild form of the early illness, known as radiation sickness. It is often necessary to give a large dose locally and there may be local reaction in the irradiated area with temporary reddening of the skin or blistering similar to that which occurs in sunburn. Delayed local effects

that may occur in these patients are scarring, less commonly necrosis and rarely the later development of cancer in the irradiated tissues. It is now apparent that there may be delayed general effects, a small proportion of patients in later years developing anaemia or leukaemia.

The part of the body irradiated

39. Experience has shown that there is a difference in the general effects of radiation according to the part of the body which is irradiated. Even quite a large dose of radiation given to a portion of a limb will usually produce no general ill effects whereas a similar dose directed to an equally large volume of tissue in the upper abdomen, for example, may produce severe immediate illness.

The type of radiation

40. The severity of effect produced by radiation may also depend to some extent upon the type of radiation concerned, since radiations differ in their powers of penetration and in their destructive effects. For example, fast neutrons are about ten times more potent than X- or gamma rays in causing cataract in the lens of the eye, although these three forms of radiation differ very little in their capacity to cause the early acute form of illness.

The age of the individual exposed

41. It has long been known to radiotherapists that young children are more likely than adults to develop reactions after irradiation. Further evidence on this point comes from a recent report on the inhabitants of the Marshall Islands, who were exposed to radioactive fall-out after the thermonuclear test explosion in that area of the Pacific Ocean in the spring of 1954. A consistently greater fall in the number of white corpuscles in the blood occurred among children than among adults, and a similar age-difference in response was noted also in regard to loss of hair.

The frequency of radiation effects

42. Reasonably good estimates have been made of the numbers likely to develop the acute illness under varying conditions of whole-body irradiation. Thus, every member of a population receiving a single whole-body dose of 500 r of gamma-rays would become ill shortly afterwards; if the dose were 150 r, only about half would do so; and, if it were of the order of 50 r, sickness would be extremely rare. It is much more difficult to assess the proportion likely to suffer from the delayed effects; all that can be said with certainty is that it would be small.

The Early Effects of Exposure to Radiation

43. The following description of the effects of a single heavy dose of gamma rays to the whole body is based on observation of the bomb-victims in Nagasaki and Hiroshima. It must be repeated that, in peacetime, exposure at this level could result only from an accident which would rarely, if ever, occur and that, even then, only those in the immediate vicinity of the disaster would be affected.

Effects of heavy dosage

44. The first effect of exposure of the whole body to a heavy dose of gamma rays of the order of 500 r is a sensation of nausea developing suddenly and soon followed by vomiting and sometimes by diarrhoea. In some people, these symptoms develop within half an hour of exposure; in others, they may not appear for several hours. Usually, they disappear after two to three days. In a small proportion of cases, however, the symptoms

persist; vomiting and diarrhoea increase in intensity; exhaustion, fever, and perhaps delirium follow; and death may occur a week or so after exposure.

45. Those who recover from the phase of sickness and diarrhoea may feel fairly well, although examination of the blood will reveal a fall in the number of white cells. Between the second and fourth weeks, however, a new series of ailments, preceded by gradually increasing malaise, will appear in some of those exposed. The first sign of these developments is likely to be partial or complete loss of hair. Then, from about the third week onwards, small haemorrhages will be noticed in the skin and in the mucous membranes of the mouth, which will be associated with a tendency to bruise easily and to bleed from the gums. At the same time, ulcerations will develop in the mouth and throat, and similar ulceration occurring in the bowels will cause a renewal of the diarrhoea. Soon, the patient will be gravely ill, with complete loss of appetite, loss of weight, and sustained high fever. Feeding by mouth will become impossible, and healing wounds will break down and become infected.

46. At this stage, the number of red cells in the blood is below normal, and this anaemia will increase progressively until the fourth or fifth week after exposure. The fall in the number of white blood cells, noted during the first two days after exposure, will have progressed during the intervening symptomless period, and will by now be reaching its full extent. The changes in the blood-count seriously impair the ability to combat infection, and evidence from Nagasaki and Hiroshima shows that infections of all kinds were rife among the victims of the bomb. Many of those affected die at this stage and, in those who survive, recovery may be slow and convalescence prolonged; even when recovery appears to be established, death may occur suddenly from an infection which in a healthy person would have only trivial results.

Effects of lighter dosage

47. The radiation effects described above are the most severe which can follow a single whole-body dose of 500 r of gamma rays and still allow some hope of survival; but at least half of a population so exposed would die. With smaller doses, fewer people would develop symptoms and the illness would become correspondingly less severe; thus, with a dose of 100 r, not more than about 15 per cent of the exposed population would be affected, the illness would be comparatively mild, and very few, if any, would die.

Effects of exposure to 'fall-out' in the vicinity of an explosion

48. The radiations considered above have been those occurring within one minute of the detonation of a nuclear weapon. These radiations have been called the 'prompt' radiations to distinguish them from those emitted by the radioactive dust, or fall-out, which settles over a wide area in the vicinity of an explosion. The fall-out may itself be active enough to cause radiation illness of a type similar to that described above and, in addition, it may contaminate and damage the skin with which it comes in contact.

49. Following the firing of a thermonuclear weapon in the region of the Marshall Islands, the fall-out on one island was so heavy that it was compared to snow, and the inhabitants received an estimated average whole-body dose of 175 r. This fall-out did not cause any deaths, but it did produce a mild illness with early sickness and diarrhoea, a fall in the number of cells in the blood, loss of hair, and some ulceration of the skin contaminated by radioactive material. The skin lesions, caused largely by the higher local

dose of beta-radiation emitted by the fall-out, appeared about two weeks after exposure on those parts of the body which had not been protected by clothing, and took the form of spotted areas of increased pigmentation, from most of which the skin peeled off as if it had been sunburnt. In about 20 per cent of cases, deeper ulceration of the skin occurred but all wounds healed satisfactorily.

Relationship between dose and incidence of effects

50. For the purposes of assessing risk and defining standards of safety, it is necessary to know the nature of the relationship between the dose of radiation and the effect induced. This relationship may be a simple linear one in which the incidence of the particular disease increases strictly in proportion to the dose received, or it may be a curvilinear one in which, with each successive and equal increment in dose, the incidence increases not by an equal but by a progressively greater amount. All the evidence suggests that the relation between dosage and radiation effects occurring within a few weeks of exposure is of the latter type, and that the curve shows a 'threshold' level, implying that a certain quantity of radiation must be exceeded before these particular effects are produced.

The Delayed Effects of Exposure to Radiation

51. Delayed effects of radiation which have been observed locally in tissues heavily irradiated are atrophy and fibrosis of the skin and underlying soft tissues, and sudden breakdown or necrosis of tissues such as bone and cartilage. In rare instances cancer has subsequently developed in the damaged tissues. Cataract has occurred if the lens of the eye has been irradiated. The delayed general effects of radiation which are known, are the development of severe anaemias and leukaemia; in addition, evidence is beginning to accumulate from observations made on animals that irradiation may cause some shortening of the normal life-span. In our report we have dealt in considerable detail with leukaemia, because experience in Japan following the atomic bomb explosions in 1945, and the results of our own investigation on the incidence of leukaemia among irradiated patients, have provided more precise information on the effects of different levels of exposure than is available for any other of the delayed effects.

52. The knowledge that long-term effects may be produced by radiation is in itself an insufficient basis for assessing the risk that any of them will develop as a result of a particular dose. For this purpose, it is necessary to estimate, from national mortality statistics, the incidence of the condition in the absence of exposure to radiation additional to that from natural sources, and then to compare this figure with the incidence of the same condition in a population that has been exposed to radiation. If an increase is demonstrated, the frequency with which the condition develops at different levels of radiation dose must be determined, and the relationship between the dose and the incidence of the disease must be evaluated. Only then is it possible to assess the hazards, if any, associated with the different uses of radiation.

INDUCTION OF LEUKAEMIA

53. Leukaemia is a disease in which uncontrolled over-production of the white blood-cells occurs. It is at present invariably fatal, although some forms may run a protracted course over many years. Several kinds of leukaemia are described according to the type of cell mainly affected. Usually, there is an increase in the number of the affected cells in the blood, associated with the appearance of immature forms of the cell in question. In some cases, however, the numbers in the blood may fall below normal

through failure to liberate the cells from their site of formation in the bone marrow; the disease is then known as aleukaemic leukaemia.

54. In many countries the death rates from leukaemia have shown a steady rise in recent years. In 1920 the crude annual death rate from this condition for both sexes in England and Wales, for example, was 11 per million persons; in 1954 it was 49 per million. Some of this rise has undoubtedly been due to an improvement in diagnosis but it seems probable that this is not the whole explanation and that, for a reason as yet unknown, there has been a real increase in the national death rate from leukaemia.

55. It is known that leukaemia may be induced in animals as a result of exposure to radiation. Case reports have appeared from time to time of patients who have developed leukaemia after exposure to radiation for the treatment of various diseases, and there have also been a number of reports of radiologists dying from leukaemia. Such isolated reports do not of themselves prove that the relationship is one of cause and effect, but the matter has now been put beyond doubt by a series of recent observations on the incidence of leukaemia under conditions in which an estimate could be made of the degree of exposure to radiation.

Leukaemia following a single exposure: atomic bomb explosions

56. The most recent information, for which we have to thank the United States National Research Council, covers all cases of leukaemia recorded by the Atomic Bomb Casualty Commission in Nagasaki and Hiroshima between January, 1947, and August, 1955. Vital statistics allow an estimate to be made of the number of cases of leukaemia that would have been expected to occur over a similar period in a Japanese population not exposed to radiation from the bombs but otherwise comparable to the surviving populations of Nagasaki and Hiroshima. Calculations have been made for the combined totals of the survivors of the explosions in both cities (Appendix A).

57. During the eight years from 1947 to 1954, about 25 deaths from leukaemia would have been expected in an unexposed Japanese population of the same size and having the same age and sex distribution as the combined populations of survivors from both cities. Over the same period, however, 91 proven and 14 suspected cases have been recorded among those present at the time of the explosion and still resident in one or other city at the time of diagnosis. The difference between the expected and the observed number of cases is so great that it is most unlikely to be due to chance.

58. The difference between the numbers expected and those observed becomes even greater if the most heavily irradiated survivors are considered separately. Only for Hiroshima are adequate details available of the distances from the centre of the explosion at which the individual survivors had been exposed. In the absence of radiation, it is unlikely that even one case would have occurred among the number of survivors less than 1,000 metres distant, yet 15 cases have been found. Further, there is a much higher incidence among those who developed the early acute illness than among those who had, at the most, only mild symptoms.

59. An examination of the incidence of leukaemia in relation to the distance from the explosion has been made for the survivors in Hiroshima, where the concentric distribution of the radiation was not affected to the same degree as in Nagasaki by the irregular distribution of the radio-active fall-out. The dose from the prompt radiation decreases as the distance from the explosion increases. In survivors who were 2,000 metres distant

or more, the incidence during the period January, 1947, to August, 1955, was about 2 cases in every 10,000 persons. Among those between 1,500 and 2,000 metres distant, the incidence was about 3 to 4 cases per 10,000 persons, and for those at the shorter distances of between 1,000 and 1,500 metres and under 1,000 metres it was respectively about 28 and 128.

60. To make an accurate estimate of the relationship between the dose of radiation and the incidence of the disease, one would have to substitute doses expressed in roentgen units for the distances from the centre of the explosion. It has not been possible to obtain reliable estimates of these doses, which should include not only the contribution from the gamma rays but also that from the neutrons emitted by the explosion and that from the radioactive fall-out. Tentative estimates of the gamma ray dose received by people standing in the open can be made from the information published in 1950 by the United States authorities in 'The Effects of Atomic Weapons'. These estimates suggest that the dose at under 1,000 metres would not be less than 1,400r, and at 1,250 metres about 350r. At 1,750 metres it would be about 50r, and at 2,000 metres about 8r. As a dose of 1,400r or more would kill everyone exposed to it, survivors who were within 1,000 metres of the explosion must have been heavily protected. An unknown proportion of the survivors at all the other distances must also have been protected to some extent because they were either indoors or, if outside, shielded by buildings. For this reason, it is not possible to indicate with any great confidence the average levels of dose received by survivors at different distances from the bomb and, in view of the uncertainty about the actual doses received by the exposed population, one cannot infer with certainty whether the relationship between dose and the incidence of leukaemia is a curvilinear or a linear one.

61. For the Japanese cases which occurred up to the end of 1954, the average length of the period between exposure to the bomb and the first appearance of symptoms was about 6 years. It is clearly important to determine whether there has been any tendency for cases to occur less frequently in subsequent years. The morbidity rate has therefore been examined year by year in both Hiroshima and Nagasaki, and it has been found that the recorded incidence has remained approximately constant in Hiroshima in the period 1948 to 1954, and in Nagasaki in the period 1950 to 1954 (Appendix A). This finding suggests that there is no sharply-defined peak year of occurrence, but that with this type of exposure the incidence of leukaemia rises, after a variable latent period, and then remains approximately constant up to at least the ninth year.

Leukaemia following repeated exposures: radiotherapy

62. Before 1955, there had been a report of leukaemia developing in two patients given X-ray treatment for ankylosing spondylitis. In 1955, two further publications directed attention to this possibility, and another reported the occurrence of leukaemia in young children who had been given X-ray treatment to the chest in infancy for suspected enlargement of the thymus gland. In an attempt to obtain further evidence on the occurrence of leukaemia as a delayed effect of irradiation, and in particular on the relationship between the dose received and the incidence of the disease, we have sponsored a survey of patients treated for ankylosing spondylitis with radiation.

63. Ankylosing spondylitis is a disease which affects chiefly the joints of the spine, and to a less extent other joints, particularly those of the pelvis and the shoulders. It usually starts in early adult life and is about six times more frequent in men than in women. It causes severe pain and reduced mobility and, unless treatment is given, the affected joints may gradually lose their freedom of movement and the back become progressively stiffer. In

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severe cases all spinal movement is lost, chest expansion is greatly diminished, and the movements of other major joints restricted. The popular description, 'poker back', is a very apt one.

64. Some patients with this condition are benefited by X-ray treatment, which is given to relieve pain and increase mobility and which may permanently halt the progress of the disease. As treatment usually takes the form of irradiation of the whole spine in one course of radiotherapy, it involves exposing a large section of the body directly to the X-rays. In some patients one course of treatment does not suffice, and further courses have to be given, either to the spine or to the major joints, or to both. Indeed, this group of patients was chosen for our investigation because the treatment is so extensive that it more nearly approaches whole-body irradiation than that given for any other non-malignant condition.

65. An analysis has been made of the hospital records of between 13,000 and 14,000 patients, all of whom had been treated with X-rays at some time during the twenty-year period 1935 to 1954. Thirty-eight of these patients developed leukaemia, an incidence of only about one-third of one per cent; yet calculations based on the national death rates over the same period show that even this low incidence is about ten times greater than would have been expected in the absence of irradiation. The possibility of such a difference being due to chance is so remote that we shall ignore it.

66. Caution is necessary, however, in interpreting this finding. It is not possible to conclude immediately that the increased number of deaths from leukaemia is related to the X-ray treatment, in the way that the increased death rates among previously healthy people in Hiroshima and Nagasaki can be attributed to exposure to the radiations from the bombs. The possibility has to be considered that death from leukaemia would, even in the absence of treatment by irradiation, be a more frequent occurrence among sufferers from ankylosing spondylitis than among the normal population, or alternatively that ankylosing spondylitis in some way increases a patient's susceptibility to irradiation.

67. By courtesy of the Ministry of Pensions and National Insurance, it has been possible to examine the records of a group of about 400 male patients with ankylosing spondylitis who had never at any time been treated with X-rays. The fact that no increased incidence of leukaemia was found in this group suggests that ankylosing spondylitis does not of itself predispose a patient to the development of leukaemia. To confirm this point, it would be necessary to examine the records of a much larger group of unirradiated patients; X-ray treatment is, however, so widely used for ankylosing spondylitis that it may be difficult to do this.

68. Clear evidence was, however, found in our main investigation for the existence of a relationship between the dose of radiation and the incidence of leukaemia. The dose was estimated in two different ways, firstly by calculating the total amount of energy absorbed in the whole body, and secondly by calculating the dose of radiation received in certain parts of the bone marrow. The first method demonstrated a curvilinear relationship between the incidence of leukaemia and the radiation dose, whereas the second method resulted in a linear relationship. Fortunately, over the range of doses likely to be met with in ordinary civil conditions, the difference between the two results is negligible. The theoretical implications of the two possible relationships are, however, very different and important and point the way to considerable future research. The data upon which the findings are based are summarised in Appendix B.

69. The average length of time between the first exposure to X-rays and the diagnosis of leukaemia was about six years. This period cannot be directly compared with that observed in the Japanese cases, as many of the patients had had several courses of radiation before leukaemia was diagnosed, and it is not known which particular course was the effective one or whether all the courses may not, to some degree, have affected the development of the disease. Nevertheless, it may be concluded from both series of cases that the latent period for radiation-induced leukaemia is shorter than for radiation-induced cancers.

Leukaemia following chronic exposure

70. We have no precise knowledge of the incidence of leukaemia under conditions of chronic exposure. It has been reported that, relative to the numbers at risk, there are about nine times as many deaths from leukaemia among American radiologists as among other American physicians. This figure is based on a study of the obituary notices published in the Journal of the American Medical Association from 1929 to 1948, in which both the professional occupation and the cause of death are usually reported. In about a quarter of the notices, however, the cause of death was not reported and thus a bias may have been introduced into the results of the study. A review of all the published papers on this subject shows that there may well be an increased death rate from leukaemia among American physicians as a whole, compared with the general population, and in particular among American radiologists, but it is not possible to estimate the extent of the increase with any certainty.

General conclusions on the induction of leukaemia

71. The results of the investigations carried out by the Atomic Bomb Casualty Commission in Japan, and of our own study of the occurrence of leukaemia in patients with ankylosing spondylitis, leave no doubt that ionizing radiations can induce leukaemia in man, and that the average latent period between exposure and the development of the disease is only a few years. In neither of these situations were the conditions of exposure similar to those of persons engaged in work associated with a possible radiation hazard. Those exposed occupationally tend to receive radiation in small doses over long periods, and it is not yet known whether the dose-response relationship based on short periods of heavy exposure is directly applicable to such conditions.

INDUCTION OF CANCERS

72. The evidence for the induction of cancers by radiation consists chiefly of reports of the occurrence of cases under circumstances which make it reasonable to suppose that some at least were radiation-induced, and of the apparently increased frequency of a particular type of cancer, itself rare in the normal population, in persons exposed to heavy doses of radiation. Most of the information comes from the case-records of patients treated with radiotherapy and from those of workers in certain special occupations who in the past received very heavy doses of radiation in the course of their work. It is noteworthy that tumours following radiotherapy tend to develop in tissue already severely damaged by radiation, and that, compared with leukaemia, a much longer period—up to 20 years or more—usually elapses between the first exposure to radiation and the clinical appearance of the disease.

Cancer of the lung

73. The mines of Schneeberg and Joachimsthal are rich in a variety of ores and, since the latter part of the last century, pitchblende, an ore containing radium and other radioactive elements, has been extensively worked there. It had long been known that the miners were liable to die in middle-life from a respiratory disease locally named 'mountain sickness'. It is now recognised that this condition is one of cancer of the lung and it is generally accepted that there is a strong connexion between the excessive mortality from this disease and the high radioactive content of the air of the mines. Investigations have suggested that, up to 1939, nearly one-half of the miners who had died had contracted lung cancer.

74. The first decay-product of radium is a gas, radon, which in its turn disintegrates, giving rise to a series of products, all of which are solids. Radon, being a gas, diffuses through the rocks containing the radium ore, and escapes into the atmosphere of the mines. The inhalation of radon is known to constitute a serious hazard, and the International Commission on Radiological Protection has advised that the concentration of this gas in the inspired air should not exceed 0·0001 microcuries per litre. A series of measurements of the radon content of the air of the mines, made between 1924 and 1939, showed that the concentration of radon must then have been on the average about thirty times greater than the maximum permissible level since laid down. The serious hazard incurred in breathing such an atmosphere comes, not only from the radon itself but also from its solid daughter-products which, being attached to dust particles in the atmosphere, may be retained in the chest and may irradiate the tissues of the lungs for long periods.

75. The average latent period for the induction of lung cancer in these miners was about 17 years, and calculations have shown that the dose to the lungs during this period would have been equivalent to about 1,000 r. This calculation assumes that the radiation dose is spread evenly over the lungs, but it may well be that some areas of the lung, depending on the sizes of the radioactively-charged dust particles which are inhaled, may be subjected to doses of more than 10,000 r over a whole working life. It is consistent with other knowledge that tumours could be induced under these conditions, particularly when it is remembered that radium itself and many of its daughter-products emit alpha particles with high biological efficiency.

76. The only conditions in which an increased incidence of lung tumours has been observed in association with radiation are those in which there is an increased risk of inhaling radon and the other daughter-products of radium. In theory, however, the inhalation of radioactive material in particulate form, either as a result of fall-out from nuclear weapon explosions or in the vicinity of nuclear reactors, could lead to the accumulation of a high radiation dose within the lungs. Such particles would not be uniformly distributed within the lungs but would tend to aggregate on discrete small areas of the bronchi, which would thus be subjected to a high radiation dose, with the result that in the long run lung cancers might be produced in some people. In this country appropriate measures are always taken to eliminate the hazard in the vicinity of nuclear reactors, and it would be extremely unlikely to occur as a result of fall-out except in conditions of actual warfare. There is no evidence that external irradiation by X- or gamma rays can cause lung tumours in man.

Cancer of the bones and joints

77. Radium and the daughter-products of thorium, when assimilated into the body, tend to be held for long periods of time in the bones where, if

in sufficient concentration, they may give rise to local destruction and disease. A number of artificially produced radioactive isotopes, of which the most important are strontium and plutonium, also show this predilection for bone. Radioactive strontium exists in several forms, one, strontium 89, having a half-life of 53 days and another, strontium 90, of 28 years, while the half-life of plutonium 239 is about 24,000 years. A warning of the potential danger from these artificial elements is given by past experience of the effects of the natural elements radium, mesothorium and radiothorium after they have gained entrance to the body and become fixed in bone (Appendix N).

78. Our knowledge of these effects comes mainly from the case-records of former workers in the luminising industry and of a group of patients given radium compounds internally in the course of treatment. Stringent controls are now enforced in the luminising industry to protect the workers, and the prescription of radioactive substances for treatment has been controlled by legislation.

79. Since 1925 there have been many reports of illness and death occurring among a group of workers engaged in the painting of watch and clock dials with luminous paint, most of whom had been in the industry during the period from 1916 to 1924. Luminous paint is compounded of zinc sulphide and radium, and, formerly, varying mixtures of radium, mesothorium and radiothorium were also used. It was customary for dial painters to apply their paint with fine brushes, the points of which they 'tipped' between their lips before painting. In this way they swallowed radioactive material, some of which became lodged in the skeleton. If large amounts were swallowed, death sometimes occurred, within about three years, from severe anaemia, haemorrhages, and infections, particularly of the bones of the jaw. Those who had ingested smaller quantities of paint often developed cystic and necrotic changes in the bones which might cause 'rheumatic' pains or fractures. Occasionally, these changes progressed and cancer of the bones appeared. Such tumours usually developed more than fifteen years after the first exposure to the hazard.

80. Similar effects have occurred in patients given radium compounds internally for the treatment of mental disease or for various rheumatic and other affections, and in people who, for quasi-medicinal reasons, have consumed large amounts of 'radioactive water'. In animals strontium 90 has been shown to produce similar biological effects.

81. It is possible to estimate the amount of radium in the body of a living person, if there is good evidence that no other radioactive element is present in addition to the normal components of the body. Measurements carried out on those who have been exposed to unknown mixtures, such as luminous compounds, are difficult to interpret. So far, no person is known to have developed radiation-induced bone cancer who had less than 3·6 microcuries of radium in his body, unless either mesothorium or radiothorium was also present; the lowest radium content, in the presence of one or other of these elements, has been 0·52 microcurie at the time of appearance of the tumour. On the other hand, it seems certain that early non-cancerous cystic changes in bones have developed with a body-content of as little as 0·4 microcurie of radium alone. These amounts of radium are to be contrasted with the maximum permissible level for body radium, which, as laid down by the International Commission on Radiological Protection, is 0·1 microcurie.

82. Bone cancer has also been reported after the use of X-rays in the treatment of non-malignant bone tumours and some infections. Such cancers have occurred only after very heavy doses of radiation and have originated

in the area of the body treated. The risk of the development of bone cancer at the levels of X- or gamma radiation experienced under modern occupational conditions is insignificant.

Cancer of the skin

83. Cancer of the skin was the earliest form of radiation-induced tumour to be described in man. Radiation dermatitis of the hands, forearms and face was common among the early radiologists and radiological technicians, and cancer often occurred in the damaged skin. By 1911 no fewer than 54 cases had been described; the occurrence of these tumours diminished as radiologists learned to take the necessary precautions.

84. Since the early part of the century, records have accumulated of the occurrence of skin cancers following X-ray or radium treatment. In some instances, these tumours have followed the injudicious use of X-rays for mild skin affections, or even for the removal of facial hair. The latent periods have usually been long, ranging in a recently reported series of 13 cases from 12 to 56 years, with an average of 33 years. Although it is usually impossible to make any accurate retrospective assessment of the doses of radiation received, the severity of damage to the skin suggests that, in these cases, they must have been of the order of several thousands of roentgens.

Cancer of the thyroid gland, the pharynx and the larynx

85. A number of cases of cancer of the thyroid gland have been reported among children, some years after they had been given X-ray treatment for conditions including suspected enlargement of the thymus gland, bronchitis, infected tonsils and adenoids, and enlarged glands in the neck. In many instances, the children were less than one year old when irradiated. In a series of cases irradiated for suspected enlargement of the thymus gland, the average latent period between irradiation and the establishment of the diagnosis of cancer of the thyroid gland was only about 7 years. Perhaps the most important feature of these cases is the comparatively small dose of radiation responsible for induction of the tumour, in contrast to the large doses associated with the induction of cancer in adults; cancer of the thyroid gland has developed in a child after a recorded dose as low as 250 r. It is possible that hormonal factors may be involved in addition to the direct effect of irradiation.

86. A few reports have drawn attention to the development, many years later, of cancers of the pharynx and larynx in patients who have had X-ray treatment for such conditions as tuberculous glands of the neck. The latent period is long, averaging about 20 years, and periods of more than 30 years have been recorded. In most cases, the irradiation was given in the early days of radiotherapy and there is practically no information available about the size of the radiation doses that were employed.

EFFECTS ON THE BLOOD OTHER THAN LEUKAEMIA

87. Observations have shown that a fall in the numbers of red cells, white cells and platelets in the blood may occur in persons exposed to radiation in the course of their work. There is little direct information on the dose-response relationships, but it seems possible that, even with whole-body doses of gamma rays as low as 1 r per week, slight changes can occur in the white-cell count of especially susceptible people. Certainly, with doses much in excess of 1 r per week, a general depression occurs in the white blood cell count. A reduction in the numbers of red cells and platelets may occur at a later stage, and in some persons continued exposure may lead to severe degrees of anaemia.

Aplastic anaemia

88. If not detected in time, radiation-induced anaemias may endanger life, particularly when the red bone-marrow is itself so severely damaged that the red-cell deficiency cannot be made good by the production of new cells; this condition is known as 'aplastic anaemia'. The diagnosis is not easy to make, and the condition can easily be confused with aleukaemic leukaemia unless a full examination of the bone marrow is carried out. This diagnostic difficulty was encountered during the investigation of leukaemia among patients treated with X-rays for ankylosing spondylitis. Particular attention was paid to deaths reported as being due to aplastic anaemia but, when these cases were fully investigated, evidence was found that a number were, in fact, aleukaemic leukaemia; eventually, only four deaths could with any certainty be ascribed to aplastic anaemia out of a total of some 50 deaths from leukaemia, aplastic anaemia and allied diseases combined. Similarly, only six cases of aplastic anaemia were reported from Nagasaki, compared with over 40 cases of leukaemia in the same city. It seems clear, therefore, that aplastic anaemia is a rarer delayed effect of radiation than leukaemia.

INDUCTION OF CATARACT

89. The term 'cataract' implies an opacity in the normally transparent lens of the eye, varying from a tiny granule which does not cause any definite impairment of vision, and which may disappear, to a large plaque resulting in blindness. It has been known for some time that exposure of the eye to X-rays can lead to cataract formation, but the large doses which appear to be necessary for its induction are only likely to occur under very unusual conditions. For all practical purposes, therefore, the production of cataract by X-rays is not an occupational hazard, although it was discovered in 1948 that the condition had developed among a group of physicists exposed to neutron irradiation during the operation of a cyclotron.

90. In the following year there were reports from Japan of an increased incidence of cataract in the populations of Hiroshima and Nagasaki. The extent of the increase cannot be determined with precision, but it is significant that, of 98 cases of cataract among survivors of the Hiroshima explosion, 85 occurred in persons who were within 1,000 metres of the centre of the explosion and would thus have been subjected to neutron- as well as gamma-irradiation. Confirmatory evidence of the high dosage which they had received is provided by the fact that most of them had suffered epilation of the scalp and that two subsequently developed leukaemia.

EFFECTS ON THE SKIN OTHER THAN CANCER

91. In the paragraphs dealing with the induction of skin cancers by irradiation, it was noted that cancers develop mainly in skin which has been subjected to such heavy doses of radiation as to be obviously damaged. Most of our knowledge of the less serious delayed effects on the skin has been obtained from observation of the results of therapeutic irradiation with X-rays, during which the skin may be exposed to large doses of radiation directed to underlying tissues. With doses of 1,500 r or more, a certain amount of permanent skin-damage is likely to occur, but it will not be particularly severe unless a large area has been irradiated. Larger doses, however, say of 4,000 r or more, are often followed by obvious skin-damage, the texture becoming thinner, and the surface being usually covered with dilated blood vessels. In such cases, the skin may be very sensitive and prone to infection, and it is in this type of damaged skin that radiation-induced tumours are most likely to develop.

92. The hair follicles and glands of the skin may also be affected by radiation. A dose of the order of 300 to 400 r will cause temporary loss of hair, and with higher doses, perhaps 700 r or more, hair-loss may be permanent. It is a common finding that, owing to the destruction of the sweat glands, heavily irradiated skin permanently loses its ability to sweat. After doses of the order of 1,500 r, the sebaceous glands are destroyed and the skin loses its normal greasy texture.

EFFECTS ON THE KIDNEY AND LUNG

93. It has been reported that therapeutic doses of X-rays to the region of the kidneys may affect their function and lead to the development of high blood pressure which may prove fatal. The damage described has followed the treatment of certain rare tumours with large doses of radiation and it is unlikely that such effects will occur under other conditions of exposure. It has also been reported that pneumonitis, sometimes fatal, has followed radiotherapy directed towards the chest.

SHORTENING OF THE LIFE-SPAN

94. A number of reports based on observations made on animals suggest that exposure to ionizing radiations may lead to a reduction in the expectation of life. No evidence has yet been published that this occurs in man.

The Effects of Exposure to Radiation during Pregnancy

Abortion and stillbirth

95. After heavy doses of radiation, a pregnant woman may miscarry or give birth to a stillborn child. Information from the Atomic Bomb Casualty Commission shows that in Hiroshima and Nagasaki there were higher abortion and stillbirth rates among pregnant women near the explosion than among those at greater distances. Of 98 pregnant women in Nagasaki who were within 2,000 metres of the centre of the explosion, about 23 per cent of those who had severe radiation illness miscarried, in comparison with only about 4 per cent of those who did not develop any severe illness, and with about 3 per cent of women who were between 4,000 and 5,000 metres distant. It is apparent that abortion and stillbirth as a result of irradiation during pregnancy do not constitute a problem unless the dose of radiation is large.

Effects on the children of women irradiated during pregnancy

96. There is considerable evidence, both from the case records of patients treated with radiotherapy and from reports published by the Atomic Bomb Casualty Commission, that heavy irradiation of pregnant women can lead to the birth of children who are either abnormal at birth or who later develop in an abnormal way. The case records of women therapeutically irradiated during pregnancy describe a number of different developmental abnormalities in their children, the most striking of which is the condition known as 'microcephaly'; one such case was found during the course of our investigation of patients treated by X-rays for ankylosing spondylitis. The underlying cause of this condition is a partial failure of the development of the brain, as a result of which the head is smaller than that of a normal baby. All grades of the condition exist, ranging from the most severe, in which the child usually has to be maintained in a mental institution, to others in which there is only slight impairment of development and mental powers.

97. There are published records of eleven mentally-retarded children in Nagasaki and Hiroshima who were exposed before birth at a distance of between 700 and 1,200 metres from the centre of the explosion. Ten of the mothers of these children suffered acutely from the effects of radiation, and the eleventh probably did so. The head circumferences of all eleven children were appreciably less than those of unirradiated Japanese children of the same age-group and, in the cases among Nagasaki children, smaller than those of children exposed before birth at distances of between 4,000 and 5,000 metres from the explosion, where the dose of prompt radiation would have been less than 1 r. The evidence from Hiroshima suggests that children irradiated between the twelfth and eighteenth weeks of intra-uterine life are more likely to develop microcephaly than children irradiated either before or after this period.

The Effect on Fertility of Exposure to Radiation

Permanent sterility

98. It is well-established that irradiation may reduce the fertility of men and women, and even render them permanently sterile. In men, a single dose of 500 r to the testes would probably produce permanent sterility. The dose to the ovaries likely to produce the same result in women would depend to some extent upon the age of the woman concerned; a woman nearing the end of her reproductive life would require a smaller dose, about 300 r, than a woman in her early reproductive years. These levels of dose are so high that, if they were received in the course of whole-body irradiation, the individual would develop the early acute illness already described. It is extremely unlikely, therefore, that permanent sterility would be induced in any one accidentally exposed to a large whole-body dose of radiation, unless the acute illness had been manifest.

99. Under modern conditions of occupational exposure, for example among radiologists and radiographers, there is no evidence of any impairment of fertility. Furthermore, there is no suggestion that female radiographers suffer from radiation-induced menstrual disturbances which might be accompanied by diminished fertility.

CHAPTER IV

THE GENETIC EFFECTS OF RADIATION

Introduction

100. Nowhere in our report have we been more conscious of the difficulties of the task which we have undertaken, and of the limitations of the knowledge at our disposal, than in considering the genetic effects of radiation. The established scientific evidence in this field provides but an insecure basis on which to frame answers to the many important questions that are now being asked. Consequently we have been forced to make many assumptions of questionable validity and our conclusions must be regarded as provisional and treated with a measure of reserve. An essential part of future studies will be the collection of more detailed vital statistics. Moreover, it must be realised that genetic studies inevitably tend to be slow and that sufficient knowledge on which to base firm conclusions will be accumulated only after many years of intensified fundamental research.

The Material Basis of Heredity

101. In man and other sexually reproducing animals, every individual arises from a single living cell, which is formed by the fusion of two germ cells, an egg cell from the mother and a sperm cell from the father. Soon after it is fertilised, the egg cell divides into two; each of these divides again, to give a total of four, and this process is repeated until there are enough cells to give rise to all the organs and tissues of the body, among them the sex glands from which in time new germ cells will be formed.

Chromosomes and genes

102. Each cell contains within it a nucleus whose essential component is a number of microscopic thread-like structures, the chromosomes. These are aggregates of sub-microscopic particles—the genes—which determine the hereditary nature of the individual. The total number of genes in a cell is not known with any accuracy but it is certainly high, perhaps thousands or even tens of thousands in a human cell. Each chromosome carries a large number of them arranged in order along its length, so that each gene has its own special place, or locus, in a particular chromosome.

Cell division

103. The nucleus of each human germ cell carries 24 chromosomes, all of them different from one another. When egg and sperm come together, their nuclei also fuse, so that the fertilised egg contains a nucleus carrying 48 chromosomes constituting 24 pairs. The members of a pair, derived one from the mother via the egg and one from the father via the sperm, normally correspond to each other both in the gene loci which they carry and in the order in which these loci are arranged. When the fertilised egg, or any later cell in the body, is about to divide, a replica is first formed of each of the 48 chromosomes. This makes it possible for the two cells so produced to receive sets of chromosomes exactly like each other and like those of the parent cell. The cell divisions immediately preceding the formation of germ cells, however, follow a somewhat different pattern, which

results in the egg or sperm receiving only one member of each pair of chromosomes; thus the number that the egg or sperm contains is reduced to 24.

Gene mutation

104. In the normal course of events, each cell possesses a set of genes identical with those of the cell from which it is derived. Occasionally, however, a sudden change occurs in a gene, which is converted into a slightly different form. The altered form of the gene is spoken of as a new allele and the process of change is known as mutation. Once such a mutation has occurred, the gene is reproduced and passed on in the new form at all subsequent cell divisions. Thus, each locus can come to be represented in the population by a number of these variants or alleles.

Homozygotes and heterozygotes

105. Having but one chromosome of each kind, the germ cell carries only one allele at each gene locus, whereas the body cells, with a pair of each kind of chromosome, carry two alleles. These two alleles may or may not be exactly the same. An individual bearing two identical alleles is said to be homozygous at that particular locus; one with two different alleles is said to be heterozygous at the locus. A homozygous individual clearly must have received the same allele from each of his parents and he will pass it on to all his offspring. A heterozygous individual must have received different alleles from his two parents, and he will on the average pass on one of the two to half his offspring and the other to the other half. This sorting out of the genes when they are distributed to the offspring of a heterozygote is a direct result of the halving of the number of chromosomes during germ cell formation and is known as segregation.

Gene reassortment

106. If an individual is heterozygous for two or more loci, the process of segregation will result in his genes being reassorted into new combinations in his offspring. Thus, in the process of reproduction, the various alleles at the different loci are continually being reassorted into an immense variety of combinations, with the result that each person has a particular hereditary constitution not exactly like that of anyone else. Each of us is, in fact, genetically unique, with the exception of identical twins, who are produced when the fertilised egg—very early in its development—splits into two parts each of which gives rise to a complete individual.

Dominant and recessive alleles

107. Some alleles produce a noticeable effect only on those individuals who are homozygous for them. Such alleles and the characters which they determine are spoken of as recessive. Other alleles have some effect even when the individual is heterozygous, and the characters which these determine are described as dominant. Among the numerous genes which have been studied in man and other animals, all gradations are known between the extremes of fully recessive alleles, which have no effect at all on the heterozygote, and fully dominant ones which have as strong an effect on the heterozygote as on the homozygote.

108. A dominant allele which is being transmitted in a family will be manifest in every generation unless it dies out. A recessive allele, on the other hand, can be transmitted to later generations by an individual who shows no sign of carrying the allele in question. The character produced by such

a gene will appear from time to time in the population, in families where both parents carry the gene; this is especially likely to occur where there is a marriage between cousins.

Sex-linked genes

109. There is a special category of genes whose transmission is connected with the determination of sex. The sex of an individual is determined by one particular chromosome pair. In the female, both chromosomes of the pair are similar; in the male, one is of another type. Thus, the structure of the female sex chromosome pair can be represented as XX, that of the male as XY. Genes carried on these chromosomes are said to be sex-linked, but the Y chromosome, having few geni loci, has little effect on most hereditary characters. If a male contains an abnormal allele on his X chromosome, he will show its effect, even if the same allele situated on one of the two X chromosomes of a female would be recessive. If a recessive sex-linked gene is uncommon, it will occur only very rarely on both the X chromosomes of a female and the characteristics it produces will therefore not often be found in females. They will be commoner among males, who will be affected whenever their single X chromosome contains the allele. The apparently normal females can, however, transmit the chromosome carrying the affected gene to the next generation, and so act as carriers of the abnormality. The classical example of this type of condition is the disease haemophilia, which appears in males but is transmitted by apparently normal females.

Continuous variation

110. Not all gene differences have effects which are sufficiently distinct to be recognised by their segregation among the members of a family. As can be seen in human stature and intelligence, for example, some characteristics vary by continuous gradations over a wide range which is regarded as normal. Such characters are believed to be controlled by the combined action of a large number of genes, the effects of which are supplementary and each so small that they are not individually distinguishable.

Causes of mutation

111. Reference has already been made to the process of mutation by which one allele changes into another. We do not know all the factors which cause mutation. It is believed to be due sometimes to chance disturbance of the complex molecules which constitute the genes, and sometimes to external influences such as certain chemicals or natural background radiation. We shall discuss later the extent to which naturally occurring mutations are likely to be attributable to background radiation, but from observation of organisms other than man it is known that other causes can be important. Present evidence suggests that mutations, whatever their origin, are for the most part random changes not specifically related to the nature of the stimulus or to the needs of the individual.

Mutation rate

112. So far as is known, all genes are subject to mutation, and, over the population as a whole, mutation is continually occurring at a definite but very low rate (Appendix C). Factors influencing mutation increase the rates at which the genes change; they do not produce changes of a novel type. Most genes, if not all, are susceptible to such factors, though some may be affected more readily than others. No methods are yet known for stimulating the mutation of only one particular gene, or even of a small selected group of genes.

Mutation and natural selection

113. The hereditary variation found in human or other populations is the result of mutations which occurred in past generations. New alleles which cause abnormalities harmful to those who possess them tend to be eliminated from the population. Thus, a new dominant allele causing death before the reproductive age, or for some other reason preventing the individual from leaving descendants, is eliminated in one generation. Those with less drastic effects are eliminated with a speed related to the severity of the handicap which they impose. Recessive alleles, which give rise to harmful conditions only when they are in the homozygous state, are also eliminated, but very slowly, since the allele can continue to exist or to spread in the heterozygous state without contributing any significant handicap to the perpetuation of the line. Thus, in the case of both dominant and recessive harmful alleles, natural selection is constantly operating towards their removal from the population. Equally, it will operate even against genes whose effects are valuable to the individual during his own lifetime, if they reduce his chances of leaving offspring.

114. For alleles which increase the chances of leaving offspring the situation is the reverse. Those individuals in which these effects are manifest are more than ordinarily likely to propagate their kind, so that their descendants tend to become the predominant type. Many, but perhaps not all, of the genes which tend to increase fertility will also have effects which are useful in other ways to the individual in whom they are manifest; they can be considered as generally advantageous.

115. Between these clear-cut examples there are genes with every gradation of effect. The manifestations of genetic abnormality in the individual may vary from the trivial to the disastrous. The action of some genes is delayed until after the end of the reproductive period of life and they are therefore largely immune from the eliminating influence of natural selection. There are genes which are harmful whether the individual carrying them is homozygous or heterozygous; there are others which are advantageous to heterozygotes and harmful to homozygotes. The relative prevalence of any particular allele, in any particular population, can be understood only in the light of the relationship between the environment and the advantages or disadvantages of the condition to which the allele gives rise.

Genetic equilibrium

116. As natural selection is constantly operating towards the elimination of harmful genes from the population, the incidence of the conditions to which they give rise would steadily decrease, were it not that they are being replenished by the occurrence of new mutations. Their frequency will therefore tend to reach the level at which their loss by selection is balanced by new mutation. A state of genetic equilibrium has then been reached. This is the situation with regard to many of the more harmful abnormalities in man, the incidence of which remains relatively steady in the population despite the failure of those affected to leave normal numbers of offspring.

The general effects of increasing mutation rates

117. The above considerations suggest the broad qualitative effects which can be expected to follow an appreciable increase in mutation rates. At this present time, the advantageous alleles that have appeared in the past are already widespread and will have become part of the normal constitution of the population. An increase in the rate at which they are produced can therefore have little effect. The harmful alleles, on the other hand, have been restricted to a low incidence by the operation of natural selection. A

significant increase in the rate at which these are produced will therefore have a more easily detectable effect. The general inference is that increasing the mutation rate in a human population would have a relatively much greater effect upon the incidence of harmful than upon that of harmless or of advantageous hereditary traits.

Mutation and the adaptability of populations

118. Mutation provides the means by which the human race, through hundreds of thousands of years, has successfully adapted itself to its environment. There is no reason to believe that this adaptation has proceeded by sudden and conspicuous changes in human characteristics. The consensus of opinion is that evolution has occurred by a succession of small variations from the average, which conferred a slight but eventually important advantage in relation to the trend of environmental change. It is the existence of this galaxy of hereditary traits, varying only slightly from the accepted normal, that has conferred adaptability upon the human race. The recurrence of a small but steady incidence of harmful mutations is the price that has to be paid for this asset.

The Genetic Effects of Radiation: Basic Principles

119. There is as yet little direct information about the genetic effects of ionizing radiations on man and, for reasons which we examine later, the few observations that have so far been made present many difficulties of interpretation. We have therefore to rely on information obtained from experiments on other organisms. The experimental evidence is itself incomplete and largely derived from observations on forms of life other than mammals, but a general picture is beginning to emerge which appears to be consistent for the organisms which it has been practicable to study. The question arises, however, to what extent it is justifiable to draw inferences concerning man from the reactions of remote organisms observed under the artificial conditions of laboratory experiment. Since the genetic mechanism in man is the same as that in other animal and plant species, and since the animals and plants that have been studied all show the same type of genetic response to ionizing radiations, it would be unreasonable to suppose that the response in man will do other than follow the same general pattern. On the other hand, we do not think that conclusions derived solely from observations on other organisms offer a secure basis for quantitative estimates concerning man and, except where we have explicitly stated the contrary, we have not used them for this purpose.

Effects of radiation on germ cells

120. Ionizing radiation will have genetic consequences only in so far as it affects any of the germ cells or the cells ancestral to them in the reproductive organs. It may then have one of three results: the affected cells may die, their chromosomes may be broken, or the genes may be caused to mutate.

121. Death of a germ cell, or indeed of any cell ancestral to it, can have no genetic consequence, because this very death will terminate the lineage of cells to which it belongs.

Chromosome structural change

122. Chromosome breakage may also lead to death of the cell lineage. Broken chromosomes may fail to reunite or they may join up to give new forms of chromosomes which are incapable of passing through the process of cell division in the normal way; either of these circumstances leads to

subsequent death of the cell lineage. If the damage occurs in an immature germ cell in the sex gland, the cell lineage will usually die before mature germ cells are formed, and there will be no genetic consequences. If it occurs in a mature germ cell which later participates in fertilisation, the ensuing embryo will usually die early in gestation, and the ultimate genetic effects will be minimal.

123. Chromosome breakage may, however, have a different outcome if the fragments reunite in new patterns which are capable of passing through cell division. The resulting kinds of structurally changed chromosomes may be transmitted to apparently normal offspring, and at least one type of change will manifest itself by the occurrence of repeated abortions or malformations among the descendants of the irradiated individual. Experiments on mammals indicate that this inherited effect will appear only if conception takes place within a few months of irradiation.

124. Such structural changes of the chromosomes are induced especially by large single doses of radiation, for example heavy doses of X-rays or the prompt radiation from atomic bombs. They are induced only rarely by long-continued exposure to low-intensity X- or gamma radiation, although relatively small doses of neutrons or alpha particles are more effective in bringing them about.

125. Although they may cause partial sterility or abortion in their carriers, major structural changes of the chromosomes do not as a rule bring about other kinds of abnormality in individuals bearing them. For this reason, and also because of their low rates of spontaneous occurrence and induction by chronic irradiation, and of the probability of their having an adverse effect on fertility, chromosome structural changes are likely to be of comparatively little importance among the radiation hazards to man.

Induced gene mutation

126. The third and, from the genetical point of view, the most important effect of exposing germ cells to additional radiations is the induction of increased gene mutation. Since all germ cells from time immemorial have been continuously exposed to some radiation from natural sources, it would be surprising if exposure to additional radiation were found to induce any novel types of mutation. The results of experiment support this view; the types already known recur, but at an enhanced rate.

Proportionality of induced gene mutation to additional radiation

127. There is no known threshold for the induction of gene mutations by radiation: that is to say, any additional exposure, no matter how small, must be expected to raise the mutation rate, if only by a minute amount. Furthermore, to judge by our experience up to the present, it is probably true that the rise in the rate of mutations is directly proportional to the amount of additional exposure. This law is known to hold good, for such organisms as have been studied, when the radiation dose is fairly high. It is also known to hold good for the induction of one class of lethal genes in the fruit-fly, *Drosophila melanogaster*, by X-ray doses as low as 25 r. In this chapter of our report we are chiefly concerned with doses well below this level; but, for the present, there does not appear to be sufficient evidence to warrant the assumption that there is any real departure from this law even at the lowest doses, and proportionality has therefore been accepted as the basis of what follows.

Accumulated dose to germ cell lineage

128. Cells arise only from pre-existing cells. Mature germ cells are produced from a line of ancestral immature cells which have been present at every instant during the individual's life, from conception onwards. It will be remembered that mutated genes reproduce themselves as faithfully in a cell lineage as do the normal genes from which they arise. In consequence, the mutated genes arising throughout a germ cell lineage will be accumulated in the mature germ cell, and a given radiation dose will therefore have the same order of effect whether it is given over a short or a long period of time. In other words, long continued exposure to low-intensity radiation induces as much gene mutation as a single exposure to high-intensity radiation, provided that the total dose is the same. Experiments have shown that this probably remains true even when the dose is split into a series of small fractions, and no matter what interval elapses between the separate irradiations. Thus, in contrast to most other types of biological response to radiation, damage to the genetic material cannot be repaired and the effect from repeated exposures is cumulative.

Genetically effective dose to a population

129. This cumulative effect of radiation indicates that the genetic effects of exposure will depend on the ages of the individuals exposed as well as on the dose they receive. If, for example, all are past the reproductive age, the genetic effects will be nil; if they are younger, the possible number of offspring they may have is of importance. The age distribution of those exposed is therefore an important factor to take into account when estimating the consequences to future generations of additional radiation.

The Effects of Increased Mutation Rates on the Incidence of Disease in Human Populations

130. In approaching the problem of making some quantitative assessment of the genetic effects of radiation upon a human population, we have been very conscious of the inadequacy of the evidence in two essential respects. First, we do not know the dose of radiation required to double the mutation rate of any specified human gene; secondly, there is reason to suspect that the radiosensitivity of human genes may vary considerably, so that it could be very misleading to treat them as an approximately uniform group which would respond to any particular dose of radiation with a standard increase in mutation.

131. It is, however, possible to give a general idea of the effects of an increase in the mutation rate of particular human genes without raising the question of dose or degree of uniformity in sensitivity to radiation. We have therefore taken selected examples of diseases in which genetical factors are known to play an important part, and have attempted to assess the effects in a human population of doubling the spontaneous mutation rates of the genes concerned, without specifying the agent or agents causing this increased rate of mutation.

132. The role of heredity in the production of disease ranges from that of a predisposing to that of a preponderating cause. Thus, there is some evidence that heredity is a factor influencing susceptibility to tuberculosis; but this could be of no significance unless the individual were infected with tubercle bacilli. On the other hand, achondroplasia, a form of dwarfism, is probably determined entirely by genetic factors, in the sense that no known modification of the environment can prevent its appearance in those who possess the necessary gene.

133. In making our assessment we have confined our attention to those conditions which impose a significant handicap and which are determined, entirely or to an important extent, by hereditary factors. To put it another way, our object has been to give an assessment of the social load that would be imposed upon a population, like that of this country, by increasing mutation rates. We propose to consider two possible situations: first, when the mutation rate of every gene concerned is supposed to have been doubled in one generation only, thereafter reverting to its former level, and secondly when the rates having been doubled remain at that new level generation after generation.

THE EFFECTS OF DOUBLING MUTATION RATES ON DISEASES DUE TO A SINGLE GENE

134. For abnormal genes with effects which are masked in any way, either by normal alleles—as in recessive traits—or by the influence of environment, mutation has less immediate or less apparent consequences than for genes with effects which are directly manifest, as in dominant traits. Severely disabling dominant diseases reveal the results of recent mutation most readily. Sex-linked traits show a less rapid response. The effects of genes which produce recessive traits are masked until two genes come together in the homozygous state; hence, the results of their recent mutation will be less noticeable, although in the course of time their full effects will appear. Where a gene is common and mainly benign, but can occasionally cause disease, the results of its recent mutation will be scarcely detectable.

135. The effects which might be expected to result from an increase in mutation rates can most easily be calculated for diseases known to be caused by single genes (Appendix D). For this purpose we need to know what proportion of cases in a given generation is due to recurrent fresh mutation. The incidence of the particular disease in the population must be ascertained; so must its mode of inheritance (dominant, partially recessive, recessive or sex-linked), the degree to which it handicaps or favours the affected individuals—as shown by their length of life and reproductive capacity—and the modifying effect, if any, of environmental factors. This information is available for only a relatively few conditions and much more research will be required before we can feel reasonably confident in making estimates for groups of diseases. To illustrate the different types of effect, we have chosen three examples about which we have some fairly accurate information.

(*i*) *A dominant trait*

136. Achondroplasia (chondrodystrophia) is a dominant form of dwarfism. Although there are many clinical types, it will be assumed for the present purpose that the condition is due to a single gene with a manifestation which is independent of environmental factors. The incidence at birth, according to Danish estimates, is one in 9,400. Biological fitness is greatly reduced on account of high stillbirth rates and also, in adult life, on account of low fertility, especially of females. The chance that an affected individual will have offspring is estimated at only 1 in 5. The majority of cases are believed to arise through fresh mutation.

137. Doubling the mutation rate of the causal gene for one generation would produce an 80 per cent increase in the incidence of the condition in the first generation, that is the incidence at birth would rise to nearly one in 5,000. The excess would, however, rapidly disappear and within five or six generations the incidence would return to normal (Fig. 1a). If the

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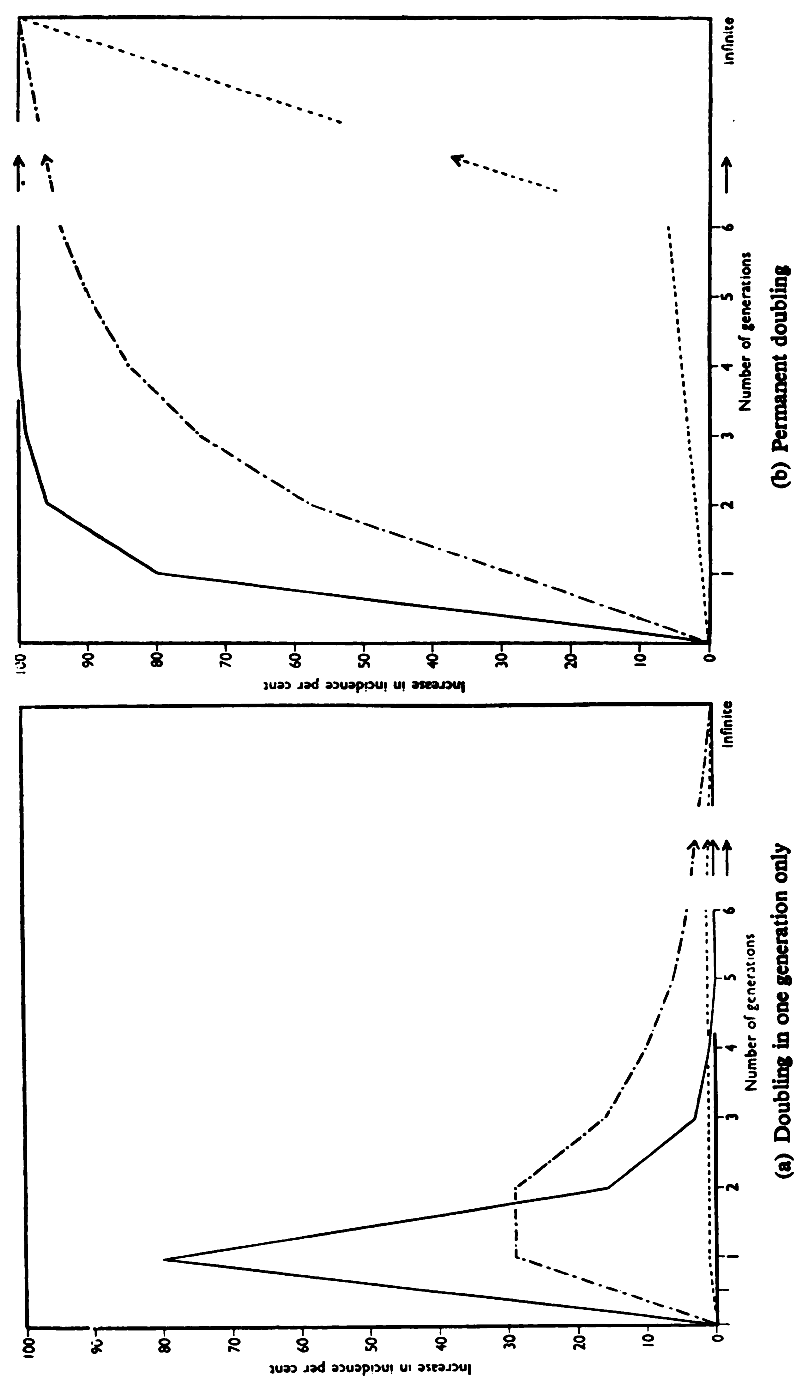

Key:
Achondroplasia (dominant) ————
Haemophilia (sex-linked) —·—·—
Phenylketonuria (recessive)·········

FIG. 1. The effects of doubling the mutation rates for three hereditary conditions, expressed as the percentage increase in incidence (Appendix D, Table 2D).

mutation rate were permanently doubled, the incidence would rise to a level close to double the present figure (i.e. 100 per cent increase) within three or four generations (Fig. 1b).

(ii) A sex-linked trait

138. The term haemophilia covers a group of sex-linked traits in which there is impairment of blood clotting. For the present purpose it will be assumed that all severe cases are caused by abnormal alleles at a single locus. The incidence is at least 1 in 12,000 of the male population at birth and the chances that an affected male will survive and have descendants is about one in eight. Females carrying the abnormal gene are healthy and have normal chances of reproduction. Doubling the mutation rate for one generation would produce a 29 per cent increase in incidence in the following generation, that is, the incidence would rise to about 1 in 9,300. In the next generation this level would be sustained but thereafter it would sink back moderately quickly towards the previous level (Fig. 1a). If the mutation rate were permanently doubled, the incidence would rise to 90 per cent above the previous level in about six generations, and thereafter slowly approach the point where the incidence was doubled (Fig. 1b).

(iii) A recessive trait

139. Phenylketonuria is an example of a deleterious recessive trait associated with severe mental deficiency. Its incidence in the population at birth is about 1 in 40,000 and, although the early mortality is not high, the chance that an affected person will have offspring is practically nil. A doubling of the mutation rate in one generation would cause an increase of one per cent in the incidence of the disease in the first generation. If allowance were made for the effects of inbreeding, a further small increase would be predicted sometime after the second generation and this would be followed by a very slow return to the previous level (Fig. 1a). The response to a permanent doubling of the mutation rate would be a slow rise by almost equal increments. It would take more than 50 generations of 30 years each to increase the incidence by 50 per cent, and many more to approach an increase of 100 per cent (Fig. 1b).

THE EFFECTS OF DOUBLING MUTATION RATES ON BROAD GROUPS OF DISEASES

140. The three examples given above illustrate the kind of result to be expected from doubling the mutation rates of genes representing each of the three classical types of genetical effect. A large number of dominant, sex-linked and recessive diseases are known; many of them are rarities but together they may account for a relatively large proportion of serious hereditary disability in the population. However, to give an idea of the extent of the problem of hereditary disability, and the total results of doubling the mutation rates of the genes which are responsible, common categories of illness must be considered. Our information, from the genetical point of view, is unfortunately not often precise and, in addition, the effects of genes are in many cases modified by environment. Mental diseases and mental deficiency, when taken together, account for nearly half the hospital beds provided in this country and are the most extensive inclusive category in which hereditary causes are known to be important. We shall first consider severe mental defect and then the two main types of mental illness.

(i) Severe mental defect

141. The incidence in the population of cases of severe mental deficiency which survive has been estimated to be about 1 in 500. It is higher than this at birth but subsequently reduced by the heavy mortality in early life. Beyond

this, there are grossly affected individuals, many with malformations of the nervous system, who are stillborn or who do not survive early infancy but who would have been mentally defective had they lived; the incidence of such cases is at least 1 in 200. The number of cases of severe defect at all ages surviving in England, Wales and Scotland may be nearly 100,000. The fact that not far from 30,000 hospital beds are provided for these cases indicates the extent of the medical care required.

142. In this broad category of disease, conditions with dominant inheritance include epiloia (sebaceous adenoma with tuberose sclerosis), several types of acrocephaly, hypertelorism and neurofibromatosis. In such diseases, many of the severely affected cases, say one half, can be attributed to recurrent fresh mutation. Doubling of all the mutation rates, for one generation, would have a large effect in the subsequent generation, and a permanent doubling of the mutation rates would soon permanently double the incidence. We may suppose that diseases such as these, collected together, form four per cent of all surviving cases of severe mental defect, as shown in Table 1. If all mutation rates were doubled, this would add 50 per cent to the numbers of these cases in the first generation, that is to say two new cases in every 100 in the whole category, an increase which would mean a thousand extra cases requiring medical care.

TABLE 1

Effect of doubling mutation rates of genes concerned with severe mental deficiency: cases per generation classified according to probable causation

Type of Diagnosis	Probable causation	Surviving cases of severe mental deficiency under present conditions (i)*	(ii)†	Increase in first generation if mutation rates are doubled (i)*	(ii)†
Acrocephaly ... ... Epiloia ... ... Neurofibromatosis ... *et cetera*	Dominant gene	4·0	2,000	2·00	1,000
Microphthalmos ... Gargoylism ... ... *et cetera*	Sex-linked gene	0·5	250	0·15	75
Amaurotic idiocy ... Cerebral diplegia ... Phenylketonuria ... 'True' microcephaly ... *et cetera*	Recessive gene ...	40·0	20,000	0·40	200
Mongolism	Environmental influence plus common genetical susceptibility	12·5	6,250	?+	?+
Others	Miscellaneous, including birth injury and infection	43·0	21,500	?+	?+
All	—	100·0	50,000	3·0	1,500 (approximate figures)

* (i) percentages based upon all cases in the category.

† (ii) numbers which would occur in a generation of 20 million births.

143. A few rare diseases causing severe mental defect are known to be due to sex-linked genes, for example one type of microphthalmos and one type of gargoylism (Hurler's syndrome). The contribution of this group to the total number of defectives is small and can hardly exceed half of one per cent of severe cases. Nevertheless, for the reason given above, diseases of this kind must be due to genes with significant mutation rates: if these rates were doubled in one generation, the incidence at birth of the diseases would be increased by nearly one-third, as in haemophilia. After a doubling of the mutation rates in one generation, there would be an increase of 0·15 per cent in the total number of cases of severe mental defect, or 75 extra cases requiring medical care.

144. Other important known genetical causes of mental defect are recessive conditions, such as the two kinds of amaurotic idiocy, phenylketonuria, cerebral diplegia, and 'true' microcephaly. Known conditions caused in this manner account for 20 per cent of low grade mental defect and the same type of causation may easily account for twice as much as this. The case frequency of these traits individually is low, that is about one in 40,000 in the population at birth; about 20 conditions of this type are already known and perhaps another 20 may exist undetected. Since these diseases are all very deleterious, so that those affected scarcely ever have offspring, it is generally accepted that the genes causing them arise continually by spontaneous mutation. As previously explained, the reason for this assumption is that the incidence in the population can only be maintained if loss of genes through failure of reproduction is balanced by an equivalent appearance of new mutations. From this consideration the mutation rates can be estimated for these recessive traits but the method is indirect and the results are imprecise; they are likely to be too high. A doubling of mutation rates in one generation would cause a one per cent increase in the incidence of each lethal recessive trait the original incidence of which is one in 40,000. If 40 per cent of severe mental deficency were determined in this manner, doubling mutation rates would cause an increase of 0·4 per cent in the whole category or 200 extra cases requiring medical care.

145. The problem of mongolism, a disease responsible for between 10 and 15 per cent of all cases of severe mental defect living in the population requires separate consideration. There is strong evidence of a genetical element in the causation but maternal age is also a very significant factor. The hereditary predisposition must be very common and only harmful in exceptional circumstances. Although a slight increase in incidence might be expected as a result of doubling mutation rates, the nature of the predisposition is so imperfectly understood at the present time that it does not seem useful to make a numerical estimate for this condition.

146. As shown in Table 1, 43 per cent of the cases are not yet accounted for. Among these, there must be a large group in which injury or infection is the main cause, perhaps 15 per cent of all cases of severe mental defect. A still larger number are of quite unknown origin, although genetical factors may have some influence. Furthermore, there may be a residual proportion of cases due to relatively common genes, acting either singly or in combination with one another, on which the effect of increased mutation could be appreciable. It is impossible to make quantitative predictions about mutation for this group with unknown causation but the number of cases ordinarily caused by fresh mutation must be very small.

147. The conclusion is that, after doubling the mutation rates, an overall increase of three per cent in the category of low grade mental deficiency in one generation is possible; in a generation of 20 million births the known

surviving cases would number 50,000 and an increase of three per cent would mean 1,500 additional cases requiring care. If the mutation rate remained permanently doubled, the incidence in the population would, on the most pessimistic assumptions, eventually, after very many generations, double also and twice as much medical care would accordingly be needed.

(*ii*) *Mental illness*

148. Current theories concerning the genetical factors underlying mental illness imply that a small but substantial proportion of cases must owe their origin to the recurrence of mutations. This is almost certainly true for Huntington's chorea, a rare dominant disease. For the common disease groups, schizophrenia and manic-depressive reaction, the situation is less clear. According to figures for 1954, there were approximately 63,000 cases of schizophrenia and 31,000 cases of manic-depressive reaction under hospital care in England, Scotland and Wales. Since these diseases account for about half of all mental illness, it may be worthwhile to attempt a rough estimate of the effects of changing mutation rates upon their incidence.

149. Theoretically, any genes responsible for conditions, like mental illness, which lower biological fitness to a marked degree would have been eliminated from the population, or would have become very rare, unless they had been continually replaced by fresh mutation. However, there are uncertainties about the relevant facts concerning the genetics of schizophrenia and manic-depressive reaction. First, the incidence of these diseases in the population is not accurately recorded; secondly, the biological fitness of predisposed and even of affected individuals has not been fully investigated; thirdly, the nature of the genetical contribution is known only by surmise; and, fourthly, nothing definite is known about possible compensating mechanisms which might, if they existed, make unnecessary the assumption of gene replacement by spontaneous mutation.

150. Calculation, on the basis of elementary and simplified assumptions about gene action, leads to the conclusion that doubling the mutation rate might have the effect of raising the incidence of schizophrenia by a factor of one per cent and of manic-depressive reaction by a factor of 1·4 per cent in the first generation. It is not possible to estimate the number of extra hospital beds which this proportional increase in frequency of genetical predisposition would imply. We can, however, obtain an idea of the expected number of extra chronically incapacitated patients from the calculations set out in Appendix E. The total number of such cases which would appear among the first generation of 20 million births after doubling mutation rates would be 200 schizophrenics and 200 manic depressives. The number of extra patients needing psychiatric care at one or other time during their lives, on account of these genetical predispositions, would be from 5 to 10 times as great. A permanent doubling of mutation rates would have in each succeeding generation an effect similar to that in the first generation. Thus, over a very long period of time, the number of cases would slowly increase until the limiting value of twice the initial number was approached.

(*iii*) *Blindness*

151. A frequent cause of severe disability is blindness. The extent of the morbidity is shown by the fact that every year, in England and Wales, 12,500 new cases of blindness are registered. These include cases of developmental abnormality, tumours, metabolic diseases and the results of injury and infection. The genetical background is extremely varied but at least half of the hereditary cases can be attributed to single genes, often recessive like those causing retinal degeneration, though a few are dominant like that for

retinoblastoma; some are known to be sex-linked. High myopia is believed to have a complex genetical background. The important cases from the present point of view are those with onset in early life and about three-quarters of all such cases of blindness are thought to be hereditary. About 300 children between the ages of 0 and 15 years are registered as blind annually. Severe cases in the causation of which mutation is likely to play an important part will mostly be in this group which includes aniridia, microphthalmos and retinoblastoma. The mutation rates for these dominant diseases are listed in Appendix C. Assuming that these figures apply to England, Wales and Scotland, we can estimate that, in one generation of 20,000,000 births, there would be 80 cases of aniridia, 80 cases of microphthalmos and (using the mean of three estimates) 560 cases of retinoblastoma due to spontaneous mutation in ordinary circumstances. These numbers would be almost doubled if the mutation rates were doubled for one generation. If the mutation rates were permanently doubled, this increase would be continued until the total incidence of these diseases was doubled, as was calculated for achondroplasia.

152. Numerous cases of blindness due to recessive conditions are known, and some have sex-linked inheritance. Figures for the incidence of these traits are not well enough established for the effects of doubling mutation rates to be estimated.

(iv) *Neonatal deaths, stillbirths and congenital malformations*

153. An increase in mutation rates would be expected to have an effect upon the abortion, stillbirth and neonatal death rates and upon the incidence of congenital malformations. These deaths and malformations are known to be caused in large part by the environment of the unborn child, which may be affected by illnesses and other conditions in the mother. Many may be due to single recessive genes, some to chromosome abnormalities and others are known to be caused by immunological incompatibility between mother and foetus. For these reasons we have not found it possible to make detailed calculations of the kind used above for other conditions, but it is certain that the total effects of doubling mutation rates in one generation would be slight. Observations on these foetal conditions in actual circumstances where the mutation rates might have been increased have been made on human populations and are discussed below in paragraphs 162 to 170.

THE OVERALL LOAD OF ILLNESS IMPOSED BY DOUBLING ALL MUTATION RATES

154. We have expressed the opinion that, from the standpoint of the social load imposed, mental diseases constitute the most important single category of disease which is determined to a marked degree by heredity and which is serious, in the sense both of being highly harmful to the individual and of making heavy demands on medical resources. We are aware that this is only an opinion and that others may have different views. We believe, however, that it will be conceded by all that the mental diseases contribute a very substantial proportion of the total number of those suffering from serious hereditary disorders. It seems reasonable, therefore, to suggest that the total increase in the social load due to serious hereditary illness of all kinds, which would follow doubling all mutation rates, would be unlikely to exceed more than a few times the estimates we have given for mental defect and mental illness combined.

155. Hereditary diseases are sometimes thought of as incurable, and it is true that present knowledge provides no grounds for believing that cures will

be found for some of the grosser forms. With the advance of medical science, however, it has become possible to alleviate many hereditary conditions or even to maintain the affected patients in good health. A classical example is diabetes mellitus, into the causation of which a hereditary factor enters in many cases. Before the discovery of insulin, the majority of diabetics were destined for invalidism or premature death. Now with its aid they live essentially normal lives. With further advances of medical knowledge, it may be expected that an increasing number of hereditary conditions will be brought into this category, and thus the load of suffering from illness of this kind be reduced. It should be realised, however, that the preservation of those afflicted by hereditary conditions will increase their chances of having children and so lead to an increased prevalence of the condition in the population and with it an increased need for medical services.

156. From the point of view of the long-term effects on the population, it must be remembered that, even though most of the new recessive alleles produced by an increase of mutation rates will not meet a like partner in the first generation after they are produced, and will therefore remain concealed, they will still exist in the population. They will in fact persist until, at some later time, two carriers of like genes happen to mate. The recessive effects will then become manifest in some of the children of such matings. If such genes are harmful, these affected children will produce fewer offspring than the normal, so that there will be a reduced chance that the genes will be passed on to later generations. In this way mutated genes can eventually die out. The extinction of the gene will have been brought about, however, only by the failure of some affected individual or individuals to reproduce at the normal rate, a circumstance which may sometimes be merely unfortunate but in other cases may be the expression of a hereditary defect which causes great suffering. Thus, if the mutation rate is increased, and a crop of newly mutated recessive genes produced, they will continue to cause harmful effects for many generations.

The Effects of Increased Mutation Rates on Hereditary Traits showing Continuous Variation about the Normal

157. Although in any human population we can find individuals who are physically, biochemically or mentally abnormal in a relatively gross way and whose abnormality can be traced to single gene differences, most of the variation between human beings is not in fact of this kind. Even in that greater part of the population which we should describe as normal, no two individuals are alike: they vary by imperceptible gradations over a wide range in respect of many characters such as physique, general well-being, life-span, intelligence and so on. Some of the variation is hereditary but some is due to differences in environment—in the circumstances under which the individual lives and has grown up. The hereditary portion of this variation is believed to be due to the combined action of many genes which supplement one another in producing their effects. These genes cannot be distinguished one from another, and their effects have therefore to be measured in a way differing from that used where a gene has consequences sufficiently drastic for it to be followed as a separate entity. The importance of heredity in such cases is expressed by estimating the proportion of the variation which is traceable to gene effects. The properties of the system of genes are inferred from observations on the amount of this variation which is shared by relatives and on the change in the proportion from generation to generation. In particular, the effect of mutation will be measured by the increment it adds to the variation in each generation.

158. One difficulty should be observed at the outset. The success of this method of approach depends on the ability to measure separately the proportions of the variation attributable to the combined effects of the genes, and the proportion attributable to environmental factors external to the individual. Even in animals and plants, under the conditions of controlled experiment, this is not always easy. In man, whose parents give him not only his genes but also the home and environment in which his early and most formative years are passed, the separation of hereditary and environmental effects is always extremely difficult. Observations on, for example, children brought up by foster parents are of some assistance; but, even so, the conclusions at which we have been able to arrive must be regarded as rough estimates and treated with due caution.

159. Genetic theory leads us to expect that, since mutation brings new gene differences into the population, the basic effect of an increase in the mutation rate will be to increase the variation shown by these characters, that is to raise the numbers of the more extreme types at the expense of the more central, average individuals. Very little information is available, however, even from experimental animals and plants, about the magnitude of the effect to be expected. Such few observations as we have (Appendix F) suggest that in any generation the variation due to new mutation is but a small fraction of the heritable variation observable, and, of course, a still smaller fraction of the total variation, which includes that due to the environment. Indeed, the available data would lead us to expect that hundreds of generations of mutation would be needed to build up the variation which is seen in a human population. A doubling of the mutation rate for a few generations would therefore be expected to have only the most trivial effect on the variation in such characters, and even a persisting doubling of the mutation rate would take very many generations to approach its full effect, which at most would be to double the variation.

The distribution of intelligence

160. The effect on the distribution of intelligence—or, more accurately, of the score in intelligence tests—of an increase in the hereditary variation, such as would be expected to result from a raised mutation rate, is considered in Appendix G. Extensive studies have been made of the intelligence score and something is known about its distribution in the population and the extent to which it is inherited. Increase in the variation, that is in the spread of the distribution, will lead to an increase in the numbers both with markedly low and markedly high scores. Furthermore, the more extreme the class under consideration, the greater the increase in its numbers relative to the overall change in the variation. Thus, from the table in Appendix G, it will be seen that a doubling of the heritable variation could lead in the long run to nearly a tripling of the numbers falling short of an intelligence score of 70 and conventionally regarded as requiring special schooling. On the assumption that the average score did not fall, a corresponding increase would be expected in those with the high scores of over 130.

161. The increase in the two extremes of the distribution may, however, not be symmetrical. Evidence from experimental organisms shows that, where a character has been subjected in the past to much selection in a particular direction, new variation is likely to produce a disproportionately large increase of the more extreme types in the direction opposite to that towards which selection has been pushing the character. This would still be true when the variation is being increased by irradiation. It seems probable, in the light of man's evolutionary history, that he has been subjected to fairly

intense natural selection for increased intelligence. It might therefore be expected that an increase in the variation, resulting from a raised mutation rate, while leading to some increase in the fraction of those who are highly intelligent, would lead to a greater—perhaps much greater—increase in the other extreme fraction with low intelligence. In addition to the calculation which assumes that the average intelligence score remains constant and so assumes symmetrical increases at the two ends of the distribution, Appendix G includes a calculation which assumes an asymmetrical effect, the overall average of the population falling but the proportion of children of grammar school ability remaining constant. The disproportion in the increase of the low end of the distribution is of course increased, a doubling of the variation raising the proportion with a score of less than 70, perhaps by as much as four or five times. It should be remembered, however, that these calculations apply to the situation when the increase in variation has reached its full extent. We have already seen that such data as are available suggest that a permanent increase in the mutation rate would take hundreds of generations to produce its full effects on hereditary variation.

Observations on Populations Exposed to Radiation

162. An alternative approach to the problem before us, and one which in addition might provide direct evidence of the relation between the dose of radiation and increased incidence of hereditary traits in man, is to observe the effects on human populations which have been exposed to ionizing radiations. Three such studies have been carried out, two on American radiologists and the other on the Japanese populations who were in Hiroshima and Nagasaki at the time of the atomic bomb explosions. For various reasons, the evidence from each is inconclusive, even that from the extensive study by the Atomic Bomb Casualty Commission in Japan.

Possible indicators of change in mutation rates

163. Among the possible indicators of a change in the mutation rate are changes in the sex ratio at birth, the congenital malformation rate, the stillbirth rate, the neonatal death rate, the weight at birth, the weight at nine months, and measurements of the head and body. Changes in the sex ratio may be used as an indicator of genetic damage. The inheritance of abnormal genes in the sex chromosomes has been considered in relation to haemophilia. Experimental observations have shown that abnormal genes which kill the infant long before birth can be carried on the sex chromosomes and there is reason to believe that this may be true in man. Such genes will necessarily disturb the sex ratio at birth; mothers with such mutations will have too few sons, and, in rare cases, fathers too few daughters.

164. It is known, from both human and experimental evidence, that genes can produce abnormalities which are evident at birth. Estimates of the congenital abnormality rate vary with different observers from just over one per cent to about six per cent, owing to lack of agreement on what shall be reckoned an abnormality as well as to real differences in populations with respect both to their environments and to the frequencies of the causal genes. Further, genes are not the only cause of such abnormal conditions. Both clinical and experimental evidence suggests that maternal ill health, particularly infectious disease and malnutrition during pregnancy, can produce them. There is no definite information as to what proportion of cases of malformation should be attributed to the effects of single genes; nor are the forms of inheritance understood.

165. The stillbirth and neonatal death rates are also influenced by a variety of factors. The evidence that abnormal genes can play a part is based

partly upon experimental genetics and partly upon investigations of family histories. On the evidence available at the present time, it is difficult to estimate the extent of the part played by genetical causes.

Genetic studies on radiologists

166. The evidence provided by the studies of congenital abnormalities in the offspring of American radiologists is inconclusive for two reasons. First, no measurements were made of the radiation doses which were received by the radiologists in the course of their work and it is virtually impossible to deduce these in retrospect. Secondly, the data were obtained by postal questionnaire, to which only three-quarters of the radiologists replied and little over half the other specialists who were used as a control group. Whether those who chose to answer were representative of the total is open to question. As the magnitude of the effects observed was small (slight rises in the incidence of twinning, foetal death and congenital malformation), one cannot exclude the possibility that the increases were due to statistical bias in the data rather than to the radiation exposure, or alternatively that statistical bias in the other direction may have partly concealed a somewhat larger increase than was observed.

Studies on Japanese populations

167. The Atomic Bomb Casualty Commission's genetic study was much more extensive. An attempt was made to assess the prompt radiation dose received by each individual; and in each city those remote from the burst constituted a control population with which to compare those close to it. More than 80,000 subsequent pregnancies were followed, and a third of the children were re-examined at nine months of age.

168. The final report on the genetic programme of the Atomic Bomb Casualty Commission has not yet been published; but through the courtesy of the United States authorities, and especially of Dr. James V. Neel and Dr. William J. Schull, we received copies of the draft and are permitted to refer to it. The data present many difficulties of interpretation for several reasons. First, the radiation dose was not known with any accuracy. Second, the parents with different degrees of exposure were not entirely comparable in various characteristics, such as maternal age at birth of the child, to which the congenital malformation rate is related; for this reason, even if there were no effect due to exposure, the children of the highly exposed parents would be expected to differ in their congenital malformation rate from those of the slightly exposed. Complex statistical procedures are necessary to allow for this. Even more open to error is the comparison between the children of exposed parents and those of parents who were entirely unexposed. The latter group of parents included immigrants from other cities or from rural areas after the time of the bombing, and some who were away from home at the time, and the effect of these factors on, say, the congenital malformation rate is quite unknown. Thirdly, the number of people who survived high exposures was not large and therefore there were comparatively few births in this group; estimates of the incidence of congenital malformations and other abnormalities are consequently of low statistical precision, being open to relatively large disturbances through the operation of chance. Fourthly, only small effects would be expected in any given generation, even if the mutation rate had been raised many times.

169. Any opinion on a report which is still only in draft must be regarded as provisional. In our view, however, the data suggest an effect of the bomb radiations on genetic factors in prenatal survival, as shown by the sex-ratio at birth. The evidence for this effect is not highly significant statistically

and any change which was induced in the sex-ratio is unlikely to have exceeded 2 per cent per 100 r exposure of one parent. This appears to be the only positive conclusion that might perhaps be drawn but it is possible, for several of the measurements, to set upper limits to the changes that might have occurred without being detected. From the nature of the evidence a possible doubling, but not more than doubling, of the congenital malformation rate, or a 50 per cent rise in the stillbirth rate, following exposure of one parent to 200 r, might have escaped detection.

170. Although it was possible to set an upper limit to the increase in sex-ratio, congenital malformation rate and stillbirth rate, we were unable to do so for the increase in mutation rates of the genes responsible. For this purpose it would first be necessary to know what proportion of prenatal death or malformation is in ordinary circumstances due to newly mutated genes and what proportion to genes already present in the population. We cannot, therefore, derive from the Atomic Bomb Casualty Commission's data any estimate of the mutation-rate-doubling radiation dose for man.

The Radiation 'Doubling Dose' for Human Mutation Rates

171. At this stage it becomes necessary for us to attempt to give a quantitative estimate of the magnitude of the effect of any given dose of radiation on the mutation rate in human populations. This is an extremely difficult task, since not only have we as yet too little precise information on which to base an accurate estimate, but also it is by no means simple to know in what terms the effect should best be measured.

172. We have seen that all genes mutate spontaneously. The spontaneous mutation rate (s) of any particular gene can be considered to be made up of two parts; some of its mutations (x) will be provoked by the naturally occurring radiation, while others (y) will be due to other influences, so that $s = x + y$. It would be easy to find a theoretically adequate measure of the effect of increased radiation on mutation, if all mutations were caused by radiation of some kind, y would then be zero; and, since we have seen (paragraph 127) that radiation-induced mutations increase in simple proportion to the amount of radiation, it follows that, if the amount of radiation were doubled, the mutation rate would be doubled also, and so on. We could express the effect of increased radiation in terms of the 'doubling dose', that is the quantity of radiation required to double the spontaneous mutation rate. It is clear that the doubling dose under these circumstances would be equal to the naturally occurring radiation.

173. However, as we shall see later (paragraph 178), there is good evidence that in the few well-studied animals spontaneous mutations are not due solely to radiation, and therefore we cannot safely assume that y is zero. The situation would remain fairly simple, provided x and y were always in the same proportion, so that we could assume that a certain constant fraction of the spontaneous mutations of each gene is caused by natural radiation; but this also seems unlikely to be the case. Older parents have accumulated higher doses of radiation than younger parents; if all genes were equally sensitive to radiation, the frequency of all new mutations should then show the same increase in the children of older fathers as in the children of older mothers. It is found, however, that for some human genes, but not for others, the increase is more marked in children of old fathers than in those of old mothers. This fact suggests that the genes which exhibit this relationship to paternal age differ from other genes in that their mutation is dependent in an important way on something other than radiation, perhaps on the number

of cell divisions since conception, which is much greater for sperm than for eggs. There is also some evidence from experiments on flies and other organisms, in which high doses of radiation were employed, that certain genes are more radiation-sensitive than others. In view of these facts, it is only safe to assume that the same may be true of human genes, and that for each gene the spontaneous mutation rate is built up of both an x and a y fraction, which do not always bear the same proportionate relation to each other.

174. If this is so, an amount of extra radiation which will double the mutation rate of the most radiation-sensitive genes will have a much smaller effect on the more radiation-tolerant ones. It is then impossible to give any one figure which will measure the effect of radiation on the whole set of genes. However, in practice we still know so little about human mutation rates that we can, provisionally, make some simplification of the theoretical considerations. We can attempt to assess the effects of increased radiation in terms of that dose of radiation which will double the spontaneous mutation rate of an adequate and representative sample of the most sensitive genes. This would be a minimum estimate of the doubling dose. By 'adequate and representative' we mean that we must consider a sufficient number of the more sensitive genes to get examples of all the different kinds of genetic effects. Fortunately there is no reason to doubt that, if one considered a fairly large number of the most radiation-sensitive genes, they would contain examples of genes with all possible kinds of effect. We shall assume that this is indeed so, and, further, that there are sufficient genes with roughly the same degree of radiation-sensitivity for us to employ the concept of a representative doubling dose of radiation of the kind which we have been discussing.

175. The attempt to estimate a figure for the minimum representative doubling dose in man is beset with many difficulties. We have as yet no useful direct evidence. The only data which might provide information about actual increases in human mutation following irradiation are those from the investigations of the results of the atomic bombs in Japan, and those on the offspring of radiologists, and, as we have already seen (paragraphs 162-170), in neither is the material sufficient to lead to any firm conclusions. At the present time, therefore, we are driven to making indirect estimates.

176. Perhaps the most firmly based line of argument towards an indirect estimate is one which leads us to an assessment of a minimum figure above which the doubling dose must almost certainly lie. Let us suppose that all human spontaneous mutations are radiation-induced; then, provided the mutation rate increases in direct proportion to the radiation, the doubling dose would be the same as the quantity of natural radiation received. The only way of escape from this argument would be by the supposition that for human genes the mutation rate is not directly proportional to the radiation, but that they are comparatively insensitive to small doses up to the level of natural radiation, and relatively much more sensitive to doses slightly greater than this. There is no evidence in any other animal for such an effect, but a few experiments in plants, the results of which are not entirely consistent, have suggested an effect of this kind, though only a slight one. It therefore seems safe to argue that, even under the most pessimistic assumption that all human spontaneous mutations are induced by radiation, the doubling dose could not be less than the normal amount of natural radiation. It will be seen, in the chapter on exposure levels, that in this country, over a period of 30 years this amounts to about 3 r to the reproductive organs. We may therefore take this figure as the lower limit of our estimate.

177. The next step is to try to determine whether the actual value of the doubling dose is quite near this limit or considerably above it. There are several ways in which we can proceed. We may first ask whether the Japanese data are compatible with a doubling dose as low as 3 r, or whether, if the value were as low as that, one would have to anticipate rather striking effects in place of the almost complete absence of definitely significant results which was actually observed. Calculations show that, if some more or less plausible assumptions are made, the absence of definitely recognisable effects in the Japanese data does not contradict a doubling dose as low as 3 r, although it is of course more easily accounted for if the real doubling dose is considerably higher.

178. Our lower limit for the doubling dose was based on the supposition that all spontaneous human mutations are caused by radiation. If natural radiation accounts for something less than 100 per cent of spontaneous mutations, then this lower limit would be raised accordingly. In experimental animals, one can determine what fraction of spontaneous mutation is due to radiation by measuring the effects of several different large radiation doses and extrapolating the results to the naturally occurring dose. Even among experimental animals, it is only for fruit flies that we yet have sufficient information to do this with much confidence. It turns out in this case that natural radiation accounts for only about one ten-thousandth (0·0001) of their spontaneous mutations. In trying to extend this result to man, we have to take into account two considerations. The first is that the longer the time elapsing between the conception of an individual and his reproducing, the greater the dose of radiation he will accumulate. The second is that, if the genes are equally radiation-sensitive in two species, the fraction of the spontaneous mutation induced by natural radiation will be smaller if the spontaneous rate is large than it would be were the spontaneous rate small. If one compares the lengths of the pre-reproductive periods of man and flies one finds that man has time to accumulate about 1,000 times as much radiation as the fly. We are much less certain about the comparison of their spontaneous mutation rates, since figures for man are not available for many loci (Appendix C). However, estimates have been made that the human mutation rate is probably about five times as great as that in flies. We should then find that the fraction of the human spontaneous rate due to radiation can be estimated at 1,000/5 times the fraction which holds for flies. i.e 200 × 0·0001, or about 2 per cent.

179. The argument given above is based on the hypothesis that the sensitivity of human genes to radiation, that is the mutation rate induced per roentgen, is the same as it is in flies. As has been repeatedly pointed out, we have no definite evidence about the radiation-sensitivity of human genes, by which this assumption could be checked. The mouse is the only mammal for which we have any evidence on the radiation-sensitivity of genes. The induced mutation rates for a small number of genes have been roughly measured for this species. The experiments suggest that its genes are about ten times as sensitive as fly genes; but it should be noted that this figure depends very largely on only one of the seven genes tested. However, in order to be cautious we may make the hypothesis that mouse genes are ten times as sensitive as fly genes, and that human genes are similar to mouse genes. According to this hypothesis, we must increase our estimate of the fraction of human spontaneous mutation rate due to radiation by ten times, from 2 per cent to 20 per cent.

180. According as we suppose the radiation-induced fraction of the spontaneous mutation rate of man to be 20 per cent or 2 per cent, we arrive

at estimates for the doubling dose that are five or fifty times the naturally occurring radiation, that is 15 r or 150 r. It must be pointed out, however, that the calculations which have just been given have involved a number of quantities which are still only imperfectly known; for instance, the spontaneous mutation rates of flies, mice and particularly men. One cannot therefore, on this basis, absolutely exclude the possibility that the doubling dose may actually lie somewhat below 15 r.

181. Various other theoretical methods have been suggested for utilising the data about experimental animals to calculate a doubling dose for man. They all lead to values within the same rather wide range as we have just reached. Moreover, they all involve even more conjecture about quantities on which we have little precise information, such as the comparative numbers of gene loci in man and other animals. We shall not attempt to summarise them here.

182. There is another rather different type of approach to the problem; that is, to compare the values of the minimum representative doubling dose in the animals and plants for which we have the most reliable data (Appendix H). They mostly run from about 25 r upwards, many of them being between 25 and 60 r. Only a few types of organisms have yet been studied in detail, but taken as it stands this evidence would suggest that all doubling doses lie in about the same range, and it is therefore possible that man's may do so too. It is unfortunate, though easily understandable, that none of the fully investigated organisms has a lifetime comparable in length to that of man, and this suggests the necessity of caution in applying the results to man. But one might expect, *a priori*, that evolutionary processes would have acted to reduce the radiation sensitivity of the genes of organisms with long pre-reproductive periods, so that they would have higher doubling doses. Thus, this line of approach would lead us to expect the human doubling dose to lie above 25 r.

183. The discussion in the last few paragraphs has been given at some length in order to bring out the great uncertainty of our present knowledge of the doubling dose for most human genes. Mustering all the arguments at our disposal, we can only come to the conclusion that it almost certainly lies above 3 r, but that it may be as much as 150 r or even more. Any statement which goes beyond this can only be phrased in terms of probabilities, and depends on a judgment made by balancing all the different lines of argument against one another. In this tentative fashion, we should advance the view that there is little likelihood that the representative value lies between 3 r and 15 r; and that, although we cannot exclude the possibility that for some human genes the doubling dose may be less than 30 r and for others more than 80 r, the best estimate which we can make, in the light of present knowledge, is that the representative value lies between 30 r and 80 r.

184. It remains to consider what dangers would arise if we have, for lack of adequate information, materially over-estimated the value of the doubling dose for human genes. Even if we suppose that it is actually as low as the minimum that we can reasonably entertain, namely 15 r, it is extremely improbable that in times of peace the whole population, or a large fraction of it, will receive an additional dose of this magnitude from industrial or other sources. We need not therefore anticipate any general danger. There may, however, be small groups of people, for instance those employed in certain industrial processes or receiving medical treatment involving X-rays, who may be exposed to doses near the representative doubling dose. Have they grounds for fearing any disastrous effects on their descendants? In the first place, it is obvious that if, for reasons of age or other considerations, they do

not reproduce after the period of exposure, no genetic effects at all will eventuate. If they do reproduce, there are two aspects to be considered: the effect on their immediate offspring and the effect on their later descendants.

185. We shall consider first the immediate descendants. It has been calculated on theoretical grounds that at the present time, without any additional radiation, approximately one human germ cell in ten (10 per cent) carries a new mutation. The great majority of these are recessive, and only very rarely have an effect on immediate offspring. Probably not more than one in a hundred is a dominant, the action of which will be seen in the next generation. Thus, a doubling of the mutation rate might lead to an increase of one in a thousand (0·1 per cent) in the numbers of harmfully affected children in the next generation. This must be compared with the present chance that the children born in a family will be congenitally defective. At present, about four per cent of all babies are stillborn or die shortly after birth, while another two per cent survive but are malformed; and in addition a considerable number in later years develop diseases or abnormalities in which hereditary constitution is a preponderating cause. Thus a doubling of the mutation rate in one parent would only add to the chance of producing a defective child an additional 0·1 per cent. above the present level of about seven to eight per cent.

186. A more realistic estimate of individual genetical risk can be obtained from the figures given in paragraphs 141 to 150. For example, the ordinary risk that any pair of parents will produce an imbecile or an idiot—that is a case of severe mental defect—which survives, is about one in 500. The increased proportional risk for parents in both of whom mutation rates have been doubled is three per cent; this means that the risk of their having a child with severe mental defect which survives is one in 485. If only one parent is affected, the risk would be increased by a factor of 1·5 per cent, so that the chance would then be about one in 493. Similarly, the risk of producing psychotic offspring might be increased by a factor of one per cent if mutation rates were doubled in both parents and by half this amount if only one was affected. The likelihood of miscarriage, stillbirth, or foetal malformation would probably be even less increased; compared with the changes in incidence which occur, for example, at different maternal ages or between the first and later births, these alterations would be inappreciable. The risks of occurrence of specific dominant or sex-linked traits, such as those listed in the table of human mutation rates (Appendix C), would indeed be proportionately much more markedly affected; but, because of their rarity, the risks of these abnormalities are ordinarily considered negligible for the individual and, even after being nearly doubled, they would remain so.

187. In ordinary circumstances, if a parent carries any given allele, the chance that one of his offspring receives it is one in two; a grandchild has one chance in four of receiving it and a great-grandchild has one chance in eight. The same rule applies to an allele which has arisen by fresh mutation in the parent. Thus it follows that the extra risk of disability, which applies to the children of an individual who has been exposed to doses of radiation causing mutation, will be halved in each subsequent generation of his offspring, provided that the level of mutation rate in the rest of the population has not also been raised. Moreover, it must be remembered that under natural conditions every human being already carries a certain number of harmful recessive genes, the results of spontaneous mutation in the past. There is therefore no reason why an individual in whom the mutation rate has been doubled, or increased by some similar figure, need fear that he runs an appreciable risk of founding a 'bad line' of descendants.

188. One may conclude that, if a relatively small group of prospective parents receives a doubling dose of radiation, no noticeable effects will be produced either on their immediate offspring or upon their descendants. For levels of radiation up to the doubling dose, and even some way beyond, the genetic effects of radiation are only appreciable when reckoned over the population as a whole and need cause no alarm to the individual on his own account.

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CHAPTER V

EXISTING AND FORESEEABLE LEVELS OF EXPOSURE TO RADIATION

Introduction

189. Throughout the whole of his evolutionary history man, like all living organisms, has been exposed to small but variable amounts of ionizing radiation from his natural surroundings. To these he has now added similar radiations from his own inventions. In their biological action these differ but little from each other, and all must be taken into account when assessing the present hazards from ionizing radiation. We shall first consider those inescapable radiations which come from the natural background and, thereafter, those which are derived from sources controllable by man.

190. It will be clear from an earlier chapter of this report that relatively heavy doses of radiation are required to impair the health of the individual and such doses are rarely associated with the ordinary circumstances of civilian life. The use of radiation for medical purposes or occupational exposure to sources of radiation may be associated with the possibility of high doses, but every precaution is taken to safeguard the patient, and the employee is protected by nationally and internationally recognised recommendations which limit the doses received occupationally to levels considered to be safe.

191. On the other hand, our knowledge of the genetic effects of radiation is less precise and it is believed that doses of radiation which have no known effects on the health of the individual may be of genetic consequence. Doses received from all sources, however small, have therefore to be assessed in the light of their possible genetic implications. It has been seen that ionizing radiation can have genetic consequences only in so far as it affects the reproductive organs—the gonads—and it is thus the dose received by the gonads up to the end of reproductive life which must be estimated in all cases. At the levels of dose with which we are mainly concerned, the genetic effects of radiation can be calculated only in relation to the population as a whole. It is therefore in terms of the total gonad dose* to the population that the following estimates of the exposures from various sources have been made

Radiation from Natural Sources

Cosmic radiation

192. Cosmic radiation reaches the earth from interstellar space. The atmosphere surrounding the earth has substance and acts as a filter, absorbing almost all the dose to which otherwise we should be exposed. In general, the longer their path through the atmosphere, the more the radiations will be attenuated; thus, the dose at sea level is less than that at high altitudes.

* The total gonad dose has been calculated on the basis of the considerations set out in paragraph 129.

193. Cosmic radiation has several components—protons,[1] fast and slow mesons,[2] electrons[3] and neutrons[4]—which differ in their relative contribution to the natural background of radiation according to the altitude; at sea level the most important are mesons, electrons and neutrons. Mesons and electrons are considered to be, for equal physical doses, of about the same biological effectiveness as gamma rays. Fast neutrons and protons of high energy may be several times more damaging to the individual but are less likely to induce gene mutation. Neglecting this possible variation in effectiveness and assuming equal biological efficiency for all the particles, one derives a dose at sea level equivalent to 0·028 r per annum from cosmic radiation. Virtually all of this is highly penetrating radiation and can be assumed to irradiate the whole body, including the gonads, almost uniformly.

Terrestrial radiation

194. A few of the naturally occurring elements, particularly the heavy elements, are radioactive, thorium and uranium being the chief primary sources. These two elements are only feebly radioactive and each has a half-life measured in many millions of years. Each atom, in its radioactive disintegration, is transmuted to a daughter atom which is also radioactive and which in turn disintegrates to another radioactive atom, the process being continued until ultimately a non-radioactive stable atom of lead is formed. All the daughter elements decay much more rapidly than the original parents, thorium and uranium, and many of them emit gamma rays as well as nuclear particles in their disintegration.

195. Thorium and uranium are almost universally distributed in trace quantities in rocks and soils, areas of granitic rock usually having higher concentrations than sedimentary rock. Occasionally, the concentration is considerably greater than normal, sufficient to make the area worth mining, but even in these rich lodes it is usually only of the order of one per cent or less.

196. As a consequence of the presence in many soils of the radioactive daughter products of uranium and thorium, emission of gamma rays occurs widely over the land surfaces of the earth. Brick and stone necessarily contain traces of these radioactive substances and, inside houses built of such materials, radiation is added from this source. On the other hand, substantial structures of brick and stone offer slight shielding from cosmic radiation.

197. The amount of radiation contributed from the earth and from buildings varies from place to place even in the same country and any average figure can be only an approximation, but 0·078 r per year would perhaps be representative of the amount received inside buildings on the surface of the body by the inhabitants of this country. The corresponding figure in the open would perhaps be 0·048 r per year. Measurements show that about 37 per cent of the dose of gamma rays is absorbed superficially and filtered off before reaching the internal tissues and organs: allowing for this and esti-

[1] *Protons* are nuclei of hydrogen atoms and carry a unit positive charge. Fast moving protons are a minor component of cosmic rays at ground level. Neutron radiation also ejects protons from hydrogenous material.

[2] *Mesons* are unstable particles with masses intermediate between those of electrons and protons. Energetic mesons constitute the main component of the more penetrating cosmic rays.

[3] *Electrons* are the smallest constituent particles of atoms. The electron carries the elementary negative charge of electricity. It has a positively-charged counterpart of equal mass called the positron. The soft or less penetrating component of cosmic rays consists essentially of positrons and electrons and about an equal number of gamma rays.

[4] *Neutrons*, see Chapter II, paragraph 19.

mating the amount of time that a person spends in the open, the average dose to the gonads of persons in this country from external gamma radiation is estimated to be 0·043 r per year.

Atmospheric radon

198. One of the decay products of uranium is radon, a gas which diffuses out of the earth and buildings and from minerals such as coal. In general, its concentration in the atmosphere is extremely low, about 0·3 of a micro-microcurie* per litre of air, but in cities such as London, where much coal is burnt, it may sometimes reach ten times this amount. In these circumstances, the dose of gamma rays from the further disintegration-products of radon may almost equal the dose of radiation from cosmic sources. The average dose to the gonads from this source, however, probably does not exceed 0·001 r per year from the atmosphere and an approximately equal or slightly greater amount from the gas absorbed into the body from the lungs.

Radioactive constituents of the body

199. Among the normal constituents of the body are the elements carbon and potassium, each of which has a radioactive isotope occurring naturally as a minute fraction of the total element. The radioactive isotope potassium 40 forms 1/8000 of natural potassium and emits both beta particles and gamma rays. An average value for the potassium content of the body is 0·21 per cent by weight, and measurements suggest that the figure for the gonads does not differ greatly. Calculation on this basis gives an estimated dose to the gonads of about 0·02 r per year. Naturally occurring radioactive carbon, carbon 14, constitutes one part in a million millions (10^{12}) of natural carbon; it emits only beta particles. Taking body tissue to be 18 per cent carbon, one derives a dose from this source of about 0·001 r per annum to the gonads.

Total gonad dose from natural sources

200. Information on the total dose to the gonads from natural sources of radiation is summarised in Table 2. It will be seen that, from all sources, the total is roughly 0·1 r per annum, or about 3 r per generation of 30 years (Appendix J).

TABLE 2

Estimated dose rates to gonads from natural sources of radiation

Radiation source	Estimated average dose rate to gonads in roentgens per year
External radiation	
Cosmic rays (sea level)	0·028
Gamma rays from the earth	0·043
Radon in air	0·001
Internal radiation	
Potassium 40	0·020
Carbon 14	0·001
Radon and decay products	0·002
Total	0·095

* $\frac{1}{1,000,000,000,000}$ of a curie.

Radioactivity in bone

201. Within the body, radioactivity is probably highest in bone. The naturally occurring radioactive heavy elements are contaminants of food and water as well as of soil and they and their disintegration products, of which radium is the most important biologically, are absorbed to a very limited extent from the gut. The body removes them from the circulation and stores them in bone. In this way the radioactivity of bone builds up gradually throughout life but there is no evidence that the rather higher doses in bone compared with other tissues are deleterious. Table 3 gives the estimated doses to bone from these sources.

TABLE 3

Estimated radium content and radiation dose rates to bone-cells

Geographical conditions	Estimated radium in skeleton at age 35 in micro-microcuries	Dose rate to bone-cells in equivalent roentgens per year		
		radium	external sources	total
Average areas	60	0·037	0·08	0·12
Active areas ... ... (See Appendix J)	1,100	0·37	0·18	0·55

Radiation from the Appurtenances of Civilisation

202. Since the discovery of X-rays and radioactive materials man has adopted them increasingly for certain of his needs. In medicine they now provide invaluable, and often irreplaceable, aids to diagnosis and treatment. They are also used extensively in industry and in the amenities of modern civilised life. It is necessary, therefore, to assess the dose that these developments contribute above that received from the natural surroundings.

Diagnostic X-rays

203. For some time it has been realised that by far the largest contribution is made by diagnostic X-rays. Continuing efforts are therefore being made to assess the dose of radiation given to the whole population in this way. The problem of accurate assessment is beset with difficulties and at present any estimate must be based upon very imperfect data. The two basic requirements and the best means of fulfilling them from available figures are as follows:

(i) *The number of radiological examinations made annually, subdividea according to the sex and age of the persons examined and the part of the body under examination.* It is possible to make from published figures a reasonable estimate of the total number of X-ray examinations made within the National Health Service. A minimum figure for the year 1955 would be 12,200,000. To this must be added an unknown but relatively small number for hospitals outside the National Health Service and for private practice. This number has been estimated at rather less than half a million, giving a total of 12,650,000.

The division of this very large figure into sex, age and type of radiological examination has been made on the basis of information supplied by five hospitals only and covering some

21,000 patients (two London teaching hospitals and three others). While there are certainly differences between these hospitals, they are not so wide as to make an approximate calculation unwarranted. The sample has therefore been accepted as representative.

A very important source of radiation to the gonads is provided by radiological pelvimetry, which measures the size of the pelvic outlet of the woman in relation to the actual or potential size of her infant; a second important source is X-ray examination of the abdomen in pregnancy. Information of the frequency with which such examinations are made has therefore been specially sought from a rather wider group of nine hospitals. On the assumption that these are representative, the results have been applied to all live births occurring in hospitals in England and Wales.

In addition to the 12,650,000 X-ray examinations already mentioned, certain others are undertaken for special reasons and these bring the estimated total during 1955 up to nearly 18,000,000. The additional examinations include mass miniature radiography, dental radiography, and examinations of service personnel and mineworkers, but each of these types of examination represents a relatively unimportant source of radiation to the gonads and the effect of any error in estimating their contribution will be slight in relation to the total.

(ii) *The average dose to the gonads—male, female and foetal separately—produced by the diagnostic irradiation of each separate part of the body.* For the purposes of calculation the figures used, which cover X-ray examinations of 24 different parts of the body, have been derived almost entirely from one London teaching hospital where careful measurements have been made and precautions taken to restrict the irradiation of the gonads to the lowest possible level. They are therefore almost certain to be minimum figures. It should be realised that with the X-ray tube only slightly misaligned the dose may sometimes be multiplied many times.

204. Bringing together these two sets of figures—the estimated numbers and ages of persons irradiated and the estimated average gonad dose delivered to them with specified examinations—leads to two important general conclusions. It shows, first, that almost the whole population dose is accounted for by a relatively few sites of examination, principally the hip, the lumbar spine, the lower abdomen and the pelvis. The far more frequent examinations of chest, head and limbs make relatively unimportant contributions. Secondly, according to the present calculations, the amount of radiation reaching the reproductive organs of the people of this country through diagnostic radiology is as much as 22 per cent of that derived from natural sources. Indeed, in view of the minimum figures adopted in these calculations, the contribution of diagnostic radiology may well be very considerably higher than 22 per cent. It undoubtedly forms the most important source of man-made irradiation and its application has been steadily increasing in recent years (Appendix K).

Radiotherapy

205. At present there is little information about the contribution to the population dose from therapeutic irradiation. Although its main use is for patients with malignant disease, the majority of whom are beyond the child-bearing age, some younger patients are treated for non-malignant conditions such as ankylosing spondylitis. Less penetrating X-rays are widely used in the treatment of a large number of diseases of the skin, and the artificially

produced radioactive element iodine 131 is now being administered for hyperthyroidism.

206. It is as yet not possible to state a figure for the population dose to the gonads from this source of radiation. Rough assessments would suggest that it is considerably less than the dose from diagnostic radiology but probably greater than that from any other source. The problem is one upon which research is required.

X-ray fluoroscopy for shoe-fitting

207. X-rays are used commercially for fitting shoes but, with modern equipment and good practice, it appears that the number of machines in operation would probably deliver not more than 0·1 per cent of the dose to the gonads received from natural radiation.

Luminous watches and clocks

208. Watches and clocks with luminous dials depend for their luminosity upon the rays from radium or other radioactive material used in the paint. Measurements and calculations suggest that the average wrist-watch contains about one-fifth of a microcurie of radium. A calculated dose to the gonads from wearing such a watch is about 0·01 r per year.

209. From the information given by the trade, it can be deduced that there are in use about three million men's watches and about a million women's and children's watches with luminous dials. In addition, there may be about ten million luminous alarm clocks. On this basis it can be estimated that the population dose from this source is about one per cent of the natural background.

Television sets

210. Cathode-ray tubes for television sets are capable of causing the production of X-rays. In general, however, the operating voltages are comparatively low and the X-rays are readily absorbed by the walls of the tubes and by protective screens. It can be estimated that the population dose from this source is at present much less than one per cent of the dose received from natural radiation.

Cosmic radiation in aircraft

211. Since the amount of radiation from cosmic sources is greater at high altitudes, the doses received by persons in aircraft have been investigated and the population dose calculated. The additional dose averaged over the whole population is at present insignificant compared with that received from the natural background.

Occupational Exposure to Radiation

Medical and industrial workers

212. Men and women have been exposed to ionizing radiation in the course of their occupations for over half a century. In the early years after the discovery of X-rays and radioactive materials many suffered injury, but safe practices have gradually been elaborated and standards of safety laid down. In this country since 1921 the British X-ray and Radium Protection Committee, and later the Medical Research Council's Committee on Protection against Ionizing Radiations, have considered the available information and made periodic recommendations on occupational exposure levels. Equivalent levels have been advocated by the International Commission on Radiological Protection.

213. Control of radiation exposure may be effected in two ways. In the first, the intensity of radiation in the vicinity of the source is measured at frequent intervals and, provided that the intensities are always below those levels accepted by international agreement as being without danger, no separate check is required on the exposure of the individual. In the second, the doses actually received by the individual are recorded, usually by means of a photographic film which he carries and which blackens on exposure to radiation, so allowing the dose received, if any, to be estimated. For twenty years the National Physical Laboratory has provided a service whereby such films are issued on demand and subsequently read. The worker and his supervisor can thus keep a check on the doses received to ensure that the accepted weekly levels of dose are not exceeded.

214. The provision of film badges has now been taken over by the National Radiological Protection Service which serves the majority of people known to be occupationally exposed, other than those employed by the Atomic Energy Authority or in the many hospital departments which process their own films. The Service will be available to those employed in the many and increasingly varied industrial applications of ionizing radiation as well as to research laboratories.

215. The available records have been sampled and analysed to assess the total dose from these sources to the population but, as precise records are not available from all branches of industry, the contributions from some sources are estimates only. It has not been possible to make as accurate an assessment as for the employees of the Atomic Energy Authority because the number, sex and ages of those exposed are not known with any precision and the monitoring, when carried out, is not as complete.

216. On the basis of figures from the hospitals which make use of this service, it has been roughly calculated that about 60 per cent of the medical workers at risk are women; in industry and research, women constitute only 15 per cent. It is estimated that in total about 14,000 people are employed, of whom half are women. After allowance for the fact that in women radiation is more completely absorbed before it reaches the gonads, it is estimated that the average gonad dose for both men and women would be about 2·5 r per year. Because the ages are not known with precision, it is not possible to make an accurate estimate of the genetic dose to the population as a whole, but after making certain assumptions we have reached a figure from this source of 1·6 per cent of natural background radiation (Appendix L).

Atomic Energy Authority employees

217. The Atomic Energy Authority now employs about 7,000 people who are exposed or potentially exposed to radiation in the course of their work. All employees liable to be exposed wear film badges that are examined weekly or monthly, and the sex and age of each individual are known. Thus it has been possible to calculate, with considerable accuracy, the doses received by the employees in relation to their expectation of parenthood. The average dose to the Authority's employees from all occupational sources of radiation is 0·4 r per year. The results of personnel monitoring show that in all recent years no employee has had an average weekly dose exceeding the maximum permissible and that about 90 per cent of the persons exposed to radiation averaged less than one-tenth of the maximum permissible weekly dose. It has been estimated that the gonad dose averaged over the population as a whole is about 0·1 per cent of the natural background.

Contamination of the World by Fall-out from the Explosion of Nuclear Weapons

218. Nuclear weapons differ in their construction and size and the same type of weapon can be detonated in different ways—under water, on land or in the air. These variations lead to differences in the radioactive dust produced, in its distribution and in the rate at which it falls out from the atmosphere on to the earth. Except in the immediate vicinity of a nuclear weapon explosion, the ionizing radiations to be considered arise from radioactive particles.

Radioactive fission products

219. Radioactive fission products, formed when the atoms are split, become mixed with the vapourised material from the bomb and with any earth, water or debris caught up in the explosion. Large particles fall quickly and are deposited close to the site of explosion; small particles are carried up with the hot gases to heights which vary with the power of the explosion. They subsequently travel in air streams for considerable distances and times, depending on the size of the particles and the height to which they were carried.

220. The particles reaching this country from the distant explosion of a typical nuclear weapon detonated over land are spherical in shape and consist of fused silica and metallic oxides impregnated with fission products. The vast majority are smaller than 0·001 of a centimetre in diameter. Particles from the thermonuclear tests in the Pacific atolls differ, in that they consist of calcium oxides or carbonates from coral and are irregular in shape. Both varieties are radioactive.

221. Different types of weapon produce a similar mixture of fission products in slightly different proportions, but the overall rate of decay of radioactivity is almost the same in all types. The total radioactivity decays to one-tenth of the original level for each seven-fold increase of time in days, as measured from the moment of detonation; thus, if the radioactivity is one unit on the first day after detonation, it is 1/10 unit at seven days, 1/100 unit at 49 days, and so on.

222. At the time of atomic explosions some normally stable elements may become radioactive by virtue of the capture of neutrons. Such induced activities are for all practical purposes short-lived and therefore of little importance when long-term hazards are being considered.

223. Since January 1951, continual watch has been kept by the Atomic Energy Authority on the radioactive fall-out reaching this country from nuclear devices exploded in other parts of the world. The activity in rain water at selected sites is recorded continuously. In addition the atmosphere is sampled daily by the collection of dust on a cylindrical filter through which about 1,500 kilogrammes of air are passed, and the radioactivity of the filter papers is determined (Appendix M).

Radioactivity in air

224. When the ordinary type of atomic bomb is exploded in Nevada, the dust-cloud rises to a height of about 40,000 feet. It then travels eastwards with the winds which prevail at that height and diffuses both vertically and laterally. It may or may not pass over this country on its first circuit round the world; if it does so, it will usually appear about 5 days after the explosion. The cloud continues circling the earth, and peaks of activity can be detected over any particular place in its path at about monthly intervals. The total radioactivity per unit volume of air falls progressively with time

owing to decay of the radioactive elements, to increased spread of the cloud and to deposition on the surface of the earth; approximately half the available material is deposited every 22 days.

225. Clouds from thermonuclear explosions behave differently because of the far greater height, approximately 100,000 feet, to which the debris is carried. Diffusion downwards from the stratosphere is a very slow process, and months after a thermonuclear test explosion most of the radioactive debris is still at these great heights.

226. Dust clouds from distant tests passing on the first circuit over this country are usually too high to impart measurable activity to air at ground level. Subsequently, at the peak periods, concentrations are in the region of five radioactive disintegrations per minute per cubic metre (dpm/m^3) of air. From April, 1952, to December, 1955, the mean concentration of activity from all bombs exploded in that period was 0·5 dpm/m^3. The corresponding average activity from naturally occurring radon decay products in the air was measured at one of the sampling stations and found to be 130 dpm/m^3. The debris from the thermonuclear tests in the Pacific in 1954, much of it by now already decayed, has mostly still to come down, but it is not expected to exceed 0·1 dpm/m^3 in the next few years. The dose to a person fully exposed to air with a radioactivity of 0·5 dpm/m^3 has been calculated to be one millionth of a roentgen per year.

Deposited radioactivity

227. The radioactive fall-out is cleared, sooner or later, from the air by deposition. Rain contains the bulk of deposited activity and continuing measurements have been made since 1951 of the radioactivity of rain water collected from specially treated roofs. Any radioactive dust deposited on the roofs in spells of dry weather is washed off and included with the next sample of rainwater. From these measurements the amount of radioactivity deposited per square mile can be determined for each explosion.

228. The dose that a man standing in the open in this country would receive from the deposition of radioactivity from all bombs so far exploded has been estimated. It has been assumed that all the radioactivity remains on the surface of the earth and that none is lost, as we know some will be, by drainage and weathering. Including all ordinary atomic bombs exploded before December, 1955, and calculating all the radioactivity which they have contributed and will contribute over the next 50 years, it is found that the total dose which a man continuously out of doors, night and day, would receive is 0·005 r. To this dose from ordinary atomic bombs must be added the dose from thermonuclear weapons. For these latter the dose from the radioactivity still to be deposited is more important. It can be estimated that the accumulated dose from thermonuclear weapons is 0·002 to 0·003 r with another 0·027 r still to come.

Total radioactivity from weapons already exploded

229. All these doses together add up to about 0·035 r from weapons already exploded. This is a maximum dose. The loss of radioactivity from weathering has not been taken into account, nor has the protection afforded by buildings in and around which most people in this country spend a large part of their lives. It would be realistic to divide the dose by three for weathering and by seven for protection afforded as a result of time spent in houses. The average inhabitant of this country may therefore receive in the next 50 years between 0·001 and 0·002 r from this fall-out, or 0·02 to 0·04 per cent of the radiation that he will receive during the same period from natural surroundings.

230. If the firing of both types of bomb were to continue indefinitely at the same rate as over the past few years, there would be a build-up of activity gradually reaching a plateau in about a hundred years time which, on the same basis of calculation, would give the average individual a dose over a period of 30 years of 0·026 r or about 0·9 per cent of what he would receive in the same period from natural sources.

231. The most impressive feature of these figures for exposure from fall-out is the very great effect of a very few thermonuclear explosions. This is not surprising since their power is measured in equivalents of millions of tons of TNT compared with the thousands of tons for atomic bombs. If the rate of firing this type of weapon increases, the radiation exposure will be altered proportionally.

SPECIAL HAZARDS FROM RADIOACTIVE FISSION PRODUCTS

Particulate contamination

232. The total radioactivity in the air from fall-out is measured daily, and determinations are made of selected individual radioactive substances. The activity will arise from particles which may be inhaled into the lung. It can be calculated, however, that not more than one or two particles of the more highly active substances are breathed by any person in the course of a year (Appendix M). Although the radioactivity of these particles is minute, it is concentrated in a few million-millionths of a cubic centimetre (10^{-12} c.c.) and the possibility of their creating a hazard must therefore be considered. The International Commission on Radiological Protection has concluded that the critical volume of tissue to be taken into account is of the order of one cubic centimetre, which is a larger volume than could be heavily irradiated by a few such radioactive particles. It seems unlikely that particulate contamination of the air from the fall-out from test explosions would constitute a problem in ordinary civilian life.

233. Radioactive material from the air is deposited and accumulates on the ground where it may contaminate drinking water and agricultural crops. After deposition it becomes possible to make routine measurements of the present concentration of the more dangerous radioactive substances. By December, 1955, this concentration amounted to 0·011 curie of strontium 90 and 0·0002 curie of plutonium 239 per square mile. The continuing fall-out from explosions which have already taken place will cause a rise, to a maximum by about 1965, of around 0·045 curie of strontium 90 per square mile. These figures should be viewed against the background of the fact that the top one foot of soil has always contained on the average about one curie per square mile of the equally, if not more, dangerous naturally occurring radium.

Strontium 90

234. From the point of view of general contamination of the world, the hazard from deposited strontium 90 might, according to present ideas, be greater than that from external radiation, since strontium, like radium, is ordinarily retained in the body and deposited in bone. The average concentration of radioactive strontium in rain water over a period of three years ending December, 1955, was 1·7 micro-microcuries per litre and, since most water passes through soil before being drunk, the activity reaching human beings through drinking water is extremely small.

235. Strontium 90 is deposited on herbage and soil and is then absorbed into plants. Man and animals consuming the leaves of plants will therefore receive strontium 90 in food as well as in water. The hazard is greater for

grazing animals than for man, since animals may crop herbage from wide areas of contaminated pasture. Man, moreover, relies for his food more on grains and roots, which are not sites of concentration of strontium, and on animal produce from which the animal has removed most of the strontium to its bones. Cows' milk contains strontium, but fortunately the cow in its metabolic processes secretes calcium into the milk in preference to strontium.

236. The importance of radioactive strontium, compared with other long-lived fission products produced by exploding nuclear weapons, derives from four factors; its relative abundance among the fission products, its facility for following calcium through the human food chain, the ease with which it is absorbed, and the fact that, once absorbed, it is stored for long periods in the bones of the body. In bones it forms more or less localised deposits which, judging by animal experiments and according to analogy with the action of radium compounds on human subjects, can if present in sufficient amounts give rise to bone tumours or, by irradiating the neighbouring bone marrow, to aplastic anaemia or leukaemia. There is evidence that the young are more susceptible to its action than adults. Such measurements as have been made of strontium 90 in human bone suggest that the highest levels are at present about a thousand times less than is considered permissible for those occupationally exposed.

Plutonium

237. Plutonium 239, another very long-lived radioactive element, is also a potentially dangerous contaminant since, like strontium, it is deposited in bone. The amount of activity from plutonium in fall-out is small relative to that of strontium 90, its solubility is low, and less than one-tenth of one per cent of the amount taken by mouth is absorbed. The hazard from plutonium in fall-out debris is thus very small.

Caesium

238. Very sensitive methods of measuring the radioactivity of the human body have now been developed and, on the records obtained in recent months, there has been some indication of gamma radiation suspected to be due to the long-lived fission product caesium 137. Although it is not possible yet to identify with certainty the source of this radiation, calculations have been made on the assumption that it is due to this isotope. Caesium is not concentrated in any particular organ of the body and, on the basis of present information, is unlikely materially to affect the figure given above for the dose from the fission products deposited on the ground.

Other important isotopes

239. Of the other elements in mixed fission products strontium 89 and barium 140 behaves similarly to strontium 90; iodine 131, which is easily absorbed, is highly concentrated in the thyroid gland. These three isotopes, having half-lives which are only a matter of days, are of very little significance in relation to the long-range fall-out from atomic bombs, since most of their radioactivity will have decayed before they are deposited. In relation to the heavily contaminated areas within a few hundred miles of the explosion of a thermonuclear bomb, however, they will be of great importance.

TOTAL GONAD DOSE FROM MAN-MADE SOURCES OF RADIATION

240. Information on the total dose to the gonads from man-made sources of radiation is summarised in Table 4, the dose from each source being expressed as a percentage of the dose received from natural sources. It will be seen that the total dose amounts to approximately 25 per cent of that already received from the natural background.

TABLE 4

Summary of estimated population doses of radiation to the gonads expressed as percentages of natural background

Source of Radiation	Approximate dose to gonads as a percentage of natural background
Natural background	100
Diagnostic radiology	at least 22
Radiotherapy	?
Shoe-fitting	0·1
Luminous watches and clocks	1
Television sets	much less than 1
High altitude flying	insignificant
Occupational exposure:	
Radiology and Industry	at least 1·6
Atomic Energy Authority	0·1
Fall-out from test explosions	less than 1

Nuclear Warfare

241. Atomic bombs were developed for their capacity to create blast, which was the chief cause of casualties at Hiroshima and Nagasaki. The additional effects of the detonation of a nuclear weapon are due to the release of other forms of energy—heat and ionizing radiation. The ionizing radiations produced by the weapons are the new feature of military operations. Of the prompt radiations, produced at the moment of explosion, neutrons are the direct result of the process of nuclear fission. Gamma rays are also a by-product of fission but most of these are produced immediately after the detonation by the enormous quantities of radioactive fission products. The rapid ending of the gamma-flash at ground level after an explosion is due partly to the ephemeral nature of the radioactivities of many of the fission products and partly to the very rapid removal of the debris in the up-draught of hot gases.

242. The effective range of the prompt ionizing radiations from an ordinary atomic bomb explosion is less than that of the thermal radiation, and at Hiroshima and Nagasaki the range within which death and severe injury from ionizing radiations were encountered was about one mile, as compared with up to three miles for severe flash burns and five miles for indirect blast effects.

243. The notable feature of the ionizing radiations is that, in contrast to the heat rays, they are very penetrating. Clothing which may be adequate to shield the body from heat flash is 'transparent' to these rays, and even four inches of concrete transmit half the radiation at a distance of one mile from the atomic bomb burst. Thus, at ranges relatively close to such a weapon, people in stoutly constructed buildings might survive the effects of the heat and blast waves but suffer from the damaging effects of the penetrating gamma rays and neutrons.

244. From the comparative ranges of the heat flash and ionizing radiation it will be seen that distance is a factor of great importance. With gamma-rays, as with heat, the intensity falls in proportion to the square of the distance and is diminished by an attenuation factor. It is unlikely that the prompt radiations from thermonuclear weapons would be relatively more significant than those from atomic weapons. The hazard from radiation is

therefore only one of the immediate effects of nuclear weapon explosions, and a relatively minor one in a holocaust. Its particular importance lies in the delayed and distant effects, which arise from the radioactive fission products.

245. At Hiroshima and Nagasaki the atomic bombs were exploded high in the air so as to obtain the maximum effects from blast. Virtually all the fission products were therefore carried up with the hot gases and must have been enormously diluted before being deposited gradually round the world. However, some did fall to earth locally and presumably contributed to the delayed effects recorded by the Atomic Bomb Casualty Commission.

246. When bombs are exploded in or near the surface of the ground or near the surface of water, much debris highly contaminated with fission products is flung into the air and the large particles are deposited more or less locally. Some of intermediate particle size is carried by the local winds and gradually diffuses and settles down-wind. The best known phenomenon of this character following a test-explosion arose from the thermonuclear device exploded on 1st March, 1954. According to the Press release in February, 1955, of the United States Atomic Energy Commission, the area over which fission products settled to give a radiation dose which might well have been lethal to a man unable to take shelter was about 7,000 square miles. Over a considerably larger area, conditions in the open would have been hazardous to man and beast. The size and shape of these lethal and hazardous areas will vary with the conditions of the explosion and of the local meteorology. Nevertheless, the inferences are plain: weapons such as these can be devastating, not only locally over areas measuring hundreds of square miles, but in their more distant effects, which may occur over thousands and tens of thousands of square miles.

CHAPTER VI

ASSESSMENT OF THE HAZARDS OF EXPOSURE TO RADIATION

247. We have reviewed the effects of radiation both upon the exposed individual during his own lifetime and upon his descendants. It is now necessary to relate these effects to dose levels, and to attempt to assess the consequences of exposure to those levels of radiation which now occur or might conceivably come about in the future.

248. It will be recalled that human beings have always been exposed to radiation from outer space and from traces of radioactive materials in their surroundings and in their own bodies. The problem is, therefore, to assess the effects of any additional radiation to which they are or may be subjected, rather than to define a new experience. Our ignorance of the results of exposure to radiation is great and much intensive research is required in many fields; but the naturally occurring background of radiation to which mankind has long been adjusted can be taken with some confidence as a safe standard of reference, whether we are considering irradiation from outside the body or from radioactive materials taken into the body and stored, temporarily or permanently, in its tissues.

Differences between individual and genetic effects

249. Different considerations enter into the assessment of the significance of any particular level of exposure, according to whether we are concerned with effects on the individual or with genetic effects. In respect of the effects on the individual, we are concerned with doses received throughout the whole of life. In respect of genetic effects, however, we are concerned only with doses to the gonads received up to and during the reproductive period of life. In this country the average age of mothers conceiving children is about 28 years; of fathers, two to four years older. We have therefore taken the period of 30 years to represent the average length of time during which the germ cell lineage of the human population is effectively exposed to radiation. From a genetic point of view, any exposure to radiation after reproduction has ceased is irrelevant, since any mutations produced will not be passed on to future generations.

250. The second important difference relates to the accumulation of the effects of exposure on separate occasions. The genetic effects of radiation are cumulative; a mutation persists once it has been produced in a germ cell lineage, and to this are added any further mutations that are induced in the same reproductive cells. Since dose and effect are proportional, we are concerned, from the genetic point of view, with the total dose of radiation which has been accumulated up to any particular time in the reproductive period of life.

251. The position with regard to the effects of successive doses on the individual is incompletely known. It is certain that a dose of radiation which would produce acute effects if given as one single exposure may produce no similar effects if spread over a longer period. Moreover, if a sufficient period has elapsed after recovery from the acute effects, the individual may again recover from a further exposure which produces acute symptoms. Uncertainty

arises in relation to some important delayed effects of radiation where, in view of the results on the increased incidence of leukaemia in repeatedly irradiated cases of ankylosing spondylitis, we must now entertain the possibility that repeated exposures to radiation may combine to produce certain irreversible changes in the tissues exposed.

Dosage and Effects on the Individual

Acute effects

252. The effects which appear within the first 48 hours are seen only as a result of accidents and in those within a short distance from an atomic bomb explosion, or occasionally after exposure to the heavy doses of radiation which may be necessary in the treatment of serious illnesses by radiotherapy. They are, therefore, not important in ordinary civilian circumstances.

253. The same considerations apply to the acute effects on the blood occurring up to two months after exposure to a single dose or to a few heavy doses of radiation. Similar effects occurring later can, however, be produced by doses of radiation, given continuously or repeated at short intervals, which would not be sufficient to produce any symptoms in the first few hours. Formerly, it was thought that continuous exposure to 1·0 r per week would produce no effects on the blood but, after some individuals were found to be affected by this dose, the figure recommended by national and international bodies as the maximum permissible level was reduced to 0·3 r per week.

Delayed effects

254. Of the delayed effects, the one about which we have most information is leukaemia. It also appears to be the most easily induced and seems, at present, to be the most important as far as radiation of the whole body is concerned. We have therefore taken the incidence of leukaemia as a measure of the doses of radiation that are capable of producing delayed effects. The statistical evidence indicates that an increased incidence of leukaemia can be demonstrated after exposure to doses of radiation which might, in exceptional circumstances, be met with in civil life. For example, after either a single exposure of 200 r, or a few exposures which in total amount to 200 r, there is a noteworthy increase in the small chance of developing this disease. What we do not know for certain is whether there would be an increase if a total dose of 200 r were spread over many years. Be this as it may, however, any risk that there may be from such a dose appears to be within the range of risks of other kinds commonly incurred in industrial and professional life.

255. We consider, therefore, that an individual could, without feeling undue concern about developing any of the delayed effects, accept a total dose of 200 r in his life-time, in addition to radiation from the natural background, provided that this dose is distributed over tens of years and that the maximum weekly exposure, averaged over any period of 13 consecutive weeks, does not exceed 0·3 r. We recommend, however, that the aim should always be to keep the level of exposure as low as possible.

Internal radiation

256. The problem of irradiation by radioactive materials taken into the body is in some ways a more complex and difficult one. The material is often concentrated in a particular tissue, such as bone, where it may give rise to malignant change. Owing to the short distance that the rays from a particular concentration of radioactive material can penetrate the tissues, the dose of radiation is extremely variable from place to place in the body.

Nevertheless, on the basis of the known levels of external radiation that can be tolerated, and of experience gained from the accidental ingestion of substances such as radium, 'permissible' levels of exposure for a large number of radioactive isotopes have been agreed. No single level that comprehends them all can be given, since each isotope emits its own characteristic amount and type of radiation, dies away at its own rate, and is absorbed and excreted at a rate dependent on its chemical form and its method of entry into the body. However, it has now been possible to estimate for many different isotopes the concentrations that it is safe to accumulate.*

Dosage and Genetic Effects

257. Our conclusion in the chapter on genetics was that 'For levels of radiation up to the doubling dose, and even some way beyond, the genetic effects of radiation are only appreciable when reckoned over the population as a whole and need cause no alarm to the individual on his own account'. In other words they are essentially problems for society as a whole, to be assessed in terms of the load of medical care that may, in different circumstances, be imposed on the population.

258. In ordinary circumstances only a small fraction, perhaps one or two per cent, of the hereditary abnormalities which appear in a generation can be attributed to fresh gene mutations. For the offspring of any given parents the risk from increasing the mutation rate is very slight. Nevertheless, if the whole of a large population is exposed to enough radiation appreciably to affect mutation rates, an increase even in this small fraction may add up to a large number of new cases. However, it is only if members of an irradiated group or their descendants intermarry over several generations, and do not mate with the unirradiated population, that there is likely to be a disproportionately greater manifestation of hereditary defects among the descendants. Such an extreme degree of inbreeding is unlikely to occur. A fraction of the community can, therefore, without significant genetic risk to their progeny or harm to the population as a whole, receive doses of radiation which would be likely to have serious effects if applied to the whole population (see paragraphs 185–187).

259. From the genetic point of view, we have therefore to consider radiation dosage under three headings:

(i) The dose which the individual can accept without undue concern about its possible effects on his own progeny.

(ii) The dose which can be accepted by a fraction of the population whose occupation exposes them to more than the dose of radiation received by the ordinary member of the community.

(iii) The dose which can be accepted by the whole population.

Dose to the individual in relation to genetic effects

260. We have concluded that doses up to, and somewhat beyond, the 'doubling dose' need cause no undue concern to the individual as regards his own offspring. Further, we gave reasons for believing that the values for the doubling dose of radiation for human genes may be, in general, in the range of 30 r to 80 r. We consider, therefore, that an individual could

* Precise data for a large number of isotopes are given, for example, in the Recommendations of the International Commission on Radiological Protection (1955) (*Brit. J. Radiol.*, *N.S.*, Suppl. No. 6).

reasonably accept a total dose to the gonads of not more than 50 r from conception to the age of 30 years, in addition to that received from the natural background. There will be no undue risk to the offspring of parents over this age provided the rate of exposure laid down in paragraph 255 is not exceeded.

Dose to occupationally exposed groups

261. Similar considerations apply to groups of the community whose occupation exposes them to more than the usual dose of radiation. Provided that such groups do not in aggregate total more than one-fiftieth of our population, we consider that all their members could each safely receive a total gonad dose of up to 50 r from conception to the age of 30 years, in addition to that received from the natural background.

Dose to the whole population

262. In the chapter on genetics we tried to give an indication of the load on the community that might be imposed by a doubling of mutation rates. It will be generally agreed that such a load should not voluntarily be accepted. In relation to genetic changes in the whole population, the significant figure is the total gonad dose of radiation which is received by all those capable of reproduction. A relatively high dose to a fraction of the population can only be offset by a correspondingly low dose to the remainder. If, therefore, we are to contemplate the possibility—and the necessity to develop the beneficent uses of atomic energy and ionizing radiations forces us to do so—that a significant fraction of our population may in future be allowed to receive doses of radiation of a similar order to the doubling dose, then it becomes additionally important to ensure that the dose of radiation to the rest of the community shall be held at the lowest possible level.

263. In view of the inadequacy of present knowledge, however, we do not feel justified in naming any specific figure as a limit for the average exposure of the whole population. It is nonetheless highly desirable that such a figure should be named as soon as possible and we understand that the International Commission on Radiological Protection has this matter under consideration. In the meantime, we feel bound to state our opinion that it is unlikely that any authoritative recommendation will name a figure for a permissible radiation dose to the whole population, additional to that received from natural sources, which is more than twice that of the general value for natural background radiation. The recommended figure may indeed be appreciably lower than this; and we consider that those on whom rests the responsibility for authorising the development and use of sources of ionizing radiation would be well advised to keep this possibility in mind.

The Hazards from Radiation

264. In the light of these general criteria, we may now proceed to examine the various hazards, actual and potential, to which the population in a country like our own may be exposed in consequence of the increasing use of ionizing radiation. In doing so, it will be necessary to distinguish between the hazards of peacetime experience and those which may be encountered in war.

PEACETIME HAZARDS

265. The development of modern civilisation has led to the increasing use of processes which produce radiations. Its future progress will come to depend, to an ever increasing extent, upon their further exploitation in power production, in industry, in medicine and in agriculture, as well as in basic

scientific research. It would be impossible to abolish their use without denying ourselves services upon which we have already come to rely; and, in the future, nuclear energy will constitute a major, if not the most important, physical factor upon which our civilisation will depend. In assessing the hazards consequent upon its use, it is therefore necessary to maintain a sense of perspective and to weigh, as society has done in the case of steam-power, electricity and the internal combustion engine, the risks entailed against the advantages to be gained from the employment of this newer source of power. The risks are twofold: those to the individual and those to the population as a whole. In regard to the former, we may indicate our own assessment by saying that, in view of the small numbers likely to be employed in atomic power production and of experience already gained in the effectiveness of protective methods, it seems probable that a given amount of power might be made available to the community at a smaller cost in accidents, illness and disability than that involved in present methods of mining and power production. The novel aspect of the situation lies in the possible genetic risk to the community as a whole. The considerations which we have put forward suggest that, although this is at present small and seems unlikely ever to be large, it is potentially important. We propose now to indicate the relative hazards of particular uses and to point out where and how these may be expected to increase or should be curtailed.

266. In the chapter on exposure levels, the contributions of the various man-made sources of ionizing radiation to the total exposure were expressed as percentages of the radiation received from natural sources; and it was shown that today the population of this country receives, from man-made sources, a dose of radiation equivalent to at least one-quarter of that from the natural background. In itself this figure, which amounts to less than 1 r over a period of 30 years, can give rise to no immediate apprehensions; but its significance should not be disregarded. From the point of view of population genetics, all extra radiation is undesirable, and it is at least a portent that, in the half century following the discovery of ionizing radiation, man has increased his exposure levels by about 25 per cent. It does not therefore seem to be too early to suggest where the use of ionizing radiation might be restricted.

Medical diagnostic radiology

267. The greatest contribution in this country to the increased exposure to radiation comes from medical diagnostic radiology, the application of which has been steadily increasing in amount and scope in recent years. A large proportion of the genetically significant dose derived from diagnostic radiology is contributed by relatively few types of examinations, of which fluoroscopic and radiological examination of the female pelvis, and examinations of the hip joint and lumbar spine in males, are important examples. Clearly, the small genetic risk to the community and to individuals must be weighed against the possible great advantage and even necessity of the radiological examination to the particular patient. The final decision must be made on medical grounds. There can, however, be no doubt that the risk could in many instances be reduced, not only by a reduction of the actual number of examinations carried out on young people, but also by the use of modern methods of X-ray examination and by strict limitation of the X-ray beam to those parts of the body which have to be exposed. From the point of view of the dose distributed over the population as a whole, and from the point of view of the special risks to individuals associated with examinations involving heavy exposure, we are of the opinion that the time has arrived for a review of present practice in diagnostic radiology.

Radiotherapy

268. The dangers associated with the heavy exposures employed in radiotherapy are well recognised, and the illnesses for which it is used are usually so serious that the risks can be fully justified. In addition, many of the patients treated for cancer are of an age when the genetic risk will be small. Nevertheless, treatment by radiotherapy of certain non-malignant conditions, particularly in children, could with advantage be reconsidered from the point of view of reassessing the risks entailed against the benefits conferred.

269. The increasing use of therapeutic amounts of radioactive isotopes must also be carefully assessed and controlled, although at the present time the total used is probably of little practical significance from a genetical point of view. Should, however, their use become widespread in relation to common diseases of early or middle life, they might make a significant contribution. The risks from the use of 'tracer' amounts of radioactive substances in diagnosis and investigation are at present very small.

Occupational exposure: Atomic Energy Authority employees

270. For a number of years the main group of persons exposed to radiations were workers in the X-ray and radium departments of hospitals. Later, this experience was supplemented by careful observations of those employed in atomic energy projects and the various atomic industries. We have already seen that, as a result of all this experience, the maximum permissible level of radiation for workers was fixed at 0·3 r per week, and this figure is still regarded as a satisfactory working level provided that it is not maintained for years on end. There is, however, no large factor of safety in this figure and, as we have seen above, other limitations are needed for those exposed over very long periods of time. The excellent practice of the Atomic Energy Authority, now the main industrial employer of persons occupationally exposed to radiation, is shown by their record that the average exposure of their employees is less than half a roentgen yearly, and that their contribution to the population gonad dose is only about 0·1 per cent of that contributed by the natural background. These, however, are average doses; but, even if one takes the maximum exposure of those employees engaged in special tasks, this has only occasionally and for short periods approached the maximum permissible dose of 0·3 r per week. In view of the expected development of atomic energy as a source of power, this evidence of the awareness of the risk, and of the care with which it is being met, gives justifiable grounds for reassurance.

Radioactive effluents

271. Concern has been expressed about the possible emission from some nuclear reactors of radioactive particulate matter, which on inhalation might lodge in the lungs and give rise ultimately to cancer. This possible risk is well recognised and has been effectively dealt with.

272. Similar concern has been felt about the disposal of radioactive wastes. It is true that the amounts of radioactive material involved are formidable, but by treatment, by long-term storage to allow of decay, by concentration to relatively small bulk and subsequent burial in sealed containers or disposal in the depths of the ocean, there seems no reason to doubt that this problem can continue to be solved. In this country strict control of the hazard is exercised by legislation.

Occupational exposure: hospital employees

273. The handling of large quantities of radioactive isotopes in the treatment and investigation of patients in hospitals raises many difficult problems

but, with adequate care and training of the workers concerned, experience shows that the doses received can be kept down to one-tenth or less of the maximum permissible levels.

274. In this country a code of practice, formulated by the Ministry of Health on the advice of the Medical Research Council, has been drawn up for the National Health Service. This lays down the relevant permissible levels of exposure as well as giving instructions and suggestions for the implementation of these in practice.

Other industrial employees

275. The control of radiological hazards in non-atomic industry will be more difficult to codify owing to the wide variety of circumstances in which ionizing radiations are used. Measurements of the doses received by many workers are made periodically, but it is difficult to avoid the impression that industrial personnel are, in general, less aware of the hazards of radiation than those engaged in the fields of medicine and atomic energy. The record of the Atomic Energy Authority shows the standard that is attainable and the practicability of being satisfied with nothing less.

Definition and review of safety standards

276. All these problems are under close and continuous scrutiny. Expert national committees, notably those of the Medical Research Council and Ministry of Health, work in close conjunction with international bodies such as the International Commission on Radiological Protection (itself associated with the World Health Organization), so that a wealth of experience is brought to bear on all these problems, and the standards of safety are under constant review.

Miscellaneous sources of radiation

277. There are several other sources of radiation which at present contribute only very small amounts but which cannot be disregarded. The practice of routinely examining the feet by X-rays when fitting shoes is of dubious value and, in view of the possibilities of multiple exposures to children, may even be dangerous. On the basis of avoiding any unnecessary exposure, it is in our view hardly justifiable. We hope that the procedure will be abandoned, except when prescribed for orthopaedic reasons.

278. The contribution of radiation from watches and clocks with luminous dials is also small but real. The main hazard is to workers in the luminising industry but the risk from the widespread use of such instruments is not entirely negligible. We recognise that there are circumstances which require the use of instruments with self-luminous dials. For the majority, however, there is no such necessity and their wider use constitutes an avoidable, if small, risk which could be minimised for all concerned if the amount of radioactive material in these instruments was reduced to the lowest possible level.

279. It has been recognised that television sets give rise to very small amounts of X-rays, but at the present time radiation from this source does not constitute either a personal or a significant genetic hazard. This applies to domestic sets working at normal operational voltages but, near special types of high voltage projection equipment used commercially, the radiation may reach significant levels, although it is improbable that in practice any operator would be appreciably exposed. Nevertheless, the possibility of television equipment giving rise to radiations should be borne in mind when

considering the design and operation of such instruments. So far as sets used by the general public are concerned, most of the radiation is normally absorbed in the apparatus itself and is insignificant at the usual viewing distances.

Test explosions of nuclear weapons

280. It is impossible to explode a nuclear weapon without liberating radioactive matter into the atmosphere. As described in the chapter on exposure levels, the radioactive material diffuses all over the world and in the course of time is gradually deposited on the surface of the earth and comes into contact with human beings. Continuation for an indefinite time of testing at the same rate as over the last few years would gradually increase the contamination of the atmosphere, until in about 100 years time the average individual in this country would receive a dose of external radiation to the gonads of 0·026 r in 30 years of his life, an amount which represents only one per cent of that received from the natural background. The individual and genetic effects of such a dose of external radiation would be insignificant.

281. Account must be taken, however, of the particular hazard from radioactive strontium in the fall-out. The maximum permissible level of strontium 90 in the human skeleton, accepted by the International Commission on Radiological Protection, corresponds to 1000 micro-microcuries per gramme of calcium. But this is the maximum permissible level for adults in special occupations and is not suitable for application to the population as a whole or to children with their greater sensitivity to radiations and greater expectation of life. It is known that radiostrontium is more heavily concentrated in the bones of young than of adult animals, and the few measurements on human bones indicate that at the present time those of children contain about ten times the concentration found in those of adults. We consider, therefore, that the maximum allowable concentration of radiostrontium in the bones of the general population, with its proportion of young children, should not be greater than 100 micro-microcuries of strontium 90 per gramme of calcium.

282. In this country, measurements on human bones of the radiostrontium content, derived from the nuclear explosions that have already occurred, show that the irradiation from this source is now reaching about one-thousandth of the maximum permissible occupational level; and calculation of the fall-out likely to come, if the present rate of firing continues, suggests that this level may be increased ten-fold in the course of several decades. The present level would produce no detectable increase in the incidence of ill-effects. It is evident, however, that we are now accumulating radiostrontium at an appreciable rate and that a close watch will need to be kept on this increase.

283. In the light of knowledge at present available, we should feel that immediate consideration were required if the concentration in human bones showed signs of rising greatly beyond one-hundredth of that corresponding to the maximum permissible occupational level.

284. We are well aware of the inadequacy of our knowledge of the biological effects of radioactive strontium and of the urgent necessity to obtain further information. Nevertheless, recognising all the inadequacy of our present knowledge, we cannot ignore the possibility that, if the rate of firing increases and particularly if greater numbers of thermonuclear weapons are used, we could, within the life-time of some now living, be approaching levels at which ill-effects might be produced in a small number of the population.

285. The general conclusion to be drawn from a consideration of the hazards inseparable from the application of ionizing radiations in peacetime is that at present there is no cause for alarm ; but that, as all such radiations are potentially dangerous, their use should be the subject of constant and close scrutiny, and that adequate justification should be required for their employment on however small a scale. There is a limit to the amount of radiation which any population or any individual can accept and we cannot afford to expend, without careful forethought, the margin which is now available to us.

WARTIME HAZARDS

286. We have given a brief résumé of the effects of nuclear warfare. From this it will be seen that there are three broad categories of effect: those within the range of the actual explosion, those within the contiguous area in which radioactive fission products settle, and world-wide effects due to the contamination of the atmosphere.

287. Within close range of the explosion, nuclear radiations are but one element in the destructive effect. Blast and heat would be of major and probably of more immediate importance in producing casualties but survivors, unless heavily sheltered, would have been exposed to such an intensity of radiation that they would be at risk of developing each and all of the effects that we have described.

288. Explosions of atomic weapons always give rise to radioactive fission products, the heavier particles of which settle in the vicinity. With a ground burst of a thermonuclear weapon, the area of intense fall-out may cover hundreds of square miles. Within this area, those who were not in shelter, and did not remain under cover until the radioactivity of the fall-out had decayed substantially, would be exposed to intensities of radiation sufficient to produce the effects described in all grades of severity. Outside this area, there would be another zone, measured in thousands of square miles, where significant intensities of radiation would occur and where a proportion of those exposed would be at risk of serious consequences.

289. It must be emphasised that these doses, from the point of view both of the individual and of the general population, are several thousand times greater than those we have considered as possible peacetime hazards.

290. The importance of the effects of atomic warfare which would be relayed through contamination of the atmosphere to parts of the world remote from the actual conflict, would depend upon the number and type of bombs exploded. Given a sufficient number of bombs, no part of the world would escape exposure to biologically significant levels of radiation. To a greater or less degree, a legacy of genetic damage would be incurred, and an increased incidence of delayed effects on the individual would probably be induced. Although it is difficult to imagine the general occurrence of radiation intensities which would eliminate the entire human race, atomic warfare on a large scale could not fail to increase for many generations the load of distress and suffering that individuals and all human societies would be called upon to support.

CHAPTER VII

SUMMARY

291. The future development of civilisation is bound up with the exploitation of nuclear energy. Its use, like that of other sources of energy, entails risk, but the risk is controllable and, within limits, can be accepted. It is the scale and not the nature of the hazard that is new, for human populations have always been exposed to natural radiation of low intensity. (Paragraphs 1–14.)

THE NATURE OF RADIATION AND ITS ACTION ON LIVING CELLS

292. Ionizing radiations are so described because they cause the formation of electrically charged particles, ions, in the matter through which they pass. The common types of penetrating radiation are X-rays, gamma rays, alpha and beta particles, and neutrons. Alpha particles cannot penetrate tissue beyond a fraction of a millimetre but gamma rays, and X-rays produced by extremely high voltages, can traverse the whole body. (Paragraphs 15–20.)

293. The biological effects of radiation are related to the intensity of radiation and to the period of exposure. The basic unit of radiation dosage which has been generally used is the roentgen (r). All living tissue can be killed if exposed to sufficiently high doses of radiation. The effects of dosages below those which damage tissues irretrievably may be modified by processes of healing, so that the response to a dose of radiation which is spread over a long time may be much smaller than, or quite different from, the response which would occur if the same dose were given in a very short time. This does not apply to the important type of genetic effect, called gene mutation, produced by the irradiation of reproductive cells, the consequences of which are cumulative and irreversible. (Paragraphs 21–27.)

THE EFFECTS OF RADIATION ON THE HEALTH OF THE INDIVIDUAL

Sources of information

294. Our knowledge of the effects of ionizing radiations on human beings comes from four main sources: from the uses of X-rays and radium in the treatment of disease, mainly of cancer; from a study of the occupational hazards of medical radiologists, workers in the luminising industry, and miners of radioactive ores; from a study of the victims of atom bomb explosions; and from experiments on animals. (Paragraphs 28–34.)

The harmful effects of radiation on man

295. Almost all the effects of ionizing radiation on tissues are essentially deleterious. The benefits to the individual patient of the eradication of a malignant tumour by radiotherapy result from selective damage to the tumour cells. The nature and severity of radiation injury is determined by the type and dosage of radiation received, the part and extent of the body irradiated, the length of the period of exposure, and the age of the persons exposed. The harmful effects may be classified into those which develop within a few weeks of exposure, and delayed effects which may not make their appearance until many years after exposure. (Paragraphs 35–42.)

Effects occurring within a few weeks of exposure

296. The effect of exposing the whole body to a single dose of gamma radiation of the order of 500 r is such that all the persons so exposed would develop acute illness and at least half would die. In civil life, exposure to such a dosage could occur only under the most exceptional circumstances. With smaller single doses, for example of 100 r, not more than 15 per cent of an exposed population would suffer acute illness and very few, if any, of those affected would die. After a single dose of 50 r, acute illness would be very rare. The relationship between the dose of radiation received and the effects that may be produced within a few weeks of exposure is not one of strict proportionality; with each successive and equal increment of dosage the response increases by a progressively greater amount, at least until very large changes have been produced. (Paragraphs 43–50.)

The delayed effects of radiation

297. Delayed effects of exposure to radiation may occur at any time after the end of the second month. Disorders of the skin and underlying soft tissues and of bone may occur and there may be subsequent development of cancer. Cataracts, severe anaemias and leukaemia have been caused and there is evidence from animal experiments that exposure to radiation may cause death at a prematurely early age. (Paragraphs 51–52.)

Leukaemia

298. Leukaemia is a disease in which there is an uncontrolled over-production of white blood corpuscles. Experiments on animals have shown that the incidence of leukaemia is increased by irradiation. Clear evidence that the same is true of man comes from two main sources: a study by the Atomic Bomb Casualty Commission of the incidence of leukaemia in Hiroshima and Nagasaki, and a survey under our sponsorship of the incidence of leukaemia among patients treated by radiation for ankylosing spondylitis. (Paragraphs 53–55.)

299. Ninety-one proven and fourteen suspected cases of leukaemia have been recorded in Hiroshima and Nagasaki between 1947 and 1954 among those present at the time of the explosion and still resident in the cities; the expected incidence in an unexposed but otherwise comparable population is twenty-five. The difference is greater than would be attributed to chance. Moreover, there was a much higher frequency of occurrence among those who had developed early acute radiation illness and among those who had been nearer to the centre of the explosion. The latent period, that is the average length of the period between the explosion and the first appearance of symptoms of leukaemia, was about six years. The evidence suggests that with this type of exposure to radiation the likelihood of developing leukaemia, after its initial rise, remains approximately constant up to at least the ninth year. (Paragraphs 56–61.)

300. Ankylosing spondylitis is a disease in which the joints, particularly those of the spine, progressively lose their freedom of movement. In the treatment of this condition very extensive areas of the body are exposed to irradiation. The records of between 13,000 and 14,000 patients, who had been treated with X-rays between 1933 and 1954, have been studied. Up to 1955, thirty-eight of these patients developed leukaemia, an incidence which, although only about one-third of one per cent, is about ten times greater than the normal expectation. No increased incidence of leukaemia was found among 400 patients who had not been treated by irradiation, but the number is too small to exclude completely the possibility that ankylosing spondylitis

may of itself predispose its sufferers to leukaemia; nor can the possibility be excluded that these patients are more liable than the average person to develop leukaemia after irradiation. Nevertheless, there is clear evidence of a correspondence between the dosage of radiation received and the incidence of leukaemia. The average length of the latent period between the first exposure to X-rays and the diagnosis of leukaemia was about six years. (Paragraphs 62–69.)

301. The conditions of exposure to radiation in Hiroshima and Nagasaki, and in the treatment of ankylosing spondylitis, are not comparable with the irradiation in small doses over long periods which might be received by persons engaged in work with a possible radiation hazard. Some evidence has been presented suggesting an increased death rate due to leukaemia among radiologists but our knowledge of the occurrence of leukaemia under conditions of chronic exposure is too scanty to allow any reliable conclusions to be drawn. (Paragraphs 70–71.)

Cancers

302. Two characteristics of cancers induced by radiation are noteworthy: the tendency of tumours to arise in tissues already severely damaged by radiation, and the long latent period, twenty years or more, before they appear. (Paragraph 72.)

303. A study of the pitchblende miners of Schneeberg and Joachimsthal suggests strongly that inhalation of the radioactive gas radon may lead to cancer of the lung. The latent period has been put at seventeen years and the dosage to the lungs over that period at about 1000 r and in some parts of the lung much higher. In theory, the inhalation of radioactive particles in the fall-out from atomic explosions or in the vicinity of nuclear reactors could also lead to cancer of the lung, but the former hazard is extremely unlikely in peacetime, and steps are always taken to ensure that the latter does not occur. (Paragraphs 73–76.)

304. Radium, mesothorium, plutonium and radioactive forms of strontium are accumulated by and retained in bone. Until the enforcement of stringent controls, cancer of bone occurred among workers in the luminising industry as a result of swallowing radium-containing paint. The latent period was more than fifteen years. (Paragraphs 77–82.)

305. Cancer of the skin was the earliest form of radiation-induced tumour to be described in man. By 1911, before the adoption of modern safeguards, fifty-four cases had been described among the pioneers of radiology. The doses of radiation which have led to the formation of skin cancers must have been several thousand r. (Paragraphs 83–84.)

306. Cancer of the thyroid gland in children has been a sequel to irradiation of the neck for enlargement of the thymus gland. This form of cancer is distinguished by its short latent period (about 7 years) and the comparatively low dosage of radiation required to induce it. However, it is not unlikely that other factors are involved here in addition to the direct effect of irradiation. (Paragraphs 85–86.)

Other delayed effects

307. A fall in the number of red cells and white cells in the blood may follow exposure of the whole body to even moderate doses of gamma radiation. If not detected in time a condition known as aplastic anaemia may occur. (Paragraphs 87–88.)

308. Cataract formation is known to have been caused by neutron irradiation, but for all practical purposes the production of cataract by X-rays is not an occupational hazard. (Paragraphs 89-90.)

309. Delayed effects of radiation on the skin extend from a temporary loss of hair after local dosages of 300r–400r to severe and permanent damage after local exposure to single dosages of 1500r or more, or to repeated doses totalling 4000r or more in a number of weeks. It is in the skin damaged by these higher doses of radiation that tumours, when they occur, are most likely to develop. (Paragraphs 91–92.)

310. Miscarriage and stillbirth may be a consequence of irradiation during pregnancy, but they do not constitute a problem unless the dose of radiation is large. A number of different developmental abnormalities have been described in the children of women treated by irradiation during pregnancy, the most conspicuous defect being microcephaly, a partial failure of the development of the brain. Eleven cases so classified are recorded in children irradiated before birth in Hiroshima and Nagasaki.
(Paragraphs 95–97.)

THE GENETIC EFFECTS OF RADIATION

311. The assessment of the genetic effects of ionizing radiations is subject to special difficulties. We believe that we have formed as fair an assessment as is possible in the light of present knowledge, but our conclusions must be regarded as provisional. (Paragraph 100.)

The material basis of heredity

312. The physical determinants of heredity are genes, carried on chromosomes in the nuclei of cells. Chromosomes are present in pairs; one member of the pair is of maternal origin, the other of paternal origin. There are twenty-four pairs of chromosomes in human beings; the number of genes is not known, but may well be many thousands. (Paragraphs 101–103.)

313. The two genes which occupy corresponding positions on the two chromosomes of a pair are spoken of as alleles of each other. Alleles of different kinds arise by the process of mutation and are thereafter reproduced faithfully in their altered form. (Paragraph 104.)

314. Some genes produce the same effect whether they are paired with like or with unlike alleles. Such genes, and the characters they determine, are described as dominant. Other genes produce a noticeable effect only when paired with similar alleles; these, and the characters they determine, are described as recessive. There is every gradation between these two extremes. A recessive gene can be transmitted in a family by an individual who gives no signs of carrying it. (Paragraphs 105–108.)

315. Sex difference is determined by a special pair of chromosomes, and the genes carried on these chromosomes are said to be sex-linked.
(Paragraph 109.)

316. So far as is known, all genes are subject to mutation, and mutation occurs spontaneously all the time at a very low rate. Factors influencing mutation appear to affect only the frequency with which it happens. New alleles of harmful effect are eliminated by natural selection until equilibrium is reached with the rate at which they are introduced by fresh mutation. Recessive alleles are eliminated much more slowly than dominant alleles.
(Paragraphs 111–118.)

Basic principles of the genetic effects of radiation

317. There is little direct knowledge of the genetic effects of ionizing radiations on man, but with certain reservations it is justifiable to draw upon our knowledge of the effects of radiation on other organisms.
(Paragraph 119.)

318. Ionizing radiations have genetic consequences only in so far as they affect the reproductive cells or the cells ancestral to them in the reproductive organs (gonads). Two kinds of effect may have genetic consequences: the chromosomes may be damaged or the genes may be caused to mutate more frequently. Chromosome changes of the kind that can persist are only rarely produced by long continued exposure to X-rays or gamma rays of low intensity. They are likely to be a comparatively unimportant radiation hazard. (Paragraphs 120–125.)

319. It is the frequency of gene mutation that is increased by radiation; there is no evidence and little likelihood that radiation produces entirely new kinds of genes. The rise in mutation rate is probably directly proportional to the amount of additional exposure to radiation, and any additional exposure, however small, must be expected to raise the mutation rate, if only by a minute amount. (Paragraphs 126–127.)

320. Damage to genetic material is cumulative and irreparable. Long continued exposure to radiation of low intensity induces as much gene mutation as a single exposure to an equal dosage of radiation of higher intensity.
(Paragraph 128.)

321. The age-distribution of those exposed to radiation has an important bearing on the future consequences of its effects. The genetic consequences of the irradiation of individuals beyond the age of reproduction are of course nil. (Paragraph 129.)

Effects of increased mutation on the incidence of disease in human populations

322. The role of heredity in the production of disease ranges from that of a predisposing to that of a preponderating cause. The effects which might be expected to result from an increase in mutation rates can most easily be calculated for diseases known to be caused by single genes, but for relatively few such diseases have we sufficient evidence of the kind upon which such a calculation must be based. (Paragraphs 130–135.)

323. Achondroplasia, haemophilia, and phenylketonuria have been taken as examples of diseases believed to be caused by single genes. If the mutation rates of these genes were to rise to, and remain at, twice their present values, the incidence of the diseases for which they are responsible would ultimately, though at very different rates, rise to nearly twice their present frequencies. Calculations suggest that the incidence of achondroplasia, a dominant form of dwarfism, would rise 80 per cent above its present value in a single generation; haemophilia, a sex-linked disease, would take about six generations to rise by 90 per cent in frequency; and phenylketonuria, a recessive disease associated with severe mental deficiency, would take more than fifty generations to increase its frequency by one half. (Paragraphs 136–139.)

324. Mental diseases, the most important single category in which hereditary causes are known to be important, account in all for nearly half the hospital beds provided in this country. There are grounds for believing that a doubling of the mutation rates of the genes concerned with their causation would, in one generation, increase the frequency of low-grade mental deficiency by three per cent, and of the two principal types of mental illness,

schizophrenia and manic depressive reaction, by about one per cent. If the mutation rates were to remain at twice their present values, the incidence of mental diseases might on the most pessimistic assumptions double also, but would only attain this value after very many generations.

(Paragraphs 140-150.)

325. When all serious illnesses with a hereditary element in their causation are taken into account, it is unlikely that the burden put upon society by a doubling of mutation rates would exceed by more than a few times the contribution made by the increase of mental disease.

(Paragraphs 151–154.)

326. It must be remembered that a harmful recessive gene gives no outward evidence of its presence until chance brings it together with another of its kind. The crop of newly mutated recessive genes caused by an increase of mutation rates could cause suffering over many generations.

(Paragraphs 155–156.)

Hereditary traits showing continuous variation about the normal

327. Most of the variation between human beings is not of the sharp kind that can be traced to the action of single genes. Characters such as physique, intelligence and length of life vary over a wide range by imperceptible gradations, and the hereditary portion of this variation is believed to be due to the combined action of many genes. (Paragraphs 157–159.)

328. The basic effect of an increase in mutation rates upon such characters, here exemplified by scores in intelligence tests, will be to increase the numbers of the more extreme types at the expense of the more average individuals. A doubling of the mutation rates for a few generations would be expected to have only the most trivial effect upon their variation. The effect of a permanent doubling of the mutation rate would be, at most, to double the variation, and this would take hundreds of generations to achieve.

(Paragraphs 160–161.)

Observations on populations exposed to radiation

329. Three direct studies have been made on the children of human beings who have been exposed to ionizing radiations. Two, on the children of American radiologists, were for a variety of reasons inconclusive; the third is the extensive study made by the Atomic Bomb Casualty Commission on the children of those who were in Hiroshima and Nagasaki when the atomic bombs exploded. All three studies are limited to observations on the first generation, so that little genetic effect would yet have become manifest even if the mutation rate had increased. (Paragraphs 162–166)

330. The evidence assembled in the report of the Atomic Bomb Casualty Commission is beset by many difficulties of interpretation, but we believe that it reveals, in the children of those who were the more heavily exposed, a slight but significant change in the sex ratio at birth which might be due to genetic damage. From the nature of the evidence a doubling of the rate of incidence of congenital malformations, or a 50 per cent rise in the stillbirth rates, might have escaped detection if either had occurred. The evidence does not allow us to make any useful estimate of the radiation dose which doubles the mutation rate in man. (Paragraphs 167–170.)

The 'doubling dose' in man

331. An assessment of the sensitivity of human genes to radiation is particularly difficult. Any such estimate should be based upon a sample of genes large enough to be representative of all the effects they exercise, for it cannot be assumed that all genes are equally radiosensitive, nor that the

proportion of the spontaneous mutation rate which can be attributed to natural radiations is the same for different genes. (Paragraphs 171–175.)

332. If all mutations were indeed due to radiation, then the dosage which doubled their frequency would be expected to be equal to that received from natural sources, namely, a dosage to the gonads of about 3 r in thirty years. The available evidence suggests, however, that the percentage of human mutations that are caused by natural radiation might lie between 2 per cent and 20 per cent, and if this is so the doubling dose will lie between 15 r and 150 r. (Paragraphs 176–181.)

333. The direct estimates which have been made of the doubling doses for a variety of plants and animals mostly run from 25 r upwards. It is true that none of the more fully investigated organisms has a lifetime comparable with man's, but there are theoretical grounds for believing that the organisms with the longer pre-reproductive periods might be expected to have the less radiosensitive genes. (Paragraph 182.)

334. The evidence at our disposal, though far from adequate, leads us to conclude that there is rather little likelihood that the real value for the doubling dose for human genes lies between 3 r and 15 r; and that, although we cannot exclude the possibility that for some human genes the doubling dose may be less than 30 r and for others more than 80 r, the best estimate that we can make in the light of present knowledge, is that the value in general lies somewhere between 30 r and 80 r. (Paragraph 183.)

335. Even if the doubling dose were as low as the minimum we can reasonably entertain, namely 15 r, it is extremely improbable that in times of peace more than a small fraction of the population could receive an extra dose of this size. The prevalence of naturally-occurring hereditary abnormalities is such that, if comparatively few individuals received such a dose, there would be no noticeable effect on their immediate offspring or on their descendants even over several centuries. For levels of radiation up to the doubling dose, and even some way beyond, the genetic effects of radiation are only appreciable when reckoned over the population as a whole, and need not cause alarm to the individual on his own account. (Paragraphs 184–188.)

EXISTING AND FORESEEABLE LEVELS OF EXPOSURE TO RADIATION

336. Doses of radiation which are of no known significance to the individual may have genetic consequences. Exposure levels must therefore be expressed in terms of the total dosage to the gonads received by the population as a whole during the period of reproductive life. (Paragraphs 189–191.)

Radiation from natural sources

337. The natural sources of radiation are cosmic rays and the naturally-occurring radioactive elements. From all such sources an individual in this country receives, on the average, a total gonad dose of about 3 r over a period of thirty years. (Paragraphs 192–201.)

Radiation from the appurtenances of civilisation

338. Over the past sixty years man has made increasing use of X-rays and radioactive materials in medicine, industry, and ordinary civil life. The additional gonad doses received from these sources by people of this country are expressed as percentages of the gonad dose which they already receive from natural sources. (Paragraph 202.)

339. We have conducted a limited survey which suggests that the additional dose received from the various forms of diagnostic radiology may well

be higher than 22 per cent, the major amount of which is accounted for by examination of a relatively few sites of the body. The contribution made by the use of radiation in medical treatment cannot be accurately estimated; it is probably much less than that made by diagnostic radiology but greater than that received from any other artificial source.

(Paragraphs 203–206.)

340. Watches and clocks with radioactively luminous dials contribute about one per cent of additional radiation. X-rays from television sets account for much less than one per cent. The contribution from X-ray apparatus used in shoe-fitting is not likely to exceed 0·1 per cent. (Paragraphs 207–210.)

341. The contribution arising from the work of the Atomic Energy Authority is the most accurately known, and is about 0·1 per cent. A study of the records of the National Radiological Protection Service has put the contribution from other occupational sources at about 1·6 per cent.

(Paragraphs 212–217.)

Contamination of the world by fall-out from the explosion of nuclear weapons

342. Continual watch is kept by the Atomic Energy Authority on the radioactive fall-out reaching this country from nuclear devices exploded in other parts of the world. From the bombs exploded up to the present time, the population of this country may expect to receive, over the next fifty years, additional radiation amounting to between 0·02 per cent and 0·04 per cent of the radiation which will be received over the same period from natural sources. (Paragraphs 218–229.)

343. If the firing of bombs were to continue indefinitely at the same rate as over the past few years, radioactivity would gradually accumulate to a level at which an inhabitant of this country would receive an average dose of 0·026 r over a period of thirty years, or about one per cent of that which he would receive in the same period from natural sources.

(Paragraph 230.)

344. The contribution to this figure from thermonuclear explosions, relative to their numbers, is very great. If the rate of firing of weapons of this type increases, exposure to radiation will be significantly raised. (Paragraph 231.)

Special hazards of radioactive fission products

345. It is unlikely that the inhalation of radioactive particles present in the air as a result of fall-out would constitute a problem in ordinary civil life. (Paragraphs 232.)

346. The deposition of radioactive strontium is probably a greater hazard, because it is soluble and, if ingested, is deposited and retained in bone. Measurements which have been made of radioactive strontium in bone show that the highest levels are at present about a thousand times less than is considered permissible for those who are occupationally exposed.

(Paragraphs 234–239.)

Atomic war

347. Atomic bombs were developed for their capacity to create blast, but for persons exposed in the open the heat flash is equally to be feared. The ionizing radiations produced immediately after explosions have a much greater penetrating power than the heat rays, but the range at which they cause death or immediate injury is somewhat less. The hazard from radiations is therefore only one of the immediate effects of atomic explosions. Their peculiar danger lies in their distant and delayed effects.

(Paragraphs 241–246.)

ASSESSMENT OF THE HAZARDS OF EXPOSURE TO RADIATION

348. An attempt is made to assess the medical and genetic consequences of exposure to radiation at the levels of dosage which occur now or which might conceivably come about. The naturally occurring level of radiation can be accepted as a standard of reference, because it is a level to which mankind has long been adjusted. (Paragraphs 247–248.)

349. In considering the genetic effects of radiation, we are concerned with the sum, over the whole population, of the total gonad dose received by its members from conception until the end of reproductive life. (Paragraphs 249–250.)

350. In considering the effects of radiation upon the individual, we are concerned with his whole span of life, and with the rate at which the radiation is received as well as with its total dosage; and we must have regard to the possibility that the severity of the effects produced by radiation may increase in more than equal proportion to the dosage that is received. (Paragraph 251.)

Dosage and effects on the individual

351. The acute effects of radiation which appear within two months of exposure to a single dose or a few heavy doses do not enter into ordinary civil calculations; nor is it feared that they may be produced by repeated exposures to doses that do not exceed 0·3 r per week. (Paragraphs 252–253.)

352. Of the delayed effects of irradiation of the whole body, leukaemia is probably the most easily induced. We consider that an individual could, without feeling undue concern about developing any of the delayed effects, accept a total dose of 200 r in his life-time, additional to that received from the natural background, provided that this dose is distributed over tens of years and that the maximum weekly exposure, averaged over any period of 13 consecutive weeks, does not exceed 0·3 r. We recommend, however, that the aim should always be to keep the level of exposure as low as possible. (Paragraphs 254–255.)

Dosage and genetic effects

353. The genetic effects of radiation are essentially problems concerning the future welfare of the population as a whole. (Paragraph 257.)

354. It follows from the nature of the genetic effects of radiation that a small fraction of a population can, without harm to its members, receive dosages of radiation which would be likely to have serious genetic effects if applied to the population as a whole. We feel that an individual, considered as such, can accept a total gonad dose of not more than 50 r, from conception until the age of thirty, additional to that received from the natural background, without undue concern for himself or his offspring, but that the number of such individuals should not exceed one-fiftieth of the population as a whole. (Paragraphs 258–262.)

355. Our present knowledge does not justify us in naming any specific figure as a limit for the average dose of radiation which might be received by the population as a whole. It is highly desirable that such a figure should be named as soon as possible; and we understand that the International Commission on Radiological Protection has this matter under consideration. In the meantime, we feel bound to state our opinion that it is unlikely that any authoritative recommendation will name a figure for permissible radiation dose to the whole population, additional to that received from the natural background, which is more than twice that of the general value for natural background radiation. The recommended value may, indeed, be appreciably lower than this. (Paragraph 263.)

The peacetime hazards from nuclear radiation

356. Nuclear energy may become the principal source of power. So far as its use affects the small numbers likely to be employed in its production, we believe that nuclear energy might make power available at a lower cost in accidents, illnesses and disability than that incurred in connexion with other sources of power. What is novel in the use of nuclear energy and the other, increasing, uses of processes producing radiations is the genetic risk to the community as a whole. The risk from civil usage is at present small, and seems unlikely ever to be large; but from the point of view of population genetics all possible extra radiation should be avoided, and it is not now too early to suggest where we might restrain its use. (Paragraphs 265–266.)

357. With regard to occupational exposure we consider that the record of the Atomic Energy Authority shows the standard that is attainable and the practicability of being satisfied with nothing less. (Paragraphs 270–276.)

358. We consider that the time has come for a review of present practice in diagnostic radiology, and of certain uses of radiation in the treatment of non-malignant conditions, particularly in children. Among the less important sources of radiation, we hope that the use of X-rays in shoe-fitting will be abandoned except when prescribed for orthopaedic reasons; that watches and clocks with radioactively luminous dials will be confined to necessary uses; and that the X-ray hazard from television tubes, at present negligible, will be borne in mind if special types of high voltage equipment come to be widely used. (Paragraph 267–269 and 277–279.)

Test explosions of nuclear weapons

359. The genetic effects to be expected from present or future radioactive fall-out from bombs fired at the present rate and in the present proportion of the different kinds are insignificant. They might not be so, if present rates of firing were increased and particularly if a greater number of thermonuclear weapons were tested. (Paragraph 280.)

360. So far as radioactive fall-out may affect the individual, we believe that immediate consideration would be required if the concentration of radioactive strontium in bone showed signs of rising greatly beyond that corresponding to one-hundredth of the maximum permissible occupational level. (Paragraphs 281–284.)

Wartime hazards

361. The area in which a greater or lesser proportion of those exposed would be at serious risk from the radioactivity released by the ground burst of a thermonuclear weapon is measured in thousands of square miles. If a sufficient number of nuclear weapons were exploded, no part of the world would escape biologically significant degrees of exposure or the load of distress and suffering to individuals and society which such exposure would entail. (Paragraphs 286–290.)

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CHAPTER VIII

CONCLUSIONS

362. On the basis of the considerations in this report we feel justified in drawing the following conclusions in relation to the use of ionizing radiations in peacetime :

1. *Limitation of the use of all sources of radiation*

Adequate justification should be required for the employment of any source of ionizing radiation on however small a scale.

2. *Dose levels to the individual*

(*a*) In conditions involving persistent exposure to ionizing radiations, the present standard, recommended by the International Commission on Radiological Protection, that the dose received shall not exceed 0·3 r weekly, averaged over any period of 13 consecutive weeks, should, for the present, continue to be accepted.

(*b*) During his whole lifetime, an individual should not be allowed to accumulate more than 200 r of "*whole-body*" radiation, in addition to that received from the natural background, and this allowance should be spread over tens of years; but every endeavour should be made to keep the level of exposure as low as possible.

(*c*) An individual should not be allowed to accumulate more than 50 r of radiation *to the gonads,* in addition to that received from the natural background, from conception to the age of 30 years; and this allowance should not apply to more than one-fiftieth of the total population of this country.

3. *Dose level to the population*

Those responsible for authorising the development and use of sources of ionizing radiation should be advised that the upper limit, which future knowledge may set to the total dose of extra radiation which may be received by the population as a whole, is not likely to be more than twice the dose which is already received from the natural background; the recommended figure may indeed be appreciably lower than this.

4. *Fall-out from test explosions of nuclear weapons*

(*a*) The present and foreseeable hazards from *external* radiation due to fall-out from the test explosions of nuclear weapons, fired at the present rate and in the present proportion of the different kinds, are negligible.

(*b*) Account must be taken, however, of the *internal* radiation from the radioactive strontium which is beginning to accumulate in bone. At its present level, no detectable increase in the incidence of ill-effects is to be expected. Nevertheless, recognising all the inadequacy of our present knowledge, we cannot ignore the possibility that, if the rate of firing increases and particularly if greater numbers of thermonuclear weapons are used, we could within the life-time of some now living, be approaching levels at which ill-effects might be produced in a small number of the population.

5. *Recommendations regarding specific uses of radiation*

(*a*) All sources of radiation, both medical and industrial, should be under close inspection, in order to ensure that the high standards of protection now

attainable against the absorption of ionizing radiations, and against radioactive materials, are generally observed. Those using radiations should be instructed in the precautions to be taken, and no unnecessary or unauthorised person should be allowed to engage in such occupations. A personal record, not only of doses of radiation received during occupation but also of exposures from all other sources, such as medical diagnostic radiology, should be kept for all persons whose occupation exposes them to additional sources of radiation.

(*b*) Present practice in medical diagnostic radiology should be reviewed, with the object of clarifying the indications for the different special types of examination now being carried out and defining more closely, both in relation to the patient and to the operators, the conditions which should be observed in their performance.

(*c*) The uses of radiotherapy in non-malignant conditions should be critically examined.

(*d*) The small amounts of irradiation from miscellaneous sources, such as X-ray machines used for shoe-fitting, luminous watches and clocks, and television apparatus, should be reduced as far as possible.

6. *Collection of vital statistics*

As an essential basis for future studies of the genetic effects of radiation, further data are required on the genetic structure of human populations; to this end, there is an urgent need for the collection of more detailed information, when births, marriages and deaths are registered.

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APPENDIX A

The Incidence of Leukaemia among the Survivors of the Atomic Bomb Explosions at Hiroshima and Nagasaki

Information concerning the cases of leukaemia which are known to have occurred among the survivors of the atomic bomb explosions at Hiroshima and Nagasaki has been supplied by the Atomic Bomb Casualty Commission of the National Research Council of the U.S.A. By the end of August 1955, the diagnosis of leukaemia had been confirmed in 125 patients; in all these cases blood smears and, when necessary, bone-marrow and autopsy material, had been examined by a member of the Commission's staff. In 18 other cases, the diagnosis was suspected but the evidence was less conclusive; and in a further 5 the diagnosis was still under review.

Sixty-one of the confirmed and four of the suspected cases occurred among persons resident in Hiroshima at the time the diagnosis was made, and it is possible to relate these cases to the numbers of persons who survived the explosion and who were recorded as residing in the city subsequently. The incidence of leukaemia among survivors at various distances from the hypocentre of the explosion is shown in Table 1A. The incidence was substantially higher among those who were close to the hypocentre than among those who were more distant from it—128·0 per 10,000 among those less than 1,000 metres away, against 1·6 per 10,000 among those more than 3,000 metres away. Separate incidence rates for persons at each distance who showed major radiation symptoms shortly after the explosion and for persons who did not show such symptoms have been published by Moloney and Kastenbaum (1955).

TABLE 1A

The incidence of leukaemia among survivors of the Hiroshima atomic bomb explosion exposed at various distances from the hypocentre; persons subsequently resident in Hiroshima City only

Distance from hypocentre at time of explosion (m.)	No. of survivors on 1.10.50*	No. of cases of leukaemia		Incidence per 10,000 persons (total cases)
		Confirmed	Suspected	
Less than 1,000 ...	1,250	16	0	128·0
1,000–1,499	10,350	28	1	28·0
1,500–1,999	18,450	6	1	3·8
2,000–2,999	30,350	7	0	2·3
3,000 or more	37,700	4	2	1·6
All distances	98,100	61	4	6·6

* The numbers of survivors have been rounded off to the nearest 50. The estimates differ slightly from those published by Moloney and Kastenbaum (1955) in accordance with data provided by the Atomic Bomb Casualty Commission.

Comparable figures are not available for the incidence of leukaemia in the unexposed population of the rest of Japan, but since leukaemia is invariably fatal it is reasonable to use mortality figures to provide an estimate of the incidence of the disease which might have been expected if no explosion had

taken place. National mortality figures are, however, based on the causes of death given on death certificates, and they are not necessarily suitable for comparison with figures obtained after an intensive search for cases and after the submission of each case to expert clinical scrutiny. In fact, the use of national mortality data is justified only by the fact that the number of deaths attributed to leukaemia among survivors who were 2,000 metres or more from the hypocentre and who cannot have received more than very small amounts of radiation, is close to the expected number calculated on the basis of these data. Table 2A therefore presents for comparison the numbers of cases of leukaemia known to have occurred during the eight years from 1947 to 1954 among residents of Hiroshima who survived at different distances from the hypocentre, and the numbers of deaths from leukaemia which would have been expected in a similar period among populations of the same size and the same sex- and age-distribution, subjected to the age- and sex-specific mortality rates from leukaemia observed in the whole of Japan in 1952. It can be seen that the observed incidence among survivors who were less than 1,000 metres from the hypocentre is 100 times greater than the mortality which would have been expected.

TABLE 2A

A comparison between the observed and the expected incidence of leukaemia among survivors of the Hiroshima atomic bomb explosion exposed at various distances from the hypocentre; persons subsequently resident in Hiroshima City only

Distance from hypocentre at time of explosion (m.)	No. of cases with onset in the 8-year period 1947–54		No. of deaths expected among the survivors in an 8-year period*	Ratio of total cases observed to expected
	Confirmed	Suspected		
Less than 1,000 ...	15	0	0·15	100·0 : 1
1,000–1,499	28	1	1·32	22·0 : 1
1,500–1,999	6	1	2·33	2·6 : 1
2,000–2,999	6	0	3·96	1·5 : 1
3,000 or more	4	2	4·83	1·2 : 1
All distances	59†	4	12·59	4·7 : 1

* Calculated from the Japanese mortality data for 1952. In calculating the numbers of expected deaths, certain assumptions had to be made about the rate of change of the numbers of survivors in the different age groups, and the figures must be regarded as approximate estimates.

† Two cases referred to in Table 1A are omitted, since the onset of symptoms in one patient was in 1955 and in another patient, who died in April 1955, the date of onset is unknown; the latter patient was exposed at a distance of 2,400 metres from the hypocentre.

The data available for survivors of the Nagasaki explosion are less detailed than those for survivors of the Hiroshima explosion. Altogether 32 confirmed cases and 11 suspected cases are known to have occurred among survivors who subsequently resided in Nagasaki. Of these, 32 and 10 respectively occurred during the eight years 1947-54, and the corresponding total number of deaths which might have been expected in that period on the basis of the national mortality data is approximately 12·3.

Table 3A shows the year of onset of the disease for each of the 108 cases which have occurred among the survivors resident post-war in Hiroshima or Nagasaki, together with estimates of the annual incidence rates. The rates in

the first two years may be under-estimated, since medical organisation was incomplete at that period; in 1953 and 1954 they are almost certainly under-estimated since new cases continue to be discovered and some of the patients give histories of one or more years' duration. The data provide no evidence of a sharp peak in incidence at any particular period after the explosion, nor any clear indication that the incidence has yet begun to diminish by the end of the ninth year.

TABLE 3A

The incidence of leukaemia in different years among survivors of the Hiroshima and Nagasaki atomic bomb explosions; persons subsequently resident in Hiroshima City and Nagasaki only

Year of onset	No. of cases at Hiroshima		No. of cases at Nagasaki		Incidence per 10,000 persons (total cases)		
	Confirmed	Suspected	Confirmed	Suspected	Hiroshima	Nagasaki	Hiroshima and Nagasaki
1946 ...	0	0	0	1	0·0	0·1	0·05
1947 ...	3	0	2	0	0·3	0·2	0·3
1948 ...	7	3	2	1	1·0	0·3	0·7
1949 ...	5	0	1	2	0·5	0·3	0·4
1950 ...	6	1	5	1	0·7	0·6	0·7
1951 ...	11	0	7	3	1·1	1·0	1·1
1952 ...	11	0	6	2	1·1	0·8	1·0
1953 ...	10	0	2	1	1·0	0·3	0·7
1954 ...	6	0	7	0	0·6	0·7	0·7
Part 1955 or date unknown	2	0	0	0	—	—	—
All years	61	4	32	11	6·6	4·5	5·6

Reference

Moloney, W. C. and Kastenbaum, M. A. (1955). Leukemogenic effects of ionizing radiation on atomic bomb survivors in Hiroshima City. *Science*, 121, 308.

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APPENDIX B

Leukaemia and Aplastic Anaemia in Patients Treated with X-rays for Ankylosing Spondylitis

(A summary of the findings of the investigation sponsored by the Medical Research Council Committee reporting on the hazards to man of nuclear and allied radiations)

The investigation had two main objectives: first, to see whether the incidence of leukaemia and of aplastic anaemia was abnormally high in patients treated with X-rays for ankylosing spondylitis; and second, if the incidence of those conditions was found to be raised, to determine the quantitative relationship between incidence and dose of X-rays.

RESULTS

The incidence of leukaemia and aplastic anaemia

The case records of 13,352 patients (11,287 men and 2,065 women) were studied; these patients had been treated at 81 radiotherapy centres and sub-centres during the period 1935–54 inclusive. Slightly fewer than half of them were known to have been alive in 1955 or to have died earlier; the remainder were lost to follow-up at various dates between 1935 and 1954. They had therefore been under observation for periods of between one and twenty years, with an average for the whole group of just under five years.

49 of the patients studied were found to have developed leukaemia, aplastic anaemia, or myelofibrosis (a condition considered to be possibly a variant of leukaemia), and, of these, 46 had died by the end of 1955; 28 of them were certified as having died from leukaemia, 13 from aplastic anaemia and 1 from myelofibrosis. Three other patients with leukaemia and 1 with aplastic anaemia were found, who had been certified as having died from other causes.

With the co-operation of the Registrars-General, special efforts were made to recognise all patients in the series who had died of leukaemia or aplastic anaemia. The great majority of such cases have probably been traced despite the incomplete follow-up, but there is reason to believe that a few more might be revealed if the follow-up could be made complete.

The numbers of deaths from leukaemia and aplastic anaemia which could be expected under normal conditions were calculated from the national death rates for those diseases in the general population: they have been estimated as 2·9 and 0·3 respectively up to the end of 1955, but these estimates are certainly too high, as they are based on the assumption that all the patients untraced in 1955 were, in fact, alive at the end of 1955. In the compilation of the numbers of observed deaths from leukaemia and aplastic anaemia for comparison with these figures, only deaths *certified* as due to these conditions could be used, since the figures for expected deaths are based on death-certificate data. Thus enumerated, the numbers of observed deaths from leukaemia and aplastic anaemia are respectively 28 and 13. The differences between the observed and expected deaths are highly significant for both diseases.

Three of the observed cases of leukaemia died within the first year, and in these cases it was assumed that leukaemia was already present at the time of first treatment. If these 'co-existent' cases, and the corresponding expected mortality in the first year after treatment, are omitted, the observed and expected numbers of deaths from leukaemia for 1935–55 are 25 and 2·4 respectively.

Clinical review of cases

All the relevant data for each case were reviewed, and it was concluded that many of the patients certified as having died of aplastic anaemia had, in fact, been suffering from aleukaemic leukaemia. The diagnosis of aplastic anaemia was substantiated in only 4 cases and it was, therefore, not possible to examine the relationship between the incidence of this condition and the dose of X-rays received.

The relationship between the incidence of leukaemia and the X-ray dose

This relationship has been determined for male patients only, in order to avoid difficulties introduced by the possibility of there being a sex-difference in susceptibility to leukaemia; the 3 'co-existent' cases were excluded, and there then remained 37 cases of leukaemia for study.

Details of X-ray treatment were obtained from the case records for 1,878 men, a sample of approximately 1 in 6 of the whole group of 11,287 male patients. The X-ray dose was expressed in two ways. By the first method, the total energy absorbed in the whole body was calculated, and expressed in megagramme-roentgens (Mgm.r.); by the second, the maximum dose in the spinal marrow was determined and expressed in roentgens (r)*. The number of man-years at risk following each level of dose, calculated by each of the two methods, was estimated for the whole group of male patients and was related to the number of cases of leukaemia. The results of these calculations are given in Tables 1B and 2B, which show the crude incidence rates per 10,000 men per year at all levels of dose.

TABLE 1B

The numbers of male patients developing leukaemia and the crude incidence rates after different doses of radiation (measured by the total amount of energy absorbed by the whole body)

	Amount of treatment: whole-body integral dose (Mgm.r.)						
	0	Less than 7·5	7·5–14·9	15–22·4	22·5–37·4	37·5–52·4	52·5 or more
Number of men developing leukaemia ...	—	1	9	9	10	4	4
Crude incidence per 10,000 men per year ...	0·5†	0·7	4·7	5·1	11·3	22·6	60·2

* For the second method extensive measurements were made on a 'phantom' man.

† The rate given for 'no treatment' has been estimated from the national vital statistics for all forms of leukaemia, and weighted to allow for the fact that not all the patients in the series were certified as dying from leukaemia. If lymphatic leukaemia is excluded (as may be more appropriate) the rate is 0·3.

TABLE 2B

The numbers of male patients developing leukaemia and the crude incidence rates after different doses of radiation (measured by the maximum amount received at a point in the spinal marrow)

	Amount of treatment: maximum dose to the spinal marrow (r)						
	0	Less than 500	500 to 999	1,000 to 1,499	1,500 to 1,999	2,000 to 2,749	2,750 or more
Number of men developing leukaemia ...	—	2	8	8	8	6	5
Crude incidence per 10,000 men per year ...	0·5*	2·2	4·1	4·2	11·3	13·0	17·6

* The rate given for 'no treatment' has been estimated from the national vital statistics for all forms of leukaemia, and weighted to allow for the fact that not all the patients in the series were certified as dying from leukaemia. If lymphatic leukaemia is excluded (as may be more appropriate) the rate is 0·3.

CONCLUSIONS

Deaths from leukaemia have been found to be greatly increased among the patients studied, and it is believed that this increase is the result of exposure to X-rays; the possibility that sufferers from ankylosing spondylitis are unusually sensitive to the action of X-rays cannot, however, be excluded.

Both methods of estimating the dose show a relationship between the crude incidence of leukaemia and the dose of X-rays, with an increase in the incidence over the whole range of dose studied. With neither method is there any evidence of a threshold below which no increase in incidence is produced. Both sets of results, therefore, suggest that even very small amounts of radiation will have an appreciable effect if given to a large enough population. The method based on calculation of the whole-body energy absorption, however, shows a disproportionately greater increase at high levels of dose, whereas that based on calculation of the spinal-marrow dose shows a simple proportional increase at all levels of dose. It is of considerable importance to determine the reason for this discrepancy. If the true relationship is of the curvilinear type suggested by the first method, it remains a theoretical possibility that very small doses (which could not be tested in the investigation) will have no leukaemogenic effect at all; but such a possibility can almost be ruled out if the relationship is linear.

A full account of the investigation will be published by H.M. Stationery Office as a report in the Medical Research Council's Special Report Series.

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APPENDIX C

The Spontaneous Mutation Rate in Man

In certain circumstances the spontaneous mutation rate of human genes, expressed as the number of mutations per locus per generation, can be estimated with a fair degree of reliability. A direct count of cases due to fresh mutation can be made for a gene which has a dominant effect. This number can also be directly inferred for sex-linked traits shown only in the male. If the incidence of a trait in the general population is known, the mutation rate can be determined from the proportion of cases due to fresh mutation.

When the effects of a gene are very disadvantageous a different line of argument can be used, even though the gene may not be fully manifest in the heterozygous state. The principles on which the indirect estimation of mutation rates can be made were laid down by Haldane (1932), and they are used in the formulae given in Appendix D. The assumption is made that the human population is in a state approaching genetical equilibrium; it is supposed that disadvantageous genes could not have persisted in the population unless their extinction by selective mortality were balanced by the recurrence of mutation.

In the case of dominant or sex-linked characters associated with very high mortality, the direct measurement of mutation rate can be supplemented by the indirect argument. Estimates which are entirely indirect are untrustworthy, but they have been made for a variety of genes recognised only by their recessive effects. One difficulty with recessive traits is that allowance has to be made for the effects of inbreeding. Another likely source of error is that genetical equilibrium can be maintained not only by mutation but also by a slightly advantageous effect in the heterozygote. Hence, indirect estimates are likely to be too high.

There is a further general difficulty; this arises from uncertainty as to whether or not a single locus is involved in determining the trait under consideration. Mutations at two or three loci might produce similar characters; this apparently occurs in haemophilia and also in achondroplasia. In these circumstances any estimate of mutation frequency, direct or indirect, based upon accepting one locus as the hereditary cause of the trait, would be too high by a factor depending upon the frequencies of the component causal genes.

Estimates of mutation rate in man are given in Table 1C. Those for dominant traits are based upon the direct method, though they can all be indirectly supported; the estimates for sex-linked genes are calculated indirectly, but are supported by direct observation of pedigrees almost certainly containing freshly mutated genes; those for recessive traits are all indirectly estimated.

I should like to acknowledge here the assistance given me in connection with Appendices C, D, and E by Dr. H. Harris and Dr. D. A. Sprott.

TABLE 1C

Estimates of the spontaneous mutation rates of some human genes

Trait: Name	Trait: Mode of inheritance	Mutation frequency of the causal gene (per million per generation)	Region	Source	Date
Epiloia (tuberose sclerosis)	Dominant	8	England	Gunther and Penrose	1935
Achondroplasia ...	Dominant	45	Denmark	Mørch	1941
Aniridia	Dominant	5*	Denmark	Møllenbach	1947
Microphthalmos (without mental defect)	Dominant	5	Sweden	Sjøgren and Larsson	1949
Retinoblastoma ...	Dominant	15	England	Philip and Sorsby	1947
		23	U.S.A.	Neel and Falls	1951
		4	Germany	Vogel	1954
Partial albinism (with deafness)	Dominant	4	Holland	Waardenburg	1951
Haemophilia (severe type)	Sex-linked	20	England	Haldane	1935
		32	Denmark	Andreassen	1943
		27	Switzerland and Denmark	Vogel	1955
Muscular dystrophy (Duchenne type)	Sex-linked	95	U.S.A.	Stephens and Tyler	1951
		45	N. Ireland	Stevenson	1953
		43	England	Walton	1955
Albinism	Recessive	28	Japan	Neel *et al.*	1949
Ichthyosis congenita ...	Recessive	11	Japan	Neel *et al.*	1949
Total colour blindness...	Recessive	28	Japan	Neel *et al.*	1949
Infantile amaurotic idiocy	Recessive	11	Japan	Neel *et al.*	1949
Amyotonia congenita ...	Recessive	20	Sweden	Böök	1952
True microcephaly ...	Recessive	49	Japan	Komai *et al.*	1955
Phenylketonuria ...	Recessive	25	—	(Appendix D)	—

* This estimate differs by a factor of 2 from that given by the author, but it is based on the author's material.

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APPENDIX D

Calculation of the Quantitative Effects of Spontaneous and Induced Mutation Rates in Diseases Caused by Single Genes

In order to estimate the quantitative effect of doubling the mutation rate or of raising it by any given factor, it is necessary to calculate the proportion of cases of the disease in question in each generation which can be attributed to spontaneous mutation. This can be done by using the indirect method of calculating mutation rate; the steps in the argument are set out in Table 1D. The abnormal allele is represented by **a**, and the normal by **A**.

TABLE 1D

Calculation of the number of cases due to spontaneous mutation in diseases caused by single genes

Steps in the calculation	Dominant trait: Formula	Dominant trait: Example: achondroplasia	Sex-linked trait: Formula	Sex-linked trait: Example: haemophilia	Recessive trait: Formula	Recessive trait: Example: phenylketonuria
1. Sex affected ♂ or ♀	♂ or ♀	—	♂	—	♂ or ♀	—
2. Genotype responsible for the disease	Aa	—	a	—	aa	—
3. Frequency of genotype in population (where q is the frequency of a)...x	2q	1/10,000	q	1/12,000	q^2	1/40,000
4. Comparative loss of fitness associated with the genotype	(1–F)	4/5	(1–F)	7/8	(1–F)	1
5. Mutation rate of A to a per gene per generation ......m	q (1–F)	40×10^{-6}	q (1–F)/3	24×10^{-6}	q^2 (1–F)	25×10^{-6}
6. Proportion of genotypes (or cases of the disease) due to fresh mutation in each generation .... d	(1–F)	4/5	(1–F)/3	7/24	2q (1–F)	1/100
7. Frequency of abnormal genotypes due to fresh mutation:— (i) in each generation of births .... x × d	2m	80×10^{-6}	m	24×10^{-6}	2mq	$0{\cdot}25 \times 10^{-6}$
(ii) among 20×10^6 births (♂ and ♀) over a period of 30 years ...	$40m \times 10^6$	1,600	$10m \times 10^6$	240	$40mq \times 10^6$	5

In this table three hereditary diseases, achondroplasia, haemophilia and phenylketonuria are used to show the methods applicable respectively to dominant, sex-linked and recessive traits determined by single genes. For the present purpose the population is assumed to be in genetical equilibrium; the loss due to unfitness of genotypes is balanced by recurrent natural mutation. The figures for achondroplasia are derived from Mørch (1941); in Step 3, 1/9,400 has been rounded off to 1/10,000. For haemophilia the figure (7/8) in Step 4 has been taken from Andreassen (1943) and that in Step 3, (1/12,000), agrees with estimates by Haldane (1935). The figures in Steps 3 and 4 for phenylketonuria are derived from Jervis (1939) and Munro (1947). Doubling the spontaneous mutation rate on one occasion would increase the frequency of each disease by a proportion, shown in Step 6, in the next generation.

The results given in Step 7 (ii) in Table 1D apply only to the first generation after doubling. The quantitative effects in subsequent generations can be ascertained by substituting appropriate new incidence figures and repeating the steps of the calculation. Calculations made by this method and extended to cover six generations yield the results shown in Table 2D; the theoretical limiting values obtained after an infinite number of generations are also given. The results are shown graphically in Fig. 1 (p. 32). Two situations are considered: (*a*) the effect of a single doubling of mutation rates in one generation, and (*b*) the effect of permanently doubling the mutation rate.

TABLE 2D

The effects, expressed as the increase in incidence per hundred cases, of doubling the mutation rates, (a) *in one generation only, and* (b) *permanently, for three hereditary traits*

(*a*) *Doubling in one generation only*

Number of generations	Increase in incidence per hundred cases		
	Achondroplasia (Dominant)	Haemophilia (Sex-linked)	Phenylketonuria (Recessive)
0	0	0	0
1	80	29	1
2	16	29	1
3	3	16	1
4	1	10	1
5	0	6	1
6	0	4	1
Infinite	0	0	0

(b) Permanent doubling

Number of generations	Increase in incidence per hundred cases		
	Achondroplasia (Dominant)	Haemophilia (Sex-linked)	Phenylketonuria (Recessive)
0	0	0	0
1	80	29	1
2	96	58	2
3	99	74	3
4	100	84	4
5	100	90	5
6	100	94	6
Infinite	100	100	100

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L. S. PENROSE

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APPENDIX E

Estimate of the Incidence of Cases of Schizophrenia and Manic Depressive Reaction due to Spontaneous Mutation

The calculation of (d), the proportion of cases which can be attributed to fresh mutation in each generation, for the conditions discussed in paragraphs 148–150, can be set out as shown in Table 1E.

TABLE 1E

Calculation of (d), *the proportion of cases due to fresh mutation in each generation, in schizophrenia and manic depressive reaction*

Steps in the calculation	(i) Schizophrenia		(ii) Manic depressive reaction	
	Formula	Example	Formula	Example
1. Sex affected ♂ or ♀	♂ or ♀	—	♂ or ♀	—
2. Genotype responsible for predisposition to the disease ...	aa	—	Aa	—
3. Frequency of genotype in population (where q is the frequency of a) x	q^2	1/100	2q	1/200
4. Comparative loss of fitness associated with the genotype ...	(1–F)	1/20	(1–F)	1/70
5. Mutation rate per gene per generation ... ... m	q^2 (1–F)	1/2,000	q (1–F)	1/28,000
6. Proportion of predisposed cases due to fresh mutation in each generation ... ... d	2q (1–F)	1/100	(1–F)	1/70

(*i*) *Schizophrenia*

This is a type of mental disease which has its onset at about the age of 30 years on the average. There are many degrees of severity, and males are more often affected than females. The genetical predisposition occurs in subjects who are, according to Kallmann (1938), homozygous for a specific recessive gene; it has an incidence of about 1/100 in European populations (Fremming, 1947). On the basis of a rough survey of hospital data, it is assumed here that only one-tenth of predisposed subjects become chronically incapacitated. Neglecting sex differences, the fertility of these incapacitated patients is reduced by 1/2 (Essen-Möller, 1935). The total loss of fitness of predisposed genotypes is thus 1/10 × 1/2 = 1/20. It follows that d=2q(1–F)=1/100, as shown in Table 1E. In a generation of 20 × 10^6 births the number of incapacitated people would be 20,000, of whom 200 would be cases due to fresh mutation.

(ii) Manic depressive reaction

This is a type of mental disease with mean age of onset at about 40 years. There are many degrees of severity, and females are more often affected than males. The genetical predisposition, which is commonly believed to depend upon a dominant gene (Marrell, 1951), has a frequency in the population of about 1/200; Mayer-Gross, Slater and Roth (1954) quote 0·35 per cent for Scotland and 1 per cent for Sweden and Denmark as the total morbidity risk of affective psychosis. The incidence of chronic breakdown among those who are predisposed can be estimated at one-seventh and, for such incapacitated patients, fertility is reduced, according to observations by Essen-Möller, by a factor of 1/10. Hence the total loss of fitness for predisposed genotypes is $1/7 \times 1/10 = 1/70$. It follows that $d = (1-F) = 1/70$. In a generation of 20×10^6 births there would be 14,000 people incapacitated, of whom 200 would be cases due to fresh mutation.

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APPENDIX F

The Effect of Changing the Mutation Rate on Characters Showing Continuous Variation about the Normal

Member genes of a polygenic system cannot be followed as individuals in our observations. We must therefore measure mutation by the increment it adds to heritable variation, instead of measuring it by the frequency of change, as is done in the case of individually traceable genes.

The increment added to the variance per generation has been estimated for spontaneous mutation in two hair characters in *Drosophila* (Clayton and Robertson, 1955; Durrant and Mather, 1954; Paxman, 1955). Technically this is a difficult operation, but the estimates agree surprisingly well for each character when allowance is made for various possible sources of bias. The two characters agree, also, in showing an increment of about 1×10^{-3} of the amount of heritable variation estimated by Clayton and Robertson to be present in a normal population of *Drosophila*. The estimate may easily be too small by a factor of 2 or 3, but seems unlikely to be out by a factor of 10. This finding suggests that the relation between the amount of heritable variation in a population and the mutation rate is roughly linear, and certain theoretical considerations point the same way. It also shows that a marked increase in the mutation rate for a few generations would have only a trivial effect on the heritable variation of the population, and that, even with a persistent increase, the new equilibrium showing the full effect of the raised mutation rate in raising heritable variation would take very many generations to achieve.

Clayton and Robertson also record the result of irradiating the adults of each generation with an X-ray dose of 1,800 r. The increment added in each generation to the heritable variation available to selection was about ten times the spontaneous increment. Thus the dose which doubles the effect of spontaneous mutation would be some 200 r as measured by this criterion. But the new heritable variation available to selection in these experiments seems to have represented only about one-sixth of all the new heritable variation as measured directly by the increase in phenotypic variation. If we take the overall total therefore, 200 r must have produced about six times the spontaneous increment, so that the doubling dose becomes just over 30 r. This is more in keeping with the figure obtained from lethal mutation, though it might have been expected that the polygenic figure would be higher, not lower, than the monogenic, because of the way in which mutations can balance one another's effects in a polygenic system. It would, in any case, be unwise to place great confidence in these calculations, both because other experiments of the same kind have given results even more difficult to assess, and because doses as heavy as 1,800 r produce so many lethal mutations and so much structural change in the chromosomes that the polygenic effects may well be quantitatively distorted.

In attempting to extrapolate from these findings to the effects of irradiation in natural, including human, populations, two points must be borne in mind. Firstly, the flies which yielded these observations were from inbred lines, so that any mutation in the polygenic system would add its quota to the increase in variation. In a natural population, however, variation is already present, and mutation from one allele to another, where both already exist

and are not uncommon in the population, could add little if anything to the variation. Only mutation in genes whose alleles are rare or absent in the population will contribute materially to an increase in variation, and the contribution will fall off as the alternative alleles become more common. So if most of the member genes of a polygenic system are already varying, mutation will add correspondingly little to the total variation. On the other hand, if, as seems likely, many of the genes which can prospectively contribute to the variation of a polygenic system are not doing so because only one allele is present or at least common, new mutation increases the number of loci contributing to the variation which will then increase correspondingly. The roughly linear relation between mutation and variation suggested by the experiments is thus to be regarded as a maximum effect, the closeness of approach to it depending on the initial conditions of variation prevailing in the population in question.

The second point concerns the properties of heterozygotes. The linear relation would cease to hold if there were any innate advantage of individuals heterozygous for the genes over others homozygous for them. A situation analogous to balanced polymorphism would then arise, and at equilibrium the heritable variation would become independent of mutation rate. No such heterozygotic advantage was detected in the variation arising by mutation, even though Paxman made a special search for it; nor is it likely on general grounds. Such an advantage must, however, remain as a possible, even if unlikely, additional reason for regarding a linear relation as representing the maximum, rather than the regularly realisable, effect of mutation on variation in populations such as those of man. In other words a permanent doubling of the mutation rate would not be expected to do more, and under some circumstances might do less, than double the heritable variation in the population.

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K. Mather

APPENDIX G

The Effect on the Distribution of Intelligence of Increasing the Heritable Variation

Under conditions of natural selection the effect of raising the heritable, and hence the total, variation of expression of a character, some particular expression of which is the fittest, must in general be to lower the average fitness in the population; though where the mean departs widely from the most advantageous expression of the character the fitness of selected groups might be raised. Furthermore, where all expressions of the character may be displayed, the overall fall in fitness must be directly related to the increase in variation. A fall in fitness is not, however, to be translated directly into social load when we are considering the consequences of raising the heritable variation in such a character as mental capacity in man.

The intelligence score of an individual is derived from his performance in a series of tests, and the frequency distribution obtained from a population must therefore reflect the structure of the tests. The distribution obtained is generally treated as being normal, but in fact certain disturbances occasionally appear, and they seem to be of a type which no simple transformation or statistical adjustment can remove. It is very likely that such discrepancies spring from innate features of the test, and they should not, in any case, be allowed to obscure the essentially normal nature of the distribution. Normality will in fact be assumed in the following discussion. It is considered that by avoiding the use of very narrow ranges and, more particularly, by discusssing relative rather than absolute effects, broadly valid conclusions should be attained.

I am informed by Professor F. A. Peel of the Department of Education in the University of Birmingham, that in educational discussions the distribution of intelligence scores is taken as normal and is standardised to a mean of 100 with a standard deviation of 15. The scores have been found to approximate to I.Q.'s as measured by the original tests, and are commonly referred to as 'I.Q. scores'. In these terms, Professor Peel further informs me, children with an I.Q. of less than 70 are generally regarded as educationally sub-normal and as requiring education in special schools; those with an I.Q. of betwen 70 and 80 are regarded as needing special teaching in ordinary schools; and those with an I.Q. of over 115 as being of grammar school quality. These figures are to be taken only as general guides since they are applied neither rigorously nor uniformly throughout the country.

For information and advice on the estimation of the heritable variation in respect of I.Q., I am indebted to Dr. J. A. Fraser Roberts. Sibs show a correlation close to 0·5 in this character. Data on parent-offspring relations are less full, but suggest a similar figure, so that there is no good reason to postulate any over-dominance or heterozygous advantage. Taken on their face value, such figures would indicate virtually complete genetic determination, but there is strong assortative mating in respect of this character and there is also the effect of a common home environment to be taken into account in assessing the genetic meaning of these familial correlations. Observations on twins and foster children would seem to indicate $\frac{3}{4}$ as the fraction of variation which is heritable. However, lest this should be an over-estimate, parallel calculations have been made, assuming fractions of $\frac{1}{2}$ and $\frac{3}{4}$ as likely to straddle the true situation. Should even the figure of $\frac{1}{2}$ be too high the effect of increasing the heritable component would be correspondingly smaller. Assortative mating has been disregarded, as we may

reasonably assume that its incidence would not be affected by alteration in the amount of heritable variation, so that its relative effect would remain the same.

Calculations have been made of the effects of raising the heritable variation (V_H) to 1·25, 1·50 and 2·00 times its present value, assuming that environmental variation (V_E) remains unaltered. Thus, taking V_H to be $\frac{3}{4}$ of the total variation (V_T) we have:

$$V_H = \tfrac{3}{4}V_T = \tfrac{3}{4} \times 15^2, \text{ and}$$
$$V_E = V_T - V_H = \tfrac{1}{4} \times 15^2,$$

so that doubling the heritable variation would give us the new total:

$$V_T' = V_E + 2V_H = (\tfrac{1}{4} \times 15^2) + (2 \times \tfrac{3}{4} \times 15^2) = 1\tfrac{3}{4} \times 225 = 393{\cdot}75$$

and the new distribution of I.Q. would have a standard deviation of 19·84, the mean remaining at 100. The proportion of individuals expected to have, for example, an I.Q. of less than 70 can then be found as the area in the tail of the distribution cut off by the ordinate falling short of the mean by a normal deviate of $\frac{30}{19{\cdot}84} = 1{\cdot}512$.

The results of this and similar calculations are shown in Table 1G, where they are expressed as values relative to the proportion calculated as falling into corresponding classes with the distribution as it is now assumed to be. Thus the assumed present distribution ($\bar{x} = 100$, $s = 15$) gives 2·27 per cent of individuals with an I.Q. of less than 70. With the heritable fraction at $\frac{3}{4}$ of the total and doubled, 6·53 per cent fall below a score of 70, making a relative value of $\frac{6{\cdot}53}{2{\cdot}27} = 2{\cdot}88$. In other words, on these assumptions, doubling the heritable variation would nearly treble the number of children with an I.Q. of less than 70 (i.e. those needing to be taught in special schools, as judged by a common convention of today). In addition to the relative changes in the numbers with an I.Q. of less than 70, figures are also given for those in the classes with I.Q. less than 75 (a figure sometimes taken to indicate the need for special schooling), I.Q. between 70 and 80 (special teaching), and I.Q. over 115 (grammar school).

TABLE 1G

The effects of raising the heritable variation on the frequencies of different intelligence groups

Intelligence group	Present proportion per cent of population (assuming a normal distribution)	Assumed present heritable fraction	Factor of increase relative to present proportion with heritable variation raised to: (constant mean)			(falling mean)
			1·25	1·5	2·0	2·0
I.Q. < 70	2·27	$\frac{1}{2}$	1·31	1·62	2·26	3·24
		$\frac{3}{4}$	1·46	1·94	2·88	4·51
I.Q. < 75	4·78	$\frac{1}{2}$	1·22	1·42	1·82	2·50
		$\frac{3}{4}$	1·32	1·63	2·17	3·24
70<I.Q.<80	6·85	$\frac{1}{2}$	1·09	1·16	1·27	1·59
		$\frac{3}{4}$	1·13	1·22	1·33	1·75
I.Q.>115	15·87	$\frac{1}{2}$	1·09	1·17	1·30	1·00
		$\frac{3}{4}$	1·13	1·24	1·42	

In these calculations the variance has been assumed to change without alteration of the mean, so that the proportion of high I.Q. increases with the proportion of low I.Q. A further calculation has been made in which, as the variance increases, the mean is allowed to fall so as to keep constant the proportion with I.Q.'s of over 115. This is intended to illustrate the kind of result which would be obtained if mutation were preponderantly, but not wholly, degradatory. The relative changes for doubled heritable variation on this assumption are also shown in the table.

It should be remembered that these changed proportions would be achieved by corresponding increases in the mutation rate, only when equilibrium had been reached or at least closely approached, that is to say, after very many generations.

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APPENDIX H

The Doubling Dose of Radiation for Various Plants and Animals

In paragraphs 171-182 we were concerned with finding some quantitative measure of the effectiveness of radiation in causing mutations, with the purpose of using this estimate to establish maximum levels of exposure which are genetically tolerable. In this context, one must be particularly careful not to under-estimate the effects of radiation. In order to express its influence in terms of a 'doubling dose', we should try to arrive at a figure for the lowest dose of radiation which changes mutation in a way which is effectively equivalent to a doubling of the mutation rates of every gene. For those genes with very low spontaneous mutation rates a doubling of the rate will be relatively unimportant. Among the genes with relatively greater spontaneous mutation rates, some may be more sensitive to radiation than others. What we need to estimate is the dose of radiation which doubles the mutation rate of a sample of these more sensitive genes sufficiently large to be physiologically representative of mutations in general. This might be called a 'minimum representative doubling dose'.

After pointing out (paragraph 176) that this can scarcely be less than the naturally occurring dose of radiation, we discuss the possible modifications of this bedrock minimum in terms of an argument which was originally largely due to J. B. S. Haldane (1948). This consists of an attempt to estimate the fraction of the spontaneous mutation rate in man which can plausibly be attributed to natural radiation. The argument proceeds by analogy with the conditions in other organisms, and particularly those in the fruit-fly (*Drosophila*) and the mouse.

In *Drosophila*, suppose that:

f = the fraction of spontaneous mutations due to natural radiation,

r = the rate of natural radiation (in r per day),

m = the rate of mutation induced by 1 r,

s = the spontaneous mutation rate,

t = the average age at reproduction (in days).

Then we shall have:

$$f = \frac{m\,r\,t}{s}$$

Similarly if capital letters represent the same factors in man, we shall have

$$F = \frac{M\,R\,T}{S}$$

Now the spontaneous mutation rate and the rate of induced mutation (s and m) are much better known for flies than for man (S and M). Thus, the procedure is to arrive at F by first finding as good a value as possible for f and then modifying this according to the relation,

$$F = f \times \frac{M\,R\,T\,s}{m\,r\,t\,S}$$

In his original presentation of the argument, Haldane adopted for f a value (0·001) which had been calculated by D. E. Lea (1946). However, it has recently been pointed out (Spiers, 1956) that Lea based his calculation on an estimate of natural radiation of 2·2 milliroentgens (mr) per day, which is about eight times greater than that accepted at the present time (paragraph 200). The value for f has to be reduced accordingly. For the sake of simplicity we have taken it as 0·0001 (paragraph 178), although this may be a slight underestimate.

This value for f is based on an estimate of 0·15 per cent sex-linked lethals. Lea argues, in agreement with other authors who have considered the matter, that there may be about 1,000 genes capable of mutating to sex-linked lethals in *Drosophila*; his value for the spontaneous mutation rate can therefore be expressed as $1{\cdot}5 \times 10^{-6}$ per locus. The statement (paragraph 178) that the human spontaneous mutation rate is probably about five times as great as this is based on the observation that the mutation rates of several human genes are around 10 per million per generation (cf. Table 1C).

The opinion (paragraph 179) that mouse genes are more sensitive to radiation than those of *Drosophila* (i.e. have a higher value for m) is based on the work of W. L. Russell (1954). It is disputed by some authors; but if one adopts it, and takes it to suggest the hypothesis that human genes are also more sensitive, the result is to increase the value of F, and thus to lower the estimate of the minimum representative doubling dose. In order to be as cautious as possible, we have therefore adopted these assumptions.

The estimate (paragraph 185) that one human germ cell in ten carries a new mutation is a minimum figure arrived at in calculations by H. J. Muller (1950, 1954).

Some typical figures for doubling doses derived from experiments on plants and animals are given in Table 1H. These figures are open to a wide margin of statistical error, as the number of spontaneous mutations was always small; in most instances values smaller or larger by a factor of 2 are not excluded.

There have been many other reports of experiments in which mutations were induced by ionizing radiations, especially in plants and lower organisms; however, in the great majority of these, either the control series was too small for any spontaneous mutation to be observed, or the apparent mutants found were not confirmed by genetic test. The figures quoted here are probably representative.

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(*a*) *Text*

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TABLE 1H

Some typical figures for the doubling dose of radiation in various higher organisms

Group	Genus and species	Cell stage irradiated	Genes studied	Doubling dose (r)	Source of information (*see list of references*)
					No.
Plant	*Zea mays*	Pollen	Four recessive visibles	28	1
	Oenothera organensis ...	Pollen	Self-incompatibility	60	2, 3
	Prunus avium	Pollen	Self-incompatibility	60	2, 3
Insect	*Drosophila melanogaster* ...	Spermatozoa	Sex-linked lethals	50	4
	Drosophila melanogaster ...	Spermatozoa (aged) ...	Sex-linked lethals	140	5, 6
	Drosophila melanogaster ...	Spermatogonia ...	Sex-linked lethals	8	7
	Drosophila melanogaster ...	Oocytes and oogonia ...	Nine recessive visibles	390	8
	Drosophila melanogaster ...	Spermatozoa	White eye	60	9
Mammal	*Mus musculus*	Spermatogonia ...	Dominant visibles, semi-steriles, sex-linked lethals	50	10
	Mus musculus	Spermatogonia ...	Seven recessive visibles ...	50	11, 12

(*b*) *Table 1H*

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APPENDIX J

The Dose of Radiation Received in Human Tissues from Natural Sources

All living organisms absorb ionizing radiation from sources which are either present in their environment or incorporated in their own tissues. The most important 'external' radiations are cosmic rays and the radiations arising from elements of the uranium and thorium series present in the earth or the air and from the potassium 40 content of the earth and of vegetable matter. The 'internal' irradiation arises chiefly from the potassium in tissues, but there is also a small contribution from carbon 14 and in some cases soft tissues within bone receive radiation from very small amounts of radium present in the skeleton. Radon in the atmosphere, besides contributing its quota to the external radiation, may also add to the internal dose, by access of the radioactive material to the tissues via the lungs. Because alpha rays and beta rays are so readily absorbed, even by the elements of low atomic number which comprise the soft tissues, the dose from external sources may be regarded as entirely due to cosmic rays and gamma rays. The dose from internally absorbed radioactive materials, however, arises in large measure from beta rays and, when present, from alpha rays.

The radiation dose to human tissues from this normal background is small compared with doses known to cause immediate somatic change, and its significance is presumably to be sought in possible long-term effects; of these the genetic and carcinogenic actions of ionizing radiation would appear to be the most likely. The critical tissues considered in this appendix are, therefore, the gonads and the osteocytes of the Haversian systems in bone. An estimate of the dose to these tissues is made as far as present data allow, and although this is attempted for a number of different localities, it must be emphasised that knowledge of the basic data is far from complete.

All doses are expressed as soft-tissue doses in rads, and where calculations have involved the quantity 'W,' the energy per ion pair formed in air, a value of 34 eV has been taken as representative of recent experimental determinations of that constant.

SOURCES OF EXTERNAL RADIATION

Cosmic rays

The cosmic ray ionization intensity in air at sea-level (and geomagnetic latitudes above 41° N) has been variously quoted in the literature over a range of from 1·5 to 2·8 ion pairs/cc/sec. The differences have arisen largely in the interpretation of high-pressure ionization-chamber measurements, and the correct method of analysis appears to be that used by Clay and his co-worker (1938), and re-examined and supported by further experiments by Burch (1954). The mean of three values given by these workers and corrected to ionization in free air is 1·92 ions/cc/sec. Converting this value to a tissue dose gives:—

cosmic ray dose-rate to soft-tissues = 0·028 rad/year.*

This dose-rate applies to any soft tissues in the body, including the gonads and osteocytes, and is typical for most locations at the earth's surface above latitude 41° N. It may be some 20 per cent less in basements of tall buildings, which absorb the 'soft' cosmic ray component; but any considerable

* 1 rad corresponds to a dose of about 1·07 r in soft tissues.

reduction in dose-rate will only occur in deep underground situations. At a height of 10,000 ft. the cosmic ray dose-rate is increased by a factor of 3 or more, but it will be seen later that cosmic rays contribute only a fraction of the total tissue dose, and that, in consequence, the tissue dose-rate at this height might be only some 40 per cent greater than at sea-level.

The dose-rate of 0·028 rad/year is less than the figure 0·035 rad/year, given, for example, by Libby (1955). For the reasons given above, the lower figure appears to be the correct one and is retained in the present analysis. It is a matter of some discussion whether or not the small component of slow particles in cosmic radiation has a higher R.B.E. than the fast-meson component, the R.B.E. of which has been taken as unity. Some mutations are reported as being less effectively produced by heavily ionizing particles; in a few other cases the R.B.E. for heavy particles has been found to be in the region of 5. In the extreme case, the slow-particle component would not appear likely to add as much as 0·01 rad/year to the effective cosmic ray dose. On the other hand, some shielding of the soft gamma ray component of the cosmic radiation by building structures occurs, which reduces the dose below the unshielded value. It would seem best, in view of this and the possible effect of a higher R.B.E., to accept the unshielded value of 0·028 rad/year as the best estimate for sea-level and latitudes above 41° N.

Local gamma rays

Under most conditions of life, gamma radiation from local surroundings is responsible for the greater fraction of the external radiation dose. Measurement of this contribution, however, has been made in comparatively few places.

Some measurements of local gamma ray dose-rates in Leeds and Aberdeen (Spiers and Griffith, 1956) are summarised in Table 1J. The results cover only limited types of situation, but the concordance between the dose-rates in brick and concrete buildings, whether in Leeds or in Aberdeen, suggests that they may be fairly representative of the dose-levels in areas which are not specially radioactive and in buildings not made of granite. The dose-rates in Leeds determined with a lightly shielded counter, are about 20 per cent higher than the background measurements previously reported, but this effect was shown to be due to the difference in shielding in the two measurements. The results in Table 1J represent the local gamma ray dose-rates under conditions of light shielding.

TABLE 1J

Measurements of local gamma ray dose-rates in Leeds and Aberdeen (Spiers and Griffith, 1956)

Type of building (or 'out-of-doors')	Location	Local gamma ray dose-rate (rad/year)
I. All granite ...	(*a*) Aberdeen—laboratory	0·107
	(*b*) Aberdeen—bell tower	0·099
	(*c*) Aberdeen—entrance hall	0·101
II. Brick and concrete	(*a*) Aberdeen—rooms on various floors	0·073
	(*b*) Leeds—room in hospital building...	0·081
	(*c*) Leeds—single-storey laboratory ...	0·080
	(*d*) Leeds—various rooms in house ...	0·077
III. Out-of-doors ...	Leeds—garden of house II(*d*) above ...	0·048

Sievert and Hultqvist (1952) and Sievert (1955) have reported measurements of the total 'cosmic ray plus gamma ray' background in Swedish houses and in out-of-door situations. Some of the Swedish results are given in Table 2J where an allowance has first been made for the cosmic ray fraction and the residual ionization converted to tissue dosage. The mean dose-rates recorded were based on measurements in about 70 houses. Variations in dose-rate were most marked in Type 3 houses where, in some, values 50 to 100 per cent above the mean in Table 2J were recorded.

TABLE 2 J

Measurements of the total 'cosmic ray plus gamma ray' background in Swedish houses and in out-of-door situations in Sweden (Sievert and Hultqvist, 1952 ; *Sievert,* 1955)

Situation		Gamma rays only (ions/cc/sec.)	Mean dose-rate (rad/year)
Indoors, centre of room	Wooden houses	4·0	0·059
	Brick and concrete houses (Type 1)	6·2	0·091
	Brick and concrete houses (Type 2)	6·5	0·095
	Brick and concrete houses (Type 3)	14·8	0·216
Outdoors	Stockholm streets	5·8	0·085
	Over igneous rocks	3·9 to 8·3	0·06 to 0·12
	Over clay	3·4	0·05

The local gamma ray dose-rate can also be estimated approximately at some places in South-west England from geiger-counter recordings reported by Peirson (1951). Taking the counting rate given by Peirson for a 'normal' background situation, and allowing for the cosmic ray fraction, a counting rate of about 1·3 cts/min. per cm^2 of projected cathode area is deduced for this 'normal' local radiation. Assuming an over-all efficiency of the counter assembly of 0·6 per cent, and a mean gamma ray energy of 1 MeV, a gamma ray flux is deduced which corresponds to a tissue dose-rate of 0·05 rad/year, a figure reasonably in accord with measurements in Leeds and in Sweden away from areas of high radioactivity. Applying the same analysis to the recorded counting rate in St. Ives and its neighbourhood, the local gamma ray dose-rate is deduced as approximately 0·25 rad/year. Great accuracy cannot be claimed for this estimation in view of the assumptions made, but it is of the same order as the values given in Table 2J for areas of known high radioactivity in Sweden. Dose-rates of 0·3 rad/year and over have been reported in some parts of Cornwall (Wood and Willey, 1954).

Atmospheric radon

Radiation from the break-down products of atmospheric radon also add to the external gamma ray dose. This effect may be expected to be small under most conditions, but so far no measurements have been made to distinguish its contribution from that of gamma rays from the solid surroundings. Peirson and Franklin (1951) have calculated that at ground level an atmospheric radon content of 3×10^{-13} c/l produces a gamma ray flux of the order of 10 quanta/cm^2/min. Taking a mean energy of 0·8 MeV for radium B and C gamma rays, the tissue dose-rate for this flux is 0·0022 rad/year. Anderson, Mayneord and Turner (1954) have reported levels of atmospheric radon in London (in May 1953) which averaged $2\text{–}3 \times 10^{-12}$ c/l. and under these conditions the external gamma ray dose from atmospheric radon is of the order 0·02 rad/year—a contribution of nearly the same magnitude as that due to cosmic rays.

SOURCES OF INTERNAL RADIATION

Potassium 40

The following data have been used in the calculation of the dose-rate from the potassium content of the body:—

Mean potassium content of body	= 0·215 per cent
Specific β-activity of K	= 27·4 β's/sec./g. K
Specific γ-activity of K	= 3·5 γ's/sec./g. K
Mean β-ray energy of K 40	= 0·605 MeV
Mean γ-ray energy of K 40	= 1·46 MeV

Because the mean range of the beta particles of potassium 40 is only some 2 mm, the dose-rate in a given organ is determined mainly by its own potassium content. In the absence of precise values for the potassium content of the gonads, the mean for the whole body, derived from Shohl's data (1939) is adopted. The total tissue dose-rate derived from the energy released per g. of tissue, is then:—

$$\text{tissue dose-rate due to K} = 0{\cdot}018(\beta) + 0{\cdot}002(\gamma) = 0{\cdot}020 \text{ rad/year.}$$

In relation to this calculation there may be doubt as to the precise value for the potassium content of the gonads. So far it has only been possible to make flame-photometric measurements on tissues taken from two post mortem examinations. The results, obtained through the kind co-operation of Dr. F. M. Parsons of the Urological Department of the General Infirmary at Leeds, are as follows:—

Case and age		*Potassium content (mg./100 g.)*	*Sodium content (mg./100 g.)*
A. 67 yr. (testes)	(i)	190	205
	(ii)	240	172
B. 28 yr. (ovaries)	(i)	188	200
	(ii)	197	204

Three results are concordant and one (Aii) is suspect in that the sodium value is so low. They suggest, however, that no great error is being made in assuming an average potassium content of 0·215 per cent, as given by Shohl. The tissues were taken from the central parts of the gonads, and the potassium content should be representative of the average value over dimensions of a few mm. of the tissues containing the germ cells.

Carbon 14

Carbon in living systems contains approximately 1 part carbon 14 in 10^{12}, and has a specific beta ray emission of 0·2 beta particles per sec per g. carbon, with a mean energy of 0·053 MeV. Taking the carbon content of tissue as 18 per cent, the energy deposition due to the carbon 14 amounts to a tissue dose-rate of only 0·001 rad/year.

Radon and its disintegration products

An estimation of tissue dose arising from the inhalation of air containing radon can be made if, in the absence of complete information on all the factors concerned, some simplifying assumptions are made. The concentration of radon in the atmosphere is regarded as uniform, and it is assumed that the break-down products (radium A, B, C and C^1) are in equilibrium with the radon and are uniformly suspended in the air. The calculation is then made in two parts: (1) for a steady level of radon (plus disintegration

products) in body-tissues via the blood in contact with the radon in alveolar air, and (2) for a steady intake into the lungs of the disintegration products formed in the air.

The solubility of radon in water at 37° C. is 0·17, and in fat the figure is about five times higher. If the concentration of pure radon in alveolar air is C, and is regarded for the moment as free of disintegration products, the concentration of radon in the 50 kg. of aqueous tissue will be 0·17 C and that in the 10 kg. of fatty tissue will be 0·85 C, giving a mean for the whole soft tissues (63 kg.) of 0·27 C. This level of radon in the tissues is maintained, and hence it will maintain its disintegration products in equilibrium with it. Taking the effective disintegration energy of the series as 20 MeV, and assuming an atmospheric radon content of 3×10^{-13} c/l (as above) the energy deposition per g. of tissue corresponds to a dose-rate of only 3×10^{-5} rad/year, mainly of alpha radiation. Using an R.B.E. of 10 to enable this alpha dose-rate to be added to the beta and gamma dose-rates already calculated, the tissue dose-rate for the dissolved radon is 3×10^{-4} rem/year.

The dose-rate from the disintegration products formed in the atmosphere and subsequently inhaled can be calculated on an assessment of the fate of the products retained. If the products are insoluble in body fluids, little if any irradiation of the gonads could occur from inhalation; if soluble, a fraction of the retained products (the retained fraction in lungs and respiratory tract is 75 per cent, I.C.R.P., 1955) would be generally disseminated in the bloodstream. The calculation has been based on the assumption that 20 per cent of the inhaled disintegration products are effective in irradiating general body-tissues, and an exact formula has been used in calculating the equilibrium energy dissipation. The total dose-rate to soft tissues due to the inhalation of air containing 3×10^{-13} c/l of radon plus disintegration products is then 1·9 millirem per year.

The total dose-rate to soft tissues due to the inhalation of air containing 3×10^{-13} c/l of radon plus disintegration products might be expected, therefore, to be about 0·0022 rem/year. At the radon concentration of 3×10^{-12} c/l reported by Anderson *et al.*, (1954) for London air the dose-rate thus calculated would be 0·022 rem/year, i.e. a figure comparable with the cosmic ray background.

Total dose-rate to the gonads

Before summarising the total gonad-dose from all sources the effect of body-shielding on the local gamma ray dose should be considered. Measurements have now been made of this shielding factor by using a water-filled tin model. A thin-walled tube was fixed in the trunk so that a small geiger-counter could be positioned at the site of an ovary. The counter was placed

TABLE 3J

The screening factors for local gamma rays: horizontal, sitting and standing postures

Position of model	Screening factors for local gamma rays			
	Female	Mean	Male	Mean
Horizontal	0·52		0·67	
Sitting	0·58	0·56	0·70	0·70
Standing	0·59		0·72	

Mean factor for both sexes 0·63

outside the trunk to assume representative positions for the testes. Measurements were made in a laboratory (site IIb in Table 1J) where the background was known to be steady. The cosmic ray response of the counter was allowed for by measurements made inside a cubicle shielded by a minimum thickness of 9 in. of steel. The screening factors for horizontal, sitting, and standing postures are given in Table 3J.

The correction for the cosmic ray component inside the building could only be estimated approximately, but if this were in error by as much as ±100 per cent it would produce errors in the screening factor of not more than −8 or +4 per cent. In the male, the factor varies with the position assumed but by not more than ±8 per cent. The average ratio for the two sexes should not be in error, therefore, by as much as ±10 per cent.

Summary of dose-rates to the gonads

Table 4J summarises dose-rates which may be regarded as typical for regions in this country and possibly elsewhere, where the local rock radioactivity is not specially high, and the buildings are of brick or concrete construction not incorporating specially radioactive materials such as granite or granite chips. In arriving at the dose-rates a gonad shielding factor of 0·63 has been assumed, and the local gamma ray dose is averaged for an assumed 8 hours* out-of-doors and 16 hours indoors.

TABLE 4J

Dose-rates to the gonads for a region of 'normal' ground radioactivity

Radiation source	Dose to gonads per year (rad)
External irradiation	
Cosmic rays (sea level)	0·028
Local gamma rays (Leeds, 78 millirad/year indoors ... 48 millirad/year out-of-doors)	0·043
Radon in air, 3×10^{-13}c/l	0·001
Internal irradiation	
Potassium 40	0·020
Carbon 14	0·001
Radon + disintegration products, 3×10^{-13}c/l	0·002
Total dose per year	0·095†
Dose to age 30 years	2·85†

† Includes allowance for the R.B.E. of the alpha radiation where present, and therefore also expresses the gonad-dose in rem.

Table 5J illustrates an attempt to assess the gonad-dose to populations in three different localities and in different types of building. Estimates are given for two radon-in-air concentrations but it is not known whether levels as high as 3×10^{-12} c/l persist for long periods.

It should be noted that higher dose-rates and greater differences between localities could be obtained by taking the extreme values observed in some of the Stockholm and Aberdeen sites, but an attempt has been made to assess as far as possible the conditions affecting large numbers of people. Thus, because even agricultural workers spend probably 8 hours per day or more

* This is a maximum figure and is probably an over-estimate.

TABLE 5J

Assessment of the gonad-dose to populations in three different localities and in different types of building

Location	Type of building	Dose to gonads (rad per year)*	
		Radon 3×10^{-13} c/l	Radon 3×10^{-12} c/l
Leeds	Brick	0·095	0·125
Aberdeen	Granite	0·108	0·138
Stockholm	Wood houses ...	0·095	0·125
	Type 2 houses ...	0·109	0·139
	Type 3 houses ...	0·160	0·190

* Gonad shielding factor, 0·63; exposure, 8 hours out-of-doors and 16 hours indoors.

in a brick house, their gonad-dose is not very different from that calculated for a Leeds town-dweller. The body-shielding factor of 0·63 considerably reduces the difference otherwise apparent in situations of differing local radioactivity.

DOSAGE IN BONE

Consideration should be given to the problem of dosage in bone from natural sources, in order that the significance of the ingestion of bone-seeking radioactive isotopes may be properly assessed. The dose-rate to osteocytes has been estimated, therefore, by a summation of the dose-rate from sources external to the bone and that from radium deposited in the skeleton itself.

Dose from sources external to bone

The dose-rates to osteocytes from the sources considered above will not be very different from the tissue doses already calculated. The dose-rate from potassium 40 will be less, because the potassium content of the bone is only about one-quarter of that for the body as a whole. The dose-rate from inhaled atmospheric radon and its products may also be expected to be greatly reduced, because even if the radioactive content of an osteocyte from these sources were the same as that deduced for soft tissues, the alpha particles would leave only a small fraction of their energy in the osteocyte itself. The reduced dose-rate to osteocytes for the conditions assumed in Table 4J might be put at about 0·08 rem/year.

Dose from radium in bone

The most likely value for the radium content of the skeleton for a region not exceptionally high in radioactivity would appear to be the mean content of $1·2 \times 10^{-10}$ g. measured by Hursh and Gates (1950) for subjects in Rochester, New York, U.S.A., where the radium content of drinking-water is given as $0·6 \times 10^{-16}$ g./cc. If the skeletal radium is proportional to the radium level in drinking-water, the measured level of $1·1 \times 10^{-15}$ g./cc for tap water in St. Ives (Gleuckauf and Jacobi, 1953) would imply a body radium content of $2·2 \times 10^{-9}$ g. Swedish waters are known to have very much higher radium contents, implying body radium contents approaching 10^{-7} g., but measurements of body gamma ray emission by Sievert (1953) do not suggest radium contents of this order. In fact a cross-check in 1953 between Sievert's apparatus and one in Leeds indicated that measured activities of persons not occupationally exposed to radium salts were about the same in Stockholm

and Leeds. These measurements, however, do not exclude small skeletal-radium burdens of the order of 5×10^{-9} g.*

A mean radium-burden of 10^{-10} g. has been taken as typical for a region not specially radioactive, and a mean burden of 10^{-9} g. of radium for an active region like St. Ives. Using methods similar to those given by the author elsewhere (Spiers, 1953) the mean dose-rates to osteocytes have been estimated as in Table 6J.

TABLE 6J

Estimation of the mean dose-rates to osteocytes

Conditions	Ra in skeleton (g.)	Dose-rate to osteocytes (rem/year)		
		Ra	External sources	Total
Not specially active region, e.g. Leeds 3×10^{-13} c/l radon in air ...	10^{-10}	0·037	0·08	0·12
Active region 3×10^{-12} c/l radon in air ...	10^{-9}	0·37	0·18	0·55

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F. W. SPIERS

* Recent work in America and in Germany has confirmed the value for the skeletal-radium burden as about 10^{-10}g., and suggests that burdens greater than 10^{-9}g. are unlikely.

APPENDIX K

The Genetically Significant Radiation Received from Diagnostic Radiology

The total number of X-ray examinations performed per annum

Information under this head relating to the National Health Service has been obtained from the Annual Reports of the Ministry of Health. Other information was supplied by the Services, other Government departments, and various bodies which undertake diagnostic examinations.

Hospitals operating under the National Health Service are responsible for by far the biggest proportion of the X-ray diagnostic work carried out in this country. Total figures for the number of X-ray examinations performed at these centres are available for the years 1951 and 1952; for the years 1953 and 1954 however the only information given is the number of 'units of work done'. Between 1951 and 1952 the number of examinations carried out under the National Health Service (Table 1K) increased by 13·2 per cent, and between 1953 and 1954 by 10·9 per cent. It has, therefore, been assumed that the mean of these figures, 12 per cent, would fairly represent the increases for 1952–53 and 1954–55. The number of examinations carried out in 1954 was estimated by applying to the number of 'units of work done' the ratio derived from the previous year's figures, i.e., that 1 examination equals 1·8566 'units of work done'. The figure for examinations performed in 1955, estimated on the assumption that the trend shown in the previous years continues, is, therefore, approximately 12,200,000.

TABLE 1K

The number of X-ray examinations per year carried out at National Health Service hospitals: 1951–55

Year	Number of 'units of work done'	Number of examinations
1951	—	7,738,389*
1952	—	8,756,643*
1953	18,214,310*	9,810,365†
1954	20,201,177*	10,880,506‡
1955	—	12,189,801†

*From the Annual Reports of the Ministry of Health.
†Obtained by adding 12 per cent to the figure for the previous year (see above).
‡Obtained by applying to the number of 'units of work done' the ratio derived from the previous year's figures, i.e. that 1 examination equals 1·8566 'units of work done'.

This total refers only to National Health Service hospitals. It has been assumed that hospitals outside the Health Service undertake 3 per cent of this number of examinations, i.e., 350,000, and that private medical practice accounts for a further 100,000 examinations. It is further assumed that the distribution between types of examination and sex and age of the patients examined is sufficiently similar to justify the final total of 12,650,000 being treated as a single group.

The gonad dose per examination

The values used in the calculations for radiation dose to the reproductive organs are listed in Table 2K. They are based almost entirely on the work of Stanford and Vance (1955). These workers made careful measurements on more than 1,500 patients, largely at one hospital. For males, the measuring instrument was placed close to the testes; for females, at a point on the skin over the ovaries. A subsidiary experiment on six cadavers gave the ratio of ovary-dose to surface-dose to be expected for the different kinds of X-ray examination; it did not, however, give any indication of the dose received by the reproductive organs of a foetus. Accordingly, where the site of examination is remote from the pelvis of a pregnant woman, the dose to the foetal gonads is taken to be the same as that to the mother's ovaries; where the child is in the direct beam, however, the dose has been estimated from the information given by Stanford and Vance. For salpingography and pelvimetry, the doses used are the lowest that have been published in this country. In the case of pelvimetry, it is assumed that three films are taken in each examination, although many hospitals take more. The dose for salpingography is as reported by Barnett and Bewley (1955) and that for the foetal gonads in pelvimetry by Stanford (1951).

It must be emphasised that the doses quoted in Table 2K are those produced by the techniques and methods of only one hospital; further, this hospital is one where particular care is taken to reduce the gonad doses to the minimum.

TABLE 2K

The dose of radiation received by the gonads in the course of X-ray diagnostic examination of various parts of the body

X-ray examination	Dose (mr) received by the gonads		
	Male	Female	Foetal
Head	0·8	0·2	0·2
Teeth	4·75	0·8	0·8
Shoulder	0·22	0·03	0·03
Arm, hand	0·26	0·05	0·05
Rib, sternum	0·48	0·16	0·16
Chest—large film	0·36	0·07	0·07
„ —mass miniature radiography ...	0·25	0·15	0·15
„ —special*	37	5·4	5·4
Barium swallow and meal	20	9	9
„ enema	40	20	20
Abdomen	69	200	580
Cholecystogram	1·8	15·6	15·6
Pyelogram	486	1,290	3,210
Bladder	279	690	2,610
Pelvis	1,100	210	800
Hip, femur	710	210	800
Leg, foot	3·5	0·6	0·6
Spine—cervical	1·74	0·18	0·18
„ —thoracic	22	15	15
„ —lumbar	129	713	713
Lumbosacral joint	22	220	1,540
Sacro-iliac joint	129	713	2,700
Salpingogram	—	1,700	—
Pelvimetry	—	1,280	2,680

* An average value for bronchography, tomography, etc.

TABLE 3K

The genetically significant radiation resulting from diagnostic radiology: England and Wales, 1955

Examination centres	Type of examination	Males: Examinations as per cent of total*	Males: Dose as per cent of total†	Females: Examinations as per cent of total*	Females: Dose as per cent of total†	Foetal gonads: Dose as per cent of total†
Hospitals ...	Head	3·9	0·1	3·4	n‡	n
	Teeth	0·2	n	0·3	n	n
	Shoulder	0·8	n	0·6	n	n
	Arm and hand	4·8	n	4·3	n	n
	Rib and sternum ...	0·4	n	0·1	n	n
	Chest—large film ...	11·8	0·1	12·5	n	n
	„ —special ...	0·4	0·2	0·8	n	n
	Barium swallow and meal	2·6	0·4	1·6	0·1	n
	„ enema	0·5	0·2	0·7	0·1	n
	Abdomen (including obstetric)	0·6	1·1	1·1	4·0	6·4
	Cholecystography ...	0·2	n	0·4	n	n
	Pyelography	0·7	3·0	0·6	10·7	2·3
	Bladder	0·1	0·1	0·1	0·4	0·4
	Pelvis	0·7	7·4	0·7	2·3	0·7
	Hip, femur	1·4	19·2	1·4	3·8	0·2
	Leg, foot	5·0	0·4	3·9	n	n
	Spine—cervical ...	0·4	n	0·7	n	n
	„ —thoracic ...	0·5	0·1	0·6	0·1	n
	„ —lumbar ...	1·4	2·6	1·4	10·8	0·7
	Salpingography ...	—	—	0·1	1·1	—
	Pelvimetry	—	—	0·1	3·0	15·6
	Total	36·4	34·9	35·4	36·4	26·3
	(Re-allocation of the foetal dose)§ ...		13·5		12·8	
	Total		48·4		49·2	
General Dental Service	All types	1·8	0·1	3·0	n	
Armed Services	„ „	3·4	2·2	0·1	0·1	
Mass Miniature Radiography	„ „	10·8	n	8·3	n	
National Coal Board	„ „	0·2	n	—	—	
Others	„ „	0·5	n	0·1	n	
	Total	53·1	50·7	46·9	49·3	

* *i.e.* of the total of all X-ray diagnostic examinations in England and Wales, 1955.

† *i.e.* of the total of all genetically significant radiation received from X-ray diagnostic examinations in England and Wales, 1955.

‡ n = negligible (i.e. below 0·1 per cent).

§ Allocated between the sexes in the sex-ratio at birth, 1 : 1·059.

Classification of examinations by sex and age of patient and by type of examination

Information as detailed as this is not readily available in many hospitals. It has, however, been obtained for 2 London teaching hospitals, 2 general non-teaching hospitals in the Greater London area, and a children's hospital. In the few cases where appreciable discrepancies exist, weighting was in favour of the non-teaching hospitals. Information on the number of pelvimetry and obstetric abdomen examinations carried out was obtained from 9 hospitals, 5 of them outside London. Apart from these two types of examination, the number of unborn children exposed to radiation was calculated from the number of women of childbearing age examined.

Detailed results

The results of the calculations on the genetically significant radiation received from diagnostic radiology are given in Table 3K. The number of examinations has in each case been expressed as a percentage of 17,650,000, the estimated total number of X-ray examinations of all kinds performed in England and Wales in 1955. The doses received, weighted according to the age of the patients exposed, are expressed as percentages of the total genetically significant dose from diagnostic radiology in England and Wales in 1955.

For comparison, the genetically significant radiation dose from natural radiation has been similarly calculated. Dose rates of 0·10 rad per year for males, and 0·09 rad per year for females have been assumed, and weighted according to the ages of the population as a whole. The total radiation received from diagnostic radiology is found to be 22 per cent of that received from natural radiation by the whole population, and is in addition to it.

Strictly, these figures apply only to England and Wales, but there is no reason to think that the corresponding figures for Scotland and Northern Ireland will be greatly different. Each approximation in the calculation has, however, been estimated on the low side. According to the calculations based on the present sample, therefore, the value of 22 per cent should be regarded as a probable lower limit rather than as an estimate. A realistic estimate of the radiation contribution from diagnostic radiology might be considerably greater than this figure.

A more detailed presentation of the subject than is possible here has been prepared and is to be published shortly (Osborn and Smith, 1956).

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S. B. OSBORN

APPENDIX L

The Contribution of Occupational Exposure to the Genetically Significant Dose of Radiation

In order to calculate the amount of radiation contributed to the total gonad-dose by occupational exposure, it is necessary to know (*a*) the number of persons, distributed by sex and age, who are exposed to radiation in the course of their work, and (*b*) the average gonad-dose received per person exposed. Accurate information on these points is lacking at the present time, and the following estimate is based on the best available data.

An estimate of the average gonad-dose per worker can be made by analysing the data obtained by the radiation monitoring service operated at one time by the National Physical Laboratory and now by the Radiological Protection Service; this service measures the occupational exposure to radiation for workers in the fields of medicine, research and industry, excluding the Atomic Energy Authority. It is a voluntary service and its coverage is incomplete, since many organisations, particularly hospitals, carry out their own monitoring, and there are undoubtedly others where no monitoring is done. However, the average dose recorded by the N.P.L. Service is a low one, and all the available evidence suggests that the doses recorded by the self-monitoring organisations are of a similar order. In these circumstances it has been assumed that the data obtained by the N.P.L. Service are applicable to all workers, except dentists in private practice; analysis of these data gives the figure of approximately 50 mr per week, or about 2·5 r per year, as the average gonad-dose for both males and females in all occupations.

Practically no information is available about private dentists and their assistants, but it is clear that, taking into account the number and type of examinations involved, their exposure risk is very much less than that of radiologists and radiographers; given reasonable care, the average gonad-dose they receive will be considerably lower than the figure of 2·5 r per year just quoted.

Besides those persons actually working with irradiating equipment, there are large groups of people, for example nurses in hospitals, who are exposed to some extent as a result of their work-places being near a source of radiation. These people are not regarded as being radiation workers and are not monitored; their total number however, greatly exceeds that of the radiation workers, and although the average doses are very low, the aggregate dose is undoubtedly significant.

From the Annual Reports of the Ministry of Health, it would appear that there are at present about 9,000 persons, excluding private dentists, occupationally exposed in the medical field; and it is estimated from information supplied by the Factory Department of the Ministry of Labour and National Service that there are about 5,000 persons occupationally exposed in the field of industry and research. This total of 14,000 persons is divided roughly equally between the sexes, and these are the people to whom the figure of 2·5 r per year is considered to apply. Thus, the gonad-dose received per year by this group as a result of occupational exposure, is 17,500 r to each sex. In order to allow for the group of workers exposed at a low level mentioned

above, it is considered advisable to increase this aggregate dose by about 50 per cent, i.e. to 25,000 r per year, for each sex; and a further 1,000 r should be added for each sex to cover the doses likely to be received by those private dentists who possess X-ray equipment, and by their assistants. The final total, which, it will be appreciated, is a very rough estimate, would then be 26,000 r each for males and females.

There is, unfortunately, no precise information about the age-distribution of these groups, and the best assumption that can be made at the present time is that the females are all below the mean age of reproduction (for women, about 28 years) and that the males are evenly distributed between the ages of 18 and 60 (mean age of reproduction about 32 years).

The total number of females below the age of 28 in the United Kingdom is 10×10^6. Each of these women receives a gonad dose from natural radiation of about 0·1 r per year, so that the total dose-contribution from natural radiation to this part of the female population is 10^6 r per year. Hence, at the present time, the occupational dose adds about 2·6 per cent to the genetically significant dose of natural radiation received by the female population.

For males in the United Kingdom, the population is about 5×10^6 between the ages of 18 and 32, and about 9×10^6 between the ages of 32 and 60. Only the dose-contribution for workers aged up to 32 is effective, so that the occupational exposure of genetic importance for male workers is about 9,000 r per year. The total number of males in the United Kingdom below the age of 32 is about 12×10^6, so that this section of the population receives a total dose from natural radiation of $1{\cdot}2 \times 10^6$ r per year. Thus, the occupational dose constitutes an addition of about 0·75 per cent.

Accordingly, in the United Kingdom the occupational contribution to the genetic dose from all sources except the Atomic Energy Authority is 26,000 r per year for females and 9,000 r per year for males, i.e. 35,000 r per year in all. The dose of natural radiation received by females up to the age of 28 is about 10^6 r per year, and by males up to the age of 32, about $1{\cdot}2 \times 10^6$ r per year, making a total of $2{\cdot}2 \times 10^6$ r per year. Hence, on the basis of the assumptions made, the occupational dose adds about 1·6 per cent to the genetic dose received by the population from natural radiation. It is to be noted that the contribution from the Atomic Energy Authority, reported by Farmer (1956), is 0·09 per cent.

I should like to thank Dr. G. H. Aston of the National Physical Laboratory and Mr. K. L. Goodall of the Factory Department, Ministry of Labour and National Service, who supplied some of the information on which the above estimates are based.

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APPENDIX M

The Long-range Fall-out from Nuclear Test Explosions

This appendix summarises the data available on the radiation dose to human beings resulting from the fall-out from distant nuclear explosions. Continuous measurements have been made since 1951 by the Atomic Energy Research Establishment, Harwell, of the deposition on the ground of fission products from distant nuclear explosions. Measurements have also been made of the activity in the air at ground level in the United Kingdom, and on several occasions the variation of activity with height has been explored in the atmosphere up to 50,000 ft. (Stewart, Crooks and Fisher, 1955).

Diffusion and deposition of dust clouds from nuclear explosions

The dust cloud from a weapon in the 'nominal' size-range generally remains within the troposphere, reaching a maximum height of some 25–40,000 ft. when exploded in the middle latitudes. This part of the atmosphere is comparatively turbulent, and as the cloud travels downwind it diffuses both laterally and vertically and contaminates the lower atmosphere across a broad front at great distances. The dust is ultimately removed from the atmosphere by washout in rain-water and by direct deposition on to surfaces. Measurements made in the United Kingdom have shown that the former is the more important process and that the latter can be neglected in comparison; this conclusion however is not necessarily true close to the test site, where gravitational deposition may be an important factor. The United Kingdom measurements have also shown that, on the average, one half of the fine dust from the smaller type of nuclear explosion is removed from the atmosphere by rain-water in a period of 22 days. Deposition of the dust cloud from such explosions is therefore effectively complete within 3 months of the time of burst.

The behaviour of the clouds from explosions in the megaton class is markedly different. These clouds penetrate into the stratosphere, and may reach heights of the order of 100,000 ft. Diffusion is a very slow process in the stratosphere, and material returns to the lowest layers of the atmosphere at a much slower rate than in the case of the smaller type of explosion; a significant fraction of the dust generated may remain in the stratosphere for years after the weapon is exploded. Systematic surveys of the radioactive content of the air at various altitudes, taken in conjunction with fall-out measurements made at the same time, have shown that the dust from these tests was being deposited at a rate of between 10 and 20 per cent per year.

Measurement of airborne dust at ground level

The concentration of radioactive dust in the atmosphere has been measured routinely by drawing measured volumes of air through filters and counting the resultant beta activity on a suitable geiger-counter. Ground-level measurements made at Harwell since 1951 have shown that the activity present in air is of less biological significance than that deposited on the ground. The average level of activity from nuclear explosions has been found to be less than 1 per cent of that due to natural radioactivity in the air, and the maximum level has never exceeded that due to natural radioactivity. The measurements also show that over the past 7 years an individual in the United Kingdom might, on the average, have inhaled a total of $3{\cdot}4 \times 10^4$ dpm of fission products, including $8{\cdot}7 \times 10^3$ dpm of fresh fission-products

(measured at an age of 10 days) and including the particles of relatively high individual activity (Heard, 1956). The remaining $2 \cdot 5 \times 10^4$ dpm consisted of particles of low activity, owing to their low specific-activity or prolonged radioactive decay whilst airborne. For comparison, the continuous occupational permissible breathing-level for strontium 89 dust is $1 \cdot 3 \times 10^6$ dpm *per day* over a working life. Table 1M summarises information on the particles which might have been inhaled, deduced from the detailed 'particle-size and activity' analysis. Only particles in the high and medium specific-activity groups are considered; particles in the lower specific-activity groups are of less individual importance.

TABLE 1M

Estimate of the probable number of radioactive particles in the high and medium specific-activity groups, inhaled by any given individual in the United Kingdom over the past seven years

Particle size (μ)	High specific-activity		Medium specific-activity	
	Probable number inhaled	Total activity per particle* (dpm)	Probable number inhaled	Total activity per particle* (dpm)
1–2	7	$5 \cdot 4 \times 10$		
2–3	1	$2 \cdot 5 \times 10^2$		
3–6	0·5	$1 \cdot 4 \times 10^3$	112	14
6–9	0·07	$6 \cdot 6 \times 10^3$	14	66
9–12	0·01	$1 \cdot 8 \times 10^4$	3	180

* Immediate half-life = 6 days.

The particles carry fission-product beta activity which decays approximately inversely with time, and the activities quoted in Table 1M are those at 10 days after burst. The only significant alpha activity in the particles is that of plutonium. The activity of a 10 μ-particle may vary from about 4 disintegrations *per day* to extremely low levels. The mean concentration of this alpha activity in the ground-level air over the period has been 2×10^{-17} μc per cc compared with the occupational maximum permissible limit of 2×10^{-12} μc per cc (International Commission on Radiological Protection) for breathing insoluble plutonium dust over an occupational lifetime.

Measurements of deposition

Measurements of the deposition of the activity have been carried out at Milford Haven and at Chilton, near Harwell, since 1951, and in New Zealand since February 1955. In the current system, rain-water falling on a 12 × 10 ft. polythene roof is passed through a cylindrical esparto-grass filter and is collected in a tank. The beta activity of the dried filter is measured by mounting it co-axially over a calibrated cylindrical geiger-counter. Samples of the filtered rain-water are evaporated and counted on the same counting system, so that a correction can be made for the solubility of the radioactive material. This measurement is made only periodically, since experience has shown that the solubility of material from any particular bomb-series does not vary significantly with age, and it is possible to use a mean figure.

The daily deposition records at Chilton and Milford Haven are generally similar, although they occasionally differ markedly in detail. All daily deposits

of surface activity greater than 5 mc per sq. mile observed between February, 1951, and December, 1955, have been arranged in groups, and the frequencies of occurrence of the various groups are given in Table 2M.

TABLE 2M

The range of values of all daily deposits of surface activity greater than 5 mc per sq. mile, observed at Chilton and Milford Haven between February, 1951 *and December,* 1955

Range of values of deposited activity (mc per sq. mile)	Frequency of occurrence	
	Chilton	Milford Haven
5– 25	83	76
26– 50	9	5
51–100	3	1
101–150	3	—
151–200	1	—
201–250	—	1

The highest daily deposit at the sites was 190 mc per sq. mile at Chilton and 240 mc per sq. mile at Milford Haven; these occurred about the same time in heavy rain, some 5 days after the explosion of a weapon in Nevada in the autumn of 1951. The highest deposition in a single day from a thermonuclear weapon test was 25 mc per sq. mile at Chilton and 100 mc per sq. mile at Milford Haven.

The dose from deposited radioactivity

The gamma ray dose to human beings from each individual deposition of fission products has been calculated. Little difficulty has been experienced in dealing with the fission products from individual test series in which all the explosions take place within a period of a few weeks, or in interpreting the data when a series of nominal-bomb tests takes place in an atmosphere previously contaminated from thermonuclear tests; the difference in the time-scales of the deposition processes and in the decay-rates of the samples can be used to separate the components. The major difficulty arises when the stratosphere is contaminated with the fission products from thermonuclear tests widely separated in time. If, as we believe, the fine dust in the stratosphere is deposited at a rate of only 10–20 per cent per year, then it can be shown that more than 90 per cent of the integrated dose per generation is due to the single isotope, caesium 137.

The gamma ray dose from each individual deposition of fission products has been calculated, initially for the idealised case of an individual standing on an infinite flat plane. A protection factor of 3 has then been introduced to take account of the material which is washed into drains or is otherwise removed from the topmost layer of the earth's surface; this factor is believed to be conservative, since about one-half of the material already deposited has been found to be soluble in water. An additional factor of 7 has been allowed for the shielding provided by buildings* against the gamma rays from fission products; this figure has been derived from measurements carried out

* It is perhaps noteworthy that the reduction in dose from this cause is much less than the enhanced dose received from the gamma radiation from the natural radioactivity of the building materials (see Appendix J).

at the Atomic Energy Research Establishment with the gamma rays from cobalt 60, and is based on the assumption that the average individual spends $2\frac{1}{2}$ hours daily out-of-doors.

The individual doses have been summed, and the total external dose to be received by the average inhabitant of the United Kingdom due to material deposited on the ground from bombs exploded before 31st December, 1955, is estimated to be 1·7 mr. About 75 per cent of this dose is associated with material which has yet to be deposited; the dose due to bomb dust suspended in the air near ground level is negligible by comparison. It has also been estimated that if the various types of weapon continue to be fired at the present rates for an indefinite period, the ultimate dose per individual per generation of 30 years will be 26 mr; this level will be reached in approximately 100 years.

Very sensitive methods of measuring the radioactivity of the human body have been developed in recent years, and there has been some indication, on the records obtained in recent months, of the presence of a 0·6 MeV gamma ray, which is suspected to be due to the fission-product caesium 137 arising from the fall-out; it is not yet possible, however, to identify this radiation rigorously. The radiation has been observed qualitatively on body-monitoring records obtained at Harwell, and has been reported in some detail in progress reports from the Argonne National Laboratory in the United States. The highest body-activity detected so far in the United States is found to be 4×10^{-3} μc. This activity would, if maintained, produce a total-body irradiation of 0·6 millirad per year, or about 1/30 of the dose due to the naturally-occurring isotope potassium 40 in the body. The caesium activity in the body may be expected to fluctuate in step with the rate of fall-out, if the biological half-life is about 20 days as suggested by the International Commission on Radiological Protection, and if this activity in human bones arises from the direct contamination of herbage; in this respect the caesium differs markedly from the strontium 90, which is cumulative. From the quantities expected in the fall-out and from the chemical and metabolic properties of this isotope, the caesium 137 in the human body due to uptake from food and water is unlikely, on the basis of present information, materially to affect the dose, calculated above, from the fission products deposited on the ground.

The accumulation in the human body of strontium 90 from fall-out

The rate of deposition on the ground of strontium 90 in the fall-out has been measured by radiochemical analysis and, since the spring of 1954, has been found to be approximately 6 mc per sq. mile, per year. The total at 31st December, 1955, was 11 mc per sq. mile. From the measurements of activity in the upper air, it is anticipated that the total from bombs already fired will rise in about 10 years to a maximum of approximately 45 mc per sq. mile. Should the various types of explosion continue at the present rates, the accumulated deposition is likely to rise to an equilibrium value of about 500 mc per sq. mile in about 100 years' time.

Because of its similarity to calcium, strontium is found to follow this element in the human food-chain. Measurements have accordingly been made of the strontium 90 activity per g. of calcium in samples of vegetation and soil from a number of locations in the United Kingdom (Bryant and Chamberlain, 1956). Using refined radiochemical techniques and anti-coincidence counting procedures, the activity per g. of calcium has also been measured in the bones of yearling sheep, in samples of milk, and in human bones. The representative figure for the strontium 90 activity of vegetation in the autumn of 1955 was about 35 μμc per g. of calcium. The corresponding representative

figure for sheep bones was 14 μμc per g. of calcium but in a few mountain pasture areas, values up to ten times higher have been recorded.

The maximum activity found so far in the limited number of human bone-samples in the United Kingdom is 1·2 μμc per g. of calcium in the skeletons of one-year-old children. Adult bones show lower activities, ranging from 0·05 to 0·2 μμc per g. of calcium. These levels may be compared with the maximum permissible level for strontium 90 in the body for occupational workers, recommended by the International Commission on Radiological Protection, which corresponds to about 1,000 μμc per g. of calcium. The average radiation dose to the bone from a level of 1 μμc per g. of calcium would be about 3 millirem per year, and may be compared with the dose to the bone from the background level of radium in the average person in this country (10^{-10}c), which is about 37 millirem per year, and from natural gamma radiation, which contributes about a further 80 millirem to the bone (see Appendix J).

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W. G. MARLEY

APPENDIX N

An Attempt to Estimate the Hazard from the Ingestion of Strontium 90

During the last fifty years, information has been accumulating about the induction of cancer in man and various species of animals by ionizing radiation and radioactive substances. Much of the evidence is qualitative and we have still very little reliable quantitative information on which to assess hazards.

The main risk from the absorption of bone-seeking isotopes is the delayed production of changes in bone, sometimes followed by osteogenic sarcoma, as well as blood changes consequent upon effects in the bone marrow.

The best human evidence is from the clinical investigation of patients to whom radium salts were administered for therapeutic purposes some twenty-five to thirty years ago, supplemented by earlier studies on industrial workers including those who ingested radioactive luminous paint. One of the most characteristic features of the production of malignant tumours of all kinds by ionizing radiations and radioactive materials is the very long latent period, often in the region of twenty years.

From a consideration of all the available evidence, the maximum permissible level of radium permanently incorporated into the skeleton has been fixed by the International Commission on Radiological Protection for persons occupationally exposed as 0·1 microcurie. From a comparison of the toxic effects of strontium 90 as compared to radium in animal experiments, the corresponding maximum permissible level for strontium 90 has been fixed at 1·0 microcurie. It must be appreciated that the biological effects of the radium and its breakdown products are probably largely due to alpha particles, whereas strontium 90 with its associated yttrium 90 emits only beta radiation. These differences in the energies and nature of the radiations, as well as differences in the patterns of distribution of the radioactive materials in the bone itself, probably cause the differences in biological effects. Strontium behaves chemically very similarly to calcium in bone formation and it is therefore natural to associate the two elements. The maximum permissible level of strontium 90 in the human skeleton corresponds to 1,000 micro-microcuries of strontium 90 per gramme of calcium in the skeleton, and it is this concentration which determines the dose level.

However, this maximum permissible level has been fixed for a group of adults educated in relation to the risks, under medical supervision, and working under carefully controlled conditions.

It is well-known that growing bone takes up more of the bone-seeking isotopes such as strontium 90, and concentrates them in the rapidly growing portions of the bone. It is also well known that rapidly growing tissues, such as those of children, are often particularly radiosensitive. We must also conclude from the available evidence that the damage produced by radioactive materials in bone is an integrated effect over the whole time and dose of the radiation. Since the radioactive half-life of strontium 90 is long (28 years), any material incorporated during childhood has a longer time to act than material taken up in later life. The danger is a little mitigated by the fact that radioactive strontium may not be retained in bone for as

long as radium. The biologically effective half-life of strontium 90 together with its daughter product yttrium 90 has been estimated by the International Commission on Radiological Protection as 2,700 days.

Another factor which suggests caution is that it is well-known that the irradiation of tissues in inflammatory conditions is more likely to induce tumour formation than the irradiation of normal tissues, and such conditions are more likely to occur in the whole population than in the specially selected occupational group.

It is difficult to fix precisely the lowest level at which tumour formation and other effects have occurred, owing to the fact that in many instances the material ingested by the worker or patient has been an unknown mixture of mesothorium and radium, and this uncertainty complicates the estimation of dose since the rates of decay of these substances are very different (mesothorium 6·7 years half-life and radium 1600 years).

Examination of the results of ingestion of radium by humans makes it clear that in the fixing of the maximum permissible level there is no great safety factor involved. A suggestive destructive lesion (in the dentine of the teeth) has been observed in a patient who carried a body burden of approximately 0·15 microcurie of 'radium' (probably a mixture of radium and mesothorium). Of 44 patients investigated by Looney *et al.*, 36 had body burdens of 0·4 microcurie or more; clinically recognisable, but not malignant, bone lesions were observed in 32 of these. The lowest level of pure radium producing a bone sarcoma in this series was 3·6 microcurie. Sarcomata were seen in five other patients included in this study, but these patients may well have ingested a mixture of radium and mesothorium. Among these five, the lowest level associated with the production of a tumour was 0·52 microcurie of 'radium' (mixture).

If we assume, in accordance with the agreed international recommendations, that 1 microcurie of strontium 90 carries the same risk as 0·1 microcurie of radium, then with all these considerations in mind, it would be unwise to fix the maximum allowable concentration of radioactive strontium in the bones of the general population, with its proportion of young children, at more than one tenth of the level agreed for occupationally exposed persons. That is, the maximum allowable concentration should not be more than 100 micro-microcuries of strontium 90 per gramme of calcium.

As we consider the possible effects which might be produced at lower dosage levels (that is, below one tenth of the maximum permissible level) our ignorance increases still further and we can only rely upon extrapolation from limited animal experience. It is still not possible to give a certain quantitative answer to the question of the relationship between dose and the frequency of bone changes, induction of tumours, and other effects of the radiation. At very low dose levels the incidence is so small in animals that the existence of a threshold below which no effect occurs has been postulated.

It appears however that each unit quantity of radiostrontium absorbed by the bone confers a certain probability of bone-tumour formation, the tumour development time perhaps decreasing and the tumour incidence increasing with the dose. On the whole, the experiments seem in favour of a proportionality between the frequency of tumours produced in a given length of time and the amount of radioactive material in the body even at low dose levels.

If we again assume, in accordance with the agreed international recommendations, that one microcurie of strontium 90 carries the same risk as

0·1 microcurie of radium, and attempt in this way to estimate the effects at one-hundredth of the maximum permissible level, we provisionally conclude that the effects are unlikely to be detectable. Nevertheless, if the concentration in human bones showed signs of rising greatly beyond one-hundredth of that corresponding to the maximum permissible occupational level it would indicate the need for immediate consideration of the problem.

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ATOMIC SCIENTISTS ASSOCIATION

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STATEMENT ON STRONTIUM HAZARDS

In view of the widespread concern about radiation hazards, the Council of the Atomic Scientists' Association decided to appoint a committee to study the whole problem, and to follow up the implications of last year's report of the Medical Research Council in the light of more recent information. There is particular concern at the present time about the effect of the radioactive strontium produced in H-bomb tests. The Council has, therefore, requested the committee to prepare a public statement giving its current assessment of this hazard.

The report of this committee (whose membership is given below) is as follows:

When an H-bomb is exploded at a high altitude, and in an area in which there are no inhabitants—as is said to be the case with the forthcoming British tests—there is very little likelihood of immediate injury to people, or of substantial local contamination of the oceans, even if a large amount of radioactivity is produced in the explosion. This radioactivity would be taken up into the upper regions of the atmosphere from where it would spread all over the globe and gradually descend to the ground over a period of some years. By that time only the long-lived radioactive products of the bomb would remain, and it is with the possible effects of these that we are concerned.

If H-bomb tests continue at the present rate, the dose of radiation to the reproductive organs, which may cause damage to future generations, has been estimated in the M. R. C. report to be of the order of 1% of that resulting from the natural level of radiations. Of greater import, however, is the damage which may result to the present generation, mainly from one radioactive substance—strontium-90. This substance enters into our food, chiefly in vegetables and dairy products, and it accumulates in the human body in the bones where it remains for a long time. Depending on the assumptions made about the distribution of strontium in bone, we calculate (see appendix) that by the year 1970 the radiation dose to bone from all the tests carried out up to the autumn of 1956 will range from 9% to 45% of the dose received from all natural sources, including the radium which is normally present in bone.

It is known that radioactive substances concentrated in the bone may give rise to bone cancers and other damage, and that the irradiation of bone marrow may result in leukemia, a type of cancer of the blood. The induction of bone cancers by the action of strontium-90 in the bone has been demonstrated in animals; in human beings the same effect has been observed with radium, which in some ways behaves like strontium. In all these cases, however, the amounts of radioactivity present in the bone were far greater than those that are likely to accrue from H-bomb tests. The question then arises how to apply these findings to very small doses. There is here a fundamental difficulty, in that the relationship between the damage produced and the amount of radiation is not known. If this relationship is such that there exists a threshold dose below which cancer cannot be induced, then it can reasonably be inferred that the small amount of strontium-90 which will accumulate in bone from the current H-bomb tests would not result in any harm. If, however, the number of additional bone tumours resulting from radiation is directly proportional to the dose, then even a very small dose will give rise to a small but definite probability of bone cancer. This means that in a very large population a certain number of people would contract this disease as the result of their having a small amount of strontium-90 in their bones.

The evidence is as yet inconclusive. Some animal experiments have been interpreted as indicating the existence of a threshold dose. On the other hand, in man, the occurrence of leukaemia caused by radiation suggests a simple proportional relationship. Unfortunately the question cannot be settled by experiment in a short period of time, nor is there strong guidance from theory. There is one theory of the origin of cancer (that it is due to somatic point mutation) which

implies a proportional relationship. Where the effects of strontium-90 are concerned the authorities in the M. R. C. report appear to be inclined towards the proportional hypothesis, for they state "On the whole the experiments seem in favour of a proportionality between the frequency of tumours produced in a given length of time and the amount of radioactive material in the body even at low dose levels."

If the proportional relationship is accepted, it is then possible to make a rough estimate of the number of bone cancers which may result from a given H–bomb test. The calculations given in the appendix show that an H-bomb of the type tested at Bikini in 1954, if exploded high in the atmosphere, may eventually produce bone cancers in 1,000 people for every million tons of T. N. T. of equivalent explosive power. (It has been stated that bombs hitherto exploded were equivalent, in aggregate, to 50 million tons, insofar as their strontium-90 fall-out is concerned.) These thousand casualties would be spread all over the world and occur in the course of several decades. A somewhat larger number of people might suffer other bone changes, possibly without manifesting any clinical symptoms. There is also the probability of a number of cases of leukaemia resulting, but we have not enough data to estimate this number. At the same time it should be pointed out that these casualties, although large in absolute number, represent only a fraction of those due to the natural level of radioactivity; there would be no way of distinguishing one from the other.

If other types of nuclear weapons were exploded, the number of casualties would vary in direct relation to the amount of fission products released into the upper atmosphere.

In giving these estimates it must be emphasized again that, apart from the considerable margin of error due to lack of adequate data, they are based on the as yet unproved hypothesis of a proportional relationship applying to very small doses. From this point of view they represent the most pessimistic approach. On the other hand, if this hypothesis is correct, then the figures may be an underestimate of the damage since they do not allow for the radiation dose in children before or after birth. Children are known to take up much larger quantities of strontium than adults and the likelihood of producing radiation damage in them is probably much greater for the same amount of radiation.

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APPENDIX

Estimate of strontium-90 hazard based on an assumed linear relationship between bone tumour formation and dose

1. The measurements of the ^{90}Sr content in human bone in this country, carried out by workers of the U. K. A. E. A.[1] show that by the beginning of 1956 the average amount in all age groups from 10 years upwards, was about 0.2 S. U. (1 S. U.=10^{-12} curies of ^{90}Sr per gramme of calcium).

2. If this figure is applied to the data of the world survey of strontium-90 made by the Lamont group,[2] we conclude that the ^{90}Sr concentration resulting from all the tests carried out up to the autumn of 1956 will in 1970 amount to about 4 S. U. This value is in agreement with the estimate of 4–10 S. U. made by Libby [3] for the U. S. A.

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[1] R. J. Bryant, A. C. Chamberlain, A. Morgan, G. S. Spicer. A. E. R. E. HP/R 2056, 1956.
[2] J. L. Kulp, W. R. Eckelmann, A. R. Schulert. Science, 125, 219, 1957.
[3] W. F. Libby. Proc. Nat. Acad. Sci., 42, 945, 1956.

3. The total fission yield from the tests carried out up to the autumn of 1956 is estimated[2] to be equivalent to 50 megatons. For one nominal high-yield weapon (as defined in the Indian Government's document:[4] 20 megatons with high fission yield) exploded high in the atmosphere, one may therefore expect a concentration in bone of 1.6 S. U.

4. From Hasterlik's survey[5] of bone sarcoma in people with a radium burden, we estimate that 0.1 microcurie of radium yields about 0.5% probability of bone sarcoma. (A factor of 2 has been allowed for the possible admixture of mesotherium with the radium.)

5. Since 0.1 microcurie of radium is accepted[6] to be equivalent to 1,000 S. U., applying this probability value to the population of the whole world (2.5×10^9 people) we obtain that the number of bone sarcomas from one nominal high-yield nuclear test may amount to 20,000 all over the world ($2.5\times10^9\times0.5\times10^{-2}\times1.6\times10^{-3}$).

6. The radium normally contained in the body and the external radiation from natural sources deliver to the bone a dose equivalent to 45 S. U., or 9 S. U. if one allows for the non-uniform cohcenaration of strontium in bone (Spiers[7]). This means that the dose rate from strontium in bone will amount to 4/45 or 1/9 of that due to the natural background.

[British Journal of Radiology, v. 29, August 1956: 409–417]

Radioactivity in Man and His Environment

Presidential address[1] by F. W. Spiers, D. Sc., Department of Medical Physics, The University of Leeds

A tradition, upheld almost without exception in the long history of the Röntgen Society and the British Institute of Radiology, is the delivery of an address to the Institute by the President during his term of office.

I am very conscious of the honour that falls to me to address you as President and very much aware of the responsibility of following my distinguished predecessors in the fulfilment of this obligation. In recent years, by a change of Articles of Association, the Institute has re-emphasised a characteristic that has always been implied by the nature of its membership—a wide interest in all that pertains to the science of radiation and radioactivity. Many presidential addresses given in this house bear witness to the activities and interest which spring from the Institute's constitution and to the bonds of cooperation and of service to radiology which exist between its medical and non-medical members.

It seemed appropriate, therefore, that I should speak on this occasion about radioactivity in man and his environment and give thereby a practical example of the width of interests encompassed by the Institute. Moreover, the subject I have chosen is of interest and importance to all who use ionizing radiation and is one which, although having significance in the problems of today, has also the historic interest of belonging to the scientific discoveries of the early years of this century.

The experiments of Elster and Geitel and of C. T. R. Wilson in the year 1900 first revealed the presence of a residual ionization in an electroscope and subsequent work by many investigators in the next few years showed that this ionization arose from radioactivity in the materials of which the electroscope was made, from terrestrial sources outside the electroscope and from penetrating radiation of cosmic origin. It was soon established that traces of the known radioactive elements were widely dispersed throughout the lithosphere and that few raw substances or even few metals were free of radioactivity in trace amounts. Man, it appeared, lived in a radioactive world and eventually it was to be found that, like his surroundings, man himself was also radioactive. It is certain that man has been exposed to ionizing radiations throughout his occupancy of this planet and it seems at least likely that in geologically recent times, no very great changes have occurred in the radiation intensity of his environment.

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[4] Nuclear Explosions, Government of India, 1956.
[5] R. J. Hasterlik. Geneva Conference, Vol. II, p. 149.
[6] Recommendations of Int. Comm. Rad. Protect. B. J. R. Suppl. No. 6, 1955.
[7] F. W. Spiers. Brit. J. Radiol., 29, 409, 1956.
[1] Delivered at the British Institute of Radiology on May 24, 1956.

During this century, however, the situation has undoubtedly changed, for man now produces ionizing radiations and artificial radioactivities in abundance and turns them to his own great technical advantage. It is possible, perhaps likely, that the increase now taking place in man's radiation background will be greater than the other possible changes which have so far occurred, when for example ancestral man left his arboreal home for the cave, or later even, the Englishman entered his castle! It would seem fitting to put on record in this Institute an estimate of man's natural radiation dose, a quantity which members, by their professional activities, are bound in some measure to augment.

The radiation dose received naturally by tissues of the human body arises in two days: from external sources which include terrestrial radioactivity and cosmic rays, and from internal radioactivity acquired by the body from air, food, and water. The biological significance of this background radiation dose is properly a consideration for the biologist, but any physical analysis should be oriented to give the data required for a biological assessment. Because the background dose is in any case small compared with doses known to cause immediate somatic change, long-term genetic and carcinogenic actions may be presumed to be the likely considerations. The radioactivity data will be used, therefore, to derive the dose to human gonads, and the dose to osteocytes in bone. The dose to soft tissues in the body will not differ greatlyy from that of the gonads, with the exception of lung tissue where recent work by Hultqvist (1956) will be used to indicate the possible lung dose from the inhalation of radon, thoron, and their decay products. Because α rays and β rays are so easily absorbed, even by the elements of low atomic number comprising soft tissues, the dose to the gonads from external sources may be considered to be due entirely to cosmic rays and γ rays. The dose from internally acquired radioactivity, however, arises to a large extent from β rays and, when present, from α rays.

RADIOACTIVITY IN MAN'S ENVIRONMENT

1. Surface rocks and oceans

The radio-elements which contribute significantly to the terrestrial γ radiation are the members of the U and Th series together with the naturally occurring radioactive isotope of potassium, ^{40}K. The actinium series, ^{87}Rb, ^{147}Sm, ^{14}C and a number of radioactive isotopes, found in recent years to occur naturally, do not contribute significantly to the γ-ray background, either because of the character of their energy emission, or because they are insufficiently abundant. The radioactive constituents of some rocks listed in Table I show that for an "average" granite the γ-ray emission is contributed in roughly similar degree by the elements of the uranium and thorium series and by potassium 40. Sedimentary rocks are considerably less radioactive than granite with limestone least of those listed. The alum shale is given as an example of an exceptionally high rock radioactivity in Sweden, its excess activity being due entirely to its very high uranium content. Sea water has a low content both of uranium and potassium and furthermore exhibits a very low γ-ray equivalent radioactivity because, in deep water, the thorium isotopes, including ionium (^{230}Th), the precursor of ^{226}Ra, are almost completely removed by sedimentation following adsorption on ferric and manganic hydroxide while the uranium remains largely in solution (Rankama, 1954). The equivalent radium content of sea water is about 100 times lower, and river water some 10,000 times lower, than that of sedimentary rocks.

TABLE I.—*Radioactivity in rocks and water*[1]

Material	Radio-elements per g	γ-ray equivalent g Ra per g$\times 10^{12}$	
		Components	Total
Granite (average)	U: 4 μg Th: 13 μg K: 30 mg	1·44 2·25 2·40	6·1
Granite (alps)			12·4
Clays (Yorkshire)			4·4
Sandstone (Yorkshire)			2·0
Limestone			1·4
Brick			5
Alum shales (Sweden)	U: 170 μg Th: 1·5 μg K: 35 mg	60 0·3 2·8	63
Sea water	U: $1\cdot3\times10^{-9}$ g K: 0·35 mg	7×10^{-5} 28×10^{-3}	$2\cdot8\times10^{-2}$
Thames			$\sim10^{-4}$

[1] Data mainly from Libby (1955) and Hultqvist (1956).

TABLE II.—*Radioactivity in air*[1]

Situation	Radon content 10^{13}c/l	Thoron content 10^{13}c/l
Open air:		
London	1 to 3	
Rothamstead	0·3	
Innsbruck	4·3	
Over oceans	0·01	
In houses:		
Sweden:		
Wood	5·3	0·28
Brick	9·1	0·91
Shale-concrete	18·6	0·96
Britain:		
Laboratory	0·8	
Cellar	7·8	
Air-raid shelter	118	
Joachimstal mines	30,000	

[1] Data mainly from Hultqvist (1956).

2. *Air*

The radon and thoron contents of air (Table II) depend on the escape of these gases from the ground and on the prevailing atmospheric conditions; stagnant and dusty air will retain the gases and their decay products, wind and rain will clear the air of radioactivity. The low value over oceans reflects the low concentration of the parent elements radium and thorium in sea water.

The value of 3×10^{13} c/l is often taken as representative of the radon content of air although values ten times higher have been reported by Anderson, Mayneord and Turner (1954) on some days in central London. The radon and thoron levels in houses depend very much on ventilation as well as on the building materials used.

3. *Drinking water and foods*

Table III lists values of the radium content in some drinking waters and foods according to some recent measurements by Hursh (1956) and Muth (1956). The radioactivity of tap water depends to a considerable extent on the purifying processes, being reduced by large factors where precipitation methods are used. High radioactivity is found in mineral waters and can occur in rural areas where water is taken from wells. Some values for the radium content of foods are given although no large survey of foods has so far been undertaken.

RADIOACTIVITY OF THE HUMAN BODY

Only in comparatively recent years has attention been given to the determination of the natural radioactivity of the body. Values of the total body radium content as high as 7.5 to 14×10^{9} g Ra, reported by Rajewsky (1941) and by Krebs (1942), have not been found in modern measurements of radium in cremation ashes. The recently obtained values given in Tables IV, varying from 0.05 to 0.32×10^{9} g Ra, show reasonable agreement between measurements made by different methods in different parts of the world. There is only a slight correlation of radium content and drinking-water activity and it seems likely that, as the data in Table III suggest, the major route for radium intake is through the radium content of food. The radium contents of soft tissues have been reported so far only by Muth (1956) and are included only tentatively, in order to assess the significance of the figure for the testis in a later calculation of the gonad dose.

TABLE III.—*Radioactivity in water and foods* [1]

Source	*Radium content 10^{16} g per g*
Drinking waters:	
London	<1
America:	
Cities	0·1 to 1·4
Rural areas	0·3 to 23
Germany:	
City	1·4 to 3·1
Mineral waters	130 to 240
Thermal springs	~1100
Foods:	
Milk	0·04 to 2·7
Cereals	10 to 39
Potatoes	67 to 125
Meat	80

[1] Data from Hursh (1956) and Muth (1956).

TABLE IV.—*Radioactivity of the body* [1]

Radio-element	Ra in tap water g/c.c. 10^{16}	Body radioactivity in in $c\times10^{9}$
Potassium 40		13
Carbon 14		87
Radium (1)	0·01	0·047
(2)	0·36	0·12
(3)		0·14
(4)	1·4 to 3·1	0·32
(5)	34	0·24
Radium in bone		10^{14} gRa/g
Radium in soft tissues		1·4 to $4·6\times10^{15}$
Radium in tests		$0·6\times10^{15}$

[1] Data mainly from Hursh (1956) and Muth (1956).

The highest radioactivity in the body expressed as curies is that due to ^{14}C, the amount in Table IV being calculated from the measured specific activity of carbon of biological origin, given by Anderson and Libby (1951) as 15.3 dis min g C. The β-ray energy of ^{14}C is so low (54 kc V), however, that the dose rate to body tissues from ^{14}C is almost negligible compared with that from potassium and radium.

The potassium content of the body is now reasonably well established, at about 0.2 percent of body weight, by measurements of body γ-ray emission by Sievert (1955), Burch and Spiers (1953), and Rundo (1955). Males show higher values than females but the differences are related to the fat content of the body which contains no potassium. Anderson (1956) has shown that γ-ray measurements of total body potassium correlate well with lean body mass, as determined by measurements of body water by the tritium method.

THE EXTERNAL RADIATION BACKGROUND

1. Local γ-radiation

In most circumstances the local rock radioactivity is responsible for the major part of the external radiation background. Calculations of the dose-rate over different types of rock have been made by Libby (1955) and by Hultqvist (1956) and estimates, based on formulae given by the latter, are shown in Table V for the surface radioactivities listed in Table I. The dose-rates are of the same order as those given by Libby and accord with such direct observations as have so far been made.

The dose-rates inside houses and buildings are greater than the corresponding open-air values because the building materials are disposed to give a nearly isotropic irradiation. Some measurements reported by Sievert and Hultqvist (1952) and by Hultqvist (1956) in open-air and in houses in Sweden are summarized in Table VI, where the original values in ions per second have been corrected for the cosmic ray component and then converted to dose-rates in mrad/year. The values given are the means of observations in some 1500 apartments and houses; in each class of building dose-rates were found, exceptionally, up to twice the mean. Measurements made recently in Leeds and Aberdeen (Spiers and Griffith, 1956) are given in Table VII where the observations fall into two groups, those in granite buildings in Aberdeen and those in brick or concrete buildings in both cities. The dose-rates are lower than those reported by Sievert and Hultqvist but, with the exception of the Swedish alum-shale buildings, the difference for brick dwellings in the two countries are not very great. Open-air dose-rates in Leeds and over clay and in wooden houses in Sweden are about the same and only about half the dose-rates in brick dwellings.

TABLE V.—*Calculated dose-rates over flat ground*

Situation	*Local γ-ray dose-rate (mrad/year)*
Granite (average)	95
Granite (alps)	228
Clays (Yorkshire)	82
Sandstone (Yorkshire)	37
Limestone	26
Alum shales (Sweden)	1150
Sea	0. 5
Thames	0. 001

TABLE VI.—*Measured dose-rates in Swedish houses*

Building material	*γ-ray dose-rate at room centre (mrad/year)*
Wood	50
Brick	104
Concrete with alum-shale	171
Stockholm streets	85
Over igneous rocks	60 to 120
Over clay	50

2. Cosmic radiation

The dose-rate given by Libby (1955) for cosmic radiation is 35 mrad/year and appears to be based on an ionization intensity at sea level of approximately 2.5 ion pairs/c.c/sec. Recent work by Burch (1954) on the interpretation of high-pressure ionization chamber measurements supports the analysis given by Clay and his colleagues (1938) and leads to a lower value of the cosmic radiation ionization. The mean of three values given by these workers, corrected to ionization in free air, is 1.92 ions/c.c./sec. which converts to a dose-rate of 28 mrad/year at sea level and geomagnetic latitudes above 41° N.

This dose-rate applies to all bodily tissues. It may, however, be some 20 percent less in the basements of tall buildings which absorb the soft cosmic ray component and it will increase rapidly with altitude. At 10,000 and 15,000 feet the cosmic ray ionization is known to increase to about three and six times the sea-level rate; but because cosmic radiation is responsible for only a fraction of the total background dose-rate, the total tissue dose-rates are increased only by factors of the order of 1.6 and 2.3 respectively.

It is possible that greater significance attaches to the increase in the heavy particle component of cosmic radiation with altitude, thereby changing the relative biological efficiency (R. B. E.) of the radiation for some biological actions. Some mutations, for example, are reported as being less effectively produced by heavily ionizing particles than by X-rays; on the other hand, in a few cases the R. B. E. for heavy particles has been reported to be in the region of 5. At sea level, however, the contribution to the total ionization by slow particles and the effect of an associated increase in R. B. E. for this component would appear to be unimportant.

TABLE VII.—*Measured dose-rates from local radioactivity*

Type of building	Location	Local γ-ray dose-rate [1]
		mrad/year
1. All granite	(a) Aberdeen—laboratory	107
	(b) Aberdeen—in bell tower	99
	(c) Aberdeen—entrance hall	101
2. Brick or concrete	(a) Aberdeen, rooms on various floors	73
	(b) Leeds, room in hospital building	81
	(c) Leeds, single storey laboratory	80
	(d) Leeds, various rooms in house	77
3. Out-of-doors	Leeds, in garden of house 2(d)	48

[1] Mean values given for centres of rooms and at 1 m above floor.

3. Atmospheric radioactivity

Radiation from the decay products of radon in the air can add to the external γ-ray dose, although the effect may be expected to be small and very dependent upon atmospheric conditions which determine the degree of radioactive equilibrium. Peirson and Franklin (1951) have calculated that at ground level an atmospheric radon content of 3×10^{-13} c/l produces a γ-ray flux of the order of 10 quanta/cm^2/min. Taking a mean energy for Ra B and C γ rays, the tissue dose rate for this flux is 2.2 mrad/year. The formula used by Hultqvist (1956), which includes a factor for scattered quanta, gives a dose-rate, for the same radon level, of approximately 4 mrad/year and this figure will be used in subsequent calculations. Under most conditions this contribution to the background dose-rate is trivial but for the higher levels of atmospheric radon given in Table II (or for the higest value, 2 to 3×10^{-12} c/l., reported by Anderson, Mayneord and Turner, 1954, for London air), the γ-ray dose could be of the same order as that due to cosmis rays.

Calculations of dose-rates due to thoron in air lead to values of the same order of magnitude as those for similar levels of radon activity. The dose-rates for the thoron levels given in Table II are from 0.6 to 2 mrad/year. Apart from Hultqvist's data for Swedish houses, however, no information is available on the general level of thoron in air and no allowance for thoron has been included in the subsequent tabulation of dose-rates.

THE INTERNAL RADIATION BACKGROUND

1. Potassium.

The potassium content of the body provides the chief source of internal radiation to soft tissues. Taking the average tissue content of potassium to be 0.2 per cent and using Sawyer and Wiedenbeck's (1950) constants for the radioactivity of ^{40}K as 28.3 β particles per second per g K of mean energy 0.605 MeV and 3.6 γ quanta per second per g K of energy 1.46 MeV, the tissue dose can be calculated to be:

Tissue dose rate due to K=18 (β)+2 (γ)=20 mrad/year.

Because the mean range of the β particles of ^{40}K is only some 2 mm in tissue the dose-rate in a given organ is determined mainly by its own potassium content. With regard to the gonad dose, flamephotometer measurements on testes and ovaries taken from two post-mortem examinations gave a mean potassium content of 0.20±0.02 per cent. Although observations have been made so far on only four specimens, the results suggest that the mean content assumed in the ^{40}K calculation is sufficiently representative of gonad tissue.

2. Carbon 14

The specific activity of ^{14}C in carbon of biological origin is 15.3 β particles/min/g C (Anderson and Libby, 1951) with a mean β-ray energy of 0.15 MeV. Taking the carbon content of tissue as 18 per cent, the energy deposition due to ^{14}C amounts to a tissue dose-rate of only 1 mrad/year.

3. Radon and its disintegration products in air

An estimation of tissue dose arising from the inhalation of air containing radon can be made if, in the absence of complete information on all the factors concerned, some simplifying assumptions are made. The concentration of radon in the atmosphere is regarded as uniform and it is assumed that the breakdown products (RaA, RaB, RaC and RaC′) are in equilibrium with the radon and are uniformly suspended in the air, a condition holding probably only in still air. The calculation is then made in two parts: (1) for a steady level of radon (plus disintegration products) in body tissues *via* the blood in contact with the radon in alveolar air, and (2) for a steady intake into the lungs of the disintegration products formed in the external air.

The solubility of radon in water at 37° C. is 0.17 and in fat the figure is about five times higher. If the concentration of pure radon in alveolar air is R, the concentration of radon in the 50 kg of aqueous tissue will be 0.17R and that in the 10 kg of fatty tissue will be 0.85 R, giving a mean for the whole soft tissues of 0.27 R. This level of radon in the tissues is maintained and hence it will maintain its disintegration products in equilibrium with it. Taking the effective disintegration energy of the series Rn RaA . . . RaC′ as MeV and assuming an atmospheric radon content of 3×10^{-13} c/l, the energy deposition per gram of tissue corresponds to a dose rate of only 0.03 mrad/year, mainly from α radiation. Using an R.B.E. of 10, to enable α-ray dose-rate to be added to the β and γ-ray dose-rates already calculated, the mean tissue dose-rate for the dissolved radon is 0.3 mrad/year.

If the disintegration products are present in equilibrium with the radon content of the air, these are also swept into the lungs by the inspired air and, where they are deposited, build up an equilibrium level which is determined by the inspiration rate. Following the recommendations of the International Commission on Radiological Protection, the distribution of the inhaled products will be taken to be: 25 per cent exhaled, 50 per cent retained in the upper respiratory passages and subsequently swallowed and 25 per cent deposited in the lungs. Because of the short half-lives of the decay products RaA . . . RaC′ and their low transference from the gut to the bloodstream, the fraction of the products swallowed will contribute little to general body dosage. This fraction, however, would irradiate the areas in the upper respiratory tract where the deposition was initially localised. In the case of soluble compounds, all the fraction deposited in the lungs would be taken up by body fluids; in other cases perhaps half the lung deposition would be generally disseminated. It would probably be safe to assume that some 20 per cent of the inhaled disintegration products could give rise to the irradiation of the general body tissues.[a]

For the atmospheric radon content assumed and an inhalation rate of 20 m^3/day the dose-rate to soft tissues from inhaled decay products is 0.16 mrad/year or 1.6 mrem/year. The total dose-rate to the general soft tissues of the body from inhalation of air containing 3×10^{-13} c/l, of radon and disintegration products might amount, therefore, to about 2 mrem/year, a quantity which remains small compared with other dose-rates, but one which might become significant under conditions of high atmospheric radioactivity.

[a] It can be shown that for an influx of K c/sec. of the disintegration products RaA . . . RaC′, the energy dissipated per second at equilibrium will be:

$$\Sigma(E)=3{\cdot}7\times10^{10}K\left\{E_A\left(\frac{1}{\lambda_A}\right)+E_B\left(\frac{1}{\lambda_A}+\frac{1}{\lambda_B}\right)+E_C\left(\frac{1}{\lambda_A}+\frac{1}{\lambda_B}+\frac{1}{\lambda_C}\right)+E_{C'}\left(\frac{1}{\lambda_A}+\frac{1}{\lambda_B}+\frac{1}{\lambda_C}+\frac{1}{\lambda_{C'}}\right)\right\}$$

MeV/sec where E and λ are respectively the particle energy in MeV and the transformation constant in sec. $^{-1}$.

TOTAL DOSE-RATE TO GONADS

In summarising the total gonad dose from all sources it is necessary to allow for the shielding of the gonads by overlying body tissues. The shielding factors, which reduce the dose from the local γ radiation, have been determined by using a water-filled tin model in which measurements of the normal background intensity could be made at the sites of the ovary and testis. The screeing factors were derived from the measurements after appropriate corrections had been made to allow for the cosmic-ray contribution to the instrument response. In the female, the loyal γ radiation was found to be reduced by factors of 0.52 to 0.59 depending on the position adopted by the model. In the male the screening factors varied from 0.67 to 0.72 and a mean factor of 0.63 was derived as the average for both sexes. This factor, which is unlikely to be in error by as much as $\pm$ 10 per cent is applied to all estimations of gonad dose from local γ radiation.

Table VIII summarises the dose-rates which may be regarded as typical for regions of this country, and possibly elsewhere, where the local rock radioactivity is not specially high and the buildings are of brick or concrete not incorporating highly radioactive materials. An arbitrary division of the day has been made by assuming six hours of the 24 to be spent out-of-doors.[3] Possible small additions to the total dose-rate would be not more than 2–3 mrem/year for thoron in air and 1 mrem/year for the radium content of the testis suggested by Muth's (1956) data.

Table IX illustrates the range of dose-rates deduced for various situations in this country and in Sweden. Greater differences between localities could be obtained by taking the extreme values observed in some of the Swedish and other sites, but as far as possible an attempt has been made to assess the conditions affecting large numbers of people. Thus because even agricultural workers spend some eight hours in a brick house their radiation dose is not very different from that calculated for a town dweller in the same geological area. The body shielding factor considerably reduces the differences otherwise apparent in situations of differing local radioactivity.

TABLE VIII

Radiation source	*Dose to gonads per year*
External irradiation	
Cosmic rays (sea level)	28 mrad
Local γ rays (Leeds, 78 mrad year indoors, 48 mrad/year out-of-doors)	45
Radon in air, $3 \cdot 10^{13}$ c/l	1
Internal irradiation	
Potassium 40	20
Carbon 14	1
Radom dis. products, $3 \cdot 10^{13}$ c/l	2
Total dose per year	97 mrad [1]
Dose to age 30 years	2·91 rad [1]

[1] Includes allowance for the R. B. E. of the α radiation, where present, and therefore also expresses the gonad dose in rem.

TABLE IX.—*Gonad doses*

Location and buildings	*Dose rate mrad/year*
Leeds, brick:-	
(1) outdoor worker	90
(2) indoor worker	97
Aberdeen, granite	113
Stockholm:	
(1) wood	89
(2) brick	114
(3) concrete	146
At 15,000 ft. on granite:	
(1)	244
(2)	328

[3] The assumed time out-of-doors, which is used illustratively, is probably above average for city dwellers.

The last two entries illustrate the overall effect of the sixfold increase in cosmic-ray ionization at an altitude of 15,000 feet in a situation where the rock is granite, as for example in the Andes. Two granites have been chosen (as in Table I) and the resulting dose-rates are respectively 2·2 and 1·7 times the corresponding values over the same rock at sea level.

DOSE TO LUNGS

In addition to dosage from external sources of radiation and from ^{40}K, lung tissues will receive further irradiation from inhaled particulate matter carrying the decay products of radon and thorium. Some of this radioactivity will lodge in the upper respiratory tract and be removed in perhaps a matter of hours by ciliary action. In this time, however, irradiation will have occurred from the short-lived radon product RaA. Radioactivity on the finer dust particles ($< 1\ \mu$) will enter more deeply into the lung tissues *via* the alveoli and, unless rapidly dissolved, will produce a general irradiation of the lung tissues.

Chamberlain and Dyson (1955) and Shapiro and Bale (1953) have considered the problem of dose to the trachea and bronchi and have come to approximately the same conclusion as to the order of magnitude of the dose produced by a given atmospheric radon content. Chamberlain and Dyson have shown that for a radon concentration in air of 3×10^{-13} c/l., bronchial tissues to a depth of 45 μ (the range of the RaA α particles) receive an average dose-rate of 220 mrem/year. The total dose-rate from all sources would then be of the order of 315 mrem/year. The total dose-rate from all sources would then be of the order of 315 mrem/year to the superficial bronchial tissues, a rate about three times that calculated for the general soft tissues of the body.

If it is assumed that the 25 per cent of decay products reaching the alveoli are insoluble and remain in the lungs, calculations similar to those given in an earlier section show that the average lung dose would be about 160 mrem/year for an air activity of 3×10^{-13} c/l. A very comprehensive analysis of problem of lung dosage from both radon and thoron decay products has been given by Hultqvist (1956). The dose-rate to lung tissue due to a radon concentration of 3×10^{-13} c/l. can be deduced from Hultqvist's results as 150 mrem/year. The thoron concentrations given by Hultqvist (see Table II) are about ten times lower than the radon levels and, assuming a thoron concentration of 3×10^{-14} c/l., the resulting lung dose according to Hultqvist could be about 200 mrem/year. The total lung dose from all sources would then be about 245 mrem/year if the radon only is included and 445 mrem/year including radon and thoron. A high ventilation rate (3·5 changes of the air per hour) reduces the radon and thoron doses by a factor of nearly 4, in which case the lung dose from all sources would be reduced from 445 to 190 mrem/year. It appears, therefore, that the dose-rate to lung tissue may be from two to four times that received by other body tissues when the radon and thoron concentrations in air are respectively 3×10^{-13} c/l. and 3×10^{-14} c/l.

DOSAGE IN BONE

The bone tissues likely to receive the highest dose from natural sources are the osteocytes lying in regions of bone which incorporate radium. An average dose to an osteocyte of a given size can be deduced from the radium content of the body on the assumption of uniform radium deposition. In those cases of radium poisoning which have been studied by autoradiography (especially by α-particle track photographs) the radium occurs in localised areas where the dose-rate has been shown to be as much as 10 to 20 times the average. Nevertheless, the calculation of an average dose-rate will be a satisfactory measure of the general background irradiation of bone and will serve as a means of assessing the significance of the ingestion of bone-seeking radioactive isotopes.

The dose-rate to osteocytes from sources external to the bone will be practically the same as in the calculations for soft tissues, but the potassium contribution to the dose-rate will be less because its concentration in bone is about four times less than in muscle. The resulting dose-rate from sources other than radium can them be put at about 82 mrem/year (Table X).

TABLE X.—*Background dose-rates to bone*

Conditions	Radium in skeleton	Dose rates to osteocytes in mrem/year		
		Radium	External sources	Total
Not specially radioactive region Radon in air 3×10^{-13} c/l.	10^{-10} g	39	82	121

The radium body burdens listed in Table IV suggest that for a region not specially radioactive a representative value for the radium content of the skeleton might be 10^{-10}g Ra. Using methods of calculation similar to those given by Spiers (1953) the mean dose-rate to an osteocyte of diameter 5μ would then be 39 mrem/year, making a total from all sources of 121 mrem/year. The extent to which this might vary from site to site in bone connot be estimated with any confidence because the deposition of radium in the skeleton due to a low continuous intake has not so far been studied.

ACQUIRED RADIOACTIVITY TO PRODUCE THE EQUIVALENT OF THE BACKGROUND DOSE-RATES

At a time when the accidental ingestion of radioactive materials must be regarded as at least a possible accompaniment of nuclear energy development, it is of interest to consider what quantities of acquired radioactive elements will produce dose rates equal to those already being received by body tissues from natural sources. Two fission products would seem to be particularly important in that they are long lived and are capable of being taken into the body structure. Radioactive caesium, ^{137}Cs, may be expected to follow potassium in its behaviour and distribution and strontium 90 is known to be taken up in bone.

In the case of ^{137}Cs, which emits β rays of 0.55 MeV and 1.21 MEV energy and a γ ray of 0.66 MeV, a body burden of 0.6 μc is required to give the soft tissues of the body a dose-rate equal to the background rate of 100 mrem/year. This comparative figure is relevant to any discussion of the significance of an acquired ^{137}Cs activity.

In the case of ^{90}Sr with its radioactive decay product ^{90}Y, a body burden of 45×10^{-9} c would result in an average dose-rate to osteocytes equal to that received from natural sources including a radium burden of 10^{-10} g. If, however, it is conceded that the concentrations of strontium in bone can be five times higher than those observed with radium (I. C. R. P. 1953), the introduction of this further factor of 5 reduces the amount of ^{90}Sr+^{90}Y equivalent to the background radioactivity to 9×10^{-9} c. Assuming the calcium content of the skeleton to be 1000 g, the ^{90}Sr contents can be expressed respectively at 45×10^{-12} and 9×10^{-12} c of ^{90}Sr per g Ca.

DISCUSSION

The importance of an estimate of the dose-rate from natural radiation lies in the fact that it is the inevitable accompaniment to life and has been so throughout man's evolution. It must form one basic element of comparison in any judgment of what is a tolerable level of human dosage from the new radiation sources. It would seem that the natural dose-rate, if not an essential factor in man's environment must be at least a tolerable one. Furthermore, a comparison of the natural background dose-rate with dose-rates known to be harmful will give some indication of the range within which "permissible" rates must be set—although it is beyond the proper scope of this discussion to consider what those rates should be.

In relation to genetic effects, comparison must be made with the dose received by the gonads from background radiations. It seems likely that this dose-rate is not far from 0.1 rad per year or 3 rad in a generation time of 30 years, and that, even in houses incorporating highly radioactive shales in their structure, the dose level is not much more than 50 per cent greater than this. Although reservation is necessary, in view of the paucity of information, great variations in the background dose-rates to large populations seem rather unlikely, because local γ radiation provides only part of the background dose and its contribution to the gonad dose is reduced by the shielding of overlying tissues.

The relationship of a gonad dose of 3 r in a generation to that required to double the human spontaneous mutation rate is uncertain because the radiosensitivity of human genes is unknown. The most recent estimate that can be quoted is that of Muller (1955) which suggests that the so-called doubling dose is unlikely to be much more than ten times this background dose.

The dose-rate to the soft tissues in bone is only slightly in excess of that to the gonads because the contribution from the small natural radium content of the skeleton is partly off-set by a reduced contribution from potassium. A dose-rate of about 0.12 rad per year or about 6 rad in 50 years appears to be a reasonable estimate. This is some 2000 times less than dose-rate to osteocytes which have been estimated in cases where small accidental radium burdens (of about 0.4 μg) have ben associated with radiographically detectable but not necessarily malignant—changes in bone.

It has been shown that the dose rates from natural sources to the upper respiratory tract and to the general tissues of the lung may be as much as three times the gonad dose-rate for an assumed normal atmospheric radon content of 3×10^{-13} c/1. The dose in 50 years, 15 rad, is from 100 to 1,000 times less than the order of dose estimated in cases where the occurrence of cancer has been associated causally with the irradiation of body tissues. It is probably 10,000 times less than the lung dosage in the uranium miners of Joachimstal amongst whom the incidence of lung cancer was so marked.

It is evident, therefore, that the natural background dose stands nearest in relationship to those doses thought to be genetically significant. Several orders of magnitude lie between the natural dose and those associated with carcinogenesis. Clearly in any assessment of the significance of population dosage, these differences of magnitude must be recognized and taken into account.

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BIOLOGICAL EFFECTS OF RADIATION

An open meeting was held on March 7 at the Institute of Biology under the titl "Aspects of Radiotoxicity," with the aim of clarifying some of the problems arising from the increasing level of radiation. Dr. H. O. J. Collier, in the chair,

set the evening's challenging tone to the scientist by opening the discussion with the quotation, "Man, proud man, drest in a little brief authority, . . . plays such fantastic tricks before high heaven, as make the angels weep". This challenge was taken up by Prof. J. Rotblat, who spoke on the physical aspects of radiation damage, and by Prof. L. Penrose, a member of the Medical Research Council Committee (1956) for the study of radiation hazards, who discussed biological damage, including the genetic effects.

Prof. Rotblat began by stressing the complexity of the problem. On the physical aspect alone, and confining the discussion to ionizing radiations, there is a multitude of particles to consider, from electrons to fission fragments, with widely differing energies, penetrating powers and types of secondary reactions produced. Compared, however, with the chemical, biochemical and biological processes contributing to radiation damage, the nature of which is still obscure the physics of radiation damage is fairly well understood. The passage of radiations through matter gives rise to excitation and ionization of atoms and dissociation of molecules. All these contribute to the biological action and for this reason it is necessary to consider the total energy transferred to matter rather than ionization alone. The unit employed is the linear energy transfer (sometimes referred to as the L. E. T.), which is the amount of energy transferred per unit length of path of the ionizing particles, usually measured in keV. per micron of tissue. This does not depend on the mass of the ionizing particle but only on its charge and velocity. The very marked variation with velocity explains the observed difference between the damaging action of alpha particles, protons and electrons, of the same energy. But although different particles with the same value for the linear energy transfer produce the same biological effect, the relationship between the linear energy transfer and biological damage, or the relative biological effectiveness (R. B. E.), is not at all straight-forward. Prof. Rotblat quoted examples of biological processes, in some of which the biological damage increases with linear energy transfer, in others decreases with it, and in still others first increases and then decreases. These variations indicate the existence of different mechanisms of biological action.

Turning to the doses of ionizing radiations to which man is nowadays exposed, Prof. Rotblat made a survey of the natural background of radiation, pointing out that this will differ depending on whether one considers the gonad dose, the dose to bone or to other tissue. In Great Britain the gonad dose is 0·1 r. per year, or 3 r. in thirty years; the bone dose is slightly greater and amounts to 8·5 r. during seventy years of life. Of the man-made radiations, the largest contribution is from diagnostic radiological examination. The Medical Research Council report gave a figure of 22 per cent of the natural background, but more recent evidence indicates that the actual value may be 5–10 times higher, which would bring it in line with that in the United States. The fall-out from nuclear test explosions has up to now contributed very little so far as external radiations are concerned, but internal radiation gives rise to some concern. The effect of cæsium-137 in the body, although quite small, has perhaps been dismissed too lightly, but the concentration of strontium-90 in bone is the more worrying factor.

All these doses appear quite safe when compared with the maximum permissible dose (M. P. D.) established by the International Commission on Radiological Protection, which amounts to 0·3 r. per week, or 150 times the natural background. It has, however, to be remembered that the maximum permissible dose was set up for people occupationally exposed, and on the assumption that only a small fraction of the population is so exposed. For the whole population the figure may have to be reduced by a factor of 10 or even 50, but there may be other reasons for lowering the maximum permissible dose. It is now becoming evident that the doubling dose of leukæmia may be much less than was thought. Another factor, which came to light only recently, is the shortening of life-span due to an acceleration of the natural processes of ageing. A survey of radiologists in the United States, showing a reduction of life-span by five years, may be statistically uncertain, but there is no doubt about this effect from experiments on animals. These experiments show that every irradiation results in some irreparable damage, and that the lethal dose gradually decreases with age. If this is extrapolated to human beings, it leads to an inherent limit of life-span due to the natural background of radiations. This limit may be two hundred years or more, but the grim significance of this sort of speculation becomes apparent when applied to the present maximum permissible dose, which would reduce the life-span to about thirty years from the time exposure began. There seems, therefore, to be a good case for lowering the maximum permissible dose, quite apart from the genetic hazards.

Prof. Penrose opened his discussion of the biological damage due to radiations by directing attention to the ignorance there is of the intermediate steps in their action, stating that one could only speculate with the knowledge available. He pointed out that the major damage to populations exposed to an atomic explosion would be due to heat and blast, and not to radiation. Ionizing radiations were, however, damaging even in small intensities. Their effects could be divided into immediate, during the first few weeks, and delayed. The immediate effects, such as burns and anæmia, are well known. Their relation to the dose received is not linear, lower doses producing apparently no effect. With the delayed effects, such as cataract or leukæmia, the picture is different. From the evidence on survivors at Hiroshima and Nagasaki, as well as from the findings of Court Brown and Doll on patients treated with X-rays for anklyosing spondylitis, it is clear that the incidence of leukemia increases linearly with the dose. This linear relationship between exposure and delayed effects emerged toward the end of the Medical Research Council Committee's deliberations and was the reason for the demand for a reconsideration of the maximum permissible levels.

Another point to be clarified in the public mind is the difference between somatic and genetic effects. Direct damage to the fœtus by radiations has been studied on pregnant rats and mice as far back as 1931, and the findings have been supported by observations on human fœtuses. It was found that large doses to the mother's abdomen could clearly cause fœtal damage not previously suspected. Apart from miscarriage, this could result in the birth of babies with arrested development of the central nervous system leading to a special type of microcephaly. These risks, which had been known for some time, were definitely confirmed in the populations exposed at Hiroshima. Dr. Alice Stewart and her colleagues at Oxford claim to have observed delayed deleterious effects on the fœtuses exposed at any time during pregnancy to a much smaller dose of X-rays, such as occurs during diagnostic pelvic X-ray of the mother. In these offspring there was found to be a significantly higher incidence of leukæmia, which developed one, six, or even ten years after birth. Prof. Penrose hesitated to accept these findings as fully proved, but felt that although the damage occurred only in a small proportion of the mothers X-rayed in pregnancy, nevertheless it was important.

The genetic hazard can only be considered by looking at a long-term picture. There are two processes to be considered—chromosome breakage, which disturbs nuclear harmony, and point mutations, both of which may be the result of ionizing radiations. When a chromosome breakage occurs, both theory and experiment show that the effect is proportional to the square of the dose because, to produce an effect, there must be two breakages with the possibility of joining up again to reproduce. In the case of point mutations there is a direct linear effect, and this has been established by work with *Drosophila*. In man, such mutations are far from difficult to study, and this has in part produced the impression that the genetic hazard results only in an increase in the number of achondroplasic dwarfs. Actually, an increase in the rate of mutations will increase many other far more serious conditions. Referring to the nuclear test explosions, Prof. Penrose stated that the number of fresh mutants would be small and difficult to demonstrate, and in this sense only is it insignificant, counted over the whole population the number would certainly not be negligible.

Prof. Penrose then produced some evidence of the part played by radiation on the natural mutation-rate in man. One method is to study the incidence of diseases believed to arise by spontaneous mutation in relation to the ages of parents. For chondrodystrophy and acrocephalosyndactyly the age of the father, but not that of the mother, is strikingly high. This could be explained by assuming that the gene loci concerned were not very sensitive to radiation but mutated in response to other factors. For the most sensitive loci, mother's and father's age would both be only slightly raised, as seems to be the case in retinoblastoma neurofibromatosis and epiloia. Mongolism falls into a quite different group, its incidence increasing with an increasing maternal, but not paternal, age, and it is clearly not due to point mutation.

In answer to questions on the haphazard use of radioisotopes therapeutically and experimentally, both speakers stressed that nowadays every care is taken in the use of these materials. Prof. Rotblat explained that in the case of radioiodine therapy in thyroid disease, treatment is now limited to patients of the older age group where tumour induction after many years is of less importance

than in the younger age group. Physicians in Britain have been more conservative in accepting the possession of a Geiger counter as essential for their practice than has the medical profession in the United States. The contribution from short-lived isotopes used in medicine has been calculated to be very small.

The chairman concluded the meeting by a revision of the old rules of health, suggesting that one should stay indoors to be shielded from cosmic rays, to stop drinking milk to limit the intake of cæsium 137, and to keep away from the doctor, to avoid being X-rayed.

P. J. LINDOP.

[Britsh Medical Journal, London, Saturday, March 30, 1957]

STRONTIUM-90 IN MAN

Whenever a nuclear weapon is exploded, or atomic energy is released in a reactor, over 200 radioactive isotopes are produced. All these can be harmful to man, but the main villain of the piece appears to be strontium-90. Its notoriety is due to several reasons. First, it has a high yield: one in every twenty fissions results in the production of an atom of strontium-90. Secondly, it is long-lived: its physical half-life is 28 years. Thirdly, it finds its way from the soil into food. Fourthly, it is a bone seeker. And, fifthly, it is retained in the human body for a long time; the biological half-life is about 7½ years. The last two properties lead to the possibility that the β-rays emitted from radiostrontium may produce bone lesions and sarcomas as well as leukaemias.

Although the hazards from the deposition of strontium in bone have long been recognized, too little has been known about its distribution and metabolism to assess the hazards quantitatively. Recent papers [1,2,3] giving results of a world-wide survey of strontium content in man throw some light on the subject. Strontium-90 is an inevitable by-product of nuclear power production, and by the time most of our power requirements have been met from atomic energy there will be an embarrassingly large quantity of strontium-90 to be disposed of; but this does not present a hazard at the moment, since the strontium is not allowed to escape into the open. Even in a full-scale nuclear war strontium-90 would not be the main radiological poison; its damaging action would be small compared with that of the shorter-lived isotopes strontium-89 and iodine-131. It is the relatively small amount of strontium-90 accumulating from test explosions that causes some worry. Furthermore, strontium-90 does not present a genetic hazard, since it emits only β-rays, which do not penetrate to the gonads. It is the somatic hazard, the probability of inducing bone tumours, that is the main concern.

When a nuclear weapon is exploded in the air the radioactive fragments are carried by the upward draught high into the stratosphere. In the case of a high-altitude explosion—as the next British H-bomb test is supposed to be—there is very little local fall-out, and practically all the radioactivity is taken up into the stratosphere. The rate of descent from there is very low, about 10% per year, and this allows the radioactivity to spread out over the globe, so that when it does come down, mainly with the rain, it is nearly uniformly distributed all over the world. By the time the active material has descended the short-lived radioactive elements have practically all decayed and only the long-lived ones, mainly strontium-90 and caesium-137, are deposited on the ground. Here strontium, being chemically similar to calcium, enters into the soil–plant–animal–human chain of calcium. The first step in the chain, the uptake of strontium by plants, depends on the root depth and on the calcium content of the soil. The latter varies greatly; differences by a factor of 100 are quite common. Since the uptake of strontium by the soil will be the greater the less calcium it contains, the concentration of strontium-90 per gramme of calcium in the soil, and thus the concentration in plants, may vary enormously. There is also a large variation with depth, most of the strontium being deposited in the top 4 in. (10 cm.) of soil. The consumption of vegetable food is one way of introducing strontium-90 into the body. But since most of the calcium

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[1] Libby, W. F., Proc. nat. Acad. Sci. (Wash.), 1956, 42, 945.
[2] Kulp, J. L., Eckelmann, W. R., and Schulert, A. R., Science, 1957, 125, 129.
[3] Booker, D. V., Bryant, F. J., Chamberlain, A. C., Morgan, A., and Spicer, G. S., Radiostrontium and Radiocaesium Measurements in Biological Materials to December 1956, Atomic Energy Research Establishment, 1957, London.

in our diet comes from dairy products the next step in the chain, the uptake of strontium by grazing animals, is the most important one. Fortunately there is a large discrimination factor, of about 7, against strontium consumed by the cow appearing in the milk. Furthermore, there is another discrimination factor, of about 8, in going from food to the deposition of strontium in bone, so that the amount of strontium-90 per gramme of calcium in the human bone is very much smaller than that in the soil.

The concentration of strontium-90 is expressed in terms of so-called sunshine units (S. U.), one such unit being one-millionth of a microcurie per gramme of calcium. A recent survey[3] has shown that the content of strontium-90 in the human skeleton, resulting from all H-bomb tests carried out until the autumn of 1956, will eventually reach a value of about 2 S. U. This is an average value with a very wide spread. The uptake in different bones in the skeleton varies by as much as a factor of 15, the largest concentration being in the vertebrae and sternum, the lowest in the skull. There is also a big variation with age; children aged 0–4 years take up about five times more than adults, and they have also a greater sensitivity to radiations. Finally, there are large variations in individuals of the same age group, probably due to differences in diet.

The main problem is how to assess the hazard from a given amount of strontium-90 in bone. The maximum permissible concentration of strontium-90 is 1,000 S. U., which is 500 times the average amount which will accrue from test explosions. But such a comparison is misleading, since all maximum permissible levels were established for people occupationally exposed to radiations. When whole populations are exposed the maximum level must be greatly reduced. The assessment of this hazard hinges on a fundamental question: how does formation of bone tumours vary with dose? If a threshold dose exists below which tumours are not formed, then the small doses resulting from test explosions are probably harmless. But if there is no threshold, and tumour formation is a linear function of dose, then even the smallest dose will impart a small but finite probability of a bone tumour being formed. The available evidence is meagre, and, moreover, it entails comparison of strontium with raduim, since only after exposure to radium has production of bone sarcoma in man actually been observed. There is growing evidence that the relation between the amount of exposure to radiations and the probability of developing long-term effects is of a linear nature. This was the view accepted by the M. R. C. in its report,[4] which states that "each unit quantity of radiostrontium absorbed by the bone confers a certain probability of bone tumour formation." The consequence of a linear relationship is that every H-bomb test by causing an increase of the content of strontium-90 in bone will increase the frequency of bone lesions. But much more research work will be required before an accurate assessment of the hazard can be made.

Leukaemia and Natural Background Radiation

Sir,—It is now well established that ionizing radiations can cause leukaemia in man. The detailed survey carried out recently by the U. K. Medical Research Council[1] of the incidence of leukaemia among patients treated for ankylosing spondylitis with x-rays, as well as the earlier investigations by the U. S. Atomic Bomb Casualty Commission[2] of the survivors of Hiroshima and Nagasaki during the period 1947–55, have definitely shown that the incidence rate of leukaemia increases with the strength of the radiation dose received. Although the exact relationship between the two is not definitely established, it may not be unreasonable to assume—and there is some recent evidence in support[3]—that the probability of incidence of leukaemia is directly proportional to the strength of the exposure (at least for low and moderate doses).

In the analysis carried out by the U. K. Medical Research Council, the x-ray dose was estimated in two different ways—first, by calculating the total amount of energy absorbed by the whole body, and, secondly, by calculating the maximum dose of radiation received (at a point in the spinal marrow). The whole-body dose is more relevant in the present discussion.

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[4] Medical Research Council, The Hazards to Man of Nuclear and Allied Radiations, 1956, London.

[1] The Hazards to Man of Nuclear and Allied Radiations, 1956. H. M. S. O., London.

[2] Pathological Effects of Atomic Radiation, 1956. National Academy of Sciences, U. S. A.

[3] British Medical Journal, 1956, 2, 704.

In the table given below, the figures in the last column are calculated from the corresponding data in columns 1 and 2, which are reproduced from the U. K. Medical Research Council's report. Thus, for instance, the whole-body integral dose of 7.5–14.9 megagram-roentgens (average 11.2) would correspond to a whole-body dose of about 160 roentgens for the "standard man" (mass 70 kilograms). The leukaemia incidence rate for this dose is 4.4 cases per 10,000 men per year (if we exclude the natural rate of 0.3 cases per 10,000 per year). Thus the incidence rate per roentgen per year per million persons comes out to be 2.8.

Whole-body Integral Dose in Megagram-roentgens	Crude Incidence per 10,000 Men per year	Incidence per Million per Year per Roentgen
0	[1] 0·3	
<7·5	0·7	>0·4
7·5–14·9	4·7	2·8
15·0–22·4	5·1	1·8
22·5–37·4	11·3	2·6
37·5–52·4	22·6	3·5
>52·5	60·2	<8·0

[1] The rate has been estimated from the national vital statistics for all forms of leukaemia excluding lymphatic leukaemia.

It may be seen that the incidence rate of leukemia is roughly 3 per million per year per roentgen.

An analysis of the Japanese data leads to a figure of roughly 1 per million per year per roentgen. This represents a lower limit to the incidence rate, since the gamma-ray doses have been estimated for a person in the open. Apart from the uncertainty in estimating the dose in this case, the actual radiation dose received by the survivors must have been much less, by a factor of 2 to 3, due to shielding. Thus the incidence rate is again seen to be very roughly of the order of 3 per million per year per roentgen.

Let us assume that the natural incidence of leukemia is due entirely to the action of the background radiation (to which everyone is exposed). The average natural death rate due to leukemia is of the order of 15 per million per year. In England and Wales the annual death rate from leukemia was 11 per million in 1920, and 49 per million in 1954; the reason for this increase is not yet clear. The Japanese death rate is about 16 per million per year.) A person receives a whole-body dose of 0.1 to 0.15 roentgen per year from natural background radiation—that is, from the naturally and normally present radioactivity in his own body and the radioactivity and cosmic rays in the environment. This is equal to a dose of 3 to 5 roentgens up to adult life (say, 30 years).

On the assumption that the natural incidence of leukemia is due entirely to background radiation, the figure of 3 leukemia cases per million per year per roentgen and a natural radiation dose of 5 roentgens up to adult life would lead to a natural adult incidence rate of 15 per million per year. The close agreement between the incidence rate as calculated above and the observed rate may, no doubt, be partly accidental, but it points to the high plausibility of the assumption that the natural incidence of leukemia could be due entirely to the background radiation.

Our thanks are due to Professor D. S. Kothari and Dr. M. L. N. Sastri for stimulating discussions.—We are, etc.,

SANTOSH KUMAR MAZUMDAR.
A. NAGARATNAM.

NEW DELHI.

[Reprinted from British Medical Journal, April 20, 1957]

STRONTIUM-90 IN MAN

Sir,—Strontium-90 is now frequently being discussed all over the world, and your leading article (*Journal*, March 30, p. 752) will no doubt be quoted in lay fora as an expression of the best medical opinion of this country. It is important that some of the weaknesses of this, in most ways excellent, short summary of the situation should be noted.

"The main problem is how to assess the hazard from a given amount of strontium-90 in bone." Most would agree with this quotation.

Permissible Body Burden for the Occupationally Exposed.—This has been given by the International Commission on Radiological Protection and the Medical Research Council's Committee on Protection against Ionizing Radiation as 1 μc of strontium-90 or approximately 1,000 S. U. (strontium units, μμc per gramme of calcium), and was derived as follows. From industrial experience in the luminizing industry it was considered that the minimum toxic body burden of radium—a chemical analogue of calcium and strontium—was 1 μc. Early results with experimental rodents in Chicago had indicated that the toxic dose of strontium-89 relative to radium was about 10:1. Strontium-90 and its daughter yttrium-90 liberate per disintegration about twice the energy of strontium-89. Therefore the ratio of strontium-90 to radium should be about 5: 1 as administered; but radium decays in the body to radon, a gas, much of which escapes in expired air, more in the case of recently deposited radium than from long-standing depots, and more in the case of experimental rodents than man. In terms of body burden of effective radiactive material in human bone it was to allow a factor of 2 for this, bringing the ratio strontium-90: radium=10: 1. The calculated, minimum toxic permanent body burden for strontium-90 is thus 10 μc. Allowing a safety factor of 10 a maximum permissible body burden of 1 μc was derived.

The following uncertainties are relevant. (1) Industrial radium used in the luminizing industry was a mixture containing variable amounts of mesothorium and other radioactive elements, too short-lived for their contribution to the observed toxic manifestations to be assessed at the time when the body burdens of luminizers were estimated. However, a few subjects who had received injections of relatively pure radium for medical (*sic*) reasons have been discovered: in them the minimum toxic body burden appears so far to be about 8 μc[1] as compared with recent estimates of about half a microcurie of "radium" in ex-luminizers. The minimum toxic body burden of radium cannot therefore be indicated with precision. (2) The ratio of toxic doses of radium and strontium-89 was derived from injected animals. After more chronic administration these radioactive materials may be expected to be more widely distributed in bone. (3) The comparative metabolism of radium and strontium may differ significantly in man from the rodent because of different size and bone structure.

Permissible Body Burden in General Population.—In spite of the uncertainties the maximum permissible body burden for strontium-90 remains one of the best authenticated among those derived for occupational purposes. However, because of the uncertainties it is not justifiable to use this figure as a yardstock when one is considering a general population as Libby[2] and Kulp *et al.*[3] have done. Your leading article and the Medical Research Council[4] have given the most cogent reason—the presence in a general population of foetal, neonatal, and adolescent subjects who have greater avidity for and greater sensitivity to radioactive strontium. On the other hand, with the homoeopathic doses they receive daily the young will distribute strontium much more uniformly in bone than will the adult. How much this latter offsets the former is unknown.

This matter of distribution of strontium is worthy of stress. The values given by Kulp *et al.*[3] for strontium–90 in human skeletons were not those directly measured but figures "normalized" for unevenness of distribution in different bones. Now, unless I am in error, the factor for normalization was derived from the single administration of strontium–85 to adults in the terminal phase of killing diseases. An ill adult patient, probably bed-ridden, given a single dose of a radioactive marker and dying 3 to 125 days later, is no criterion for the metabolism of a daily ingested calcium analogue, and to use this criterion to "normalize" observed data, particularly from children, is, to say the least, unscientific. In fact field studies on domestic animals[5] indicate that gross radioactivity from strontium–90 is fairly uniformly distributed throughout the bones. Moreover, the analysis of stable strontium in human bones from subjects of all ages shows a similar uniformity.[6] Strontium is a normal though apparently

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[1] Looney, W. B., Hasterlik, R. J., Brues, A. M., and Skirmont, E., Amer. J. Roentgenol., 1955, 73, 1006.
[2] Libby, W. F., Proc. nat. Acad. Sci. (Wash.), 1956, 42, 945.
[3] Kulp, J. L., Eckelmann, W. R., and Schulert, A. R., Science, 1957, 125, 219.
[4] Medical Research Council, The Hazards to Man of Nuclear and Allied Radiations, 1956. London.
[5] Cox, G. W., and Morgan, A., personal communications.
[6] Sowden, E. M., and Stitch, S. R., A. E. R. E. MRC/R.2030, 1956.

non-essential metabolite, and radioactive strontium will follow the same pathways.

Radioactive strontium–90, now universally distributed over the surface of the world, should be considered in comparison with the naturally occurring radium found in all bone, human and animal. Calculations of acceptable risks for a population from body-burdens of radioactive strontium are best made therefore using natural radium contamination as the criterion. This approach is being pursued with vigour.

Dose-response Relationship.—Meanwhile, "How does formation of bone tumours vary with dose?" Is there a relationship between natural radioactivity of bone and natural incidence of bone sarcoma? Though this has been carefully looked for, the only conclusion is that the evidence is too scanty.

Is there "growing evidence that the relation between the amount of exposure to radiations and the probability of developing long-term effects is of a linear nature"? In so far that formerly there was none at all, while recently a linear relationship has been suggested between dose to a point in the spinal marrow and the incidence of leukemia in irradiated spondylitics (Appendix B[4]), this statement is true. But radiation-induced leukemia is an odd thing. It has a much shorter latency than other radiation-induced malignant states. Some would even yet argue that it is not a malignant condition. In the experimentally induced lymphoma of the mouse the relation between total dose and fractionation of the dose is most complex and certainly not simply linear.[7]

It would be unwise therefore to use leukaemia as a model for all radiation-induced malignancies and as a justification of the thesis of somantic mutation as the cause of cancer. It is wrong moreover, to attribute to the M. R. C. report[4] opinions which it did not express. The statement, "Each unit quantity of radiostrontium absorbed by bone confers a certain probability of bone tumour formation," was not in the report but one of its signed appendices. Furthermore, the statement would suggest a linear relationship only if each unit quantity conferred the *same* probability.

In point of fact, the quantitative evidence of the carcinogenic properties of radiostrontium, and there is not much of it, suggests that the relationship is not linear and that there is some form of threshold, or effective latent period.[8,9] Therefore your conclusion, "The consequence of a linear relationship is that every H-bomb test by causing an increase of the content of strontium-90 in bone will increase the frequency of bone lesions," is as yet without foundation. The correct conclusion in the present state of scientific knowledge is that a linear relationship sets the limit and indicates the worst possible conditions, so that it would be prudent to go easy until the real facts are ascertained.

Strontium in Animal Metabolism.—*Pari passu*, it is right and proper that investigations be made on the uptake of both natural radium and the now universal strontium-90. Your leading article notes that we derive most of our calcium and strontium from milk and certain of its products. A safety factor of about 7—the cow—is built into this route by which we ingest strontium-90. Your leading article quotes a second safety factor of about 8 in going from food to the deposition of strontium in human bone. The combination of 7×8 would indeed be impressive, but is unfortunately an overstatement. Sowden and Stitch[6] find the strontium content of human bones to be, in round figures, 100 p. p. m. of ash, of which about 40% will be calcium. The strontium: calcium ratio is thus about 1:4,000. In a general diet one can derive from the data of Harrison *et al.*[10] that strontium: calcium is about 1:500. Here is the factor 8, but this includes the factor for the cow insofar as milk and its products are part of the diet of the average adult. Moreover, Harrison *et al.* calculate that the discrimination against strontium in favour of calcium by the human gut gives a factor of about 2. Unpublished observations from the same source suggest that the human bone absorbs calcium and strontium from the blood without discrimination. The best figure at the moment for the human safety factor is thus only 2. It is likely to be rather higher for alkaline earths of vegetable origin than for those derived from milk, since lactose and some amino-acids cause reduction in the factor of discrimination.[11]

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[4] Medical Research Council, The Hazards to Man of Nuclear and Allied Radiations, 1956. London.

[7] Mole, R. H., in Progress in Radiobiology, 1956, edited by J. S. Mitchell, B. E. Holmes, and C. L. Smith. Oliver and Boyd.

[8] Finkel, M. P., Radiology, 1956, 67, 665.

[9] Brues, A. M., J. clin. Invest., 1949, 28, 1286.

[10] Harrison, G. E., Raymond, W. H. A., and Tretheway, H. C., Clin. Sci., 1955, 14, 681.

[11] Wasserman R. H., Comar, C. L., and Nold, M. M., J. Nutr., 1956, 59, 371.

Strontium in Soil and Plant.—Your leading article infers that all the strontium-90 in plants is derived from the soil. But the leaf of the plant is a very good surface for the adhesion of small particles of "fall-out" and for the absorption of strontium as well as other mineral ions.[12, 13] Thus the present contamination may be much more related to the rate of fall-out than to integrated total "fall-out."—I am, etc.

Didcot, Berks J. F. LOUTIT.

SIR.—Your leading article (*Journal*, March 30, p. 752) states that strontium-90 does not present genetic hazards since it emits only β-rays which do not penetrate to the gonads. In view of the great importance of this subject at the present time, it would be interesting to know whether this statement is based on experimental evidence or merely on supposition. If the latter, then it is difficult to accept the argument without further investigation. It may be that the writer assumed that all the available strontium-90 is deposited in the bone and therefore is unlikely to affect the gonads. However, even on this assumption the β-particles can traverse distances of up to about a centimetre in soft tissues:[1] when the testes are in the inguinal canal, they must be less than one centimetre distant from bone at some stage in their journey. This may also apply to the ovaries during development.

A second possibility which may not be excluded is the effect of any strontium-90 prior to its deposition in the bone or after subsequent mobilization. If there is any similarity between the effect of strontium and lead it may be that strontium does in fact have an effect on the soft tissues in general. Lead is well known to have a special affinity for bone, but its main impact is on soft tissues, such as the nervous system and the gut. Until, therefore, these possibilities have been excluded by experiments of suitable length in mammals, such a statement as that in your leading article may convey a false sense of security.—I am, etc.

London, S. W. 17.

BRIAN H. KIRMAN.

A. E. R. E. HP/R 2017

ATOMIC ENERGY RESEARCH ESTABLISHMENT

THE RADIOLOGICAL DOSE TO PERSONS IN THE U. K. DUE TO DEBRIS FROM NUCLEAR TEST EXPLOSIONS PRIOR TO JANUARY 1956

By N. G. Stewart, R. N. Crooks, and Miss E. M. R. Fisher

1. INTRODUCTION

The size distribution of the radioactive dust particles in the cloud from a nuclear explosion depends both on the power of the weapon and on the manner in which it is exploded, whether on the ground, on a tower or high in the air. The larger particles fall to earth relatively close to the site of the explosion, but in every type of burst a considerable fraction of the total radioactivity generated is contained in fine dust particles whose rate of fall under gravity is small and which may therefore remain airborne long enough to diffuse widely throughout the atmosphere and affect large areas of the earth's surface.

The United Kingdom, being remote from all test sites used up to the present time, has been free from local effects and has provided a useful observation area in which to study the global contamination problem. In this paper, an account is given of the methods that have been used to measure the amount of radioactive material in the air above U. K., and the amount deposited on the ground, as a result of all weapons exploded between January 1951 and January 1956. Based on these measurements, an estimate is made of the integral gamma ray dose received by the average inhabitant of this country during his lifetime from all nuclear tests held to date. An estimate is also made of the dose that will ultimately be received per individual per generation if bombs continue to be exploded at the present rate.

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[12] Tukey, H. B., and Wittwer, S. H., in Progress in Nuclear Energy VI: Biological Sciences, 1956. Pergamon Press.
[13] Russell, R. S., Squire, H. M., and Martin, R. P., A. E. R. E./S. P. A. R. 3, 1955.
[1] Medical Research Council, The Hazards to Man of Nuclear and Allied Radiations, 1956, p. 5. London.

2. PROPERTIES OF THE DUST PARTICLES

2.1 The particles reaching U. K. from the smaller type of nuclear explosion are spherical in shape, having been formed by the condensation of material which has been vaporised in the explosion. They consist mainly of fused silica or metal oxides, depending on whether the weapon has been exploded near or remote from the surface of the earth, but both types are impregnated with radioactive elements more or less uniformly throughout their volume. The particles from the thermonuclear tests over the Pacific atolls consist mainly of calcium carbonate or calcium oxide but they are shapeless, possibly due to the refractory nature of the coral on which the tests have taken place. The vast majority of the particles encountered in U. K. from all types of explosion have diameters of less than 10μ ($1\mu=10^{-4}$ cm.).

2.2 The radioactive content of explosion dust falls into three categories:

(i) Fission Product Activity (β, γ)
(ii) Neutron Capture Activity (β, γ)
(iii) Alpha Activity

Fission Product Activity

The fission products contained in the fine dust are fairly representative of those generated in the parent explosion although, under certain circumstances, some may be preferentially deposited along with the larger particles near the test site. Different types of weapon produce the same assemblage of fission products in slightly different proportions, but the rate of decay of the gross activity is almost independent of these differences and varies approximately as the inverse 1.2 power of time. Thus the activity decreases by a factor of 10 for a 7-fold increase of time as measured from the instant of the explosion. Table I shows the percentage contributions made by some of the more important fission products to the beta-ray activity from the slow neutron fission of U^{235}. Only those contributing more than 5% to the total activity at any time are included in the list. At the foot of the table are given Hunter and Ballou's computed figures[1] for the beta activity associated with the fission of 10^6 atoms of U^{235}. It is often convenient to express the fission product activity of dust in terms of the number of fissions from which this activity is derived. Thus a sample which is disintegrating at the rate of 620 disintegrations per minute (dpm) twenty days after the time of the parent explosion may be said to contain 10^6 fissions (see Table I).

Activity from Neutron Capture

Although several different radioactive elements may be created by the capture of neutrons in materials close to the reacting core of a weapon, the only significant reactions to produce gamma-ray emitters are those associated with the natural uranium which may be used as the tamper material of the bomb. This uranium is subjected to intense neutron bombardment during the nuclear explosion and the following reaction takes place:

$$92^{U238} + 0^{n1} \longrightarrow 92^{U239} \xrightarrow[23.5m]{\beta-} 93^{Np239} \xrightarrow[2.33d]{\beta-} 94^{Pu239} \xrightarrow[long]{\alpha} \ldots \quad (1)$$

Chemical analysis of the debris shows that in general about one neutron is captured in this way for every fission that occurs, both in nominal bombs and in thermonuclear explosions. The U^{239} decays completely before reaching the U.K. but at four days after time of burst the Np^{239} disintegration rate reaches a peak relative to that of the fission products and accounts for about 60% of the observed activity at that time.

In addition to this, a smaller number of the neutrons in a thermonuclear explosion undergo an (n,2n) reaction with U^{238} to form 6.7 day U^{237} which is also a (β, γ) emitter.

Despite the fact that Np^{239} and U^{237} contribute much to the gross activity of the dust particles in the first few days after burst, their short half-lives and the softness of their gamma rays prevent them from adding significantly to the hazards associated with the fission products.

Alpha-Particle Activity

By far the most important alpha-particle emitter in the explosion dust is Pu^{239}. Consider first a nominal bomb of efficiency 20% with plutonium as the

[1] Hunter, H. F. and Ballou, N. E. ADC-65.

fissile material. Since one plutonium atom is used per fission, there will be a residue of four atoms/fission after the explosion. But since (equation 1) we may assume that one atom of plutonium is created per fission by neutron capture in U^{238}, the dust particles from this type of explosion will contain five plutonium atoms per fission. If U^{235} is used as the fissible material, the content will be one plutonium atom per fission; this same figure has been found to hold for thermonuclear weapons.

2.3 Between 20% and 30% of the activity contained on the spherical particles collected from the smaller nuclear explosions has been found to be soluble in water. The mean figure for the particles from thermonuclear tests over the Pacific atolls is 55%.

3. MEASUREMENTS OF DUST CONTENT OF AIR

The atmosphere has been sampled in a routine manner for radioactive dust since 1948 by attaching air filters to the aircraft which carry out the standard meteorological flights from Aldergrove, Northern Ireland. The frequency of sampling has varied throughout the period from daily to three times per week. The aircraft fly for about eight hours on a triangular course over the Atlantic; the main flying on each sortie is done at 1700 feet and at 18,000 feet with occasional descents to sea level and thus a reasonable sample of the dust content of the lower atmosphere is obtained. Each plane carries a cylindrical filter of length six inches and diameter 3½ inches consisting of a mat of esparto grass paper inside a wire cage. The collection efficiency of this arrangement is about 100% for particles greater than 3μ but this drops to 70% for the finest sizes created by nuclear explosions.

The average amount of air sampled during each flight has been measured by comparing the activity on a cylindrical filter with the activity collected on a disc filter of the same material flown at the same time, the disc being mounted in a long tube containing an orifice plate so that the air flow could be determined during flight. In this way it has been estimated that each routine filter sample contains the active dust from 1530 kg. of air.

The air has also been sampled since early 1952 over successive 24 hour periods at ground level near Harwell by fitting standard cylindrical filters to a powerful blower which provides a daily throughput of 7400 kg. of air.

The beta activity of the filters is measured by mounting them coaxially over a calibrated cylindrical geiger counter type B12E. The wall of this counter is thick enough to eliminate effectively the soft beta radiations of Np^{239} and U^{237} and the system thus provides a measure of the fission product activity alone. The measured disintegration rate of a sample of known age can be expressed in terms of fission units by using the computed curves of Hunter and Ballou. The Np^{239} and U^{237} activities are determined separately by chemical analysis.

4. RESULTS

4.1 *General.*—When a nominal bomb is exploded in, say, Nevada, the dust cloud rises to a height of perhaps 10,000–12,000 metres and travels east with the prevailing westerly winds. As the cloud travels it diffuses both laterally and vertically and ultimately contaminates the lower atmosphere across a broad front at great distances. Although on the average the cloud from a Nevada explosion crosses U. K. on the fifth day after time of burst and thereafter every four to seven weeks as it circulates round the world, the behaviour of any one cloud can depart considerably from this average and to obtain a general idea of the way in which the concentration of dust varies with distance downwind or with time, it is convenient to consider the concentrations over U. K. of the fission products from a series of explosions whose times of burst are close enough together to allow them to be considered as a single event, at least throughout a period starting some three to four weeks after the mean time of burst.

The specific activity of air above U. K. in the period following the Russian and American tests in the autumn of 1951 has been plotted in Fig. 1A. In this diagram, time is measured from the mean date of the individual explosions, and the specific activity of the air, as determined from the Aldergrove flights, has been averaged over periods of approximately fourteen days to give a figure representative of a significant fraction of the circulation path of the air, and to smooth out short-period variations. In this presentation, earlier points are

omitted since the method cannot reasonably be used within the actual period when the explosions are occurring. A smooth curve has been drawn through the experimental points and it has been found by experience that the shape of this curve is quite characteristic of explosions of near nominal size which take place in the middle latitudes of the northern hemisphere. The curve has been corrected for the radioactive decay of the fission products according to a composite decay curve computed from the sum of the individual decay curves, giving the dotted line in Fig. 1 which can then be taken to represent the decrease with time of the specific air content of the dust itself or of the longer-lived isotopes.

The decrease with time of the specific dust content of the air over the U. K. can be due to increased spread of the cloud or to deposition or both. Vertical spread above 12,000 metres is prevented by the very low diffusion rates in the stratosphere (see below). There is also a barrier of stable air in the lower atmosphere (troposphere) reaching as far as 25°N of the equator which prevents widespread diffusion to the south. Finally, direct observations of the lateral spread of Nevada clouds by sampling planes based in Gibraltar and in Scotland suggest that lateral diffusion is virtually complete by the time the clouds have made one circuit of the world. All the points in Fig. 1 refer to clouds which satisfy this criterion and we must conclude that the dotted line in Fig. 1A represents the rate of removal of atmospheric dust from the atmosphere by deposition alone and that half the material is removed in approximately 22 days.

The behaviour of the clouds from thermonuclear explosions is quite different. The fortnightly averages of the specific air activity obtained from the Aldergrove flights following the Pacific tests in the spring of 1954 have been plotted in Fig. 1B. There is no sharp decrease of airborne activity such as was obtained after the Nevada explosions, and when the correction is put in for radioactive decay it will be seen that the concentration of dust has in fact been increasing throughout the ten months following the mean date of the explosions. The explanation is that a thermonuclear dust cloud penetrates into the stratosphere and reaches a height of the order of 30,000 metres soon after the explosion. Diffusion is a very slow process in the stratosphere and material returns to the lowest layers of the atmosphere at a much slower rate than in the case of the nominal bomb cloud. The gap in the record in Fig. 2 occurs during the period when the lower atmosphere was further contaminated by the explosion of several Russian nominal-sized bombs in the late Autumn of 1954. This contamination decreased in the manner shown in Fig. 1A for this type of bomb, enabling measurements to be resumed on the residual thermonuclear contamination in January and February, 1955.

Direct confirmation of the hypothesis that the thermonuclear explosion dust is held back in the stratosphere has been provided by a series of experiments conducted in the Autumn of 1954 and repeated in the Spring and Autumn of 1955. In these experiments the concentration of activity was measured at various heights in the atmosphere up to 15,000 metres. The mean concentrations, averaged over a period of two months in the Autumn of 1954, are shown in Fig. 2 which demonstrates quite strikingly the existence of a large accumulation of fine dust in the stratosphere several months after the tests. The origin of the dust was identified by chemical analysis of the fission products.

4.2 *Mean Concentration of Beta and Gamma Activity in Ground Level Air.*— The dust clouds from Nevada and Russia pass over U. K. on their first circuit of the world before they have diffused to ground level, so that the activity does not reach high values in the surface air over this country. The peak concentrations, averaged over 24 hours, are generally in the region of 5 dpm of beta activity per cubic metre of air, but individual peaks of 25, 15, 10 and 8 dpm/m^3 have been observed. Thermonuclear explosions produce smaller concentrations because of the slow rate of diffusion from the stratosphere, the highest observed figure being 0.5 dpm/m^3. The average concentration of activity from all bombs exploded within the period April 1952, to January 1956 was 0.5 dpm/m^3, and most of this came from explosions in Nevada. The mean concentration in the eight months following the 1954 Pacific thermonuclear tests was only 0.1 dpm/m^3. Although much of the material from the thermonuclear explosions has yet to reach ground level, the average over the next few years due to bombs already exploded is not expected to exceed this figure of 0.1 dpm/m^3.

The external gamma dose rate corresponding to 0.5 dpm/m^3 of fission product activity in air has been computed by applying Chamberlain's method[2] to the gamma energy spectrum of the fission products. Including the dose rate due to Np^{239} and U^{237} the total figure is 10^{-3} milliroentgen (mr) per year to a person fully exposed to the radiation.

It is useful to compare the figures just given with those for the natural radioactive content of air. The combined disintegration rate of the natural RaB and RaC in the atmosphere at Harwell, averaged over the period November 1950 to March, 1951 was 128 dpm/m^3. The corresponding gamma dose rate is about 0.4 mr per year. The Harwell values for the RaB and RaC disintegration rates in air are, on the whole, smaller than those found elsewhere; mean values of 220 dpm/m^3 and 470 dpm/m^3 have been found at Windscale[3] and Cambridge[4] respectively while much higher figures are implied by recent Radon measurements in London.[5] Moreover, under stable atmospheric conditions, the RaB and C content of the air near ground level may reach values greatly exceeding the mean levels. The highest individual peak in the Harwell observations was 1800 dpm/m^3 which is 14 times the average value; similar values have been observed by other workers.

4.3 *Mean Concentration of Sr^{90} and Pu^{239} in Ground Level Air.*—No routine measurements have been made of the concentration in air of any individual isotope but it is possible to estimate the figures for those with relatively long half-lives. This will be done for the important isotopes Sr^{90} and Pu^{239}.

The mean fission product content of the air in the period April 1952 to January 1956, derived by calculation from the observed beta activity, was 5×10^5 fissions per cubic metre. Using an apparent fission yield of 4%, which is not inconsistent with radiochemical evidence, it follows that the mean concentration of Sr^{90} in the air within this period was 4×10^{-16} μc/c.c. The I. C. R. P. occupational m. p. l. is 2×10^{-10} μc/c.c.[6]

An upper limit for the mean plutonium concentration has been calculated in a similar manner, by allowing five atoms of Pu^{239} per fission from an atomic weapon in the nominal size range and one atom per fission from thermonuclear weapons. The mean concentration over the same period is thus found to be 3×10^{-17} μc/c.c.; the I. C. R. P. occupational m. p. l. is 2×10^{-12} μc/c.c.

5. DEPOSITION OF RADIOACTIVE DUST: SAMPLING METHODS

Dust particles which are too small to have a significant falling speed in air are removed from the atmosphere in two ways:—

(i) by washout in rainwater

(ii) by dry deposition directly onto surfaces.

Continuous measurements have been made since early in 1951 of the fission product content of rainwater falling on roofs at Milford Haven, Pembrokeshire, and Chilton, Berkshire. The roofs have been coated with chlorinated rubber paint which is chemically inactive and provides a smooth surface over which the rainwater and the entrained dust flow readily. The water is passed through cylindrical esparto grass filters of heavier gauge than is used for air filtration. The filters are exposed for 24 hour periods during rain and, after removal, are dried and their activity measured on the standard counting assembly; a suitable interval of time is allowed for the short-lived natural Radon and Thoron daughter products to decay before counting is started. Allowance is made for the solubility of the material when deriving the true deposition figures from the measured counting rates; check measurements of the solubility are made at intervals during a particular series of explosions.

The problem of the direct deposition onto surfaces is a complex one and some aspects of it are now being studied by Chamberlain at Harwell.[7] Owing to the complexity of the earth's surface it is obvious that realistic measurements of the amount of radioactive dust from nuclear explosions deposited in this way would be impossible. But since Chamberlain's early work has shown that, under certain conditions, this mechanism of deposition can be as important as rainwater deposition, it cannot be lightly ignored. It is thought, however that in the cir-

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[2] Chamberlain, A. C. A. E. R. E. Report HP/R. 551.
[3] Dunster, H. J. Private Communication.
[4] Satterly, J., Phil. Mag. 16, 584 (1908).
[5] Anderson, W., Mayneord, W. V. and Turner, R. C., Nature, 174, 424 (1954).
[6] Recommendations of the International Commission on Radiological Protection. B. J. R. Supplement No. 6, 1955.
[7] Chamberlain, A. C. A. E. R. E. Report HP/R 1261.

cumstances peculiar to deposition from the fine dust clouds from atomic and thermonuclear explosions, deposition by rain is by far the more important factor. The reasons for this view are given in the following section.

It will be shown in para. 831 that on the average 80% of the integrated gamma dose recived by an individual in U. K. from radioactive material deposited by *rain* after a Nevada explosion is due to the deposition that takes place while the cloud is crossing U. K. for the first time. It has already been stated that the concentration in the air at ground level during this first passage is generally low, and, in fact, the transfer rate from from air to ground would have to be impossibly high before it could add significantly to the rain deposition. To investigate what happens on subsequent circuits of the cloud, when vertical diffusion is complete, an experiment was carried out in which the deposition by rain in four successive fortnightly periods was compared with the dry deposition onto a glass plate covered by a film of vaseline. The results obtained in the four periods were very consistent and showed that the deposition by normal rainfall was four times that due to dry deposition on the sticky surface. It was therefore concluded that since the rain deposition from the cloud during its second and subsequent circuits of the world was less than that during the first, the rainwater measurements alone should provide a fair measure of total deposition. In any case, the method of sampling adopted ensures that any radioactive dust deposited on the collecting roofs during dry spells is washed off by subsequent showers and included in the rainwater activity measurements.

The weighting factor in favour of rain deposition is even greater in the case of dust from thermonuclear explosions. Since the dust is fed slowly into the troposphere from the stratosphere and is subsequently removed by deposition, a gradient of activity is maintained in the lower atmosphere and the concentration near ground level is very small (Fig. 3). The rain brings down activity from regions where the concentrations are comparatively high and it is estimated that for these reasons, rainwater deposition exceeds dry deposition by a factor of about twenty.

6. DAILY DEPOSITION LEVELS AT CHILTON AND MILFORD HAVEN

The daily deposition records at Chilton and Milford Haven are similar in general although occasionally differing markedly in detail. All daily deposits of surface activity greater than 2 mc/km^2 observed between February 1951 and January 1956 have been arranged in groups and the frequencies of occurrence of the various groups are given in the following table:

Range of Values of Deposited Activity (mc/km^2)	Frequency of Occurrence	
	Chilton	Milford Haven
2-10	83	76
10-20	9	5
20-40	3	1
40-60	3	----------
60-80	1	----------
80-100	----------	1

The highest daily deposit at the sites was 73 mc/km^2 at Chilton and 93 mc/km^2 at Milford Haven; these occurred in heavy rain about the same time, some five days after the explosion of a weapon in Nevada in the Autumn of 1951. The highest deposition in a single day from a thermonuclear weapon test was 10 mc/km^2 at Chilton and 39 mc/km^2 at Milford Haven.

7. DEPOSITION FROM THE 1952 NEVADA TESTS

7.1 Before proceeding to the final stage of calculating the gamma dose from the fission products deposited to date, it is important to consider in some detail the deposition measurements from one particular series of explosions in order to check that the results are in fair agreement with expectation. It is particularly suitable to choose for this purpose the Nevada tests in the period April–June 1952 since the U. S. A. E. C. have published a paper [8] summarising

[8] Eisenbud, M., and Harley, J. H. Science 117, 141 (1953).

their own deposition measurements within the U. S. during this series and a useful comparison of results can be made.

7.2 *U. K. Measurements in 1952.*—The 1952 Nevada tests consisted of eight explosions between April 1st and June 5th, the mean date of burst being May 6th. During this period and up to the time in late November when the atmosphere was further contaminated by radioactive dust from a Pacific test, the usual daily measurements were made at Chilton and Milford Haven of the activity deposited in rainwater. The quantity to be computed, to simplify comparison with the U. S. results, is the cumulative value of the deposited activity on December 6th, i. e. seven months after the mean date of burst, when deposition was virtually complete. The approximate method used was to add up the total deposition for each individual month and to calculate the probable value of this on December 6th using the Hunter-Ballou decay curve for the fission products from U^{235}. The only refinement to this method was that each heavy deposition resulting from the passing over U. K. of a single identifiable cloud on its first transit of the earth was followed through individually on its own decay curve.

The calculations show that the activity deposited at Chilton and Milford Haven amounted to 6350 dpm/m² and 8200 dpm/m² respectively on 6th December, 1952. The lower figure at Chilton is due to the fact that during an important part of the deposition period the rainfall was much lower there than that at Milford Haven, where it was very close to the average for U. K. The higher figure may therefore be taken as representative for this country.

7.3 *Comparison of British and American Results in 1952.*—In the U. S. the dust is collected on horizontal sticky plates which have the property of retaining the dust brought down by rain in addition to that deposited directly from the air. A paper has been published[8] giving an analysis of the measurements made at a network of 121 stations within U. S. during the period following the 1952 Nevada etsts. The average values of the deposited fission product activity are given at several distances between 300 and 2,400 miles downwind from the test site. The quoted figures refer to the cumulative values of the deposited activity on 1st January 1953, but using the conversion figures given in the report, these have been adjusted to a reference date of 6th December 1952 and are plotted in Fig. 8. One additional point not included among the original figures has been derived for the distance of 17,000 miles by averaging the deposition figures obtained at several places on the U. S. West coast and shown in the map in Fig. 1 of the report. This additional figure will include not only the deposition of material which has travelled once around the world but also that due to any temporary "back-flow" from the test site, and it represents therefore an upper limit for deposition at 17,000 miles downwind.

The U. K. figures have been plotted for comparison in Fig. 8, and the agreement is seen to be fairly good. No rainfall figures are given in the U. S. report for the period in question, but the difference in the mean rainfalls of U. S. and U. K. is not large enough to invalidate the comparison.

7.4 *Check on the Order of Magnitude of the U. K. Figures.*—The deposition of active material from bomb explosions in Nevada is likely to be confined to the Northern Hemisphere since the diffusion of matter across the stable air barrier is slow and is not likely to reach significant proportions in the time required for the bulk of the material to be deposited. It is therefore possible to estimate the average ground surface activity on the assumption of uniform distribution.

The number of fissions in a nominal bomb explosion is approximately 2.4×10^{24}.[9] The Hunter-Ballou decay curve shows a disintegration rate of 3.5×10^{-7} dpm per fission at +7 months. The area of the Northern Hemisphere is 2.5×10^{14} m²

$$\text{Calculated specific ground activity}=\frac{3.5\times10^{-7}\times2.4\times10^{24}}{2.5\times10^{14}}$$

$$\therefore \text{ per nominal bomb at } +7 \text{ months} = 3350\, dpm/m^2$$

The observed representative figure was 8200 dpm/m² from eight bombs exploded. Two of the bombs were described in the Press as very small and it may

[9] The Effects of Atomic Weapons, U. S. Government Printing Office, Washington 25, D. C. (1950).

be assumed as an approximation that the fission product release during the series was equal to that from six nominal bomb explosions.

$$\therefore \begin{array}{l}\text{Observed specific ground activity} = 8200/6 \\ \text{per nominal bomb at } +7 \text{ months} = 1367 dpm/m^2\end{array}$$

The observed figure is therefore of the expected order of magnitude, and the U. K. deposition measurements thus show satisfactory agreement with both the U. S. result and with the calculated values.

8. COMPUTATION OF THE GAMMA RAY DOSE FROM DEPOSITED FISSION PRODUCTS

8.1 *General Procedure.*—The gamma ray dose received over a period of 50 years due to all bombs exploded before January 1956, will first be calculated for the case of a man standing on an infinite plane surface. This will be done by calculating the total dose to the man from each single deposition and summing the series. The figure thus obtained will be larger than the dose received in practice since the shielding provided by buildings against gamma rays and the removal of activity from the earth's surface by drainage and by weathering have been ignored. Suitable adjustments to take account of these factors will be considered later in the report.

8.2 *Method of Calculation.*—Consider first the dose rate at a point above a plane uniformly contaminated with fission products of some particular age. The fission products emit a large number of gamma rays whose energies and rates of emission can be obtained from the literature. The dose rate at the point can therefore be regarded as the sum of the dose rates from a number of uniform deposits each of which emits gamma rays of one energy only. But even this simplified problem has not been solved numerically for the general case owing to the complex nature of gamma ray scattering and absorption. However, Chamberlain [2] has devised an approximate method for estimating the dose rates at various heights above a plane for all gamma energies, and this has been shown in recent measurements at A. E. R. E.[10] to give an accurate result for the particular case of Co^{60} gamma rays. There is no guarantee that the method is equally accurate at all other energies, but the error cannot be large enough to vitiate the present calculations.

Dale, Kendall and McKendrick [11] have applied Chamberlain's method to the gamma rays emitted by the fission products of U^{235} and Pu^{239} and have computed the dose rates received by a man standing on an infinite plane uniformly contaminated with those products at times between one hour and three years. The two sets of dose rates do not differ by much and those for U^{235} have been used in the computations which follow; additional values have been worked out for times between 3 and 50 years. Some of the more important quantities derived from Dale's data in the intermediate stages of computations have been listed in Table II as they illustrate several features of the fission product gamma activity. Column 2 shows how the dose rate from a specified surface activity varies with the age of the fission products; thus the dose rate from a deposit of fission products one day old is four times as great as that from an equally active deposit of products three years old. Column 3 shows how the dose rate from a particular deposit varies with time; the decrease is due mainly to the radioactive decay of the fission products. Column 4 gives the total dose received within a period of 50 years from depositions which occur at various times after an explosion; these figures allow the integrated dose from any deposition of known age to be rapidly assessed. The increase of the figures with time is due to the fact that old fission products decay less rapidly than young and therefore, for a given initial surface activity, deliver a greater integrated dose. The last column shows the integrated dose between various times T and 50 years from a deposit whose specific activity at one day after time of burst is 1 c/km². This information has been plotted in Fig. 4 which demonstrates the rate at which the dose from a particular deposit is delivered. Since no deposition has yet taken place in U. K. until after the fourth day from the time of an explosion, the earlier points are of academic interest only. The graph shows that 50% of the total dose from a deposition occurring on the fourth day after an explosion is received within the following 26 days.

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[10] Gale, H. J. Private Communication.
[11] Dale, G. C., Kendall, R. A., and McKendrick, J. C., A. W. R. E. Report HER–H7/53.

8.3 *Results of the Dose Calculations—*

8.3.1 *Atomic Bombs of approximately Nominal Size.*—Since the deposition from these bombs is completed within a few months, the calculation of the integrated dose by the procedure outlined above is a simple matter. The calculations have been done in two stages. The integrated doses from the individual identifiable depositions from clouds crossing U. K. for the first time have been calculated by combining the surface activity of these deposits with the data given in column 4 of Table II. The dose from activity deposited subsequently is determined in a similar manner, but here the depositions have been summed over fortnightly periods and the effective age of the mixed products measured from the mean time of burst of the weapons. The results of the calculations are given in some detail below for the eight Nevada explosions in 1952.

	No. of Bomb	Integrated Dose within 50 years (μr)	
		Chilton	Milford Haven
Deposition from clouds on first circuit of world	1	12	17
	2	0	0
	3	76	20
	4	8	8
	5	114	6
	6	14	166
	7	13	39
	8	59	71
Total, first time round		296	327
Residual, from subsequent deposition		56	91
Total integrated dose		352	418

The resultant integrated doses at the two sites do not differ by much, but it will be observed that the main contributions at each place come from different bombs. About 80% of the total dose is produced by deposition from clouds crossing U. K. for the first time.

The results obtained from seven test series of similar type involving the explosion of some 60 weapons are summarised in the top part of Table III. The average integrated dose from the deposited activity is 66 μr per explosion, and on this basis an allowance has been made for another 15 explosions from which dust was probably deposited in U. K. but for which no measurements were made. The figures for the number of weapon exploded have been obtained from press reports. The British tests in Australia have been neglected as the clouds from these were not detected in the North Hemisphere. The integrated dose from all bombs in the nominal size range exploded up to January 1956, based on the mean of the measurements at Chilton and Milford Haven, is 4.9 mr.

8.3.2 *Thermonuclear Explosions.*—The computation of the integrated dose in U. K. due to dust from thermonuclear tests falls naturally into two parts. Firstly, there is that part of the dose arising from material which is already on the ground and secondly, that due to material which has yet to be deposited and is at present airborne.

The first part of the calculation is a repetition of that already carried out for the nominal weapons, but there are some differences worthy of note. The integrated dose is no longer dominated by the deposition which takes place in the first few days after burst. The rate of deposition of activity is a slowly decreasing function of time of the type shown in Fig. 1B, and the mean daily deposition one year after burst adds more to the integrated dose than that at any earlier time. Again, since the time scale of the deposition is long, the records are frequently interrupted when the lower atmosphere is temporarily contaminated by dust from nominal bomb tests and a certain amount of interpolation is necessary; this presents no serious difficulty since the deposition rates are slowly varying. The results of the computations for the thermonuclear test series in the Pacific in 1952 and 1954 are given in the lower part of Table III; the figures have been separated into those based on direct measurements of deposited activity and those based on interpolation.

In order to forecast the integrated dose from active dust still to be deposited, estimates are required of the total amount of activity in the atmosphere and of its rate of descent.

An estimate of the fission product content of the atmosphere above U. K. in September 1954 can be obtained by extrapolating the graph of Fig. 2 and integrating under the curve to obtain the total fission product content of an infinite vertical column of cross-section 1 square metre. This extrapolation has been done by producing the curve of Fig. 2 linearly to a height of 10mb—the estimated initial height of the cloud—and assuming the concentration above that level to be constant. The value of the integral thus determined is 3×10^{12} fissions/m^2. The deposition rates at Chilton and Milford Haven during the same period were equivalent to 4.7×10^{11} and 2.6×10^{11} fissions/m^2/year respectively, or 16% and 9% of the content of the column per year. The same integration carried out with data obtained in January and February 1955 showed that the content of the column had increased since September to a value of 10^{13} fissions; the deposition rate had risen in proportion and was 12% per year at both sites. Whether the increased concentration is a seasonal effect or represents a gradual downwind or lateral diffusion into the atmosphere above U. K. from regions of higher concentration cannot be answered confidently. It is hoped that further observations will help to clarify this point. The present view is that the increase may be due to the slow gravitational settling of the fine dust in the rarified air of the stratosphere and that the higher levels of the stratosphere may now be depleted of activity. On this hypothesis, the value of the integral obtained in January and February 1955 may well be an overestimate of the content of the column, but it will be accepted here as the basis of the remaining dose calculations. A rough check can be made on the magnitude of the figure. It is reasonable to suppose that by the beginning of 1955 the lateral spreading of the clouds from the 1954 Pacific explosions was complete (activity reached U. K. within 12 days of the explosion on March 1st) and that the air activity above U. K. was therefore a fair sample of the world distribution. The total fission content of the atmosphere can then be calculated by summing the content of the column above U. K. (10^{13} fissions/m^2) over the area of the surface of the earth (5×10^{14} m^2); the result is 5×10^{27} fissions in the atmosphere in February 1955. This number of fissions is associated with the release of 30 megatons of energy (in the conventional units, a megaton of energy is the energy derived from the explosion of a million tons of TNT). Nothing is known of the total energy released in the 1954 Pacific tests but a typical thermonuclear weapon is commonly suposed to have a power of about 10 MT. The computed figure of 30 MT, and by implication the air activity figure from which it was derived, is therefore of the right order of magnitude and in the absence of definite weapon information, no more than this can be claimed.

The second part of the dose calculation, then, is based on an atmospheric content of 10^{13} fissions per unit column in early 1955 from the 1954 Pacific tests. A small factor of safety is introduced by assuming that deposition takes place at the rate of 20%. The computed integrated dose from the activity yet to fall is then 20.3 mr. Observations of deposition from the 1952 tests were too frequently interrupted for a similar calculation procedure to be adopted. It has been found experimentally, however, that at comparable times after burst the air concentrations and deposition rates from the 1952 and 1954 test series are in the ratio of 1:3.6. The residual integrated dose given in Table III for the 1952 tests is based entirely on the calculations for the 1954 series, using this ratio.

The figures for the Russian thermonuclear explosion in 1955 are based on the Russian statement of the power of the airburst bomb as being in the megaton range.

8.3.3 *Estimate of a Realistic Integrated Dose for U. K.*—The estimated total dose received by a hypothetical inhabitant of the U. K. standing on an infinite plane is about 35 mr in the course of 50 years from all explosions up to December 1955 (Table III); thermonuclear explosions account for about 30 mr of this total. In practice, only a small fraction of this estimated dose will be received by the average inhabitant of this country. Much of the activity brought down by rain will be washed into drains, particularly in built-up areas, or washed down crevices in the open country. This is particularly true of the activity from thermonuclear explosions, 55% of which is soluble. A factor of three to take account of this aspect would appear to be conservative. Much protection is provided by ordinary buildings against the gamma rays from the fission products which do remain in the surface of the ground. Measurements have recently been carried out at A. E. R. E. of the protection provided by a conventional semi-detached house against a uniform deposit of Co^{60} on the ground.[13] The average

[13] Stewart, N. G., Crooks, R. N., Chisholm, J. M., Gale, H. J., A. E. R. E. Report HP/R. 1782.

protection factor was 20. It can be shown by calculation that the gamma rays from Co^{60} are suitable substitutes for fission product gamma rays in this type of experiment. The protection afforded by multi-storey buildings could be greater than this but, to remain conservative, we choose a representative protection factor of seven based on the occupant of a semi-detached house who spends an average of 2½ hours per day out-of-doors. This brings the overall protection factor to 21. A more realistic estimate, therefore, of the total dose to be received within the period 1945-1995 by the average inhabitant of this country due to deposition from all bombs exploded up to January 1956 is 1.7 mr.

It is useful to calculate the probable dose per generation on the assumption that nuclear weapons continue to be exploded at the present rate for a very long time. Under these conditions, the dose within any 30-year period will build up to a flat maximum several generations hence. For the purpose of this calculation we assume, arbitrarily, that all the fission products released up to 31st December, 1955 were released within a period of three years. It can be shown rigorously that if the dose to *infinity* from a single year's explosions is X mr, then the 30-year generation dose in the equilibrium state will be 30 X mr. The calculations summarised in Table III have been extended to include the period between 50 years and infinity, and the total dose to infinity from all bombs fired to date is then found to be 56.5 mr for a person in the open. On the argument just described, it follows that the ultimate generation dose will be 565 mr under the same conditions. For the average individual (protection factor of 21) the estimated generation dose will be 27 mr.

9. DEPOSITION OF SR^{90} AND PU^{239}

9.1 *Levels of Sr^{90} and Pu^{239} on U. K. Soil.*—Since May 1st 1954, rainwater samples have been obtained at Milford Haven by collecting the rain falling within successive three-weekly periods in a polythene funnel of area 0.22 m². These samples have been analysed radiochemically for Sr^{90} by R. G. D. Osmond at the A. E. R. E. Woolwich oustation, and the cumulative curve of deposition of Sr^{90} is shown in Fig. 5 in units of milicuries of Sr^{90} per square kilometre. The level of 0.77 mc/km² in April 1954 has been deduced from previous gross fission product measurements, using an Sr^{90} yield of 4%. The mean rate of fall over the period May 1954–March 1956 is 2.3 mc/km².

Using our figure for the mean rate of deposition of stratospheric dust—12% per year—this implies that the ground content of Sr^{90} due to bombs exploded before January 1956 will pass through a maximum of 14 mc/km² in 1968. If the present rate of firing continues indefinitely, the Sr^{90} will build up to an equilibrium ground value of about 0.2 C/km².

By way of comparison, Eisenbud[13] has reported that the top foot of soil in U. S. contains an average of 0.4 C/km² of Ra^{226} and a similar figure is expected to hold for the soil in this country.

The amount of Pu^{239} deposited in the U. K., deduced from the gross fission product measurements, is found to be 0.08 mc/km² in December 1955. This is expected to rise to a value of 0.4 mc/km² from bombs exploded up to the end of 1955. If the present rate of firing continues, the plutonium content of the soil will increase steadily and will reach a value of about 0.1 mc/km² for each year of firing.

9.2 *Levels of Sr^{90} and Pu^{239} in Rainwater.*—The mean concentration of Sr^{90} in Milford Haven rainwater between December 1952 and December 1955 was 1.7 $\mu\mu$c/litre. The mean concentration in 1955 was 3 $\mu\mu$c/litre and if the present rate of firing continues indefinitely an equilibrium level of 8 $\mu\mu$c/litre will be reached in about 10 years time. The concentration of Sr^{90} in drinking water is likely to be much below that in rainwater because of filtration and adsorption in soil. The occupational m. p. l. for Sr^{90} in water is 800 $\mu\mu$c/litre.[6]

The average concentration of Pu^{239} in rainwater during the three year period ending December 1955 has been computed from the gross fission product deposition figures, the value being .03 $\mu\mu$c/litre. An equilibrium value of .08 $\mu\mu$c/litre will be reached in ten years time if firing continues at the present rate. The occupational m. p. l. for Pu^{239} in water is 3000 $\mu\mu$c/litre.

10. THE PROBLEM OF C^{14}

From the official data on the nominal bomb given in the Effects of Atomic Weapons ([9]) it is safe to assume that, of all the neutrons which are not used

[13] Hearing before Joint Committee on Atomic Energy of U. S. Congress, April 15th, 1955.
[14] Libby, W. F., Radiocarbon Dating, Univ. of Chicago Press.

up in the fission chain reaction, not more than one escapes into the air for every fission that takes place. Most of these escaping neutrons are ultimately captured in Nitrogen to form 5600 year C^{14}. As an upper limit we may therefore presume that one atom of C^{14} is formed per fission. This implies that about 300 curies of C^{14} are formed in a nominal bomb explosion. The estimated amount of 28 year Sr^{90} formed at the same time by fission is 2500 curies. But since the m.p.'s for C^{14} in air and water are greater than those for Sr^{90} by factors of 7×10^3 and 10^5 respectively (9), it would appear that the amount of C^{14} in the air presents a relatively minor hazard.

The long-term effect can best be studied by comparing the amount of C^{14} created in nuclear explosions with that which is naturally present in the world as a result of cosmic ray reactions. It follows from our previous assumptions that the present rate of firing is adding no more than .07% annually to the natural world content of C^{14}. (14) The importance of this may be gauged from the fact that the natural C^{14} in the world contributes about 1% of the normal background dose to the human body.

11. SUMMARY

The size distribution of the radioactive dust particles in the cloud from a nuclear explosion depends both on the power of the weapon and on the manner in which it is exploded, whether on the ground, on a tower or high in the air. The larger particles fall to earth relatively close to the site of the explosion, but in every type of burst a considerable fraction of the total radioactivity generated is contained in fine dust particles whose rate of fall under gravity is small and which may therefore remain airborne long enough to diffuse widely throughout the atmosphere and affect large areas of the earth's surface. These particles are removed from the atmosphere mainly by rain.

The most important radioactive components of the dust are the numerous fission product nuclei which decay at various rates and emit both beta and gamma rays of a wide range of energies. As time proceeds the shorter-lived isotopes in a representative sample of fission products decay into insignificance in comparison with those of longer half-life, and after about eight years nearly all the gamma activity stems from the single isotope Barium 137 daughter of Caesium 137 which has a half-life of 30 years. The gamma rays from fission products suspended in the air or deposited on the surface of the ground add to the general level of background radiation and increase the external radiological dose to the human body. Some of the individual isotopes may enter the human body through biological chains and produce internal effects. The most important of these is generally considered to be 28-year Strontium 90 which follows Calcium in biological systems and is deposited in bones from which it is removed very slowly.

The amount of activity deposited in rain has been measured at Chilton, (Berkshire) and Milford Haven (Pembrokeshire) since January 1951, by passing rainwater through filters whose radioactive content is then measured on a conventional Geiger counter assembly. There is no significant difference between the aggregate results obtained at the two sites. An example, given in some detail in the report, shows that the measured total deposition in the United Kingdom from the 1952 test explosions in Nevada is in good agreement with the very full data which the United States have published for the series.

The atmosphere between 500 metres and 5500 metres above the U. K. has been sampled for radioactive dust about three times weekly since 1948 by attaching air filters to the aircraft which carry out routine meteorological flights from Northern Ireland. In addition, the air at ground level near Harwell has been monitored continuously since early 1952.

The records reveal one clear and important difference between the clouds from weapons in the nominal size range (similar to that exploded at Nagasaki) and those from the large thermonuclear explosions in the Pacific. It is found that the concentration of fine dust in the atmosphere from nominal size explosions decreases in a regular and characteristic manner with time and that half the dust is removed by deposition in 22 days. The fine dust from a thermonuclear explosion, on the other hand, enters the stratosphere soon after burst and from there it diffuses downward so slowly that only 10–20% of the dust is deposited annually. Much of the activity from the 1952 and 1954 thermonuclear tests in the Pacific is therefore still in the stratosphere, and in order to determine the possible effects of these explosions in the U. K. it is necessary to form an estimate of this amount. This has been achieved by extrapolating the results obtained from systematic surveys of the radioactive content of the air between

ground level and 15,000 metres carried out in September 1954, February 1955 and September 1955.

The gamma ray dose from each individual deposit of fission products has been calculated initially for the idealised case of an individual standing on an infinite flat plane. A protection factor of three has then been introduced to take account of the material which is washed into drains or is otherwise removed from the topmost layer of the earth's surface. This factor is believed to be conservative, since about one half of the material already deposited has been found to be soluble in water. An additional factor of seven has been allowed for the shielding provided by buildings against the gamma rays from fission products. This figure has been derived from measurements carried out at A. E. R. E. with the gamma rays from Cobalt 60, and is based on the assumption that the average individual spends 2½ hours daily out-of-doors.

The individual doses have been summed, and the total external dose to be received by the average inhabitant of the U. K. due to material deposited on the ground from bombs exploded before 1st January 1956 is estimated to be 1.7 mr. About 70% of this dose is associated with material which has yet to be deposited. The dose due to debris suspended in the air near ground level is negligible by comparison. It has also been estimated that if weapons continue to be fired at the present rate for an indefinite period, the ultimate dose per individual per generation will be 27 mr; this level will be reached in approximately 100 years.

The concentration of Sr^{90} in the ground in the U. K. has been obtained from radiochemical analysis of the rainwater falling at Milford Haven. Based on the results of this analysis an estimate has been made of the possible future levels of Sr^{90} in U. K. soil should weapon testing continue at the present rate for an indefinite period.

All the important computed figures are given in the table at the end of this summary.

Summarised results

Gross Fission Product Activity in Air		
1. Mean conc. of activity in ground level air (1952–1955).	0.5 dpm/m^3	Mean activity due to the natural RaB and RaC in atmosphere (measured at Harwell) = 128 dpm/m^3.
2. Peak conc. in same period	25 dpm/m^3	Peak value = 1,800 dpm/m^3.
3. Mean Gamma dose rate to a person fully exposed to this radiation.	10–3 mr/year	
Gamma Dose from Deposited Activity		
1. Estimated dose to the average person in U. K. in a period of 50 years from all bombs exploded before 1.1.56.	1.7 mr	
2. Estimated equilibrium generation dose, reached in about 100 years time.	27 mr	
Strontium 90		
1. Mean conc. in air (1952–1955).	$4x10^{-16}\mu c/cc$ ($2x10^{-6}$mpl[1])	1. Mean conc. in drinking water is likely to remain well below that of rainwater because of filtration and absorption by soil.
2. Equilibrium conc. in air (reached in 10 years[2]).	$2.4x10^{-15}\mu c/cc$($1.2x10^{-5}$mpl)	
3. Mean conc. in rain (1952–1955).	1.7 $\mu\mu c$/1 (.002 mpl)	2. Top foot of soil contains about 400 mc/km^2 of Radium 226, an element with similar chemical properties to Strontium.
4. Equilibrium conc. in rain (reached in 10 years).	8.0 $\mu\mu c$/1 (.01 mpl)	
5. Conc. on ground on 1.1.56	4.5 mc/km^2	
6. Max. conc. on ground due to bombs already exploded (this will be reached in 1969 approx.).	14 mc/km^2	
7. Equilibrium value, reached in approximately 100 years time.	200 mc/km^2	

[1] The m. p. l's used are the occupational m. p. l's recommended by the I. C. R. P.

[2] Equilibrium figures are those which will ultimately apply if the present rate of weapon firing continues indefinitely.

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TABLE I.—*Principle components of fission product beta activity at various times*

Mass No.	Isotopes	Percent of Total Activity at Various Times after Fission								
		4d	10d	20d	50d	100d	1y	2y	5y	10y
38	Sr^{89}			5	9	10				
	Sr^{90}							5	15	22
39	Y^{90}							5	15	22
	Y^{91}			6	10	13				
40	Zr^{95}			6	11	15	7			
41	Nb^{95}				9	20	15			
42	Mo^{99}	12	7							
44	Ru^{106}				7	7				
45	$^{1}Rh^{106}$				7	7				
52	Te^{132}	8	5							
53	I^{131}	5	7	6						
	I^{132}	8	5							
54	Xe^{133}	10	11	6						
55	Cs^{137}							5	12	18
56	Ba^{137}							5	12	18
	Ba^{140}	6	10	12	7					
57	La^{140}	5	12	14	8					
58	Ce^{141}		6	10	11	8				
	Ce^{143}	8								
	Ce^{144}					7	27	30	8	
59	Pr^{143}	5	10	12	8					
	Pr^{144}					7	27	30	8	
61	Pm^{147}						6	13	22	16
Total activity (dpm/10^8 fissions)		3, 200	1, 200	620	250	110	13	4. 7	1. 4	0. 8

TABLE II.—*Basic data on dose rates from deposited fission products*

[A: Deposit of Specific Activity 1 c/km² measured at time T; B: Deposit of specific activity 1 c/km² measured at 1 day]

1	2	3	4	5
Time measured from instant of explosition T	Dose rate in μr/day from deposits A or B		Integrated Dose in μr between T & 50 years from deposits A or B	
	A	B	A	B
1 day	25	25	91	91
2 days	24	9. 3	188	75
3 days	23	5. 8	274	68
4 days	23	4. 2	349	63
5 days	23	3. 2	436	60
7 days	22	2. 2	556	54
10 days	22	1. 5	699	48
15 days	23	1. 1	896	42
20 days	23	0. 81	1, 030	37
30 days	24	0. 58	1, 260	30
50 days	20	0. 27	1, 600	22
100 days	17	0. 10	2, 400	15
200 days	17	0. 034	4, 320	9
1 year	12	0. 009	8, 340	6
2 years	6. 9	0. 0019	17, 200	5
3 years	4. 6	0. 00073		4
5 years	5. 0	0. 00046		4
10 years	5. 8	0. 00031		3
50 years	8. 9	0. 00015		

TABLE III.—*External gamma doses over 50-year period to an unprotected person in the U. K.*

Explosion Series	Cumulative Dose in milli-röntgen					
	Chilton			Milford Haven		
	1st time round	Residual	Total	1st time round	Residual	Total
Nevada, Spring 1951	0.06	0.10	0.16			0.16
Nevada and Russia, Autumn 1951	1.05	.10	1.15	1.06	0.10	1.16
Nevada, Spring 1952	.30	.06	.36	.33	.09	.42
Nevada, Spring 1953	1.13	.32	1.45	.35	.31	.66
Russia, Autumn 1953	.29	.09	.38	.25	.09	.34
Russia, Autumn 1954	.38	.18	.56	.32	.25	.57
Nevada, Spring 1955	.20	.08	.28	.19	.03	.22
Total from above	3.41	.93	4.34	2.50	.87	3.53
Estimated dose from other explosions in same size range			1.0			1.0
Total Integrated Dose from explosions in the nominal range of sizes			5.34			4.53
Pacific, 1954:						
Deposition 13.3.54–16.9.54			.92			.82
Interpolated 20.9.54–6.1.55			.92			.92
Deposition 6.1.55–15.2.55			.38			.61
Extrapolated (para 8.3.2)			20.30			20.30
Pacific, 1952:						
Deposition 31.10.52–20.3.53			.16			.16
Residual, based on comparison with Eniwetok, 1954			6.10			6.10
Russia, 1955:						
Estimated, based on size as announced			1.0			1.0
Tota Integrated Dose from thermonuclear explosions			29.78			29.91
Total Integrated Dose from all explosions prior to 1st January 1956			35.1			34.4

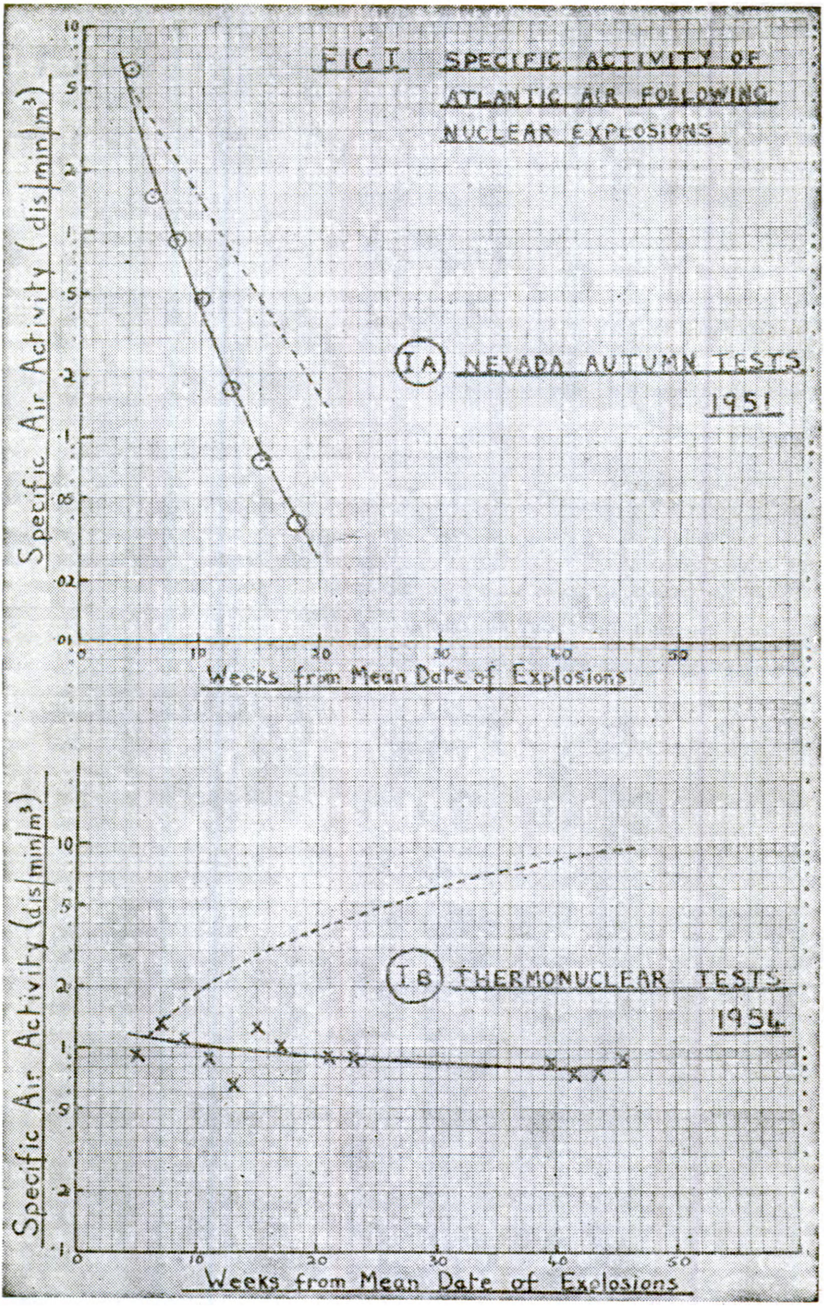
FIG I
SPECIFIC ACTIVITY OF ATLANTIC AIR FOLLOWING NUCLEAR EXPLOSIONS
IA NEVADA AUTUMN TESTS 1951
Specific Air Activity (dis/min/m³)
Weeks from Mean Date of Explosions
IB THERMONUCLEAR TESTS 1954
Specific Air Activity (dis/min/m³)
Weeks from Mean Date of Explosions

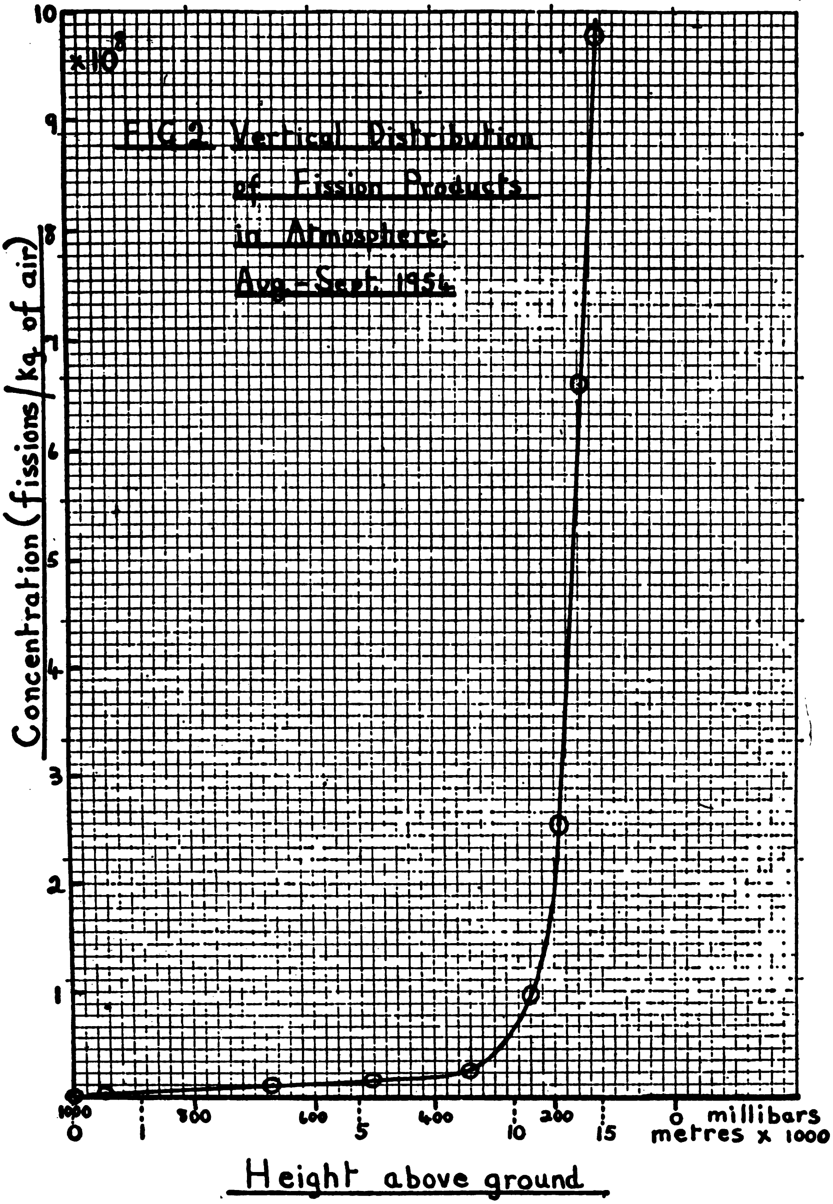
FIG. 2. Vertical Distribution of Fission Products in Atmosphere. Aug.-Sept. 1956
Concentration (fissions/kg. of air)
×10^8
10
9
8
7
6
5
4
3
2
1
1000
800
600
400
200
0 millibars
0
1
5
10
15
metres x 1000
Height above ground

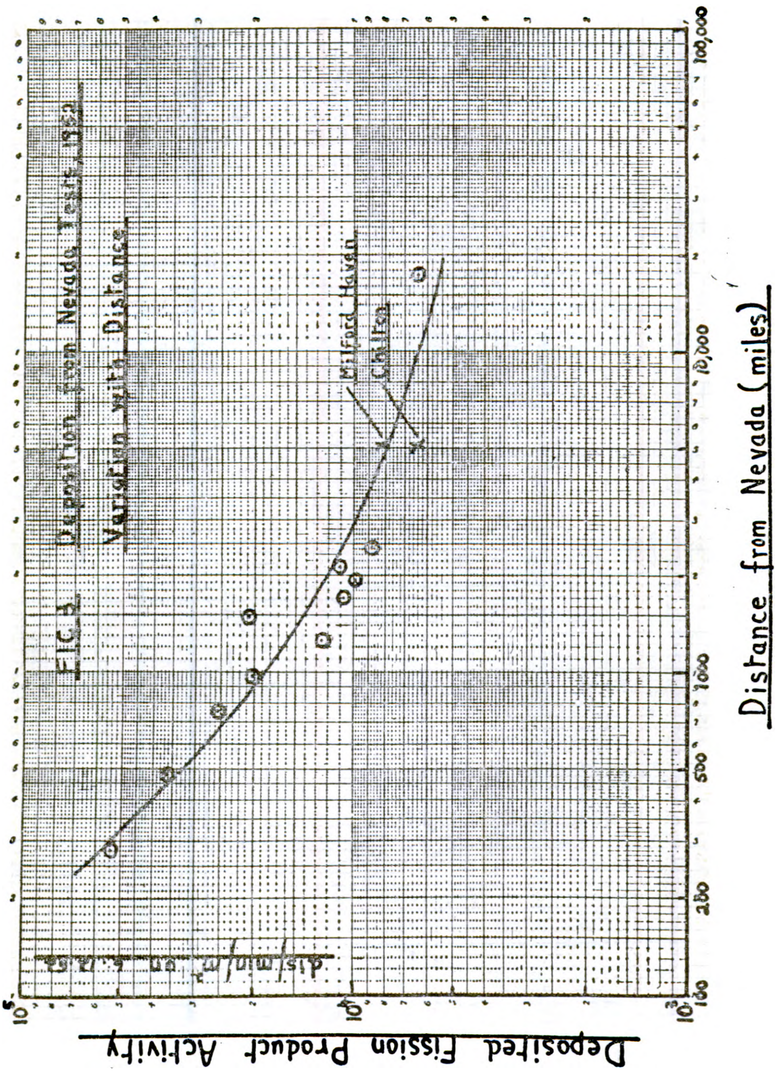
FIG. 3
Deposition from Nevada Tests, 1952
Variation with Distance
Milford Haven
Chiltern
Distance from Nevada (miles)
Deposited Fission Product Activity
100
250
500
1000
10,000
100,000

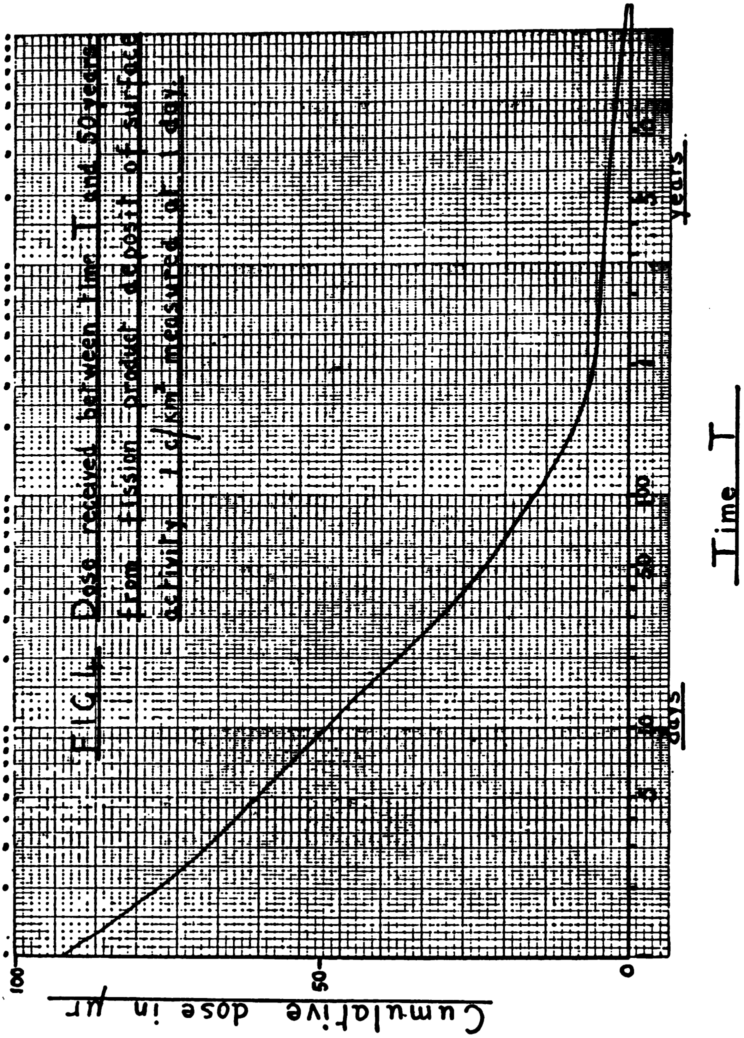
FIG. 4 Dose received between time T and 50 years
from fission product deposit of surface
activity 1 c/km² measured at 1 day.
Cumulative dose in μr
100
50
0
Time T
5
10
days
50
100
1
5
10
years

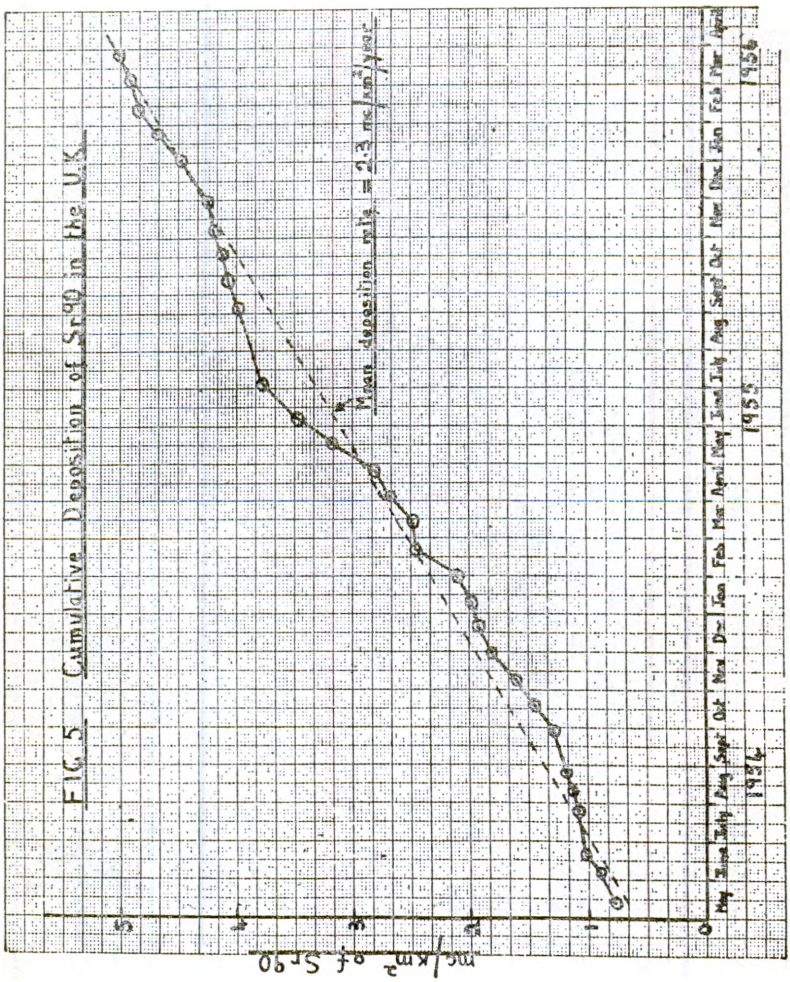
FIG. 5 Cumulative Deposition of Sr90 in the U.K.
Mean deposition rate = 2.3 mc/km²/year
mc/km² of Sr90
0
1
2
3
4
5
May June July Aug Sept Oct Nov Dec Jan Feb Mar April May June July Aug Sept Oct Nov Dec Jan Feb Mar April
1955
1956

A. E. R. E. HP/R 2056

Radiostrontium Fallout in Biological Materials in Britain

By R. J. Bryant, A. C. Chamberlain, A. Morgan, and G. S. Spicer, Chemistry and Health Physics Divisions, A. E. R. E.

1. INTRODUCTION

The general problem of the nature and effects of fallout of radioactivity from nuclear weapon tests has been considered by the Medical Research Council (1956) in Britain and the National Academy of Sciences (1956) in the U. S. Details of the physical geographical and meteorological aspects have been given by Eisenbud and Harley (1953, 1955, 1956) Stewart et al. (1955 & 1956) and Libby (1956). Libby (loc. cit.) and Martell (1955 and 1956) have described the U. S. work on the analysis of Sr^{90} in soils and biological tissues. Booker et al. (1956) have issued a preliminary report on the early results of the British work, and Bryant, Packman and Spicer (1956) have given an outline of the analytical method used at Woolwich for strontium analysis.

A variety of units have been used for reporting fallout data. It is best to use metric units where possible. The specific activity of Sr^{90} relative to calcium is expressed in micromicrocuries Sr^{90} per gram calcium and denoted by the letters S. U. Thus, in soil, vegetation, milk or bone:—

$$1 \text{ S. U.} = 10^{-12} \text{ curies } Sr^{90} \text{ per gram Ca}$$

The following physical data on the long range fallout from tests are relevant to an understanding of the uptake and distribution in biological tissues.

(i) All nuclear explosions produce Sr^{90} together with other fission products. The fission product activity is usually roughly proportional to the power of the explosion. Since Sr^{90} and Sr^{89} have gaseous precursors, they are not necessarily produced in the same physical form as the bulk of the fission product activity, and their distribution as between local and long range fallout may be different.

(ii) The physical form and distribution of the debris depends on the mode of firing and the power. In general, H bomb fission products are distributed in smaller particles, are found higher in the atmosphere, remain there longer and fallout more uniformly over the earth's surface than A bomb fission products.

(iii) The greater part of the cumulative fallout to date (1956) has been due to H bomb tests. The ratio of H to A bomb fallout is higher in the U. K. than the U. S. The ratio will increase in both countries with time, since much of the activity from H bombs already exploded yet remains to fall to earth, having a mean hold-up time in the stratosphere of the order of a decade. Fallout over temperate latitudes in the Northern Hemisphere is grossly uniform except for an excess within a few thousand kilometres distance from test sites.

(iv) Most of the fallout occurs in rain.

2. OBJECTIVES OF PRESENT WORK

Immediate objectives:

(i) Development and verification of accurate and economical analytical methods for low Sr^{90} levels in soils and biological materials.

(ii) Determination of the cumulative deposited activity, by total strontium analysis of soils, for comparison with estimates from rainfall and other methods.

(iii) Determination of current levels in human and animal tissues and food, on a sufficiently wide statitical and geographical basis.

(iv) Observations of the trend of the above with time.

Long term objectives:

(v) Investigation of the natural history of Sr^{90} in soils, and the uptake from soils to plants.

(vi) Investigation of the foliar retention of fallout by plants.

(vii) Investigation of the metabolic chain:—plants—animal tissue—milk—human tissue.

The practical importance of objectives (v) and (vii) is that this information is needed to enable present levels to be extrapolated to future conditions.

8. SAMPLING METHODS

The British sampling was begun in 1954 in consultation with Dr. L. T. Alexander of the U. S. Dept. of Agriculture. The sampling was and largely still is done by the regional staff of the Ministry of Agriculture, Fisheries & Food.

The two most important factors influencing the levels in biological materials were originally thought to be the amount of rainfall, as affecting the total fallout, and the soil conditions, as affecting the uptake from the soil to plants. It was not realised until later that foliar retention of Sr^{90} might be the dominant factor, at least on some soils. It was decided to use the sheep for sampling animal bone, on the grounds of cheapness and range of habitat.

Five farms were chosen, two in Suffolk with low rainfall and calcareous soil, and three in Wales with high rainfall and well leached acid soil. Details of these sites are given in Appendix A1, numbers 1 to 5.

All the farms carried sheep grazing permanent pastures, but the system of management of hill and lowland sheep differs. On the hill farms in summer the animals range over the mountains, where the grazing areas are often small patches of grass on hillsides interspersed with bracken. The mountain pastures are usually not limed, and may be very deficient in calcium. In the winter the sheep may be brought down to the valleys, or sent away to other areas. Some hay or other supplementary feeding may also be given.

The management of lowland sheep is different. Their grazing is usually controlled so that an area is cropped and then left to make new growth. In winter they may be folded over roots or beet tops, or given hay.

It will be seen that the amounts of rainfall, types of soil and systems of grazing the sheep are all interrelated. It will not be possible to ascribe differences in the levels of Sr^{90} in hill and lowland sheep to one factor or the other, until the underlying processes are understood.[1]

The 1954 sampling system called for soil, vegetation and sheep bone samples taken in the spring or summer. The soil samples were in fact taken from the areas grazed by the sheep—in the hill areas from the mountain pastures—but the vegetation was from hay fields. This had two effects.

(a) Hay in Wales is grown in the mountain valleys where the soil is less acid than on the hills, so that the soil conditions were not the same as for the sheep and soil samples.

(b) Hay in Suffolk is exposed to foliar absorption during the whole of the spring growing season (2 to 3 months) whereas the grazing cycle on the pastures is 2 or 3 weeks.

Thus, for different reasons, the hay samples in neither Wales nor Suffolk were representative of what the sheep were eating during the summer.

The difficulties in arranging a simple but representative sampling system are apparent. In the circumstances it is perhaps surprising that the relative activity in British samples (i. e. the ratio of vegetation; sheep bone; milk; human bone) agrees as well as it does with that found elsewhere, (para. 7.6 below).

In the 1956 routine sampling procedure, of which details are given in Appendix A2, the difficulties (a) and (b) above will be overcome by ensuring that the vegetation samples are from the same plots as the soil samples, and that the period of exposure to foliar retention of activity of the samples is similar to that of the actual pasture. It will not however be possible to distinguish the effects of rainfall, soil type, and grazing systems independently.

The decision to take samples from Welsh hill areas has introduced many difficulties, but it has been valuable in throwing up at an early stage the degree of variation in results which must be allowed for in any policy decisions affecting fallout.

Not much progress can be made with the long term objectives of the work until the relative importance of foliar and root uptake is understood. To study this, 4 sampling positions have been selected within 15 miles of Harwell, with approximately the same rainfall, but on differing soil types. At each site grass is cut from the same area every 3 weeks, and the activity compared with the fallout in rain measured over the same period. It is also planned to do growth experiments using soil from these areas. The results from these investigations are not yet complete.

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[1] Had cattle been used instead of sheep there would have been the same sort of problem, though not perhaps in so acute a form. Milk cattle are rarely nowadays grazed on unimproved soils, but the systems of management differ considerably in different areas. Moreover dairy cows are often given cattle cake or other foods not grown on the farm.

Milk sampling in the U. K. has so far been confined to a regular sample of dried, skimmed milk from a factory at Yeovil, Somerset. Use of factory dried milk has the disadvantage that it is not referable to any particular pasture, but the advantage that the bulking process smooths out variations from farm to farm. The samples are now being extended to other areas.

4. ANALYTICAL METHODS

4.1 *Methods used in Woolwich laboratories of Chemistry Division A. E. R. E.*—All methods ultimately depend on the separation of active strontium, with added carrier, as nitrate in strong nitric acid solution and include ferric hydroxide and barium chromate scavenges to remove contaminating activities. The separated strontium is stored in the presence of yttrium carrier for at least 14 days and the yttrium precipitated as hydroxide. The yttrium hydroxide is then converted to oxalate for mounting and counting.

Strontium is precipitated as carbonate, mounted and counted after allowing the yttrium to grow in again.

The method described above can be applied directly to bone ashes but for hay and vegetation ash and soil the initial concentration of the calcium and strontium in a suitable form (carbonates or phosphates) is necessary.

Full details of the methods are given in Appendix B.

Appendix B1. The determination of radiostrontium in soil by hydrochloric acid extraction.—This method is aimed at determining the total activity and carrier is therefore added at the extraction stage. The original work was done using 3M acid but 6M is now preferred. Using 3M acid, extraction of calcium tended to be low in some cases although there is no evidence that the total activity was not determined.

Calcium and strontium are precipitated as oxalates from the extract, any iron and aluminum removed as hydroxides and the calcium and strontium finally precipitated as carbonates to which the nitric acid method is applied.

Appendix B2. The determination in soil by fusion with sodium hydroxide and sodium carbonate.—Initial attack with sodium hydroxide and subsequent addition of sodium carbonate is preferred to fusion with sodium carbonate alone as a lower temperature can be used, the attack on the crucible is less and the melt readily disintegrates in water. After removal of silica from the insoluble carbonates, the method is essentially similar to that applied to the acid extract. Carrier is added to the soil before fusion. The maximum amount of sample which can be conveniently dealt with by this method is 100 gm. whereas 500 gm. can be used in the acid extraction.

Appendix B3. The determination in soil by ammonium acetate extraction.—As the aim here is to determine strontium available to the plant, carrier is not added until the extract is separated from the soil. Calcium and strontium are precipitated as carbonates which are treated as in the acid extraction method.

Appendices B4 and 5. The determination in animal and human bone ashes.—The nitric acid separation is applied directly. In the case of human bones where the amount of sample is limited and the activity low it is impracticable to measure the separated yttrium and the strontium + yttrium count is used. In a few favourable instances it has been possible to check the accuracy of this procedure by counting the separated yttrium.

Appendix B6. The determination in dried milk.—Direct application of the nitric acid separation usually gives low strontium yields. The calcium and strontium carrier are therefore concentrated by an initial phosphate precipitation.

Appendix B7. The determination in vegetable ash.—The ash is treated for the removal of silica and calcium and strontium precipitated as phosphates before the application of the nitric acid method.

Chemical yields for strontium vary from 60–80% for bone, vegetable and milk ashes and high calcium soils and from 40–60% for low calcium soils.

The yttrium yields are about 95%.

Work on this project is done in laboratories remote from others dealing with activity and specially reserved for this purpose. Frequent blanks are carried through the procedures to check the reagents, carriers etc.

All results obtained using these methods in the Woolwich laboratories of Chemistry Division will be described as Woolwich (or W) results.

4.2 *Analytical Methods used in the Health Physics Laboratory, A. E. R. E. Harwell.*—In this laboratory the determination of radiostrontium is at present confined to vegetation, animal bone and milk samples.

The chemical methods used are based on the same principles as those used at the Woolwich Outstation and only differ in minor details. They are described briefly below:

(a) The determination in ashed vegetation.

The calcium and strontium are extracted from the ashed vegetation by three successive leachings with hot 6M hydrochloric acid. The iron and aluminum are removed from the combined leachings as the hydroxides, after which the calcium and strontium are precipitated as the carbonates and purified by precipitation. Strontium carrier is added to the purified carbonates before the fuming nitric acid separation of strontium from calcium. Subsequently other radio isotopes are removed by barium chromate and yttrium hydroxide scavenges and the purified strontium stored with yttrium carrier for 18 days. Finally the yttrium is separated, mounted as the oxalate and the decay followed. The strontium is precipitated as the carbonate, weighed, mounted and the decay followed after the yttrium has grown in, in order to determine the $strontium^{89}$.

(b) The determination in bone ash and milk ash.

These are both treated in the same way and after the addition of strontium carrier, the fuming nitric acid procedure is applied directly. Otherwise the methods are identical with that for ashed vegetation.

The counting methods are identical with those used by the Woolwich Laboratories and described in the next section.

The counters were calibrated independently and interchange of sources with Woolwich has shown good agreement.

5. COUNTING METHODS (WOOLWICH LABORATORY)

Precipitates for counting are filtered on 2.1 cm. filter papers in perspex filter holders. After suitable washing and drying the papers are mounted on aluminium trays using a dilute solution of Gelva (polyvinyl acetate resin).

Three types of counter are in use.

(a) An anti-coincidence set-up with special low background G.M.4. counting tubes (7mgm/cm^2 window) surrounded by a complete ring of 9 Type G.E.21 brass bodied guard tubes. The assembly is shielded by 4 inches of steel and the background is 0.4–0.5 cpm. depending on the G.M.4. tube. The total, coincidence and anti-coincidence counts are scaled and the accuracy of the anti-coincidence count is thus continuously monitored. The efficiency for counting Y^{90} with 25 mgm. sources is about 25%.

(b) Three position anti-coincidence counters in which the background and two samples can be measured. These are normally set to count one hour in each position and the individual hourly counts and total counts from each position are recorded. The counting tubes are either E.H.M.2 (2 mgm/cm^2 window) or special G.M.4 tubes with 13 Type G.M.5 glass envelope guard tubes arranged at 180°. The assemblies are shielded by 2 inches of lead and normal backgrounds and efficiencies for Y^{90} counting are

E.H.M.2	1.5–2.0 cpm.	efficiency c. 30%
G.M.4	1.0–1.3 cpm.	efficiency c. 25%

(c) Standard counting arrangement using E.H.M.2 counting tubes and 2 inch lead shield with a background of 7 cpm. The efficiency is about 20% and this set up has been modified to permit the direct counting of bone ashes.

Type (b) is used for most of the work, type (a) being reserved for the lowest activities and type (c) for the more active animal bones.

The counters are calibrated using virtually carrier free solutions standardised by the Isotope Division, A. E. R. E. Suitable aliquots are taken, and precipitated with appropriate amounts of carrier, mounted and counted. From these results efficiency versus source weight tables (or curves) can be prepared which are used in converting cpm. to dpm.

When significant amounts of $strontium^{89}$ are present the $Sr^{90}+Y^{90}$ contribution in a mixed $Sr^{89}+Sr^{90}+Y^{90}$ source is calculated from the Y^{90} count and the Sr^{89} count obtained by difference.

With the exception of the results for human bones, the $strontium^{90}$ content is based on the counting of the $yttrium^{90}$ source, the radiochemical purity of which is checked by following its decay. The decay of the mixed strontium and yttrium sources is also checked.

6. INTERCOMPARISON SAMPLES EXCHANGED WITH U. S.

6.1 *Bone ash samples sent to U. S.*—U. K. sheep bone ash samples, from yearling animals killed in the spring of 1955 were sent to U. S. and analysed at Chicago. Results of three laboratories (two U. K. and one U. S.) are as follows, expressed in S. U.

TABLE I.—*Intercomparison of U. K. bone ash*

U. K. Ref	A. E. R. E. results		U. S. results [1]
	Harwell	Woolwich	
B3	3.0	13	31.4
B4	12.7	15	13.1
B5	5.7	5.7	5.2
B6	56	59	60.6
B7	13.4	17	18.3

[1] Martell (1955). These results are also given in Libby (1956) figure 3b, but are there wrongly ascribed to 1954;

Agreement is good, except for one sample.

6.2 *Intercomparison samples suplied by Health & Safety Laboratory, New York.*—Samples of sheep bone, hay and milk ash, and also of soil, collected from the New York area in September/October 1955, have been analysed by the A. E. R. E. (Woolwich Outstation) New York, Chicago and Pittsburgh laboratories as follows, results being in S. U.

TABLE II.—*Intercomparison of HASL samples*

	Woolwich	New York	Chicago	Pittsburgh
Sheepbone	5.6	5.8	4.45	6.98
	5.6	5.1	5.14	4.45
Hay	19.5	20		19.7
	19.8			17.9
Milk	3.1	3.3	2.4	2.4
	3.0	3.1		2.6

Agreement on the bone, hay and milk samples is excellent, especially between the Woolwich and New York laboratories.

On the soil sample, Woolwich have given results as follows but results from the other laboratories have not yet been received.

TABLE III.—*Woolwich results on HASL soil cample*

Initial attack	Sample weight	%Ca	Sr^{90} activity		S. U.
			dpm/ft^2	$\mu\mu c/m^2$	
$NaOH+Na_2CO_3$ fusion	100 gm	0.39	620	3050	17.3
2×500 ml. 3M HCl	200 gm	0.28	640	3150	25
	400 gm	0.24	580	2850	26

7. RESULTS ON U. K. SAMPLES

A check list of results on all British samples on which analysis is complete (whether analysed in the U. K. or the U. S.) is given in Appendix C.

7.1 *Soils.*—The 1955 soil samples have been analysed at Woolwich, by three methods, ammonium acetate extraction, hydrochloric acid extraction, and fusion with sodium carbonate. The agreement on total Sr^{90} between the last two methods is very fair, taking into account the low specific activity (20 to 50 dpm/kg soil) and the presence of some activity in particulate form, which makes it difficult to ensure that samples are representative.

There seems to be significantly more total Sr^{90} in the top 4″ of the Vyrnwy and Cwmystwyth soils than in those from the Suffolk stations. This may well be associated with the high rainfall (see Appendix A), but it must not be forgotten that these soils are very well leached. Stewart et al (1956) have measured the Sr^{90} fallout in rain at Milford Haven, Pembrokeshire over 3 weekly periods since the spring of 1954. Without assuming anything about the absolute levels found, their curve can be used to normalise soil estimates at different dates in 1955 to a fixed date which will be taken as 1st October, 1955. The U. K. soils were sampled in late March and early April. The factor of increase on Stewart's curve between 1st April and 1st October is 1.5. In Table I the results of the 1955 U. K. soil samples (fusion and HCl results averaged) from Appendix Cl are multiplied by the factor 1.5 and compared with three sets of results on U. S. soils, namely:—

(i) The soil taken at Ithaca N. Y. in September, 1955, depth 0–2″, by HASL for intercomparison purposes. The Woolwich results on aliquots of this soil were given in para 6.2 above.

(ii) The 17 U. S. soils (0–2″) sampled between 23rd September and 20th October, 1955 and analysed for Sr^{90} by HCl extraction by Hardy and Morse (Libby, 1956, Table 6). The mean of these samples was 2800$\mu\mu$c/m^2 (578 dpm/ft^2) and the two New York samples among them averaged 2200$\mu\mu$c/m^2 (450 dpm/ft^2).

(iii) The 6 Illinois/Wisconsin soils sampled by Alexander in late September 1955, extracted with ammonium acetate at Beltsville and analysed by Martell (1956) with mean Sr^{90} activity of 4800±900$\mu\mu$c/m^2 (970±180 dpm/ft^2). These soils were sampled to depth 8″, 80% of the activity being found in the top 2″ of unploughed soil.

TABLE IV.—*Sr^{90} in soils at 1st October 1955*

	Annual rainfall (inches)	Soil depth	Sr^{90} activity	
			$\mu\mu$c/m^2	mc/mile^2
U. K. Measurements:				
Cwmystwyth & Vyrnwy	80	0–4″	4000	10.0
Talgarth	35	0–4″	2500	6.5
Suffolk	25	0–4″	1300	3.3
Ithaca, N. Y.	42	0–2″	3070	8.0
U. S. Measurements:				
U. S. (av)	------------	0–2″	2800	7.2
New York	42	0–2″	2200	5.7
Wis/Ill	33	0–8″	4800	12.4

There is good agreement between the total Sr^{90} in the top 4″ of Cwmystwyth and Vyrnwy soils and the absolute value of the cumulative rainfall activity at Milford Haven, Pembrokeshire. Stewart et al (1956), give the total corresponding to 1st October 1955, as 4100 $\mu\mu$c/m^2 (10.5 mc/mile^2). However, the profile of Sr^{90} activity in the Welsh soils is so far unknown, and too much reliance cannot be placed on this comparison.

7.2 *Vegetation Samples.*—The results are given in Appendix C2. To express the results in S. U. ($\mu\mu$c/Sr^{90} per gram Ca) is appropriate as long as uptake from the soil is considered the major mode of contamination. When foliar retention is considered $\mu\mu$c/kg or $\mu\mu$c/m^2 of ground are sometimes more helpful units.

The notable feature of the 1955 samples (D4 to D13 and D20) was the uniformity of the results as between samples, despite the widely varying geographical and soil status of the sites. With two exceptions, all lay in the range 25 to 53 S. U. All were mature hay samples and therefore all had approximately the same period of growth and exposure to fallout.

The five samples D14a to D18/2 were taken at Chilton, Berkshire, where the soil is very calcareous (circa 100 grms, Ca per kg. soil). Rough grass, some of it dead, was collected from an ungrazed area of the former airfield. The activity ranged from 24 to 64 S. U. as compared with a S. U. value in the soil of about 0.2, thus strongly suggesting foliar retention on the dominant mode. About 25 percent of the vegetation activity was removable by washing with distilled water. Expressed per unit area of ground, these samples ranged from 50 to 85 $\mu\mu$c/m^2. From the rainfall measurements of Stewart et al (1956) the rate of fallout of

Sr^{90} during the early spring of 1956 was about 200 $\mu\mu c/m^2$/month. The Chilton results could therefore be explained without reference to soil uptake if, for example, 25% of the fallout was retained on the leaves for an average period of about 6 weeks.

Samples D24A and D25A were taken at similar airfield sites after the new season's growth of grass was available. The levels in terms of S. U. are a little lower than those of the preceding samples. In terms of $\mu\mu c$/kg and $\mu\mu c/m^2$ they are considerably lower, reflecting the lower calcium content of the spring flush of grass.

Samples D21 and D22 were taken at Cwmystwyth from a hillside sheep run similar to that on which the sheep B6 and B10 had grazed. The season was late, and the grass sparse. The activities of 370 and 510 S. U. found in the grass are in about the same ratio of 10:1 to the general level at that time as were the sheep bones taken in 1955 from Cwmystwyth to the general level in sheep bones.

7.3 *Sheep bones.*—The results of Sr^{90} analysis on 21 long bones from British sheep are given in Appendix C3.

The results for lowland sheep in 1955 and early 1956 were fairly uniform, the range of 8 samples being from 7.5 to 15.4 S. U. and the median 14 S. U. This compares with 15 S. U. estimated by Martell (1956) for the Chicago area in late 1955.

With hill sheep the results are more variable, levels as high as 59 and 183 S. U. (Cwmystwyth) 35 S. U. (Welshpool) and 65 S. U. (Haslingden) having been found. As shown in the section on sampling, it is not yet possible to say what weight to attach to excess rainfall, soil type or grazing habits in contributing to these results.

7.4 *Milks.*—Seven samples of dried milk made at a factory at Yeovil, Somerset, during the period March-July, 1955 have been analysed at the Health and Safety Laboratory, New York Operations Office, AEC, through the kindness of Dr. J. H. Harley. The mean level was 3.2 S. U. and this compares with means of 2.5 and 3.3. S. U. respectively for New York and Chicago milks over the same period as deduced from Libby (1956, Figure 1c). Similar samples dated April, 1955 and March 1956 respectively analysed at Harwell gave 4.3 and 3.8 S. U.

In samples from all three localities (New York, Chicago and Yeovil) there is evidence of a peak of Sr^{90} activity in May and June 1955 (fig. 1). Fallout was relatively heavy about this time, and the effect of this may have been accentuated by the change in the feed of the cows from hay and similar stored foods to open grazing at that time of year.

7.5 *Human bones.*—28 British human bone samples dating from October 1955 to February 1956 have analysed at Woolwich to date. Details are given in Appendix C5 and a graph of activity against age at death in Fig. 2.

The earlier samples were mostly ribs, but in future femurs will be used whenever possible. Geographically the samples are mainly from the Midlands and South East of England, with a few from Carlisle to represent the West Coast.

The highest value found so far is 1.3 S. U. in an eleven weeks old child (Carlisle), closely followed by 1.2 and 1.1 S. U. in two Birmingham one year olds, and 1.05 S. U. in a 3½ year old from Dudley. Bones from one stillborn infant have been analysed, and gave 0.45 S. U.

Adult bones show consistently low values of 0.2 S. U. or less. Much of the calcium in adult bones was of course laid down before fallout became significant.

The range of levels in British bones is almost indistinguishable from that found in the U. S. and reported by Libby (1956). The maximum in his series is 1.7 S. U. with several others just over 1 S. U. Two 1955 stillbirths from New England show 0.4 and 0.5 S. U.

7.6 *Relative levels in vegetation, bone and milk.*—The results of three series of samples are given below, median values being given except for the child, for which the maximum is used. For comparative purposes the results have been normalised to make the vegetation level equal 100, but the results in S. U. are also given in parentheses.

The three series are:

(1) U. S. results of October 1953, as given in Table I and II of Bugher (1955) and in Table 1 of Libby (1956).

(2) U. K. results of 1955

(3) The New York samples of September/October 1955, referred to in paragraph 6.2 above, with human bone results from Libby (1956).

Also for comparison, experimental results of Comar (1956) are included.

TABLE V.—*Relative Sr^{90}/Ca ratios normalised to vegetation=100*

[S. U. values in parentheses]

	Vegetation	Animal bone	Milk	Child (max.)	Stillbirth
(1) U. S. 1953	100	30	16	8	1.4
	(9)	(2.7)	(1.4)	(0.7)	(0.13)
(2) U. S. 1955	100	28	15	8.5	2.0
	(20)	(5.6)	(3.0)	(1.7)	(0.4)
(3) U. K. 1955	100	40	9	3.7	1.3
	(35)	(14)	(3.3)	(1.3)	(0.45)
Comar (exptl)	100	37	13		

8. SUMMARY AND CONCLUSION

The good agreement between independent determinations by different laboratories (paragraph 6) shows that the analytical methods of determining Sr^{90} in biological materials are adequate. The error is small in comparison with the biological variations between samples.

The soil determinations cannot yet be considered final, but it is unlikely that the estimation of total Sr^{90} by the hydrochloric acid and fusion methods is seriously in error.

There is good evidence that from the spring of 1955 onwards the levels of Sr^{90} in biological materials in the U. K. and the U. S. have been similar. This is particularly true of milk samples and human bones, but appears also in the results for animal bone and vegetation.

As Libby (1956) has stated:

"A Sr^{90} fallout probably derived from megaton weapons and nearly uniform over the world, except for local effects due to rainfall variations and to fallout from submegaton weapons, seems clearly established."

9. ACKNOWLEDGEMENTS

We are greatly indebted to Mr. K. H. Jones and all the members of the staff of the Ministry of Agriculture, Fisheries & Food who have helped in the collection of samples for this work. In particular Dr. Rice Williams and Mr. T. H. Caldwell have given most helpful advice and have provided invaluable assistance in arranging subsidiary experiments.

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APPENDIX A1

Principal U. K. sampling sites

No.	Site	Soil	Total Ca in soil gms/kg dry wt.	Altitude (feet)	Annual rainfall (inches)
1	Earl Soham Suffolk	Boulder clay	16	90	25
2	Boulge Suffolk	Sandy clay loam	3	120	25
3	Talgarth Brecon	Free draining soil on old red sandstone	2.5	1,050	35
4	Cwmystwyth Cardigan	Peat on shale (free draining)	0.3	1,200	80
5	Vyrnwy Montgomery	Peat on shale (free draining)	2	1,100	78
6	Boxworth Cambridge	Dark brown loam with chalk particles		157	20
7	Norwich Norfolk	Sandy loam with gravel		85	25
8	Princetown Devon	Sandy peat on granite		1,300	85
9	Rookhope Durham	Peaty sandy loam with podsol		1,600	56

NOTE.—All these farms carry sheep on permanent pastures. Nos. 1 to 5 were the original (1954) sites. From 1956 onwards Nos. 4 to 9 will be used, supplemented with additional samples as required.

APPENDIX A2

DETAILING OF SAMPLING PROCEDURE

1. Soil and vegetation

Samples or herbage and soil will be taken in the first fortnight of July each year, beginning in 1956. Half acre sites will be used on the chosen sampling farms, the same half acre being used each year.

Grass.—On the half acre, 10 plots, as near to one square yard as possible will be selected. The grass from each of the 10 plots will be cut with shears to about "lawn-mower" length, i. e. short enough to get the vegetation that a sheep would graze but long enough to avoid contaminating the grass sample with soil. The grass from the 10 sites will be bulked. If the bulk sample falls appreciably below 5 lbs. in weight, more yard plots will be cut, the total number being recorded.

Mat.—After the grass has been cut, a core of any grass mat there might be present will be taken from each of 10 plots, and bulked. A sampling tool will be used giving a 4'' diameter core.

Soil.—A 4'' deep core of soil will be taken from the spot where the mat had been removed. In practice, a core of mat and soil exceeding 4'' in depth may be taken, the mat removed and kept separate and the top 4'' of the core retained as the soil sample. The sample from the 10 plots may be bulked. The exact surface area must be noted.

In each future year, new square yard plots as near as possible to the original plots, but not overlapping them, should be chosen.

2. Sheep

The sheep to be taken should be in the region of 15 months old. It will have to be accepted that some sheep would spend part of their lives on pastures possibly remote from the sampling site. It is important, however, that the sheep should have spent at least the last few months in grazing pastures on or near the sampling site. Long bones only will be required.

Bones should be cleared of surface meat but need not be otherwise treated.

3. Other information

Samplers will be asked to provide:

(a) Details of terrain, rainfall, etc.

(b) Details of sheep management (i. e. whether sheep had grazed in sampling pasture continuously, and breed).

(c) Photographs of the site, and a "close-up" of the herbage.

4. Other matters

(a) Reasonably level plots should be chosen for herbage and soil samples.

(b) The plots need not be fenced off in any way unless the yield of grass per square yard is likely to be less than ½ lb. In any case, cages should not be put in position more than 3 weeks before the sampling.

(c) Sites should remain unploughed for at least 5 years.

* * * * * * *

APPENDIX C

Check list of results

SOILS

Ref.	Date	District	Ca in soil gms/kg	Sr^{90} activity to depth 4"					
				NH₄Ac		HCl		Fusion	
				$\mu\mu c/m^2$	S. U.	$\mu\mu c/m^2$	S. U.	$\mu\mu c/m^2$	S. U.
UK9	3/54	Earl Soham	26	370	0.7	210	0.15	150	0.2
UK10	3/54	Earl Soham	16.5	270	0.8	440	0.7	440	0.7
UK11	3/55	Earl Soham	16	≤410	≤0.9	880	0.6	1020	0.7
UK12	3/55	Earl Soham	8	310	0.6	690	0.9	<340	0.6
UK13	3/55	Boulge	3	≤930	≤2.7	1300	3.3	1000	2.7
UK14	3/55	Earl Soham	26	580	0.7	510	0.15	780	0.2
							0.16		
UK15	3/55	Earl Soham	11	490	0.8	920	0.8	1000	1.0
UK16	3/55	Vyrnwy	2	1700	16	2900	22	2900	22
UK17	3/55	Vyrnwy	2	1300	11	2500	16	2900	18
UK18	3/55	Cwmystwyth	0.3	1300	160	2300	150	2500	170
UK19	3/55	Cwmystwyth	0.3	1500	280	2800	150	2700	170
							180		
UK20	3/55	Talgarth	2.5	11 0	6.9	2000	8.1	1300	4.9

NOTE.—1000 $\mu\mu c/m^2$ = 1 mc/1m² = 2.6 mc/mile² = 204 dpm/ft².

VEGETATION

Ref.	Date	District of sampling	Sample	Sr^{90} Activity			Lab	Remarks
				S. U.	$\mu\mu c/kg$	$\mu\mu c/m^2$		
UK/D1	8/54	Earl Soham Suffolk	Hay	5.8	25		H	
D2	9/54	Talgarth, Brecon	Hay	7.8	10		H	
D4	6/55	Boulge, Suffolk	Hay	52	140		H	
D5	6/55	Boulge, Suffolk	Hay	43	110		H	
D6	6/55	Boulge, Suffolk	Hay	41			H	
D7	7/55	Earl Soham Suffolk	Hay	5.5	24		H	
D8	7/55	Earl Soham Suffolk	Hay	29	110		H	
D9	7/55	Earl Soham Suffolk	Hay	25	100		H	
D10	7/55	Earl Soham Suffolk	Hay	33	120		H	
D11	8/55	Vyrnwy, Montgomery.	Hay	35	250		H	
D12	8/55	Vyrnwy, Montgomery.	Hay	51	335		H	
D13	8/55	Cwmystwyth Cards.	Hay	53	270		H	
D20	8/55	Talgarth, Brecon	Hay	8.3			H	
D14A	12/55	Chilton, Berks	Grass	24			H	Rough grassland, including dead grass from previous year's growth.
D14B	2/56	Chilton, Berks	Grass	40		85	H	
D16/3	3/56	Chilton, Berks	Grass	64	490	80	H	
D17/2	3/56	Chilton, Berks	Grass	61	360	57	H	
D18/2	3/56	Chilton, Berks	Grass	44	380	50	H	
D24/A	5/56	Culham, Oxon	Grass	41	165	21	H	Rough grassland, new growth.
D25/A	5/56	Chilton, Berks	Grass	37	185	13	H	
D21	4/56	Cwmystwyth Cards.	Grass	370	1010	84	H	Sparse grass from sheep run.
D22	4/56	Cwmystwyth Cards.	Grass	510	1100	290	H	
US	9/55	Ithaca, N. Y.	Hay	19.7			W	Intercomparison sample.

SHEEP BONES

Ref.	Date	Age (years)	District of origin	Sr^{90} Activity (S. U.)		Remarks
				Woolwich	Harwell	
UK/B1	3/54	1	Earl Soham, Suffolk	1.9	1.4	Lowland farm.
B2	3/54	1	Earl Soham, Suffolk	1.7	1.2	Lowland farm.
B3	3/55	1	Boulge, Suffolk	13	3	Lowland farm.
B4	3/55	1	Boulge, Suffolk	15	12.7	Lowland farm.
B5	3/55	1	Talgarth, Brecon	5.7	5.7	Hill farm.
B6	3/55	1	Cwmystwyth, Cards	59	56	Hill farm.
B7	4/55	1	Vyrnwy, Mongtomery	17	13.4	Hill farm.
B8	4/55		U. S. (Alexander)	4.1		
B9	4/55		U. S. (Alexander)	4.4		
B10	10/55	17/12	Cwmystwyth, Cards		183	Hill farm.
B12	12/55	16/12	Welshpool, Mont	35		Hill farm.
B13	12/55	10/12	Melbourn, Cambs	11.2		Lowland farm.
B14	12/55	18/12	Clun, Shropshire	15.7		Hill farm.
B15	1/56	9/12	Petworth, Sussex	15.4		Lowland farm.
B16	2/56	10/12	Croft, Leicester	13.9		Lowland farm.
B17	2/56	9/12	Market Harborough, Leics	15.3	16.2	Lowland farm.
B18	2/56	12/12	Petworth, Sussex	15.6	16.6	Lowland farm.
B19	3/56	12/12	Grantham, Lincs	7.8		Lowland farm.
B20	4/56	12/12	Haslingden, Lancs		65	Hill farm.
B21	4/56	12/12	Talgarth, Brecon		24	
U. S.	9/55	7/12	Ithaca, N. Y.	5.6		Intercomparison sample.

MILK

Ref.	Date	District	Sr^{90} Activity S. U.	Lab	Remarks
C1	3/55	Yeovil, Somerset	1.8	N. Y.	Some confusion over references of these samples, but all spring 1955.
C2	3/55	Yeovil, Somerset	1.7, 1.8	N. Y.	
C6	4/55	Yeovil, Somerset	2.6, 2.8	N. Y.	
C7	4/55	Yeovil, Somerset			
C11	4/55	Yeovil, Somerset	3.3, 2.8	N. Y.	
C12	5/55	Yeovil, Somerset	5.3	N. Y.	
C16	6/55	Yeovil, Somerset	5.3, 5.8	N. Y.	
C19	7/55	Yeovil, Somerset	2.3, 2.8		
C9	4/55	Yeovil, Somerset	4.1	H	
	10/55	New York, N. Y.	3.1, 3.0	W	Intercomparison sample.
	10/55	New York, N. Y.	3.3, 3.1	N. Y.	
	3/56	Yeovil, Somerset	3.8	H	

HUMAN BONES

Ref.	Date	Age (years)	Bone	District	Sr^{90} Activity [1] (S. U.)
HB1	10/55	12	Ribs	Swindon	0.2 ±0.05
HB2	10/55	27	Ribs	Swindon	0.15±0.05
HB3	10/55	40	Ribs	Swindon	0.05±0.05
HB4	10/55	38	Ribs	Reading	0.05±0.05
HB5	10/55	6	Ribs	Swindon	0.15±0.05
HB6	12/55	1		Birmingham	1.20±0.07
HB7	11/55	27		Reading	0.2 ±0.1
HB8	12/55	31	Ribs	Oxford	0.06±0.01
HB9	11/55	23	Ribs	Reading	0.16±0.08
HB10	12/55	10	Ribs	Birmingham	0.57±0.03
HB11	1/56	19/12	Ribs	Birmingham	0.76±0.05
HB12	1/56	1	Ribs	Birmingham	1.1 ±0.03
HB13	1/56	3½	Ribs	Dudley	1.05±0.07
HB14	1/56	16½	Tibia	Carlisle	0.25±0.02
HB15	1/56	50	Tibia	Carlisle	0.06±0.03
HB16	1/56	65	Tibia	Carlisle	0.13±0.02
HB17	1/56	s/born	Sternum & Femur	Carlisle	0.45±0.06
HB20	2/56	16/12	Femur	Carlisle	0.8 ±0.1
HB21	2/56	3/12	Femur	Carlisle	1.3 ±0.1
HB22	1/56	33	Ribs	Swindon	0.1 ±0.1
HB23	12/55	18	Ribs	Swindon	0.2 ±0.03
HB24	12/55	40	Ribs	Swindon	0.07±0.02
HB25	12/55	34	Ribs	Swindon	Sample lost
HB26	2/56	8/12	Ribs	Birmingham	0.5 ±0.2
HB27	2/56	20	Ribs	Birmingham	0.2 ±0.05
HB28	1/56	16	Ribs	Birmingham	0.2 ±0.03
HB29	2/56	2¼	Femur	Birmingham	0.55±0.03
HB30	2/56	11⅓	Shaft	Birmingham	0.15±0.02
HB31/1	2/56	9½	Femur	Birmingham	0.25±0.02
31/2	2/56	9½	Femur	Birmingham	0.28±0.02
31/3	2/56	9½		Birmingham	0.20±0.03

[1] The errors attributed to the S. U. results are equal to twice the standard duration due to counting statis tics, and take no account of other sources of error.

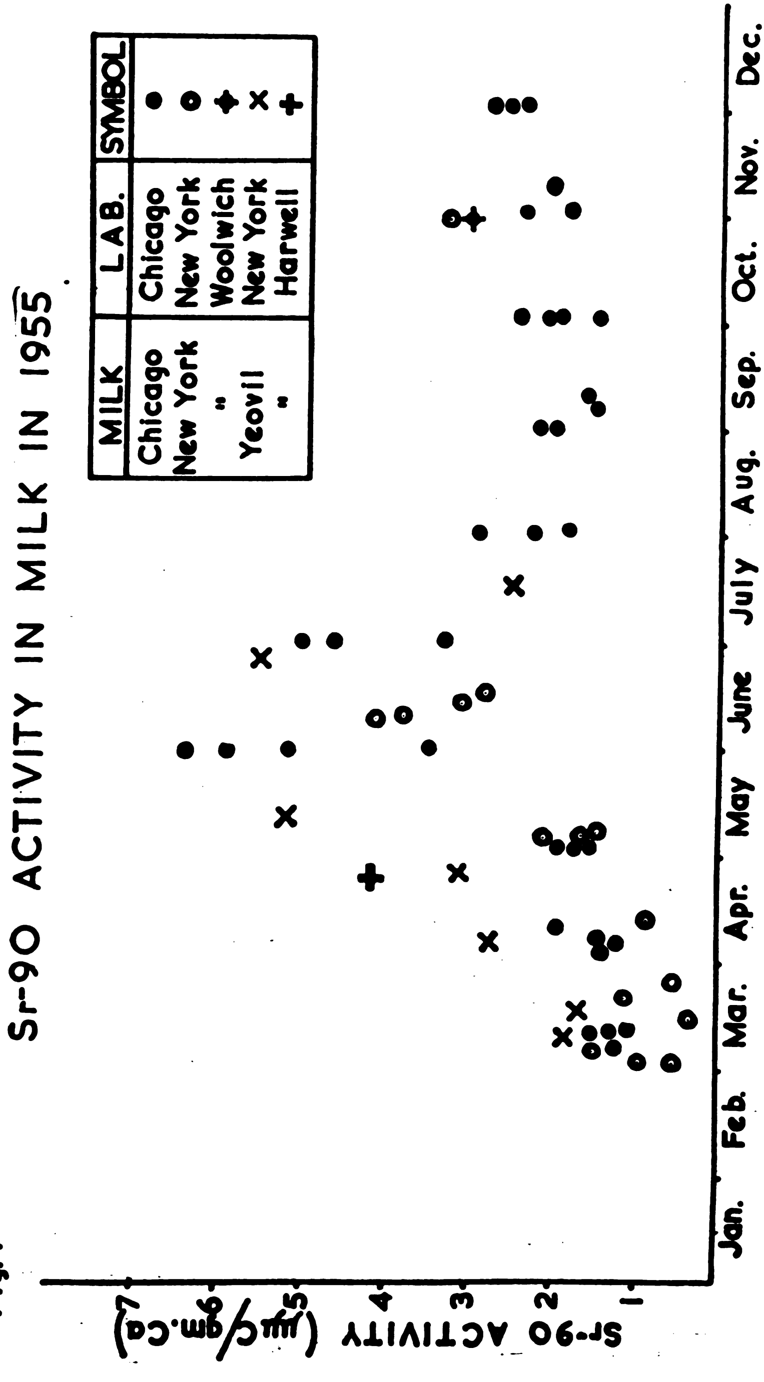
Sr-90 ACTIVITY IN MILK IN 1955
MILK | LAB. | SYMBOL
Chicago | Chicago
New York | New York
" | Woolwich
Yeovil | New York
" | Harwell
Sr-90 ACTIVITY (μμc/gm.Ca)
1 2 3 4 5 6 7
Jan. Feb. Mar. Apr. May June July Aug. Sep. Oct. Nov. Dec.

Fig. 1

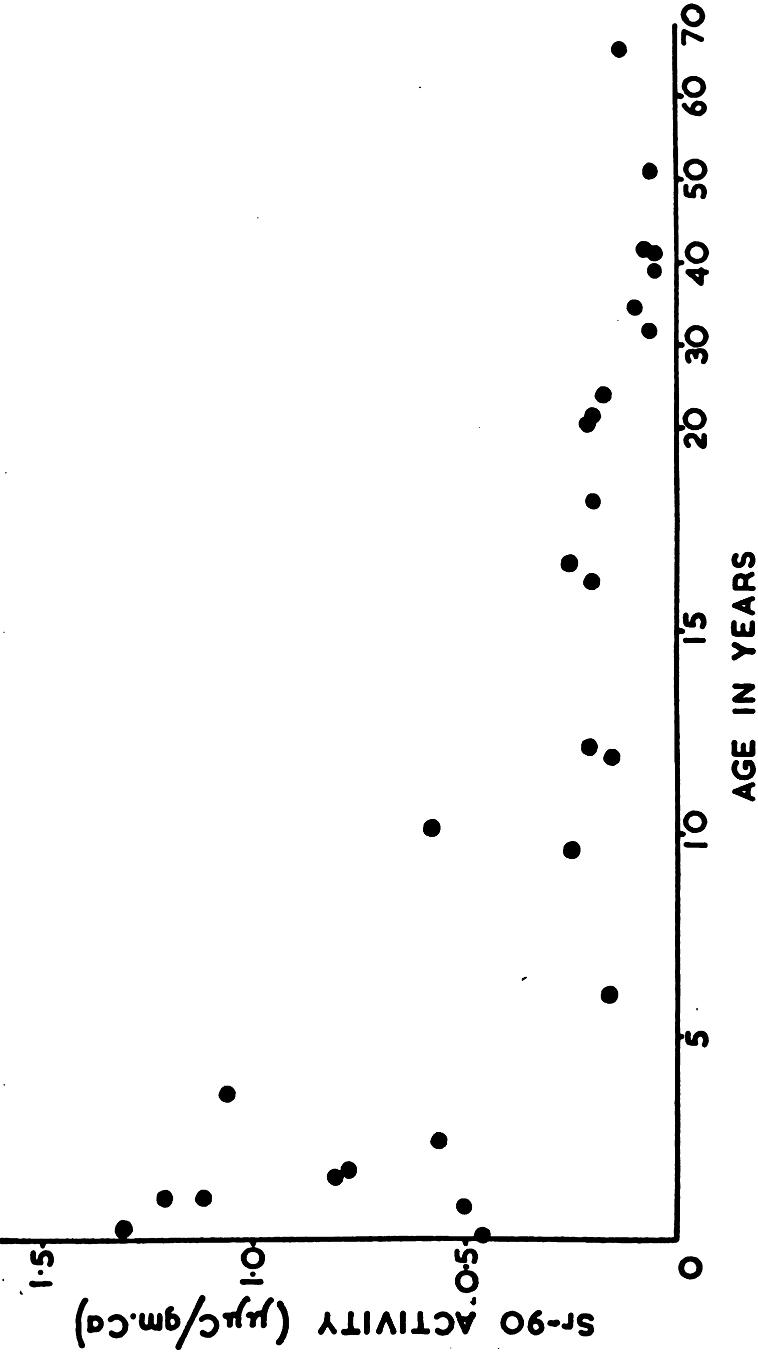

Fig.2 Sr-90 ACTIVITY IN HUMAN BONES IN 1955-6

A. E. R. E. HP/R 2182

ATOMIC ENERGY RESEARCH ESTABLISHMENT

Radiostrontium and Radiocaesium Measurement in Biological Materials to December 1956

By D. V. Booker, F. J. Bryant, A. C. Chamberlain, A. Morgan, and G. S. Spicer

1. INTRODUCTION

The methods used in the estimation of Sr.90 in biological materials, the intercomparison of results between different laboratories and the results up to the early part of 1956 were fully described in A. E. R. E. HP/R 2056.[1] The present report gives further results for Sr.90, presents some data on Cs.137 in milk, and examines in greater detail the evidence for the trend of radioactive contamination with time.

2. SR.90 IN SOIL

An effort was made to get a reliable estimate of the total Sr.90 in soil, for comparison with the rain data of Stewart et al (1956).

Three soil horizons (0–4″, 4–8″ and 8–12″) were taken in July, 1956, at each of four sites within 15 miles of Harwell, but on widely differing soil types as follows:

Site	Soil	Annual Rainfall (inches)
Grove, Berks	Clay	27.5
Aldermaston, Berks	Acid sand	27.5
Culham, Oxon	Greensand	25
Chilton, Berks	Chalk	27.5

The soils were extracted with 6M hydrochloric acid at Woolwich. This method has been shown to remove as much Sr. 90 as it got by complete fusion of the soil (HP/R 2056). The results are given in Table I.

TABLE I.—*Sr. 90 in soil (July 1956)*

Ref.	Site	Depth	gms. Ca/kg.	$\mu\mu c/m^2$	S. U.
UK36	Aldermaston	0–4″	1.60	2560	18.6
	Aldermaston	4–8″	1.52	<150	<0.8
	Aldermaston	8–12″	1.46	<50	<0.3
UK37	Culham	0–4″	3.00	2530	8.0
	Culham	4–8″	2.68	220	0.6
	Culham	8–12″	3.46	<150	<0.3
UK35	Grove	0–4″	39	1900	0.66
	Grove	4–8″	17	270	0.18
	Grove	8–12″	10	224	0.22
UK38	Chilton	0–4″	156	2180	0.15
	Chilton	4–8″	185	<150	<0.01
	Chilton	8–12″	204	<150	<0.01

Where the sign < is used, the activity was less than the minimum measurable.

The following deductions may be drawn:

(a) at least 80 per cent of the activity is in the top 4″.

(b) There is good agreement in total Sr. 90 between sites. The nature of the soil does not seem to affect the degree of retention of the fallout.

(c) The average level in this area is 2.5 mc/km^2 (6.5 mc/mi^2).

[1] Circulated to the October 1956 meeting of the United Nations Scientific Committee on the Effects of Radiation with reference A/AC82/G/R30.

3. SR. 90 AND CS. 137 IN MILK

The regular supply of skimmed dried milk from a factory at Frome, Somerset[3] has continued. Some of the samples have been analysed at the Health and Safety Laboratory at New York, through the help of Dr. J. H. Harley, and some done at Harwell or Woolwich. In fig. 1a the results are shown in terms of the specific activity of Sr. 90 relative to calcium, using the unit referred to as the strontium unit.

One strontium unit (S. U.) = One micro-micro-curie Sr. 90 per gram of calcium.

Also in fig. 1a is drawn the graph of cumulative Sr. 90 fallout in rain at Milford Haven, Pembrokeshire, given by Stewart et al (1956). The units are millicuries Sr. 90 per square kilometre. The trend of the figures with time is discussed below in para 6.

In fig. 1b is shown the Cs 137 activity of samples from the same series. The Harwell results were obtained by gamma spectrometry of the dried milk, whereas the Woolwich results were by chemical analysis. The details of the gamma spectrometric method are given by Booker (1957).

A series of samples of full cream dried milk from different regions in the U. K. was taken in October, 1956. The results of Sr. 90 and Cs. 137 analysis on these samples are given in Table II.

TABLE II.—*Sr–50 and Cs–137 in milk (October 1956)*

Area	Sr–90 μμc/gm. Ca.	Cs–137 μμc/gm. K
Somerset	4.6	28
Yorkshire	4.3	30
Cumberland	6.5	28
Carmarthen	8.0	65
Antrim	6.9	87
Londonderry	10.3	84

Calcium is about 1.2% and potassium 1.4% of dried milk by weight.

The Sr. 90 activity of 10.3 S. U. in milk from County Londonderry is similar to maxima of 10 or just over found in the U. S. (Harley et al 1956) and in Canada (Canadian submission to U. N. of October, 1956).

4. SR. 90 IN SHEEP BONES

Yearling sheep have been taken from three areas in Wales in each of the years 1954, 5 and 6. The sheep came from the same flocks each year, and the flocks were grazing the same pasture. The areas covered by hill sheep are large, and the exact soil conditions cannot be specified, but analysis of samples showed that the soil was calcium deficient, especially at Cwmystwyth (HP/R 2056, Appendix A1)

The results of Sr. 90 analysis on the long bones of these sheep are given in Table III, and shown graphically in fig. 2.

TABLE III.—*Sr. 90 in bones of Welsh sheep (S. U.)*

	Cwmystwyth Cardigan	Vyrnwy Montgomery	Talgarth Brecon
1954	18.8	7.7	1.5
1955	59, 56	17, 13	5.7, 5.7
1956	170, 151	42, 40	24, 24

The 1954 analyses were done in the U. S. Those of 1955 and 1956 have been done at Woolwich and Harwell independently, and in Table III the results of both analyses are given (Woolwich in the left).

[3] The location was wrongly referred to as Yeovil, Somerset in HP/R 2056, but the milk processed at the factory is in fact drawn from an area of 20 miles radius round it.

In fig. 2 the cumulative fallout of Sr.90 in rain is also given (this time on a log scale). It appears that the rate of increase of Sr.90 activity in these sheep has been approximately exponential, with an average increase of a factor 3 per annum, and has been about as rapid as the rate of increase of cumulative fallout.

Bones fro 10 English sheep killed in 1956 have been analysed for Sr.90. Three hill sheep from Dartmoor, Durham and Lancashire gave 53, 71 and 61 S. U. respectively, showing that the relatively high levels are not confined to limited areas in Wales. Seven lowland sheep from south and east England showed Sr.90 activity ranging from 7.8 to 15.6 S. U. Direct comparison with previous years was not possible, because the Suffolk flocks from which the 1954 and 1955 animals were obtained have been broken up, but the general level in lowland sheep seems little altered in 1956 compared with 1955.

5. SR.90 IN HUMAN BONES

Analyses of 21 bones additional to those reported in HP/R 2056 have been completed and the results are given in Table IV. Work has been concentrated on the bones (femurs) of children, since previously it had been shown that they show higher activity than those of adults. The ages at death ranged from 2 days to 14 years. 12 specimens from infants under one year of age averaged 0.62 S. U., 5 from children of 1 to 4 years averaged 0.76 S. U. and 4 from children of 7 to 14 years 0.27 S. U.

Three specimens, all from 6 month old infants, showed Sr.90 activity of 1 S. U. or greater, but none exceeded the previous maximum of 1.3 S. U. reported in HP/R 2056.

The femur HB38 from a 14 year old boy was divided into four parts transversely before analysis. The epiphyseal bone at either end showed double the specific activity of the centre of the shaft. This was to be expected since recently deposited calcium contains more Sr.90 than that laid down several years ago.

TABLE IV.—*Sr-90 in human femurs (additional to those given in Appendix C4 of HP/R 2056)*

Ref	Date	Age at death	District	Sr-90 (S. U.)	Notes
32	5/56	1 m	Herts	0.5	
33	4/56	2 m	Sussex	.15	
34	6/56	17 m	Surrey	.9	
35/1	5/56	2½ y	London	.8	Proximal end
35/2	5/56	2½ y	London	.8	Distal end
36	4/56	8 y	Middlesex	.27	
37	5/56	12 y	London	.24	
38/1	5/56	14 y	Surrey	.20	Distal subepiphysea
38/2	5/56	14 y	Surrey	.17	Distal plate
38/3	5/56	14 y	Surrey	.11	Centre of shaft
38/4	5/56	14 y	Surrey	.20	Proximal subepiphyseal
39	6/56	2 d	London	.45	
40	7/56	7 d	Kent	.15	
41	7/56	12 d	London	.35	
42	7/56	3 d	London	.8	
43	7/56	1 m	London	.4	
44	7/56	2 m	Sussex	.55	
45	6/56	5 m	London	1.1	
46	6/56	6 m	Bucks	.75	
47	6/56	6 m	London	1.2	
48	7/56	6 m	Lancashire	1.1	
49	7/56	2 y	Middlesex	.75	
50	7/56	2½ y	Essex	.70	
51	7/56	3½ y	Sussex	.64	
52	6/56	7½ y	Essex	.38	

6. TREND OF RESULTS WITH TIME

It would be valuable if the rate of increase of Sr. 90 and Cs. 137 activity in biological materials could be correlated with the physical data on fallout rates. The following indications emerge from the data:

(i) *Milk.*—There was a sudden rise in activity in the Spring of 1955 . This appears in Sr. 90 estimates in British and American data (cf. HP/R 2056, fig. 1) and applies to Cs. 137 as well as Sr. 90. This rise was probably due to a combination of two factors.

(a) An increase in the rate of fallout at that time, which is noticeable as a temporary increase in the slope of Stewart's grapt reproduced in fig. 1a.

(b) The sending out to pasture at that time of year of cows which had been given stored feed during the winter.

Since the summer of 1955 the milk levels seem to have been rather steady both in U. K. (fig. 1) and in U. S. (Eisenbud, 1957); and the spring increase was not nearly so marked in 1956 as in 1955. The median of 9 Somerset samples in 1956 is 4.2 S. U. as compared with 4.1 S. U. for 13 1955 samples.

(ii) *Sheep bones.*—There is a sharp distinction in the U. K. results between hill sheep and lowland sheep. The Sr. 90 level in the bones of the Welsh sheep seems to be increasing about as rapidly as the cumulative fallout whereas animals from S. E. England show little increase in the period 1955–1956.

(iii) *Human bones.*—Samples are too limited in number and diverse in origin to allow of satisfactory comparison, but there is no evidence of a rapid increase of Sr. 90 activity in the last year. A considerable time lag between changes in the environment and in human bone must be expected.

The tentative conclusion from the results so far is that Sr. 90 activity of tissues deriving their calcium from normal soils is increasing less rapidly than the cumulative fallout, and is probably more nearly proportional to the rate of fall-out. On very deficient soils the opposite apears to be true.

These effects may well be correlated with the importance on very low calcium soils of the root as compared with the foliar method of uptake of radiostrontium to vegetation.

7. SUMMARY OF 1956 RESULTS

The range of 1956 Sr. 90 results, and the median values are given in Table V.

TABLE V.—*Sr–90 in biological materials in 1956*

Material	No. of Samples	Sr. 90 activity (S. U.)		
		Max.	Min.	Median
Grass (acid hill soils)	9	2,180	113	370
Grass (normal soils)	27	85	23	40
Sheep bones (hills)	6	170	24	57
Sheep bones (lowlands)	7	15.6	7.8	13.7
Milk (Somerset)	9	5.7	2.9	4.2
Milk (other areas)	5	10.3	4.3	6.9
Human bones (child)	31	1.3	0.15	0.55
Human bones (adult)	8	0.25	0.06	0.20

The details of all the above samples have been given in HP/R 2056 or the present report, except for the grass samples, which will be reported more fully elsewhere.

8. ACKNOWLEDGEMENTS

We are greatly indepted to Mr. K. H. Jones and to the members of the National Agricultural Advisory Service of the Ministry of Agriculture Fisheries & Food for their help in arranging the agricultural samples. We are also greatly indebted to Dr. M. Bodian who obtained the human bone specimens for us.

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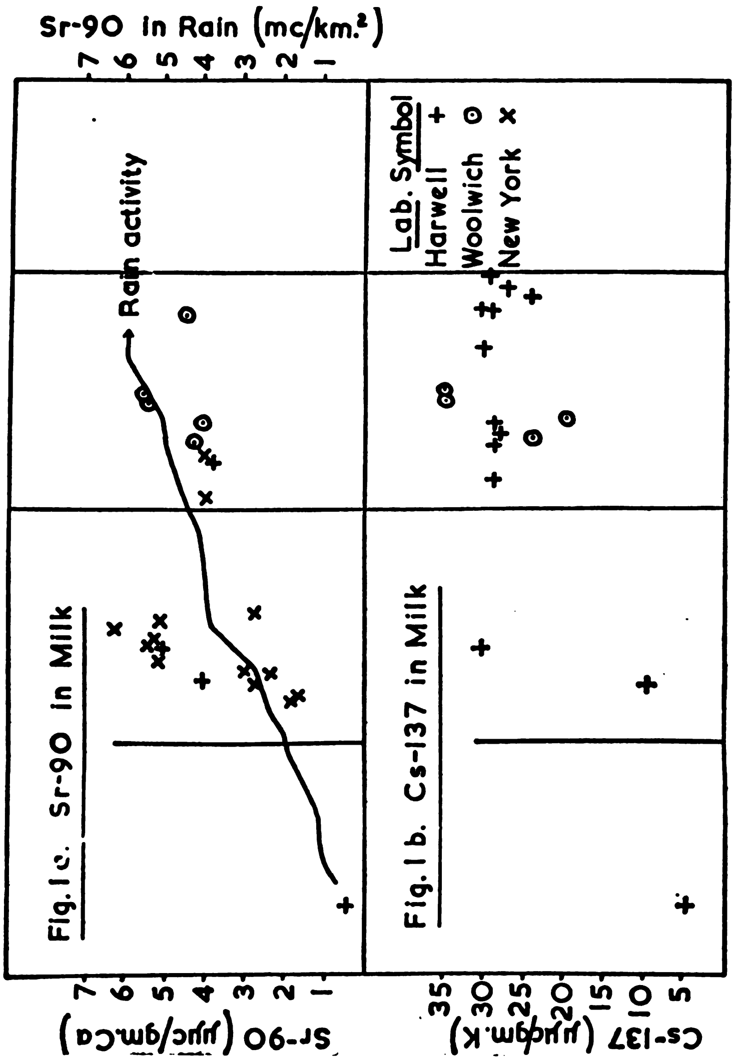
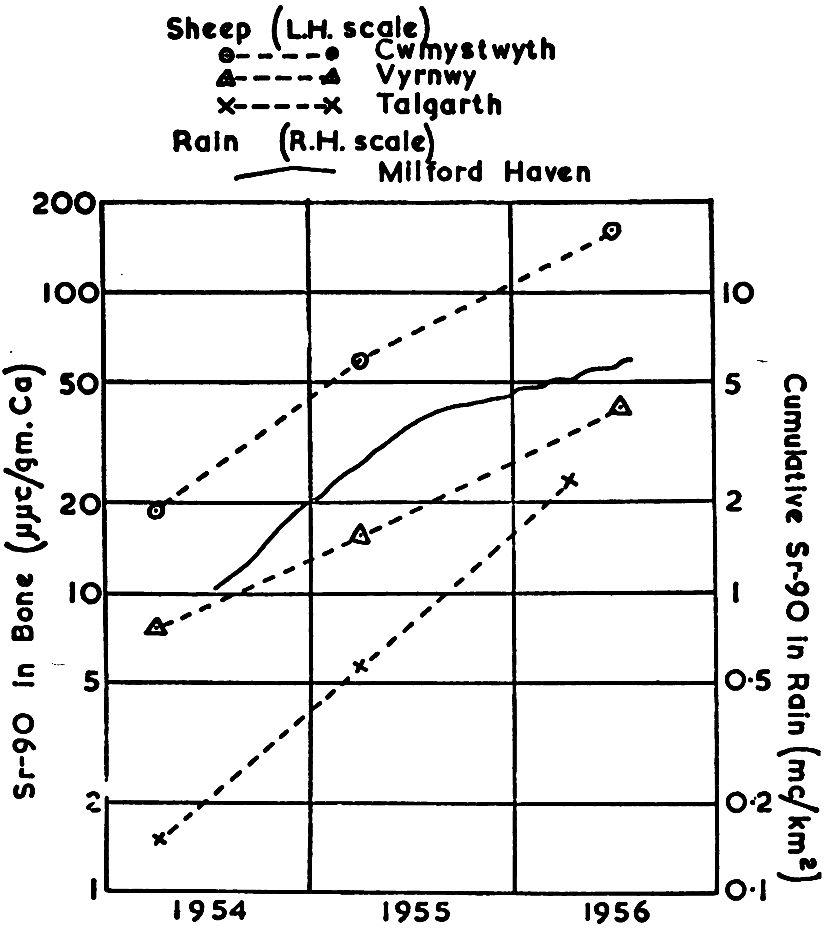

Sr-90 in Rain (mc/km.²)
Rain activity
Fig. 1a. Sr-90 in Milk
Fig. 1 b. Cs-137 in Milk
Lab. Symbol
Harwell +
Woolwich ⊙
New York x
Sr-90 (μμc/gm.Ca)
Cs-137 (μμc/gm.K)
1954
1955
1956

Fig. 2. Sr-90 in Welsh Sheep Bones



APPENDIX 3

A REPORT BY THE WORLD HEALTH ORGANIZATION ON GENETIC EFFECTS OF RADIATION

[World Health Organization, Division of Public Information, Press Release WHO/11, March 13, 1957]

GENETIC EFFECTS OF RADIATION

WHO TO PUBLISH REPORT

Throughout man's existence on earth, he has been continuously bombarded by radiations coming from outer space, from radioactive material in the earth's crust, and from natural radioactive elements within his own bone and flesh.

The intensity of this bombardment is being notably increased, in our days, by man-made sources such as medical X-ray machines, radioactive material used in medicine, and also certain material and apparatus used in science, industry and commerce; artificial radioactive elements distributed by man in nature; and, to a minor extent, shoe-fitting machines, radioactive luminous compounds on watches, etc.

Radiation has been demonstrated to be one of the agents which produces genetic mutation in a wide range of organisms from bacteria to mammals, and a Group of 20 international experts was therefore brought together by the World Health Organization (WHO) in Copenhagen last August to discuss research problems connected with the effect of radiation on human heredity.

This Group has produced a highly technical report which, together with a number of specialized papers prepared by certain of its members, is to be published by the World Health Organization within a few months. Advance copies of the text are at present in the hands of members of the United Nations Scientific Committee on the Effects of Atomic Radiation.

"Man's most unique and precious possession is his hereditary material which must determine the health and orderly development of future generations", states the introduction to the Group's report. It goes on, "This Group is of the opinion that the well-being of the descendants of the present generation is threatened by developments in the use of nuclear energy and of sources of radiation."

The report states categorically that "additional mutation produced in man will be harmful to individuals and their descendants * * *" and that " * * * all man-made radiation must be regarded as harmful to man from the genetic point of view".

Gaps in knowledge

The WHO experts agreed that present developments in the peaceful use of nuclear energy should contribute much to man's social and cultural development, and that therefore some risk must be accepted. They recognize, however, that if the dangers are to be minimized, "every possible step must be taken to reduce the exposure of man to radiations, and to understand the effects of exposure * * * Only in the light of more knowledge can decisions be taken to define more accurately the maximum amount of exposure which may be accepted by individuals and populations without risk of serious harm." The Group therefore examined some of the "larger gaps in knowledge" as they appeared at the present time, and listed some thirteen fields of genetic research in which the need for further investigation is urgent.

Cumulative effect

There are strong grounds for believing that inherited effects of radiation are additive, the experts agreed. A small amount of radiation received by each of a large number of individuals can therefore do an appreciable amount of damage to the population as a whole, although the effects may not appear for a number of generations. It is therefore desirable to limit the accumulated radiation doses received by the sex glands of individual men and women, particularly up to 30 years of age, in order to keep the average dose to the sex glands of the population as whole very low.

The most important sources of radiation to the human sex glands, the experts agreed, are at the present time from the natural radiation (normal level between 2 and 5 roentgens per individual in 30 years) and from the radiation received by patients undergoing medical X-ray examination (probable average in certain countries between 1 and 3 roentgens per individual in 30 years). If exposure during therapeutic X-ray treatment is also considered, the "total" exposure to a population might be greater. It is however difficult, the report states, to get sound data for estimating how much radiation due to therapeutic exposures is received by persons before the age at which procreation may be expected to be ended.

In connexion with the danger of radiation from medical sources, one member of the Group pointed out in a paper that radioscopic apparatus, sometimes not adequately shielded, is to be found more and more frequently in the consulting rooms of general practitioners who do not have the necessary formal training in radiology.

Artificial radioactive elements distributed in nature

In a paper by Professor R. M. Sievert, of the Institute of Radiophysics, Stockholm (Sweden), which accompanies the report, some reference is made to artificial radioactive products distributed in nature as a result of military tests. Professor Sievert recognizes that the WHO Study Group was concerned with the peaceful use of atomic energy and the effects, for instance, of the future disposal of radioactive wastes from such peaceful uses.

He points out, however, that it is essential to take into consideration the evidence obtained following atomic weapon tests, since such information is the best at present available for the study of problems in the field of artificial radioactive elements distributed in nature.

Recent measurements from large samples of foodstuffs in Sweden, Professor Sievert says, have shown that foods such as milk, beef, corn and vegetables eaten today contain artificial radioactive elements with a radiation level in many cases exceeding that due to the naturally-occurring radioactive constituents of animals and plants. It does not yet seem possible, Professor Sievert goes on, to estimate the radiation doses to human tissues, nor their distribution in time, which are necessary data for drawing conclusions of biological significance.

Professor Sievert believes that it is extremely difficult to predict what will in the future be the most important sources of radiation from artificial radioactive elements distributed in nature.

"There is reason to believe that the problems of disposal of radioactive wastes will be satisfactorily solved and that precautions in the handling and use of radioactive material will be adequate", Professor Sievert says, but goes on to warn that "accidents and unforeseen events may gradually spread radioactive substances beyond control; they could then follow unknown paths and be harmful to mankind in ways that would become known to us only after long experience."

Some conclusions

In a section entitled "Some Conclusions" the Group's report lists eight recommendations, including the establishment of more institutions and large university departments concerned with human genetics and improved teaching in this branch; the systematic registration of serious hereditary diseases; and efforts by UN Agencies toward the collection and publication of information on subjects like fertility, consanguineous marriages and parental ages which are so essential as brackground in human biological studies.

The Group was particularly impressed with the genetic hazards of man-made radiation from sources used in medicine, industry, commerce and experimental science. Both as an approach to control and as providing basic background information on radiation exposure and effects on man, it is essential, the experts agreed, that methods be found of recording exposures to individuals and populations, however difficult this may prove.

Information Submitted by the World Health Organization to the United Nations Scientific Committee on the Effects of Atomic Radiation

In the "Conclusions of the First Session" of the United Nations Scientific Committee (A/AC.82R.10 of 27 March 1956) it was stated that "This year the human

geneticists will meet at a congress on human genetics. This opportunity should be used, with the assistance of the World Health Organization to seek advice about the possibility of setting up a standard of recognition for one or more clearly recognizable medical conditions thought to be largely or solely genetic in origin."

A statement describing the United Nations Scientific Committee's request was made at a plenary session of the International Congress of Human Genetics held at Copenhagen in August 1956. There was no formal response to this request from the Congress itself. However, a Study Group on the Effect of Radiation on Human Heredity had been arranged by WHO so as to follow immediately after the International Congress on Human Genetics, and the President and a number of the members of the Conference took part in this Study Group.

A reply to the request of the United ations Scientific Committee on the Effects of Atomic Radiation was formulated during the meetings of this Study Group and the reply is attached as document A. Since this reply is to a considerable extent supplemented by the report and working papers of the Study Group, these are also attached for the consideration of the Committee.

The Study Group convened by WHO on the Effect of Radiation on Human Heredity was designed to have three objectives:

1. To provide a small symposium of papers on the effects of radiation on human heredity.
2. To formulate in simple terms the desirable lines of further research on the effect of radiation on human heredity.
3. To give some recommendations in the particular province of WHO.

Document A

REPLY TO A QUESTION RAISED BY THE UNITED NATIONS SCIENTIFIC COMMITTEE ON THE EFFECTS OF ATOMIC RADIATION

There are at the present time no convenient indicators of recent genetic damage in man, and standards which can be applied in widely separate areas for recognition of hereditary characters are extremely difficult to establish even in the best laboratory conditions. Nevertheless, new methods of recognition are being developed. Techniques are available for accurate identification of serological traits (e. g. blood antigens) and other chemical specificities (e. g. haemoglobins, serum proteins, aminoacids in urine, factors controlling clotting of blood). Ophthalmological tests are also relatively exact but require co-operation of the patient. Many hereditary conditions can only be correctly diagnosed post mortem. Thus it would seem inadvisable to make definite recommendations for standardization on a considerable scale at the present time. If however the urgency of the problem necessitates an immediate attempt to select a group of "indicator traits" then, on the basis of experience to date, the following provisional list is suitable for setting up "standards of recognition":

Retinoblastoma
Neurofibromatosis
Aniridia
Acrocephalosyndactyly
Osteogenesis imperfecta
Chondrodystrophic dwarfs of all kinds
Haemophilia
Sex-linked infantile muscular dystrophy (Duchenne type).

It is emphasized that if "indicator traits" are used in the manner implied by the question, as many traits as possible should be used in the same area at the same time. At the same time variation in the sex ratio should be studied as an index of mutation.

In setting up "standards of recognition", human geneticists and appropriate specialist physicians should consult.

STUDY GROUP ON THE EFFECT OF RADIATION ON HUMAN HEREDITY

Copenhagen, 7–11 August 1956

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Two national committees reported in 1956 on the effects of ionizing radiation on man.[1] Although difficult to compare in detail, these reports come to remark-

[1] United States of America, National Academy of Sciences (1956) Biological effects of atomic radiation; Great Britain, Medical Research Council (1956) The hazards to man of nuclear and allied radiations, London.

ably similar conclusions as to the probable effects on the descendants of populations exposed to increased amounts of such radiations.

The emphasis in both these reports was, however, on trying to set some quantitative limits to the risks in the light of existing knowledge.

The purpose of assembling the Study Group whose report is presented here was essentially twofold. The first aim was to obtain the opinions also of authorities on genetics from countries, other than those whose national committees have already stated their views. The second was to have the opinions of a number of experts on an aspect relatively little considered in the national reports—namely, the lines of research which are needed in the light of present knowledge, to increase our understanding of the genetic effects of ionizing radiations on man.

The Group met, by courtesy of the Rector of the University of Copenhagen, in the Council Room of the University, from 7 to 11 August 1956. The agenda adopted was intended to permit exploration of the views of the members on the theoretical and practical difficulties in closing present gaps in knowledge. The procedure adopted was for a number of members to open discussions either by short statements or by submission of invited papers. The opportunity was also taken to discuss a number of subjects not formally introduced.

The papers submitted are reproduced as annexes to this report and whilst the report itself represents the combined opinions of all the members of the Study Group, the annexed papers give the individual views of the authors.

The proceedings were opened by Dr. P. Dorolle, Deputy Director-General of the World Health Organization, and the Group elected Dr. A. Hollaender as Chairman.

1. INTRODUCTION

Man's most unique and precious possession is his heredity material which must determine the health and orderly development of future generations. The Group is of the opinion that the well-being of descendants of the present generation is threatened by developments in the use of nuclear energy and of sources of radiation. Both of these developments are inevitable and they should contribute much to man's social and cultural development. It would seem therefore that some risk must be accepted, but if the dangers are to be minimized every possible step must be taken to reduce the exposure of man and to understand the effects of exposure. Only in the light of more knowledge can decisions be taken to define more accurately the maximum amount of exposure which may be accepted by individuals and populations without risk of serious harm.

Radiation has been demonstrated to be one of the agents which produces mutation in a wide range of organisms from bacteria to mammals. The Group is agreed that additional mutation produced in man will be harmful to individuals and to their descendants. While there may be inherent and environmental mechanisms which modify the impact of these mutations over periods of many generations, the effectiveness of such mechanisms in man is not known. In essence then, all man-made radiation must be regarded as harmful to man from the genetic point of view.

In recent years, considerable quantitative knowledge has been accumulated on the basic mechanisms of genetics. There are strong grounds for believing that most genetic effects are very closely additive so that a small amount of radiation received by each of a large number of individuals can do an appreciable amount of damage to the population as a whole. There are, however, many gaps in knowledge particularly concerning these effects in man. These gaps will only be closed after a great expansion of general and *ad hoc* research in genetics and other fields of biology.

The Group has received the following resolution passed by the First International Congress of Human Genetics in Copenhagen and it notes and agrees (while at the same time recognizing that WHO's work is only concerned with the peaceful use of atomic energy):

"The damage produced by ionizing radiation on the hereditary material is real and should be taken seriously into consideration in both the peaceful and military uses of nuclear energy as well as in all medical, commercial and industrial practices in which X-rays or other ionizing radiation is emitted. It is recommended that the investigation of the amount and type of damage and of related genetic questions, be greatly extended and intensified with a view to safe-guarding the well-being of future generations."

The Group agrees with the memorandum, entitled "Human and Medical Genetics", which was submitted in 1955 by the Government of Denmark to the World Health Organization.[3]

This Group takes note of the report of the National Academy of Sciences of the United States of America and that of the Medical Research Council of Great Britain. It is not intended to reproduce any of the material in these reports but the Group notes the substantial similarity of the findings and recommendations of these reports and is in essential agreement with them.

2. NATURAL AND MAN-MADE SOURCES OF IONIZING RADIATION

The present sources of ionizing radiations of interest for the treatment of problems related to the genetic effects in man include the following:

Natural sources

1. Cosmic radiation.
2. Naturally occurring amounts of radium, thorium and potassium in the earth crust.
3. Content of natural radioactive elements in living tissues.

Man-made sources

4. Radioactive material and technical arrangements producing ionizing radiation (such as X-ray tubes and other particle accelerators, nuclear reactors, etc.) used in education, science, medicine, industry and commerce.
5. Sources used by the population for other purposes than those mentioned in 4 (radioactive luminous compounds on watches and other articles for common use, television sets, etc.), although such sources are much less significant than those mentioned in 4 and 6. It is important, however, that their existence be recognized.
6. Artificial radioactive elements distributed by man in nature.

Information as to the contributions to the doses received by individuals and by large population groups from the various sources listed above is summarized in Professor R. M. Sievert's paper, from which it is obvious that as regards the average dose to the gonads the most important contributions are at present those from the natural radiation (normal level: between 2 and 5 r per individual in 30 years) and from the radiation received by patients undergoing medical X-ray examination (probable average between 1 and 3 r per individual in 30 years). If therapeutic exposures are also considered, the "total" exposure to a population might be greater. It is, however, difficult to get sound data for estimating how much exposure is received in therapeutic exposures to persons before the age at which procreation may be expected to be ended.

It may be noted that at the present time the highest dose to the gonads caused by natural radiation in areas with a large population seems to exist in parts of Travancore, India, on ground containing monazite sand (possibly of the order of between 10 and 20 r per individual in 30 years).

3. IMPORTANCE OF RECORDING RADIATION EXPOSURE IN INDIVIDUALS AND POPULATIONS

From a genetic point of view the total accumulated dose is the important one and for this reason the measurement of exposure to ionizing radiations is an essential preliminary to attempts to relate dosage received to effects in man. For such measurements to be useful, the information must be recorded systematically. Unless the information is available in the form of the dose received by individuals, records of exposure would be unsuitable for many purposes and therefore some system of registration is essential. The effect of recording would almost certainly be to cut down the exposures given in medical diagnosis and treatment, since it would impress radiologists and technicians with the magnitude of such exposures. In one hospital where such recording was started there has been a 30 per cent. reduction in the total exposure of the staff. Doubtless a similar system of recording in diagnostic practice would reduce the exposure to the patients. This in itself would be a sufficient justification for introducing the procedure. It seems likely that the two national reports will already have done much to overcome the hesitation to record the dose on the part of those who would be concerned in making such records but that a recommendation from this Group would also be helpful.

[3] Off. Rec. Wld. Hlth. Org., 68, 147.

The Group is conscious that the adoption of any system of recording dosage will give rise to difficulties because it will increase the burden of work of radiologists and their staffs. Nevertheless, it feels that the importance of these procedures is such, and is so well recognized by radiologists that both those in charge of radiological departments and other physicians who use X-rays will be co-operative.

Whatever system adopted should take into account three desirable requirements:

1. That the individual will not, through lack of information, accumulate excessive exposure.
2. That information becomes available as to how much exposure to the gonads is received at each age in individuals and on an average per head of population.
3. That it should be possible to recognize the amount of exposure received by the parents of a given child. (Eventually, the information would be available for several generations.) This information is particularly valuable for purposes of genetic analysis.

The Group suspects that exposures in some industries and in scientific work are unnecessarily high. Exposures from these sources should be recorded in such a way that the dosage received can be related in individuals and populations to that received from other sources.

It seems unlikely that all countries would favour or indeed would be able to introduce the same standards of registration. Although it is expected that recommendations on mechanisms of recording will shortly be available from the International Commission on Radiological Protection, there should not be any delay in improving the standard of recording of exposures.

Whatever procedures of recording and registration are adopted will entail a large expenditure of money and effort. The need, however, is urgent. Further, the present is the appropriate time to initiate such procedures, since the introduction of atomic energy for industrial use and the extension of the use of radiation tools in biology and medicine make it possible to start with such procedures at an early stage of a period of rapid development.

4. RESEARCH

General

Additions to the understanding of the effects of radiation in man come from a very wide field of research. It is impossible to forecast what work in biology or genetics will contribute information relative to the problems. Accordingly, the Group is strongly of the opinion not only that as much experimental work as possible should be done on radiation effects on suitable organisms and such controlled observation studies as offer in man, but that there should be an intensification of all human and experimental genetic research. The Group feels that there should be the closest possible collaboration between those working in the experimental and human fields: their work is complementary. Each should be stimulating the other's research projects. This need for intensification of research in man and in other organisms raises problems of finance and of shortages of trained research workers. Both these difficulties are likely to be intensified if new areas of work, such as that on tissue cultures, chemical mutagenesis, serology, biochemical genetics and epidemiological problems of genetic disease are to develop as rapidly as is desirable. The problem of manpower shortages, in regard to both biologists and physicians, tends to be perpetuated by lack of career opportunity for those working on genetics. There is also an insufficient number of institutions where an adequate training in genetics, particularly in human genetics, can be given.

It is possible that the results of much effort in these fields will prove disappointing. Nevertheless, research workers and those supporting their work must have the courage to face the possibilities of such disappointments and still go forward.

The developments of nuclear energy would never have been made unless enormous risks of failure had been accepted. These innovations have extremely important implications among which the possible effects on man's genetic composition are outstanding. If there is to be a climate of public opinion favourable to the development of nuclear energy the peoples must be assured that investigations essential for their future health and welfare and that of their children will be undertaken on an adequate scale. This will require recognition by governments that very substantial financial provision must be made for genetic and other biological investigations essential to an under-

standing of the effects of radiation on man. Biological research in the past has suffered severely from lack of funds.

Specific

The Group does not feel that it should attempt to recommend specific research projects. Nevertheless, it seems desirable to recognize the larger gaps in knowledge as they appear at the present time. Among the fields in which the need for further work is urgent, if the genetic hazards of the irradiation of human populations are to be understood, the following appear outstanding. It should be emphasized that the rapid developments in genetics and other sciences must determine that recommendations for lines of research should only be accepted as tentative and should be revised periodically.

(*a*) *Further study of spontaneous and artificially-induced mutation.*—There is need for further study of the number and kinds of mutations produced by various doses and types of irradiation applied at different stages of the life-cycle under a variety of conditions and utilizing different kinds of organisms. The relatively limited opportunities to study irradiated human beings and their offspring should be exploited to the fullest extent possible. The appreciation of radiation-produced mutations is intimately related to a similar extension of knowledge concerning mutations that appear to arise spontaneously or as the result of the action of chemicals and of physical agents other than ionizing radiation.

(*b*) *Mutational component in the somatic changes produced by radiation and other means.*—The role of changes in the hereditary material of somatic cells in the genesis of leukemia, in other forms of neoplasms, and in alterations in the life span, is at present a controversial field whch needs clarification. The effects of low doses of radiation, including those from radioisotopes, require special study. An important method of attack on this problem is opened by recent developments in tissue culture techniques.

(*c*) *Means of protection against mutagenic agents.*—The pioneer studies which indicate the possibility that the production of radiation-induced mutations can be modified by various means have important implications for man and require extension in many directions.

(*d*) *Development of new and improved techniques for the identification of mutants.*—Efforts directed at developing more exact methods for the recognition of mutant individuals, and the distinction between the latter and phenocopies, should be intensified. It is important to prosecute studies of the frequency of a wide range of types of mutations including those with extremely small effects, recognizable only through special statistical or breeding techniques.

(*e*) *Manner of gene action.*—The phenomena of dominance, synergism and other forms of gene interaction, the multiple effects of a single gene and the role of environmental factors in the determination of traits require a great deal of elucidation, since they are highly important in appraising the effects of radiations. They should be studied both in man and in other organisms. In this connexion the prospects raised by the rapid advances being made on human biochemical specificities are of particular interest.

(*f*) *Selective factors in populations, with particular reference to the special conditions in man.*—Very little is known concerning the detailed effects of natural selection on the frequency of specific genes, constellations of genes, or cytological alterations. Such information is basic to attempts to understand the genetic composition of present and past human communities and to predict future trends consequent upon changes in radiation levels, medical practices, and social and economic conditions. These gaps in knowledge can in part be filled by the collection of relevant demographic and experimental data.

(*g*) *Patterns of mating in human populations and their genetic implications.*—A standard type of information always required in understanding the genetic composition of human populations and the effect on it of various amounts of radiation is the recording and interpretation of data on the consequences of inbreeding, assortative mating, geographical and cultural isolation and random genetic fluctuations.

(*h*) *Twin studies in man.*—These are recognized as being helpful in understanding many problems of human heredity. Such studies have already been extensively used but could be advanced by standardized registration of twins in various countries. They give useful information concerning the relative importance of hereditary and environmental influences.

(*i*) *Determination of the frequency of diseases with a significant genetic component, with particular reference to their epidemiology.*—This is fundamental for investigations on the significance of mutation as a cause of disease in man.

In this connexion central registration of human inbreeding, hereditary disease and variation is of the utmost importance. It is also of importance to know the number of people who on account of hereditary lesions have to be treated in hospitals or institutions or given social aid.

(*j*) *Study of populations of special genetic interest.*—Important information is to be obtained from the study of relatively stable, primitive communities, long isolated by geography or culture. Studies of this type require for their execution teams of persons from a variety of disciplines, such as cultural anthropologists, physicians and geneticists. It should be emphasized that the understanding of the genetic structure of contemporary populations will be greatly aided through these studies, which should be maintained continuously over a considerable period of time. The opportunity for these studies diminishes with each passing year. Among special communities to be studied are those receiving unusually large amounts of radiation, those in which the degree of inbreeding has long been very high or low, and those in which special conditions of selection have prevailed. In some investigations radiation physicists would be essential members of the teams.

(*k*) *Genetic mapping of human chromosomes.*—This is a highly specialized field in which encouraging advances are now being made. Among the possibilities to be exploited is the use of these data to aid in the identification of independently occurring mutant genes and in the study of chromosome rearrangements.

(*l*) *Cytochemistry and human cytology.*—Direct cytological observations should be conducted both on normal individuals and on those with suspected chromosomal abnormalities. Material from the individuals themselves as well as mutant cells of tissue cultures may be used in such work. Basic information concerning the ultramicroscopic structure and chemical composition of the hereditary material, and the manner in which this is altered by irradiation and other mutagens, is essential and should include information on lower organisms as well as man. The new developments in biochemistry, the emerging immunobiochemical investigation of tissue proteins, bone marrow and other tissues, the metabolic investigationes which may elucidate both physical and mental pathology, the new developments in electronmicroscopy which advanced our knowledge of the structure of human sperm all indicate the devolpment of new tools for the study of human genetics.

(*m*) *Development of further statistical methods.*—New mathematical methods have continually to be developed to deal analytically with problems which arise as the result of researches in human and in experimental population genetics. This is particularly so in relation to observations on the genetic structure of and intensity of selection in populations with regard both to traits due to single gene and those due to multiple gene effects. Special techniques requiring electronic computers will also be required for analysing data on genetic linkage in man.

5. SOME CONCLUSIONS

(*a*) The Group is of the opinion that there are too few institutions or large university departments devoted to general genetics and even fewer concerned with human genetics. It recommends the establishment of such institutions and departments and suggests that there could be no one ideal pattern. One of the benefits of such institutions would be to accustom people of different scientific disciplines having implications for genetics to work together. Physicians, general biologists, geneticists, biochemists, cytologists, serologists and statisticians are examples of the find of workers who may be needed. When such institutions are concerned with human genetics their location should have regard to the adequacy of existing medical services, to the kind and size of human populations available for field studies and to the adequacy of background vital statistics and general demographic information of the population concerned. For many purposes a population of about two million is optimal particularly for intensive epidemiological investigations. Such institutions, in addition to their research functions, could eventually serve as centres of elementary and advanced training in genetics.

(*b*) Such research departments and institutions should contribute much to teaching in general and human genetics. Medical undergraduates should all receive training in genetics and the teaching should be co-ordinated with that in radiology and in the use of radioactive substances in medicine, so that the genetic hazards of diagnostic and therapeutic procedures are thoroughly understood. Medical men training as radiologists should have specific, more advanced instruction in genetics. Health physicists, radiological physicists and radio-

logical physicists and radiological technicians should also receive instruction in genetics as part of their technical training.

It seems essential that instruction in genetics should be given to all scientists, particularly those whose work is likely to involve the use of radiation and radioactive materials in research. The principles of human genetics could with advantage be conveyed to those training in the social sciences by means of formal instruction. Finally, the Group is of the opinion that public education in genetics should be more common and adequate than it is at present.

(*c*) In the future it will be necessary from the point of view of preventive medicine and genetic hygiene to register serious hereditary diseases and defects in various populations or countries in the same way as, for instance, epidemic diseases. For that purpose, genetic hygienic ascertainment or registration will be an indispensable and necessary step. The recording of hereditary diseases and defects in various countries and regions is to be highly recommended.

(*d*) In may countries there are very few biologists or physicians properly trained in genetics. This situation will only be solved by producing more career opportunities in genetics, but may be alleviated by granting fellowships or subsidizing training at approved institutions in countries which can offer training facilities. It is possible, also, that advice and technical assistance could be given in connexion with research projects in countries with insufficient rosources in trained manpower to carry them out.

(*e*) It might be possible for a United Nations Agency to assist on request in administration or supervision of studies of specific populations over a period of years or by strengthening a research team or by giving advice on organization.

(*f*) In the past, United Nations Agencies have done useful service in contributing to the collection and standardization of vital and health statistics. It is recommended that such agencies continue their efforts and stimulate the efforts of others in the collection and publication of specific data such as fertility, consanguineous marriages and parental ages, which are so essential as background information in many studies in human biology.

(*g*) The Group wishes to call attention to the evidence that damage to body tissues produced by radiation after relatively small doses is, at least in part, mediated through effects on genes and chromosomes. There is also some evidence that the life-span may be reduced in mammals even by relatively small doses. *Ad hoc* investigations are urgently needed.

(*h*) The Group is particularly impressed with the genetic hazards of man-made radiation from sources used in medicine, industry, commerce and experimental science, etc. Both as an approach to control and as providing basic background information for relating quantitatively radiation exposure and effects on man, it is essential that methods be found of recording exposures to individuals and populations, however difficult this may prove.

There is reason to believe that radiation exposure can be much reduced, therefore, those in charge of sources of ionizing radiations should always ensure that there is adequate justification for exposing individuals to doses however small. On account of the danger to offspring resulting from irradition of the gonads by X-rays, consideration should be given to determining what efficient means of shielding the gonads could be devised and brought into general use. In addition, in every exposure the X-ray beam ought as far as practicable to be directed so that a minimum of radiation reaches the gonads.

ANNEX 1

DAMAGE FROM POINT MUTATIONS IN RELATION TO RADIATION DOSE AND BIOLOGICAL CONDITIONS [1]

(Formerly entitled "The conception that mutations accumulate following repeated irradiation")

1. ACCUMULATION

One topic which I have been requested to discuss is that of the accumulation of point mutations following repeated irradiation. An accurately additive

[1] Submitted by Professor H. J. Muller, Department of Zoology, University of Indiana, Bloomington, Indiana, USA. (This paper is a considerably modified version of that presented at the Study Group on the Effect of Radiation on Human Heredity.)

accumulation in the germ cells throughout life has as its necessary and sufficient conditions (*a*) that the induced mutations are stable, i. e. not subject to repair, (*b*) that there is no important amount of intercellular selection to alter the relative frequencies of the mutant and non-mutant cells within a given individual during his lifetime, and (*c*) that radiation given at one time does not by some long-term aftereffect influence the mutagenicity of cells irradiate at a later period. These questions will be considered in turn.

(*a*) Changes of a point-mutational nature induced by radiation have not shown, as a class, unusual instability as compared with those arising spontaneously. Although the possibility is not excluded that there may be a relatively short period, of the order of one or a few cell cycles, before a mutation becomes fully completed and permanent (as in the work of D. Lewis, 1951, on *Oenothera*), this circumstance would not in ordinary cases affect the accumulation process.

(*b*) As for intercellular selection, except for the special case of drastic lethals arising in the X chromosome of a male, which have been shown in a series of experiments with Drosophila (by Kossikov, 1936, Shapiro, 1936, etc.) to be subject to selective elimination in spermatogonia, there is no reason to expect point mutations of the usual "recessive" sort, appearing heterozygously, to influence the multiplication or survival of immature germ cells appreciably. That mature germ cells are not thus influenced was shown long ago by Muller & Settles (1927). The most pertinent evidence on this point as regards immature germ cells, in an organism related to man, is given by experiments carried out by Russell (1951, 1956) in mice to test this very question. The failure of the mutation rate to decline in groups of offspring derived from spermatozoa ejaculated at increasing intervals after spermatogonial irradiation, shows both the absence of germinal selection against the mutant cells (point b) and the essential permanence of the mutant genes (point a).

(*c*) Direct tests of the accuracy of accumulation of lethals induced in Drosophila spermatozoa have been made by comparing their frequency at a given total dose after one treatment concentrated into a short time with that after a divided treatment of the same intensity and after a protracted treatment delivered at a low dose rate. It was found that the frequency depended on the total dose regardless of its distribution in time. When the diverse experiments of this kind carried out by different investigators (see review by Muller, 1954b, page 278, citing work of Patterson, Timoféeff-Ressovsky, Ray-Chaudhuri, Makhijani, Stern and others), are all taken into consideration together, it is found that the time-intensity relation was varied over a range of about 300,000 times, without any influence on the frequency of the mutations produced. Thus, a dose delivered in divided or protracted form over a month's time was as effective as one of the same total amount given in a few minutes. Tests have also been carried out, by Kerkis (1938), by Timoféeff-Ressovsky (1934), and recently by Oster (1955), that showed an additive relation when irradiation was given successively at two widely separated stages, to the immature and mature male germ cells respectively.

The reservation must be made that mutations not of the point variety, that is, those involving gross structural changes of chromosomes, which result from a combination of two or more independently produced chromosome breaks (Muller 1938, 1940) do, as expected, show an increase in frequency when the radiation is delivered in more concentrated form, provided union of the broken ends of the chromosomes can occur to an appreciable extent during the time of the longer treatment. This condition does not hold in mature spermatozoa, the type of cell used for most of the above-mentioned timing experiments, for union of broken ends cannot occur during this stage (Muller, 1940), but it does hold in other germ cells, in which therefore more lethals of the structural type result from concentrated than from very protracted or divided treatments (Herskowitz & Abrahamson, 1956). In the experiments cited in the preceding paragraph in which both immature and mature male germ cells were used there was no test of this matter, since the intervals between irradiations were long enough to avoid interactions between the effects of different exposures.

On the other hand, in gonial cells, which allow union of broken ends during treatment, relatively few of the mutations are of the "structural" type anyway. Moreover, low doses or dose-rates, such as those ordinarily encountered in human occupational exposures, produce relatively few structural changes as compared with point mutations even in the cells (spermatids and spermatozoa) most susceptible to their production, and produce still fewer in gonia. It must further be noted that at these low doses or dose-rates the rare structural changes which

do occur must in most cases have had both or all of their constituent breaks arising as effects of the same fast particle. The frequency of these changes would therefore, in such case, be independent of the time distribution of the irradiation. For these reasons, conditions would seldom be encountered, except in oöcytes, that resulted in overall frequencies of mutations (counting, together, both those of a point and of a grosser nature) differing perceptibly from those expected on an additive relation to the radiation dose. And when point mutations only were considered, the relation would be accurately additive.

2. LINEAR RELATION TO DOSE

Another expression of this additive relation, in the case of point mutations, is shown by the linear dependence of their frequency on radiation dose. That lethals induced in Drosophila spermatozoa do vary in frequency in this way has been abundantly shown for moderate and low doses, at which most of them are point mutations, in a great array of investigations, beginning with those of Hanson & Hayes (1929) and of Oliver (1930) and proceeding through many others to those of Uphoff & Stern (1949), which brought the dose down to 50 and 25 r. In experiments involving a lesser range of dose applied to spermatozoa of Drosophila, visible, non-lethal mutations, which include fewer structural changes than lethals, were found by Timoféeff-Ressovsky to show a linear relation to dose, and a linear relation for them was likewise found by the Indiana group when appreciable structural changes were excluded by cytological examination. Russell has also found a linear relation for visible mutations resulting from the irradiation of the spermatogonia of mice with moderate doses. A linear relation for visible mutations in higher plants was found by Stadler (1928) and in lower plants, for moderate doses, by Hollaender and others (see previously cited review).

It is true that in occasional experiments with very low doses results different from those expected on a strictly linear relation have been obtained. For instance, too few induced lethals seemed to be obtained by Caspari & Stern (1948) and too many induced visibles by Bonnier & Lüning (1949). However, these experiments were carried on at levels of dose so low that small sources of error had a relatively great effect. These sources of error include, in the case of visible mutations, differences in the degree of adverse selection against the mutants as between the control and treated series, caused for instance by differences in degree of crowding. In the case of both lethals and visibles, the numbers of mutations obtained at these doses are so low as to have a relatively large statistical variation. Moreover, the proportion of those obtained which were induced by the radiation is subject to a far higher error still, since it is represented by the difference between the frequency found at the low dose and that found in the control material. Inasmuch as at doses of 25 and 50 r the spontaneous (control) frequency may be a good deal higher than the induced frequency the error of this difference may be relatively enormous. This is especially true because the spontaneous frequency itself is subject to much more variation than that of random sampling. One source of such variation lies in the origination of mutations in clusters of common origin, caused by mutations in early germ cells. Another lies in the great differences between the spontaneous mutation rate existing in different lines, which may be as great as an order of magnitude and give evidence of being caused by genes (Muller, 1928), now called "mutator genes". Finally, both the spontaneous and the induced mutation rates vary considerably according to the history of the germ cells used (e. g. Muller, 1946, Lüning, 1952). Very special techniques are necessary for minimizing these various sources of error.

In view of these difficulties it is not surprising that experiments to test the linear relation have not yet been pushed below 25 r. However, genetic and other techniques have over the course of several years been worked out at Indiana which should now allow significant results to be obtained at doses as low as 10 or even 5 r. Work on the necessary scale would require the co-operation of a group working over some two years and examining several hundred thousand cultures, a project that we estimate might cost some $18,000. We are not especially desirous of carrying it out ourselves since even if the necessary support were provided the work should inevitably entail much digression from our other activities. We should therefore be glad to co-operate in furnishing the stocks and techniques and aiding in the supervision of the work if it were to be carried on elsewhere, although if no other suitable place could be found for the project we would not exclude the possibility of our conducting it.

The fact that the relation is linear, even at 50 r, and even when the irradiation of sperm cells is protracted for several weeks, makes it very probable already that the relation is linear all the way down to zero. For in some of this work it can be shown that there must have been hours between the traversing of a sperm cell by one ionization track and its traversing by another. If, however, the linear relation can be pushed down to doses as low as 5 r (or if at this dose the frequency can merely be shown to be more nearly proportional to the dose itself than to its 1.5 or .5 power), then we would have a justification for concluding with a very high degree of assurance that the relation was indeed linear all the way down to zero. This is because the ionizations are not produced separately but occur in the course of the tracks of the fast ionizing particles (the released electrons). Thus the ionizations come in spurts and a cell either gets a spurt or it does not. With very low doses, such as 5 r or less, an individual spermatozoon would hardly ever be traversed by more than one track, that is, it would not have received more than one spurt. Hence lowering the dose would not have the effect of lessening the number of ionizations in cells that received a spurt but only of lessening the number of cells that received any spurt at all. For these very low doses, then, the mutation frequency would be proportional only to the number of cells "hit," which is necessarily proportional to dose. Therefore we could justifiably extrapolate the results from 5 r linearly all the way down to zero. We need only make the one proviso here that the mutations produced in a cell by ionizing radiation result from ionizations or activations arising in that cell itself, not from those in the medium, but there is evidence from other work (see Muller 1954b) that this is true for mutations produced by ionizing radiation in Drosophila.

3. INFLUENCE OF LOCAL CONCENTRATION OF ACTIVATIONS

Even if we assumed the linear relation to hold all the way down to zero for X-rays and gamma-rays, this still would not mean that a given mutation necessarily results from just one ionization or excitation. For, many of the ionizations and excitations are grouped together in small clusters in the course of the tracks of the fast particles and it is possible that a cluster rather than a single quantum change is usually required to cause a gene mutation or chromosome break. At first sight it might be thought that this view is contradicted by the lack of influence of intensity changes on the dose-mutation rate relation, inasmuch as this result indicates that a given number of nearby ionizations when crowded together in time are no more mutagenic than when scattered in their time distribution. However, this inference is inapplicable to the question at issue, because the crowding attained in this way is much less than that within the minute clusters formed in the course of the track of a fast particle. That a cluster of such density is in fact more effective mutagenically than the same number of scattered activiations is shown by recent work (e. g. Ives, Mickey, Muller, all 1954) proving the higher effectiveness of neutrons than of X-rays in producing both point mutations and chromosome breaks. Other evidence to the same effect lies in the lower mutagenic effectivenes apparently shown by betatron radiation having an energy of about 15 Mev, as compared with ordinary Xrays, inasmuch as the radiation of higher energy is thought to result in a somewhat lesser amount of clustering than do ordinary X-rays (Herskowitz, Muller & Laughlin, 1956). I am inclined to view the greater effectiveness of more densely crowded activations in terms of the Watson-Crick model of chromosome structure, by supposing that a hit on both complementary strands at corresponding, or nearly corresponding, points is more likely to result in a permanent alteration in the chromosome than when just one of the strands is changed.

4. COMPLICATIONS AT HIGH DOSES

The breakage of chromosomes by radiation complicates in more than one way, at high doses, the relation between radiation dose and the observed frequency of visible or lethal mutations. For one thing, the ensuing chromosome abnormalities often kill the affected cells or their descendant cells by causing chromosome bridges at a subsequent mitosis, and, short of such an effect, can lower the multiplication rate of the descendant cells or even kill them by means of the resulting aneuploidy (the abnormal proportions existing between different chromosome parts). This circumstance would not in itself affect the observed frequency of point mutations were it not for the fact that germ cells in different

stages of the reproductive and mitotic cycles differ from one another in their susceptibility to having their chromosomes broken, and that they differ in a parallel manner in their susceptibility to having point mutations induced within them. At higher doses there is necessarily more killing off of the more susceptible cells, relatively to the less susceptible ones, by means of chromosome changes, than at lower doses (as well as more reduction of the multiplication rate of those not actually killed). Now, since the cells of the groups more injured in this way are also the ones that have had more point mutations produced in them it follows that at high doses there is more selective elimination (or reduction in relative numbers) of the germ cells containing point mutations as compared with the unmutated ones than there is at low doses. Hence, at higher and higher doses the frequency of point mutations observed among the offspring will fall further and further short (in a relative sense) of the frequency with which the point mutations had actually been produced, and the graph of the observed results will bend down ever further from the straight line extrapolated from the data obtained at low and moderate doses.

It is evident that the more heterogeneous in the susceptibilities is the lot of irradiated germ cells from which the given offspring are derived, the more pronounced will this falling off from linearity be. A very marked illustration of this effect, involving only a one-and-a-half-fold increase in observed lethal mutation frequency with a fourfold increase in dose (from 1000 to 4000 r) was obtained (Muller, Abrahamson, Herskowitz & Oster, 1954) by taking offspring from copulations of Drosophila males that had occurred seven to 10 days after their irradiation as just hatched imagos. The reason the effect was here so marked was because, as Lüning's already mentioned work had shown, the germ cells released during this period were at the time of irradiation in a number of different stages, having widely different susceptibilities. Although the irradiation of a quite homogeneous lot of germ cells would, theoretically, fail to give rise to any effect of this find, this has so far remained, in Drosophila, an ideal situation, that has probably not been obtained in practice.

Even gonial cells are of differing mutagenic susceptibilities, depending, for one thing, upon whether or not they happen to be in mitosis at irradiation. As Oster (1954) has shown, gonial cells containing the condensed chromosomes of mitotic stages (produced in this case by colchicine or acenaphthene treatment) are, like other cells with condensed chromosomes, more susceptible to radiation mutagenesis. This fits in with Russell's finding that the mutation frequencies found on examination of mice derived from irradiated spermatogonia, although linear for the dose range 300 r to 600 r, fell markedly below the expectation for linearity when a dose of 1000 r was used.

In organisms such as Drosophila and, probably, moulds, in which mutations of visible or lethal expression can arise in connexion with gross structural changes of chromosomes, either as position effects or as deficiencies, the complication exists that the frequency of these structural changes rises more rapidly than the dose (approximately as its 3/2 power, Muller, 1938, 1940). The observed mutants, unless analysed for gross structural changes, will represent a mixture of these and point mutations (the latter in turn consisting of gene mutations and minute structural changes, both of which vary linearly with the dose). Thus at lower doses, where the great majority of the mutations are in the point category, the frequency will be linearly related to dose, but at high doses, where the gross structural changes become numerically important, it might be expected that the overall frequency of lethal and of visible mutations would gradually rise, to approach the 3/2 power relation. Just this is seen in the results for visible mutations observed by Stapleton, Hollaender & Martin (1952) after irradiation of spores of the mould *Aspergillus*, whereas the offspring obtained after irradiation of mature Drosophila males have in most experiments seemed to show a linear relation for lethal and for visible mutations even at high doses. The interpretation of this at first sight paradoxical result is doubtless to be sought in the fact that in these experiments with Drosophila the germ cells used had been heterogeneous enough when irradiated to result in a tendency of the frequency to fall from linearity, in consequence of selectice elimination of the products of the more susceptible germ cells, and that this tendency largely compensated for the rise above linearity that would otherwise have been produced by the ever greater relative numbers of structural change mutants arising at the higher doses.

Because of these complications results with high doses are apt to be erratic and difficult of anlysis. Thus observations with moderate doses are better suited for arriving at an understanding of the fundamental frequency-dose relationship.

5. INFLUENCE OF CELL TYPE ON INDUCED MUTATION RATE

It has long been known (e. g. Stadler, 1928, Muller, 1930) that cells of different types or tages differ considerably in their susceptibility to mutagenesis by ionizing radiation. Although gross structural changes of chromosomes show the most variation in frequency with cell type, point mutations (including what are probably changes within a gene as well as minute deficiencies and rearrangements of one to a few genes) probably have a frequency range of at least fourfold when a given dose is applied to different types of germ cells. This is to be concluded both from results on lethals arising at moderate doses (at which relatively few of the changes are in gross chromosolne structure) and from visible mutations found by cytological observation to be free of discernible changes in the chromosomes.

Putting togther the results of earlier and later studies (see review previously cited and also recent papers by Bonnier & Lüning, 1953, Telfer, 1954, Abrahamson, 1956, and Oster, 1956), the early germ cells and gonia have the lowest frequency of induced point mutations yet the highest ratio of point mutations to changes of any kind that can be demonstrated to be structural (i. e. in these cells the structural changes fall to a minimum which is relatively much lower still). At these stages, the mutation frequency and distribution of types is much the same in male and female. In the later male germ cells, the overall mutation frequency, including that of recessive lethals, rises to a sharp maximum during the period of spermatid formation and transformation (although we must omit the preceding meiotic stages from consideration here as not being well enough known in this respect). Lüning has given reasons for inferring that much or all of the exceptionally high frequency of recessive lethals induced in the spermatid period involves those connected with gross and minute structural changes of chromosomes rather than true gene mutations. The overall mutation frequency, including that of recessive lethals, then falls sharply from the spermatid period to a second minimum to the immature spermatozoa (a minimum not nearly as low, however, as the preceding one in the gonia), only to rise again within the next few days until the time of ejaculation. After insemination, within the reproductive tract of the female, the male germ cells attain and maintain at a relatively constant level their highest known frequency of recessive lethals as well as of demonstrable structural changes, except for that found in the spermatids.

In rodents, the fact has long been known that ionizing radiation has a far more damaging effect on the genetic material when applied to mature or nearly mature male germ cells than when applied to immature ones (gonia), as judged by the killing of the resulting embryos. It remained for Snell (1935) to provide evidence that these effects, and the inherited "semi-sterility" which he found also to be induced in mice, were caused by gross structural changes of chromosomes, a class of effects with which we are not primarily concerned in this paper. Later, however, evidence was obtained by P. Hertwig (1941) that at those same stages there is also a relatively high frequency of production of point mutations by ionizing radiation, just as was known to be true in Drosophila. Fortunately, in man, the period during which the germ cells of the male remain in the gonial stage exceeds by over a hundred times that of the spermatid and spermatozoon stages, so that the high susceptibility of the latter stages presents a relatively minor practical problem. Thus it is the less mutable gonia of mammals, studied mainly by Russell, which are of greater interest in assessing the genetic damage produced by radiation in human populations. As noted in section 3, however, gonia themselves do not constitute one homogeneous class so far as susceptibility to mutagenesis is concerned, but may differ considerably, according to their developmental and mitotic stage and perhaps also their physiological condition.

As for the female germ cells, the point mutation frequency in the late oöcytes of Drosophila, during the last three or four days before ovulation, attains a level almost as high as that in the nearly mature unejaculated spermatozoa, when high doses of radiation are used (Muller, H. J., Valencia, R. M. & Valencia, J. I., 1950). However, in the previously mentioned work of Herskowitz & Abrahamson it was found that lethals induced at this stage show dependence on a higher power of the dose than 1, and on the timing of the dose, as well as other peculiarities, all indicating that a high proportion of them consists of small structural changes involving two independently produced chromosome breaks. These mutations (like many of those induced in spermatids and spermatozoa),

although not strictly point mutations, must usually be classed with them operationally since the making of the distinction is commonly impracticable or even impossible.

In mammals the germ cells of females may, according to one view, remain for a long time in a stage corresponding to the late oöcytes of Drosophila. It will therefore be important to determine to what extent mamalian female germ cells follow similar principles to those of Drosophila late oöcytes in regard to induced mutations. If they are long in such a stage, we should have to admit a notable departure from linearity for female germ cells. Whatever the answer may be, however, it is to be expected that for low doses, as for most occupational and diagnostic exposures, the frequency would be linearly proportional to dose even in late oöcytes (because of any given mutagenically sensitive region being so seldom traversed by more than one track), and that the frequency for a given low dose would not be lower in them than in gonia.

That somatic cells, like germ cells, can have point mutations induced in them by ionizing radiation was first shown by Patterson (1928), using Drosophila embryos and larvae. Calculations which I made on the basis of the early results, confirmed by studies by Timoféeff-Ressovsky (1929), and more recently by Lefevre (1950), show that for given genes the frequency of point mutations is similar to that obtained for gonia, or perhaps somewhat higher. This point is of importance in considerations of those effects of radiation on the exposed individual himself, such as leukaemia and other malignancies, which might have their basis in point mutations of his somatic cells.

With the development by Puck and his co-workers of methods of culturing and subculturing human somatic cells like micro-organisms, finding and breeding lines of mutant cells (1956b), and determining the effects of different doses of ionizing radiation (1956a), the way has now been paved for carrying forward to man the exact study of the induction of point mutations and other genetic changes in somatic cells. As an early result from this study, some evidence has already been adduced (Puck et al., 1956a) that the killing effect of the radiation on the cells is, as was to have been expected, caused by chromosome structural change rather than point mutation. It is probable on a number of grounds that this genetic killing of individual cells and genetic impairment of others, caused by gross chromosome changes, lies at the basis of much of the damaging effect of radiation on the body of the exposed individual, such as epilation, leucocytopenia, destruction of the intestinal lining and other manifestations of radiation sickness, production of cataracts, retardation and distortion of growth, reduction of regenerative capacity, and (probably the most important effect) reduction of the life span (see discussions by Muller, 1950b, 1956b, Quastler, 1956, Sacher, 1956).

6. ESTIMATION OF TOTAL DAMAGE FROM POINT MUTATIONS

The prime questions regarding the damage done to posterity by a given amount of radiation are, what will the total amount of that damage be, and how will it be distributed? In our previous treatment we have discussed how the frequency of lethal or visible mutations varies with dose and with type of cell, but we have not considered the absolute frequency of such mutations for any given dose, still less the total frequency of mutations of all kinds. It is this total frequency that counts. For, as long ago shown by Haldane (1937) and as later developed by Muller (1950a), in a population at mutational equilibrium (i. e. a population in which about as many mutant genes are dying out in each generation through death or failure to reproduce of the individuals containing them as are arising anew through mutation) the average reduction in fitness of an individual lies between the total frequency of all detrimental mutations, counting equally those with large and those with small effects, and twice that frequency. If all the mutant genes were strictly recessive the lower figure (the mutation rate, μ, itself) would apply, whereas if they were all dominant enough to be eliminated as heterozygates the figure would be wice this (2μ). As Muller (*ibid.*) pointed out there is good reason for inferring the higher figure, 2μ, to be nearly correct both in Drosophila and in man. This same figure for reduction of fitness would on the whole express the proportion of individuals in the population who would have to suffer "genetic death" (selective elimination by dying before maturity or failure to reproduce) to maintain the genetic equilibrium. Some reduction of the figure for elimination rate (probably by not more than a factor of 2) might, however, have to be made to allow for some synergistic operation by

detrimental genes: a mode of action giving individuals with multiple defects a lower survival rate than the product of the survival rates of those with the separate defects.

In estimating this total mutation rate for practical purposes only point mutations need usually be considered, since the great majority both of spontaneous mutations and of those that would be likely to be produced by radiation in a human population are of this nature. The first approach toward determining the total mutation rate in any organism was made independently and simultaneously in 1934–35 by Kerkis working in collaboration with myself and by Timoféeff-Ressovsky (see his and my review papers already referred to), using descendants of irradiated Drosophila males. Special techniques were used for the detection of mutations having neither a visible nor fully lethal effect, but only reducing the expectation of survival to maturity: the so-called detrimental mutations. Both pieces of work agreed that these mutations arise some three to four times as frequently as the fully lethal mutations. Essentially similar results have recently been reported by Käfer (1952), working under the guidance of Hadorn, and Falk (1955), working under the guidance of Bonnier.

It is admitted by all these investigators, however, that there had been little chance, by their techniques, of detecting mutations that reduced survival up to maturity by less than some 5–10 per cent. Moreover, there must be many mutations, undetectable by these techniques, the detrimental effect of which occurs mainly after maturity is reached or which affect reproductive capacity rather than individual survival. Thus the estimate that in Drosophila there are some five times as many harmful mutations altogether as the number of lethals, and some 30 times the number of sex-linked lethals, is a bare minimum, possibly only half the true value. It now becomes of great importance to extend the range of detected mutations to those with still less effect, and with other types of effect, so as to throw light on the extent to which the present estimate should be raised. As in the case of the proposed investigation of low dosage, we have for some years been developing techniques for such an attack in Drosophila, but again the work would necessarily be on so large a scale that group work and considerable expenditure (comparable in magnitude with that for the low dosage project) would be required.

In absolute numbers the above estimate becomes for a dose of, say 100 r applied to spermatozoa of young Drosophila males a day or two before their mating, or applied to late oöcytes, about one induced mutation in every 12 germ cells or one in six offspring. Thus a continuation of this exposure, applied to both sexes through many successive generations, would reduce the average fitness of the individual in the equilibrium population by about a sixth (some 17 per cent.) and would cause nearly one individual in six to meet "genetic death" in consequence of the irradiation. It can further be estimated (see below) that the total effect of spontaneous mutations in Drosophila is about half as much as this; that is, the given amount of radiation, applied at the stages specified, would constitute about twice the "doubling dose". But it should be borne in mind that these present estimates are in both cases minimal ones.

7. MANNER OF DISTRIBUTION AND EXPRESSION OF THE TOTAL DAMAGE

How does this mutational damage become distributed and expressed among the descendants? The amount of damage done by any given mutant gene in a heterozygous descendant may be represented as the amount of detrimental effect it would exert when homozygous multiplied by its amount of dominance (the ratio of its effect when heterozygous to that when homozygous). Now the dominance of lethals in Drosphila has been found both in work of Stern and his co-workers (see Stern et al., 1952) and of the present author and Campbell (see Morton, Crow & Muller, 1956) to average about 0.04 to 0.05, so that even these mutant genes with extreme effects would individually reduce viability in the heterozygote by only some 5 per cent. The merely detrimental genes are suspected on theoretical grounds (Muller, 1950a) to have somewhat more dominance than the lethals, and there has recently been some direct evidence for this (Falk, 1955), but even when considerable allowance is made for this possibility the effect exerted in a heterozygote by a detrimental is expected, on the average, to be less, absolutely, than that exerted by a lethal. Thus, taking individual mutant genes of all degrees, they should average well below five per cent. in individually lowering the fitness of the heterozygote. Since at the same time the visible effects of these genes in the heterozygote, *taken individually*, usually

escape notice, it follows that the effects of mutations induced by radiation in any one generation at a frequency comparable with that above considered would not ordinarily be observed among the next or any subsequent generation. Nevertheless the total loss of fitness in the next generation, being about one in six (the minimum frequency of offspring with newly induced mutations) times, say, one per cent. (to take a bare minimum for their average expression in heterozygotes) would in a population of 1 000 000 entail the "genetic death" of at least 1700 individuals of that generation. Moreover, a comparable amount of damage would continue to be exerted for scores of generations.

The number of generations that a mutant gene persists before causing genetic death is on the average about the reciprocal of the amount of damage it does to the heterozygote, so that the average Drosophila lethal in an autosome might be expected to persist for some 22 generations. However, the average persistence of a group of mutant genes is the *harmonic*, not the arithmetic, mean of the persistence of the individual mutant genes, and this value for the Drosophila lethals investigated turns out to be about 50 generations, though with a high error (see Morton, Crow & Muller, 1956). The persistence of detrimentals must be even greater. This is the so-called accumulation figure representing not only the average persistence of the mutant genes arising in a given generation but also the average amount of overlapping, within the individuals of any given generation, of mutant genes that arose in different generations, provided that the same mutation rate has existed in successive generations for a long period and mutational equilibrium has therefore been established. Hence if the 100 r exposure above postulated has been applied to Drosophila for many generations it is to be expected that each generation would be damaged at least 50 times as much as above calculated for the first generation of offspring (in fact, by an amount equal to 2μ or in this case 17 per cent.) Moreover, instead of one individual in six carrying a mutant gene induced by the radiation each individual would contain at least 50 X 1/6, or at least eight of them, on the average. Thus, although the effects of the mutant genes would seldom be individually noticed their collective effect would in the great majority of individuals be quite appreciable. It would of course tend to give a different pattern of impairment from one individual to another.

8. THE INDUCED IN RELATION TO THE SPONTANEOUS MUTATIONAL DAMAGE

The damage caused by the induced mutations is of course intermingled with that caused by spontaneous mutations. Although the amount of the radiation-induced mutational damage is largely independent of that caused by the spontaneous mutations it is helpful, in grasping its meaning, to compare it with that of the naturally existing mutational impairment since a species is in a sense adjusted to the latter and since, in man, we have a rough pragmatic familiarity with it. For this purpose it is desirable to be able to express spontaneous mutations in the same terms as those used above for induced mutations, namely, in terms of total mutation rate and loss of fitness. This is easily done, once estimates of these total values have been made for the *induced* mutations occurring at some given dose, provided only that the frequency of some particular group of mutations, e. g. sex-linked lethals, or visibles of a given collection or category (but preferably not those confined to just one allele series) has been determined under comparable circumstances both in unirradiated and in irradiated material. For there is good reason to believe that, for point mutations, the following relation will approximately hold: spontaneous total mutations/spontaneous mutations of given category-induced total mutations/induced mutations of same category. Thus, if figures are obtainable for the last three terms, the first one (the spontaneous total) can be solved for. The particular category best determined and most used for this purpose in Drosophila work has been that of sex-linked lethals.

Any *one* particular allele series (or "locus") cannot be relied upon by itself for the above purpose because the frequencies of mutation to different series may not bear the same relation to one another for spontaneous as for radiation-induced or otherwise induced mutations (see e. g. Giles, 1952). However, there is no reason to suspect that any broad phenotypic category or section of chromatin, or a whole group of allele-series chosen for their technical convenience, will show any consistent preference as between spontaneous and radiation-induced mutability. Experimental evidence that there is no such differential susceptability in Drosophila was obtained in the observation, by Timoféeff-Ressovsky (1937),

myself (see Patterson & Muller, 1930), and others, of the similar ratio of sex-linked lethals to sex-linked visibles both in unirradiated and irradiated material (especially when allowance is made for the relatively higher frequency of deficiencies and other structural changes after irradiation).

As noted above, the ratio of "total" mutations to sex-linked lethals in Drosophila when radiation is used has been estimated to be at least 30, and we may therefore (in accordance with the preceding formula) multiply the spontaneous sex-linked lethal frequency by 30 to obtain the spontaneous total. The problem arises, however, of what observed value of the spontaneous sex-linked lethal frequency to choose. For this value has been found to vary by at least an order of magnitude from one experiment to another according to the stocks used (Muller, 1928, confirmed by later workers) and to vary by more than half an order of magnitude according to the developmental history of the germ cells (Muller, 1946 and unpublished), not to speak of variations caused by temperature and other environmental differences within the natural range. However, the upshot of a large number of studies of the spontaneous sex-linked frequency in Drosophila, by different investigators, has shown that the great majority of individuals bred at 25° C. under reasonably favourable conditions, in such manner that the germ cells used to produce the offspring do not give undue representation to those with extreme developmental histories, have a sex-linked lethal frequency averaging about 0.1 to 0.2 per cent. This is true in both sexes, but the female value appears to vary less with germ cell history and commonly to approximate 0.17 per cent. whereas the male value, which is higher (0.2 per cent.) for the sperm released very early, is a good deal lower (e. g. 0.06 per cent.) for those released in what might be called the prime of life. Taking 0.14 per cent. as a reasonable average and multiplying it by 30, our minimum figure for the total spontaneous mutation rate per gamete is 4.2 per cent. and that for the zygote is 8.4 per cent., a figure which also represents the average reduction in fitness or risk of genetic death as a result of spontaneous mutations. It was on the basis of this estimate that an irradiation of 100 r given to Drosophila in the manner specified in section 6 was there stated to constitute about twice the doubling dose, inasmuch as it had been calcuated to give an induced rate of 17 per cent. per zygote.

From the above it will be seen that, in Drosophila at least, there is much more uncertainty about the amount of spontaneous mutational damage, because of the high variability of the spontaneous mutation rate, than about that caused by any given amont of radiation applied to a known stage or group of stages. Because of this uncertainty, determinations of the spontaneous mutation rate of any particular category of mutants in Drosophila, such as a given group of "visibles", should always, in order to have significance in relation to other work, be accompanied by a yardstick indicating the general mutability characteristic of the material studied. At present the most convenient such yardstick is to be found in the sex-linked lethal rate, which must be ascertained under precisely the same conditions. Only when such a yardstick is provided can we, for example, use data on the frequency of spontaneous mutations of given types to estimate the ratio they bear to the total mutation frequency, or to the frequency of some other particular category, inasmuch as these other quantities themselves are properly expressed in relation to a corresponding yardstick.

It is true that the radiation-induced rate also varies to some extent according to the stocks used (see below), the environmental conditions, and the germ cell stages involved. These differences, however, are not of a type which would usually throw our reckoning off nearly so much as in the case of spontaneous mutations since there is more knowledge of how they may be allowed for. But they must be taken into account.

9. SPECIES DIFFERENCES AND THE PROBLEM OF EXTRAPOLATION

In view of the evidence already referred to of the variation of the radiation-induced frequency of point mutations in Drosophila according to the type of cell irradiated, and the abundant evidence that has been obtained in recent years of the influence of conditions associated with the irradiation, such as oxygen concentration, enzyme-inhibitors, etc., on the frequency (see author's review, 1954b), it would be strange if genetic differences failed to affect the result. Indeed, Dubovsky (1935) reported that some stocks of *D. melanogaster* from widely separated localities differed by a factor of about 2 in the frequency of lethals produced by irradiation of the male. It is true that such differences can

be produced in the same stock by slight differences in the timing of the germ cells used, a matter not then realized, and that stocks may also differ genetically in their natural timing, yet genetic differences of many kinds would be expected to be capable of influencing the result. In the light of these considerations, however, it is rather noteworthy that, contrariwise, even the specific difference between *D. simulans* and *melanogaster* was found by Kóssikov (1935) not to be associated with a significant difference between the induced frequencies of lethals of flies of these two kinds. This similarity may indicate that the induced frequency, like the spontaneous one (see below), even though readily altered, tends to be maintained at a certain level by some active selective processes operating on features that, perhaps as a by-product, tend to maintain susceptibility to these mutagenic factors at the level found.

However that may be, it is not to be expected that widely different species, such as those of different phyla, would have similar induced or spontaneous mutation frequencies, either total or of any given overall phenotypic class and/or chromosomal type (such as sterility mutations or sex-linked lethals), nor that they would have a similar ratio of total mutation rate to that in such a category. One reason for this disparity is that the amount and distribution of the genetic material must differ enormously as between such organisms; another is that the processes whereby the genes reach expression must be so different that a superficial resemblance in effect would provide little or no indication of a homologous genetic basis. Thus even if the frequency of production of, for example, sex-linked lethals were known in a mammal, one certainly would not be justified in applying to this figure the Drosophila factor of 30 times, to estimate the total frequency of induced mutations in the mammal.

The case is, however, different when we use as our index of relative mutation rates in two widely different species a category consisting of the average frequency of origination, in each species, of members of a single allele (or pseudo-allele) series, often called the "specific-locus rate", provided that this average has been determined through observations of a number of different series ("loci") in each species and that most of the values found for the different series of the same species show (as they have done) a tendency to be clustered within about one order of magnitude. The reasonable agreement between the results for some 12 different allele-series involving visible point mutations (including those that are at the same time lethal) after irradiation of spermatozoa of Drosophila Muller, 1954a and unpublished) and also for some seven series after irradiation of spermatogonia of mice (Russell, 1952, 1956, Kimball, 1956), justifies us in speaking of an average or modal induced mutability for such an allele-series in each species. We may then infer that differences in the detectability of the mutations of the different series, in the complexity of the genetic regions concerned, and in their actual mutability, are usually insufficient to cause inordinate discrepancies between the values for the different series.

In Drosophila the ratio between the "total" and the average single allele-series rate is at least 10 000 (e. g. Muller, 1955b) and is probably a good deal higher. This value has been obtained by multiplying the ratio of "all" detrimentals and lethals to sex-linked lethals by the ratio of the latter to the average single allele-series frequency. (These two constituent ratios have of course been obtained in different experiments, under different conditions.) Are we now justified in assuming that a mammal would have at least as high a ratio as a fly of the "total" to the average single allele-series rate, and may we therefore multiply the latter rate, as determined in Russell's irradiation experiments, by 10 000, to obtain the minimum value for the total induced mutation rate in mice?

The justification for this procedure lies almost entirely in general considerations. The main consideration is that a mammal, by no matter what criterion, stands at least as high in the scale of biological organization as a fly, and probably a good deal higher as judged by its complexity of gross and histological structure, physiology, and behaviour. It would therefore be surprising if the genetic basis of the mammal were not as least as complicated and, accordingly, compounded of as many parts (such as nucleotides) as that of the fly. This would imply also that it had at least as many, and probably more, different ways of mutating, and that any one allele-series, on the average, represented no larger, but probably a smaller, fraction of all the mutational potentialities in the case of the mammal than in the case of the fly. The several times greater DNA content of the mammalian than of the Drosophila chromosome-set tends to support this inference.

It is to be noted that this method of obtaining a minimum estimate of the total induced rate in the mouse avoids any assumptions regarding the means of defining the limits of a gene or locus, and the number of such entities. It is true that in the past the argument has usually been stated in terms of genes or loci (but see Muller, 1955, 1956a, 1957) but this has, for the present writer at least, been only a short-cut mode of expression. For, what was meant by the "specific locus" frequency was really the frequency with which mutations arose that were on operational grounds to be classed as being probably members of the same allele series, without assumptions being made as to what proportion of mutations actually occurring in the chromosome region in question would fall into the given allele category. Moreover, although 10 000 was sometimes stated to be a minimum value of the number of genes or loci, as estimated by several very different methods, the justification for using it also as the ratio of total mutations to mutations in one average allele series ("specific locus") was that, empirically, the experiments on detrimental mutations, lethals, and allele-series mutations had shown this ratio to hold, no matter what the number of genes may be, or how we define them. It is quite possible, for instance, that some of the same chromosome regions that gave rise by mutation to members of a given visible allele series also gave rise to lethals and/or detrimentals (which may or may not have been included in the count of the allele-series frequency, according to whether or not they also produced the visible effect that served as the criterion), but this was irrelevant to the determination of the ratio since all the lethals and detrimentals of sufficient detectability to be recorded as such were included in the measurement of the frequency of these classes and therefore in the "total" rate. Thus the only relevant questions concerning the validity of the extrapolation process for obtaining a minimum estimate are whether or not a sufficiently representative sample of allele series has been obtained, and whether we are willing to admit the probability of the proposition that the average allele series, as operationally defined, would constitute at least as small a fraction of the total mutation rate in a mammal as in a fly.

If we grant these points and apply our factor of 10 000 to Russell's observed allele-series rate of 25×10^{-8} mutations per r in the spermatogonia of mice, we find as our minimum estimate of the total induced frequency in this material 25×10^{-4}, which may also be expressed by saying that there is at least one mutation per germ cell for every 400 r. As for the human induced mutation rate, we can at present only say that this is what it would be if it were like that of mice, that there are no data from man as yet that are inconsistent with this, and that this rate is about an order of magnitude higher than the induced rate in Drosophila.

On the other hand, we do have for man, as well as for the mouse, some data that allow us to estimate the spontaneous mutation frequency for allele series. As this matter has recently been discussed elsewhere (Mueller, 1957), an appraisal of the validity of this evidence will not be attempted here, except to point out, first, that the determination for man has the advantage of being based on large-scale data that give, as it were, a cross section of results from different genetic lines and from different ages and conditions of reproduction, and, second, that the results of the different allele series agree reasonably well with each other and, what is more surprising, that their consensus agrees well with the average based on mice.

Here again, then, is evidence of the operation of selective processes that tend to stabilize the mutation rate, as was noted in section 8 in connexion with the radiation-induced rate. Even more striking evidence of this, in the case of spontaneous mutation is the unexpected similarity between both these human and mouse values for the spontaneous allele-series rate and that (in the neighbourhood of 0.5×10^{-5}) deduced to be characteristic of Drosphila. It is true that thus far there has only been one series of experiments (Muller, H. J., Valencia J. I. & Valencia, R. M. 1950) in which a considerable group of spontaneous allele-series rates in Drosophila has been directly determined and in which at the same time a yardstick (sex-linked lethals) was used so that the obtained rates could be converted (as proved necessary) into more typical ones. However, approximately the same figure had been reached earlier by taking the typical spontaneous sex-linked lethal rate and dividing it by the ratio found to hold between the induced sex-linked lethal rate and the induced allele-series rate. Moreover, confirmation of the order of magnitude of this value (although probably involving some reduction of the value itself) is now being obtained in another series of direct observations, checked by lethals, conducted by Schalet at the Indiana

Laboratory. In any case, such a correspondence between such different species tends to impart confidence in the estimated orders of magnitude.

When, now, the factor of 10 000 is applied to the estimated value for an allele series in man, taking for the latter the rather conservative figure of 10^{-5}, we find that the minimum estimate of the "total" spontaneous mutation rate turns out to be 0.1 per gamete or 0.2 per individual, a value higher than has commonly been suspected to apply to our own species.

10. LIGHT FROM ANOTHER SOURCE

Extrapolation of the type above discussed is not the only means of arriving at estimates of the spontaneous mutation rate in man on the basis of existing data. As explained by Morton, Crow and Muller in a parallel paper (1956, see also Crow, 1956 and Muller, 1957) several different studies of the mortality found among the offspring of consanguineous as compared with non-consanguineous matings in man agree reasonably well in giving evidence from which it can be deduced that the average human gamete carries a mutational load accumulated from past generations which if it became homozygous would be twice as much as needed to kill the individual containing it at some time between a late foetal and early adult stage. Much of this load is probably scattered among diverse mutant genes any one of which would, if homozygous, entail a relatively small risk of death. There must in addition be a considerable load of detrimental genes in the gamete that tend to cause death before or after the period studied, or that interfere with reproduction rather than survival. Moreover, in a population living under more primitive conditions than those studied, more genes would find such expression than did so in the given populations. Finally, the individual himself carries twice as many such genes as the zygote. All in all, then, the load carried, mainly heterozygously, by the zygote is probably (if expressed in terms of the damage it would do homozygously) as much as about eight "lethal equivalents."

Now this rather directly measured load does not in itself tell us anything of the mutation rate per generation. However, if there are means of obtaining a reasonable estimate, by extrapolation or otherwise, of the relative amount of expression which this load actually attains in the average individual (a matter dependent upon the degree of dominance of the mutant genes and of the frequency with which occasional homozygosity occurs), we should then have a value for the average reduction of fitness. As noted previously, this would be almost equal to μ (the total spontaneous mutation rate) if the eliminations in the given population are brought about mainly through the homozygous effects and almost 2μ if the dominance is enough for elimination usually to be caused by the heterozygous effects. Now although the data from man are insufficient to allow us to set a value for the average dominance of mutant genes there are considerations (pointed out in some of the above papers) that allow us to set some fairly reasonable limits to such a value. Moreover, the value found for Drosophila lethals lies well between these limits. It is also possible to arrive at reasonable limits for the frequency of homozygosity caused by inbreeding. If then we extrapolate by taking the value for dominance found in Drosophila, and at the same time use in our reckoning the human inbreeding factor, we reach a value for reduction of fitness of approximately 0.1 per gamete or 0.2 per individual. This in turn gives us, as the value for the "total" spontaneous mutation rate, μ=0.1 per gamete, as was estimated by the other method, explained in section 9.

It must be pointed out that the present method involves data and methods of calculation both of which are entirely separate, as well as different in character, from those used in the other mode of attack. Although extrapolation is employed at one point in the present attack—namely, for estimating the degree of dominance—this item did not enter at all into the earlier calculation. Moreover, there seems little doubt, in consideration of observations concerned with man himself (see e. g. Levit, 1935), that the dominance factor in man would at least be within the same order of magnitude as that here assumed on the basis of extrapolation. If this is true, then the estimate for mutation rate here arrived at is likewise of the right order of magnitude, at least as a minimum value. A further circumstance to be taken into consideration in evaluation of the present result is that it was not realized until the calculations were carried through that they would give a value even distantly in agreement with what had been obtained by the other method, and that no attempt was made to manipu-

late them to obtain a satisfactory fit to expectation. For these reasons, it would seem that the present result, although itself involving interpolation, lends material support, from an independent direction, to that arrived at previously.

Although the present mode of attack is concerned only with spontaneous mutations, the estimate of the total spontaneous rate, as well as of the total load, thereby arrived at, affords an important independent possibility for gauging the total mutational damage which would be produced in a human population by radiation. Before this could be accomplished, however, there would have to be some means of determining, for some limited genetic category capable of being used as an index, the relation between the spontaneous rate and the rate induced by a given dose of radiation. Possibly somatic or tissue-culture mutations, if there were good reason to infer them to be of the point type, would be useful to provide such an index. At any rate, if it were once furnished, it would then be relatively easy to combine this information with that on the total load, derived from the results of inbreeding, so as to obtain a realistic view of the all-around and long-term meaning of a given dose of radiation.

Of course we are far from the final or exact answers concerning the total frequency of either induced or spontaneous mutations, or concerning the persistence factor, for any lower organism, and much further yet from these answers for man. But the ways are opening up, and there seems good reason to believe that our present estimates for man, although involving extrapolation, may with assurance be regarded as minimal ones, and of the right order of magnitude. Before this point could be arrived at it was necessary to carry out a vast amount of work in the genetics of lower organisms, and also to collect very considerable data from man, and to consider these in connexion with one another. An increasing attack along both lines will be necessary if we are to attain the knowledge we need for the adequate protection and the fostering of our most precious trust, our genetic heritage.

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Annex 2

The Types of Mutation Produced at Known Gene Loci and the Possibility of Hitherto Unrecognized Mutations Being Induced Animal Population Irradiation Results and Work Needed[1]

It is commonly acceptable as a working hypothesis that ionizing radiations do not induce new types of mutation, but only raise the mutation rates of existing alleles. The basis of this assumption is partly theoretical and partly experimental. The theoretical argument rests on the fact that all living matter is continuously exposed to natural background radiation, and always has been so exposed; therefore, it is argued, any mutation which could be induced by ionizing radiation must already have been induced by natural background radiation at some time in the past; therefore no new type of mutation could be induced by man-made radiation. The experimental data, which are now extensive, do not disprove this; but in so far as it is essentially a negative hypothesis, and fails to specify the extent of either spontaneous or induced mutation, it is by its very nature not amenable to experimental test. Thus if in some experiment radiation exposure induces mutations of a type previously unknown, this can always be explained as nothing more than a manifestation of the limited nature of prior knowledge of spontaneous mutation; conversely, if exposure fails to induce mutations of a type previously known, that can always be explained as a manifestation of the finite nature of the experimental set-up. The hypothesis that ionizing radiations do not induce new types of mutation is therefore, like so many others in biology, unprovable and undisprovable. As such, it can only be of heuristic value; the extent of its value depends on our assessment of the extent to which it may be true, and the extent to which we are willing to use it as a guide in planning future action.

In point of fact, geneticists are willing to place so much faith in its validity that this hypothesis forms the basis of all present day estimates of the genetic hazard of ionizing radiation to man. It is therefore worth while to ask if circumstances can be visualized in which it might break down. The supposition underlying it is that man-made radiations do not differ in any essential respect from natural background radiations. In respect of dose-rate they extend far beyond the natural range, but we have no clear evidence of dose-rate thresholds for the induction of genetic effects. So far as present knowledge goes, it seems that linear energy transfer is the biologically most important characteristic of a radiation; and in this respect natural background radiation covers the whole known range, from the sparse ionization of naturally occurring gamma rays to the dense ionization produced by alpha particles and heavy cosmic nuclei. Thus there does not at present appear to be any obvious theoretical reason for expecting man-made radiations to induce alleles that were previously unknown. On the other hand, there is no theoretical basis for the converse supposition, namely that ionizing radiation can induce all known alleles; in fact, there is a certain amount of experimental evidence that it tends to induce especially the more extreme alleles at a locus.

It is worth noting that though this hypothesis has a theoretical basis which is probably valid for mutagenesis by ionizing radiations, the analogous hypothesis for chemical mutagenesis has none. There is no ground for postulating the natural occurrence in biological material of all chemical mutagens which might be synthesized in the laboratory. Furthermore, some chemical mutagens might be expected to have a relatively mild action, and induce subtle genetic changes, compared with the generally destructive action of ionizing radiation. Recent experimental work in this field, notably that of Fahmy & Fahmy (1956), supports an interpretation of this type. Furthermore, if (as seems probable) ionizing radiation is responsible for only about one-tenth of human spontaneous mutation, leaving nine-tenths to be accounted for, we should be unwise to ignore the possibility that chemical substances may be much more important than ionizing radiation as a cause of human mutation.

Thus far I have considered the gene only as the unit of mutation, its constancy between mutational events being implicit. But a gene is also a unit of action, its presence being recognizable only by its effect on the phenotype of an individual. Furthermore, the final effect of a gene, unlike the gene itself, may be extremely variable, depending upon the other allele at the same locus, the alleles at other loci and the mass of non-genetic factors, grouped together under the term

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"environment". In no two individuals are the total genotype and the total environment identical, and therefore in no two individuals can the same allele be expected *a priori* to produce identical end effects. Variability of gene expression may be very great where the end effect is putatively remote from the primary gene product, with many morphological mutants; conversely it may be relatively slight where the effect observed is believed to be close to the primary gene product, as with the blood group antigens. In so far as the practices of civilization have wrought great changes in the macro and microenvironment of man, we must suppose that they have changed and are changing the expression of many human alleles.

Many systems can be invented for the classification of human genes, and which particular system is used will depend on the interests of the user. The population geneticist is interested primarily in the biological value of a genotype. He will therefore classify alleles according to their average effect on the fitness of their carriers, that is to say on the number of zygotes that will be contributed to the next generation by a zygote of the present generation. It will be a twofold classification, according as the allele is in the homozygous or heterozygous state. Mutant alleles are probably almost always disadvantageous to some extent when homozygous, but their action in heterozygotes may vary from severe detriment through neutrality to advantage. This fact divides them into two broad classes: those which are unconditionally disadvantageous, and those which are disadvantageous in some individuals but advantageous in others. The distinction is fundamental, for it determines the nature of the forces which will maintain the allele in the population and the frequency at which it will be maintained. An unconditionally detrimental allele will be maintained at a low frequency under the opposed action of mutation to the allele and natural selection against it. On the other hand, an allele which is advantageous in some individuals and disadvantageous in others will be maintained at a high frequency, depending on the degree of advantage or disadvantage in the various individuals. Mutation will play only a minor role, or even none at all, in determining the structure of the population in respect to alleles of this type. It is therefore of importance to any assessment of the genetical hazards of radiations to man to know whether alleles of this type are of common occurrence. Unfortunately it is a problem of exceptional inherent difficulty, because we may expect that the more easily recognized genes will largely be among those with notably detrimental effects; and, conversely, that the conditionally advantageous genes will be mainly among those with minor effects and that they may, for just this reason, be difficult to recognize.

This brings me to a point where I find it necessary to voice some misgivings which I have felt for a long time about one aspect of what might be called "genetical public relations". Soon after H. J. Muller's demonstration that X-rays have a mutagenic action it was realized that they present a genetic hazard to man. At that time genes were thought of as consisting mainly, if not entirely, of common, advantageous, wild-type alleles and rare, deleterious, mutant alleles. They were unconditionally good or bad. Mutation was viewed as a necessary evil; it was something which happened, without which the species would lack the heritable variation on which future evolution depends, but it introduced into the population a load of mutant alleles which had to be eliminated by processes of natural selection. Each mutational event implied the occurrence of another mutant allele to be eliminated sooner or later through the "genetic" death of some individual if equilibrium were to be maintained.

I do not think anyone seriously doubts that this is a reasonably accurate representation of the state of affairs in respect of grossly deleterious autosomal dominant or sex-lined genes such as retinoblastoma or haemophilia. On the other hand, I think may geneticists would now doubt whether this concept is valid for more than a relatively small proportion of all human genes. Clear-cut unconditionally deleterious oligogenes may be relative rarities. They may represent only one tail of a distribution; numerically they may come far behind the polygenes, each with an effect so small as to be virtually undetectable by the methods of classical genetics, yet together of major importance because they regulate the quantitatively variable characteristics of each species through which evolution must largely operate. Now the outstanding feature of almost any quantitative character is that it has a central optimum; the extremes in either direction appear to be at a disadvantage, in respect of biological fitness, compared with some intermediate phenotype. The theoretical interpretation is that heterozygotes for genes affecting a quantitative character have a greater

biological fitness than the corresponding homozygotes; and this implies that the mutation rate may be relatively unimportant in determining the gene frequency.

The above argument has been based mainly on theoretical considerations; but there is now a great mass of observational and experimental evidence that heterozygosity is the rule rather than the exception in wild populations. If any-one doubts it, he should re-read the writings of Dobzhansky and his co-workers on wild *Drosophila* populations, of Bruce Wallace on irradiated *Drosophila* populations and of Dunn on mouse populations; or he should try inbreeding any species that is normally crossbreeding.

In the face of all this I find it disconcerting that geneticists, when writing for the public, still often base their argument on an assertion that all mutation (or very nearly all) is harmful. I say it is disconcerting in the full knowledge that I use exactly the same argument when, as happens all too often nowadays, I have to give a talk on radiation hazards to an intelligent but gentically uninstructed audience. Perhaps its attraction is that it is a relatively easy argument to put over; or perhaps we use it because one can draw quantitative inferences about the genetic load due to some unconditionally deleterious human alleles, whereas at present it is almost impossible to speak quantitatively about human polygenic characters. But, whatever the reason, it is extremely important that we geneticists should not bind ourselves to the fact that unconditionally deleterious oligogenes may constitute only a small fraction of the human genome. Furthermore, I am not entirely happy that any science can be really healthy when it has one story for home consumption and another for the rest of mankind.

The necessity for using an argument such as this stems essentially from one fact we know something about mutation in man and experimental animals, but we know very little about the effect on a population in which mutation is induced. We know enough to be reasonably certain that the current theory of mendelian populations is over-simplified and unable to accommodate some essential features of real populations; but we have not yet got a satisfactory theory to put in its place. For the present there can be only one corollary; we must have more research on the genetic structure of populations ,in the hope that the nature of the facts will become clearer and will stimulate the development of a more complete theory. This theory would have to cover the origin and loss of variation in populatons its origin by spontaneous or artificially enhanced mutation and by environmental action, and its loss by natural or artificial selection.

There have been many genetic studies of wild populations. Though in most of them the object was to study the effects of natural selection, it is only rarely that direct evidence has been obtained that the effect observed really was due to this cause. Thus the spread of melanic forms of various species of moths in industrial areas has been observed for over a century; and it has been assumed throughout that the spread was due to a selective advantage of the melanic form, following an environmental change from the relatively clean agricultural to the sooty industrial economy; but it was only last year that Kettlewell (1955) was able to confirm the validity of this assumption, by direct observation of the numbers of moths of the various phenotypes taken by bird predators. It has also been a characteristic of studies of wild populations that, with few exceptions, the material studied has been polymorphic. This must have been due largely to subjective selection by the investigator, since a polymorphic population holds an obvious interest which a monomorphic population lacks. Nevertheless, where an apparently monomorphic population has been sufficiently closely observed, it has often proved to be polymorphic, even though the polymorphism may have been cryptic. Obvious examples are the populations of various *Drosophila* species studied by Dobzhansky and his school, (vide Wallace, 1954), and the mouse populations studied by L. C. Dunn (1953). Dunn's work is of especial interest, because it shows that mechanisms whereby coadapted blocks of genes could come into existence are not peculiar to *Drosophila*. The mechanism in the mouse differs from that in *Drosophila*, but the effects are the same: suppression of crossing over and selective advantage of the heterozygous genotype in which it has been suppressed, even at the cost of a high proportion of inviable homozygotes. His findings gain significance in the light of the recent demonstration by my colleague Dr. Mary Lyon, using an induced translocation, that the region of suppressed crossing over is at least five times as long as the short segment marked in Dunn's experiments.

The study of mutation and artificial selection in the laboratory and of natural selection in wild populations are three approaches to a much more difficult study, namely that of populations with mutation rates that have been enhanced by ioniz-

ing radiations or other mutagens. Nor must it be forgotten that ionizing radiations have other genetic effects besides mutagenesis; they increase crossing over, a fact which was known before their mutagenic action was discovered, and which may be of great importance in the study of polygenic systems.

Thus far few have attempted to work with irradiated animal populations. History dictated that one of the first studies in this field should be of a human population; but the genetical work of the Atomic Bomb Casualty Commission was almost foredoomed to failure, in the sense that it was very unlikely that statistically significant observations could have been made, even on the basis of the most extreme assumption, namely that all human "spontaneous" mutation is really induced by background radiation and that the doubling dose for men is consequently as low as 3 or 4r. In the event the results were, with one possible exception, negative; but all who are concerned with planning human radiation genetic studies in the future will owe a debt to Neel and his colleagues for doing the pioneer work in this field and exposing some of the problems (Neel *et al*,. 1953). The only other genetic studies of irradiated human populations of which I am aware are those of Crow (1955) and of Macht & Lawrence (1955); in each case the irradiated population consisted of radiologists. Here also the results were, in the main, negative; and the work suffered from the further limitation that it was impossible to estimate, even roughly, the radiation dose received.

There remain the experimental studies of irradiated animal populations. Of these there have been exceptionally few; and in almost all the experimental material has been *Drosophila melanogaster*. There are two reasons for this: first, a population to be maintained under known irradiation conditions must almost of necessity be kept in the laboratory; second, to guard against the possible effects of genetic drift, the effective breeding population should be at least of several hundred individuals. These requirements of laboratory culture and population size can be reconciled only by limiting the size of the individual animal. Subject to this limitation, *Drosophila melanogaster* is the obvious choice, being exceptionally well known genetically. We hope to develop techniques at Harwell for maintaining mouse populations in the laboratory, but I am doubtful whether it would be feasible to keep free-living populations of larger animals in an irradiated space. A possible solution might be to find an isolated wild colony and irradiate its habitat; this procedure would have the inherent defect, however, that one cannot be sure of obtaining a truly comparable control population; and in this work controls are a *sine qua non*.

If anything were needed to show how wide is the gap between observational fact and existing population genetic theory, the few published studies of irradiated *Drosophila* populations would do it. The various writers have given up any attempt to interpret their observations in terms of gene frequencies, contending themselves with observing what happens in their populations and attempting to interpret their observations in terms appropriate to the polygenic systems studied. Various combinations of selection type and mutational status have been used, and various types of foundation population. Bruce Wallace (1952) has observed the effects of natural selection on biological fiitness in populations originally derived from an inbred strain but now heterogenous, which were exposed to various levels of acute and chronic irradiation. Buzzati-Traverso (1954) likewise observed the effects of natural selection in irradiated populations; but here the foundation populations were inbred and the effects observed were egg-production and the incidence of the *non-spineless* phenotype due to modification of the genetic milieu in a homozygous *spineless* population. Clayton and Alan Robertson (1955) likewise used inbred foundation populations; they observed the variance of the number of abdominal bristles and the response to artificial selection for this character. Scossiroli (1954) selected for sternopleural hairs in an irradiated population which was genetically heterogenous but which had previously been selected by Mather without irradiation and had reached a plateau.

It is too early to attempt to draw general conclusions from these experiments, but some things are clear. Buzzati-Traverso's work shows that irradiation of an inbred population can release genetic variability in a character such as egg-production, which is one of the components of biological fitness, and can thereby enable natural selection to increase fitness. Scossiroli's work shows that irradiation can release genetic variability in a heterogeneous population which has reached a selection limit, and can thereby enable the limit to be surpassed. Bruce Wallace's work shows that populations can live success-

fully under conditions of irradiation in which a large proportion of their chromosomes carry gene combinations which are lethal when homozygous, but that some of these combinations may be advantageous when heterozygous. The work of Clayton and Robertson shows that the amount of genetic variability arising spontaneously through new mutation in each generation is only a minute fraction, perhaps a thousandth, of that normally present in a *Drosophila* population; and that a part only of the additional genetic variability released by irradiation may be available for selection.

Just what the full implications are for human genetics it is impossible at present to assess; but two conclusions seem inescapable. First, it is essential to extend work of this type and to cover other species, including mammals, with a much lower reproductive potential than *Drosophila;* the results might be very different in species where the female produced only ten young instead of hundreds and selection differentials were consequently lower. Second, we have no mandate from experimental fact to extend to the whole human genome the theoretical treatment of the genetic hazard of radiations that we now apply with a fair measure of confidence to grossly deleterious gene mutations. It follows that for the present we must limit quantitative assessment to this part of the hazard alone; and this implies that the first task of human genetics must be to identify as completely as possible that part of the social load which is due to genes in this class.

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ANNEX 3

A DISCUSSION OF SOME OF THE PROBLEMS ACCOMPANYING AN INCREASE OF MUTATION RATES IN MENDELIAN POPULATIONS[1]

Problems arising from the exposure of man to irradiation are extremely numerous. They bear on many aspects of his health and his children's health. To the extent that the original exposure—medical or industrial—aims at improving man's welfare, he benefits, to the extent, however, that the exposure does him bodily harm or induces gene mutations that will harm his offspring, he suffers.

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The mutagenic effects of radiation pose problems of immediate concern to the geneticist. These problems are of three major types: the development of a theory of population genetics adequate for the formulation of predictions, the design of experiments capable of testing the theory and of supplying empirical values for various parameters, and the extrapolation of theory and experimental results to human populations.

The postulated role of mutations in Mendelian populations depends largely upon the basic concept one entertains regarding the genetic structure of populations. In the main, there exist two contrasting but not mutually exclusive concepts: the first is based upon the superiority of homozygous individuals; the second, upon the superiority of heterozygotes.

The first concept postulates that individuals of the highest possible fitness can be completely homozygous. Natural selection acting within a constant environment would favour these individuals and would tend toward the establishment of a population composed entirely of homozygous individuals. In such a population the individuals of each generation should, ideally, be identical and the individuals of one generation should be identical to those of the next. Mutations in a population such as this operate to frustrate the aims of natural selection. By definition, the new mutations are deleterious and, consequently, their constant formation prevents the population from reaching the level of fitness theoretically possible. Furthermore, under equilibrium conditions the deleterious effect of mutations on the population is a function of mutation rates and is independent of the harm done to any one individual by any one mutation. Theoretical treatments of this problem have been given by Haldane (1937), Crow (1952), and, in great detail by Muller (1950). Although no one actually believes that environmental conditions are constant or that the ideal population described above actually exists, the model is reasonable nevertheless if one assumes that near-equilibrium conditions exist at any moment and that genetic changes within populations occur slowly (see, for instance, Haldane, 1954).

The second concept assumes that even under constant environmental conditions the individual with the highest fitness is genetically heterozygous rather than homozygous. Furthermore, there need not be one ideal genotype but many. An ideal population of this sort would consist of individuals as phenotypically uniform as possible consistent with the demands of natural selection but these individuals would be genetically diverse. Similarly, individuals of one generation would not be genetically identical, even under ideal conditions, with those of the next. The selective coefficient of any gene in this type of population would be a function of the genetic situation prevailing within that population. Since the population consists of individuals of diverse genotypes, selection would be constantly shuffling gene frequencies and selective values simply because of the uncertainties associated with the formation of chance gene combinations. The details of this model have not been developed in a way comparable with the first; one can say, however, that gene frequencies under this model are primarily a function of selection and only secondarily a function of mutation rates.

These two concepts are not mutually exclusive. It may develop that one or the other is substantially correct. It may be that for some loci one is correct while for others the second applies. It is quite probable that different species differ in their genetic structure. Finally, at different times and in different places the genetic structure of a population may shift from one model to the other.

A few remarks may be made regarding the logic underlying these two concepts of population structure. Genes and chromosomes are the means by which information is passed from one generation to the next; in some cases they are the only means, in others this hereditary information is supplemented by the "spoken" word which allows individuals of one generation to communicate with those of the next. The first concept, that based on the superiority of homozygous individuals, stresses the accuracy of the transmitted information. In the absence of mutations and of environmental change, every individual of a generation would be supplied with precisely that information which has proven valuable in the past. There is no wastage through the formation of ill-adapted individuals. Furthermore, it is a moral system in that, under ideal conditions, every individual is his neighbour's equal. The second concept entails wastage; certain individuals must obtain hereditary information that is not perfectly accurate. In so far as this wastage can be equated with suffering (and it certainly can be considered in this way for human beings), the second concept is morally deficient.

What arguments, then, can be mustered to support the second concept, and to justify giving it serious consideration? First, to the extent that genes are semi-dominant, their frequencies are changed much more rapidly by the action of selection on heterozygous individuals than by that on rare homozygotes. Second, a gene that is beneficial through some semi-dominant effect need not be beneficial when hemozygous; the nature of these homozygous individuals is unimportant to the population at the time selection favours the heterozygotes. Third, the replacement of superior aa′ individuals by equally good a″a″ individuals requires that the allele a″ also be advantageous when heterozygous. Fourth, there are physiological reasons for doubting in some instances whether a single allele in homozygous individuals can actually duplicate the action of two contrasting alleles in heterozygotes.

One difficulty confronting the second concept is more apparent than real. It arises from the geneticist's inability to distinguish which of two alleles is a favourable dominant and which is a deleterious recessive (see Crow, 1952, footnote p. 285). A geneticist can detect gene effects by substitution only. Genetic changes within a population are determined by the sequence in which mutations occur. By completely ignoring the sequence of genetic change and by regarding the favourable dominant as "normal", one is forced to the absurd conclusion that the origin of each favourable dominant (or semi-dominant) lowers the fitness of the population and that the population regains its normal fitness only if the new dominant attains fixation in the population.

Finally, in reference to the first concept, I have mental reservations that stem from the assumed independence of the effect of the gene mutation on the population and the effect of the gene on individuals of the population. In other words, no matter how slight the deviation from the "normal" allele, the effect of a given class of mutant alleles is said to be proportional to mutation rate alone. In fact Muller (1948) mentions the possibility that small harmful mutations may be even worse for the population than fully lethal ones. I do not question the calculations that demonstrate this fact; I question the assumptions upon which the calculations are based and which result in a curve with such an abrupt break regardless of how infinitesimal the effect of the mutation might be.

The problems mentioned so far lie in the realm of theoretical speculations. They are problems one meets when attempting to visualize techniques employed by Mendelian populations in meeting the demands of existence, techniques compatible with the known facts of genetics. The second large class of problems arises in connection with the design of experiments aimed at testing the validity of theoretical models. Regardless of one's concept of the genetic structure of a population, obtaining experimental data to verify the concept or to furnish evidence regarding certain parameters is an overwhelming chore.

Information required for the manipulation of equations under the model that stresses homozygosity includes estimations of numbers of loci, total mutation rates, distributions of mutations in terms of their effects on various components of fitness (viability and fertility in particular), the distribution of deleterious mutations among individuals of a population, and dominance-recessive relationships. Information along these lines is being gathered, among other laboratories, at Oak Ridge and at the University of Indiana under the direction of Dr. Russell and of Professor Muller, respectively. I believe we all recognize the tremendous effort required to obtain this information.

In our laboratory we have taken what appears superficially to be a somewhat simpler approach: the simultaneous analysis of the genetic content of experimental populations of *D. melanogaster* in terms of genes affecting fitness and measures of fitness itself. The latter measure will be required for the final verification of one's concept of population structure regardless of which of the two one entertains. The chief difficulties in this approach lie in the estimation of fitness and in determining the amount of selection required to maintain this fitness. These difficulties are compounded by the necessity to limit one's studies to components of fitness and to carry out the analyses outside the population, outside even an experimental population. In studies of components of fitness one generally assumes that these components are to some degree correlated with one another and with their sum. Robertson (1955) has pointed out, though, that in a population at genetic equilibrium the components of fitness must be negatively correlated. This indicates that a technique for measuring total fitness must eventually be found if the role of mutations in populations is to be evaluated experimentally.

Difficulties associated with the determination of selection pressures within populations do not seem insurmountable at the moment. Specific genetic changes within populations offer one source of information—for instance, the increase in frequency of one particular mutation, the establishment of equilibrium frequencies, or the loss of mutations following the cessation of irradiation. Estimations of population size shed light on the extent of inter-progeny selection; that is, one can judge whether a population exists because a few parents leave many offspring or because many parents leave a few each. Furthermore, larval mortality rates can be altered substantially without changing the adult population size to any appreciable extent; manipulations of this sort will offer an approach to the study of intra-progeny selection.

The final group of problems deals with the extrapolation of theory and experimental findings to man. The first problem that comes to mind is the shift in emphasis demanded by the importance of man's intellect. In experimental material "fitness" is equated with the ability to live and to reproduce; the emphasis in eugenic studies on the differential fertility existing in relation to I. Q. and racial origins shows that the experimental concept of fitness is not completely acceptable for human populations. Furthermore, although a long life and a full life is highly desirable, length *per se* is not all-important. Although the pertinent facts lie outside the realm of genetics, I suspect that the change from a 60-hour to a 40-hour work week has added more pleasurable, livable years to the average working man's life than he has lost by way of industrial and automobile accidents. These and similar problems are concerned with values; although there may be a consensus of opinion regarding these matters, there are bound to be sharp disagreements between societies and persons and even sharp changes in the views held by the same individual at different times.

Additional problems arise, too, because man is a social animal. The two concepts of populations described earlier dealt with ideal individuals with the highest possible fitness; these concepts are applicable to populations in which, with the exception of mating, there is no interaction between individuals in determining the fitness of the population. Such concepts are inadequate for dealing with populations of social organisms in which the fitness of the population is a function not only of the fitness of the individual members but also of the interaction between individuals. It would seem that before one can approach the problem of the ideal genetic architecture of populations of social organisms, including man, one would have to solve the simpler problem of the ideal constellation of phenotypes. I do not recall having seen such an analysis for human populations.

The next problems to be discussed concern what may be described as experimental human ecology. The central problem concerns the extent by which the visible human population, or, better, the reproducing human population, differs from the initial population of fertilized eggs from which it came. How strong are the selective forces operating within human populations? Mortality figures are available for the post-natal and late pre-natal periods. Figures are undoubtedly available, too, for the proportion of individuals who remain childless throughout life. Good data concerning the mortality of individuals in the early post-fertilization periods are not available at the moment. Lacking, too, are indications of the extent to which this mortality and sterility (effective, if not actual, sterility) are selective; random elimination, of course, is ineffective in bringing about genetic changes within populations. Haldane (1954) has developed a method for estimating the intensity of selection that utilizes phenotypic measurements only; this method may prove valuable in the analysis of human populations. Other data that would shed light on the selective potentialities of human populations are those dealing with the rapidity with which resistance to certain diseases has spread within memory of man and the effectiveness of this newly acquired resistance; this information would need to include the price in terms of mortality that the affected populations paid while selection operated. Along these same lines, it would be of particular interest to determine the factors responsible for limiting the number of children per couple in many human societies. When the average number of offspring per pair falls irrevocably below two for any species, that species can no longer replace itself numerically from one generation to the next and extinction is inevitable. In some human communities the present average is but slightly above two. Since this average is determined by a combination of sociological and biological factors, some effort should be expended to determine

the actual biological limit for the number of offspring human couples can have.

If it should develop that selection is more effective in man than we have suspected, we must nevertheless be wary of those who claim that radiation will do no harm to the human species. The rate at which mutant genes enter the gene pool of a population must equal the rate at which they leave. Mutant genes leave the gene pool by the effective elimination of individuals either through death, sterility, failure to reproduce, or a tendency to reproduce at a reduced rate. Effective elimination of individuals means, for human beings, that one individual is placed at a disadvantage relative to another; in many instances the "elimination" is accompanied by mental or physical suffering. Therefore, regardless of the ability or inability of "natural" selection within human populations to forestall extinction or to maintain the "fitness" of the population as a whole, we are still forced to the conclusion that every exposure of individuals to irradiation must be justifiable in terms of the beneficial effects that exposure confers either to the exposed individual or to the population as a whole. In the light of known effects of radiation it is impossible to defend unnecessary or unnecessarily high exposures.

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ANNEX 4

THE EXPOSURE OF MAN TO IONIZING RADIATIONS, WITH SPECIAL REFERENCE TO POSSIBLE GENETIC EFFECTS [1]

The purpose of this review is to show which generally occurring sources of ionizing radiation may at present be relevant and which may be irrelevant in discussing the effects of ionizing radiations on man.

We have to consider the direct effects on human tissues, as well as the indirect effects due to mutations of somatic cells, causing harmful effects to the individual himself, or of germ cells. The latter may either involve risks for the offspring already in the next generation, consequently being of interest to the individual himself, or may—with irradiation of a large number of inhabitants—constitute a long-term problem in the entire population.

The present sources of ionizing radiations which are of interest in these connexions include the following:

Natural sources of radiation

1. Sources of cosmic radiation.
2. The natural radioactive elements, particularly radium, thorium and potassium in the earth crust.
3. The natural content of radioactive elements in man.

Man-made sources of radiation

4. Radioactive material and technical arrangements producing radiation (X-ray tubes, other particle accelerators and nuclear reactors) used under such circumstances that the user generally is aware of the presence of the radiation (e. g. in education, science, medicine, and industry).
5. Sources of radiation used for purposes in which, as a rule, only the specialist is aware of the presence of ionizing radiation (e. g. radioactive luminous compounds on watches and other articles for common use, television sets, etc.).
6. Radioactive elements artificially distributed in nature.

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THE MAXIMUM PERMISSIBLE LEVELS OF IONIZING RADIATION FOR INDIVIDUALS AND LARGE POPULATIONS

Before treating the different sources of radiation which contribute to a larger or smaller extent in producing the present level of ionizing radiation in man, a brief account of the maximum permissible doses recommended may be given.

The International Commission on Radiological Protection (ICRP) at its session in Geneva, April 1956, decided to make the following additions to their earlier recommendations:

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A *controlled* area is one in which the occupational exposure of personnel to radiation or radioactive material is under the supervision of a radiation safety officer.

For such personnel the maximum permissible levels of exposure are those specified for occupational exposure. In the case of prolonged exposure to radiation from external sources the maximum permissible levels for occupational exposure are represented by weekly doses of 600 mrem in the skin and 300 mrem in the blood-forming organs, the gonads and the lenses of the eyes.

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For any person in any place outside of controlled areas the maximum permissible levels of exposure of 10% of the occupational exposure levels.

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When genetic aspects of the effects of radiation are considered, the dose received by the whole population is of importance. Scientific data derived from human as distinct from experimental animal populations are so scanty that no precise permissible dose for a population can, at present, be set. The available information is being assessed by the Commission and other groups including geneticists. Until general agreement is reached, it is prudent to limit the dose of radiation received by gametes from all sources additional to the natural background to an amount of the order of the natural background in presently inhabited regions of the earth.

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The recommended maximum permissible weekly doses and the modified values for special circumstances, permit a desirable degree of flexibility for their application. In practice it has been found that in order not to exceed these maximum limits and also to comply with the general recommendations of the Commission "that exposure to radiation be kept at the lowest practicable level in all cases" a considerable factor of safety must be allowed in the design of protective devices and operating procedures. Therefore, under present conditions, it is expected that the average yearly occupational dose actually received by an occupationally exposed person would be about 5 rems and the accumulated dose in the employment period up to 30 years of age would be about 50 rems. Accordingly, the Committee recommends continuation of the present conservative practice as regards doses actually received by occupationally exposed personnel, to keep the accumulated dose as low as practicable especially up to age 30.

In the report of the Medical Research Council (MRC)[a] "The Hazards to Man of Nuclear and Allied Radiations," issued in June 1956, the following conclusions are drawn:

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(2) Dose levels to the individual: (a) In conditions involving persistent exposure to ionizing radiations, the present standard, recommended by the International Commission on Radiological Protection, that the dose received shall not exceed 0.3 r weekly averaged over any period of 13 consecutive weeks, should, for the present, continue to be accepted.

(b) During his whole lifetime, an individual should not be allowed to accumulate more than 200 r of "whole-body" radiation, in addition to that received from the natural background, and this allowance should be spread over tens of years; but every endeavour should be made to keep the level of exposure as low as possible.

(c) An individual should not be allowed to accumulate more than 50 r of radiation to the gonads, in addition to that received from the natural

[a] Medical Research Council, "The Hazards to Man of Nuclear and Allied Radiations", London, 1956.

background, from conception to the age of 30 years; and this allowance should not apply to more than one-fiftieth of the total population of this country.

(3) Dose level to the population: Those responsible for authorizing the development and use of sources of ionizing radiation should be advised that the upper limit which future knowledge may set to the total dose of extra radiation which may be received by the population as a whole, is not likely to be more than twice the dose which is already received from the natural background; the recommended figure may indeed be appreciably lower than this.

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In the report of the National Academy of Sciences in Washington (NAS)[8] "The Biological Effects of Atomic Radiation" the following recommendations are made.

Thus we recommend:

(C) That for the present it be accepted as a uniform national standard that X-ray installations (medical and non-medical), power installations, disposal of radioactive wastes, experimental installations, testing of weapons, and all other humanly controllable sources of radiations be so restricted that members of our general population shall not receive from such sources an average of more than 10 roentgens, in addition to background, of ionizing radiation as a total accumulated dose to the reproductive cells from conception to age 30.

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(E) That individual persons not receive more than a total accumulated dose to the reproductive cells of 50 roentgens up to age 30 years (by which age, on the average, over half of the children will have been born), and not more than 50 roentgens additional up to age 40 (by which time about nine tenths of their children will have been born).

Obviously it is generally agreed that at present it is desirable to limit the doses received by the gonads of individuals to less than 5 roentgens per year and 50 roentgens before 30 years of age and that the average dose to the gonads of the population as a whole should be kept very low; of the order of the natural background (ICRP) twice this level (MRC) or to 10 r before 30 years of age (NAS). The difference in these figures is not very important as their order of magnitude will in practice be about the same.

According to our present knowledge it seems likely that a dose of 30—80 r will (according to MRC) double the natural mutation rate in man, which is probably only to a minor fraction (perhaps about 10%) caused by ionizing radiations. The rest of the natural mutations will, to an unknown extent, be due to chemicals and to the thermal movements of the molecules. It seems to be highly desirable for mutations induced by chemicals in particular to be investigated, in order to elucidate the relative role of radiation-induced mutations.

The recommendations made by the organizations quoted above are as regards the whole population based mainly on the natural level of ionizing radiation. It is not the place here to discuss whether this is a corerct starting point, nor whether or not the maximum permissible dose levels recommended are reasonable. They are fixed after careful consideration based on our present, unfortunately very incomplete knowledge of the biological effects of small radiation doses, but are agreed to by specialists in biology, genetics, haematology, physics, and radiology having long experience in radiation protection both on the research and on the practical side.

With respect to the risks of injurious effects on man one matter may, however, be stressed. There must always be a reasonable ratio between what can be gained by the use of ionizing radiation and the risks of its injurious effects. The use in medicine of ionizing radiation for examination and treatment of patients therefore occupies an exceptional position, that has not always been taken into account in recent years, in the discussion of the problems of the general irradiation of mankind. There must certainly be a sound balance between the benefits of a good health service and the risks to the patients with respect to onset of malignant disease or genetic damage of which, however, we at present do not know very much.

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[8] National Academy of Sciences, "The Biological Effects of Atomic Radiation", Washington, 1956.

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NATURAL SOURCES OF IONIZING RADIATION

Cosmic radiation

The cosmic radiations produce the doses shown in Table I.

The values for O—4000 m have been calculated from the work of Compton and co-workers (Fig. 1) taking into consideration that some reduction due to absorption may be justified indoors, and the values for 6000—18,000 m from Millican and co-workers (Fig. 2). The values are fairly approximate, as there are many factors which are difficult to allow for at the higher altitudes, especially with regard to the unknown relative biological effectiveness of heavy nuclei rays.

Variations with time.—Major variations in cosmic radiation occur during short periods only, and the few occassions associated with an appreciable increase are so rare and of such short duration that the doses caused by cosmic radiation for a certain altitude and geomagnetic latitude may at the earth surface be regarded from the practical point of view as constant. A record of the variation in the cosmic radiation on February 23, 1956, is shown in Fig. 3. This is one of the occasions on which an extraordinary increase was observed. The dose due to this temporary increase was, at sea level, less than 0.03 millirem.

As to the long-term variations, it seems highly unlikely that any major variations in cosmic radiation have taken place during the last 2,000 years.

Variations with site.—The maximum variation between different places on the earth surface, excluding mountains of more than 4000 m high, is about 2 r per 30 years.

Doses to individuals.—The doses to individuals may be of importance for very high altitudes. The present development of communication by air makes it necessary to take into account the fact that, at very high altitudes, the maximum permissible dose of 50 rem may especially at high geomagnetic latitude already be exceeded if on the average some 10 hours per week are spent at this altitude during 10 years, which might well be possible for future personnel in aircraft. The increase in cosmic radiation on February 23 may perhaps at altitudes of 20,000 metres correspond to a dose of less than some tenths of 1 rem obtained during a few hours, therefore probably being of limited biological significance.

Doses to large populations.—The contribution to the irradiation of large population groups (>100,000) varies between 0.7 and 2.7 r or approximately between 1 and 3 rem per 30 years.

The fact that an appreciable part of the radiation can be screened off by reasonable quantities of material may be of certain value for judging the risk for stratosphere and intersteller traffic. Investigations of the biological effects of cosmic radiation at very high altitudes are however desirable, because of the lack of knowledge as to the RBE values for heavy nuclei radiations.

Natural external γ radiation

The external γ radiation in nature varies with the radium, thorium, and potassium content of the ground and of the building material in houses. The γ dose in free air produced above level ground can be calculated according to the simple formulae:[1]

Radium: dose in r per 30 years	$0.57 \cdot 10^{12} . s(Ra)$
Uranium: dose in r per 30 years	$0.20 \cdot 10^{6} . s(U)$
Thorium: dose in r per 30 years	$0.094 \cdot 10^{6} . s(Th)$
Potassium: dose in r per 30 years	$41 . s(K39)$

in which s(Ra), s(U), s(Th) and s(K39) are the contents of radium, uranium, thorium, and potassium in g element per g substance of the ground.

To obtain an estimate of the dose to the gonads in rad,[4] the doses in free air have to be multiplied by a factor of 0.5 for women and 0.7 for men (1) or on the average 0.6, to account for the absorption in the shielding part of the body. The same factor may be approximately applicable to most of the other organs For the skeleton, the factor might be considered on the average, to be about 0.8.

The doses due to natural γ radiation over ground containing various minerals are given in Table II, and the dose in dwellings in Sweden (1) are seen from Table III and Fig. 4. These are in good agreement with the few observations in other countries.

[4] 1 rad corresponds to a dose of about 1.07 r in soft tissue.

The γ radiation from the ground is absorbed by snow, as seen from Fig. 5. A snow cover of 40 cm depth and of medium volume weight absorbs about 50% of the γ radiation from the ground.

A factor of considerable importance is the relation between the time spent indoors and out of doors. Here, it is assumed that on an average in large population groups ¼ of the life is spent out of doors.

As an additional contribution to the irradiation of man from natural radioactive elements in the earth crust, the radon and thoron of the air may play an important role in special cases. In general, the content of these elements in the air is too small to contribute to the dose received by the human body by more than a few percent. In some places and during some periods, this content can be fairly high, for instance in rooms where water of high radon concentration is used or the ventilation is insufficient, (1) in cellars where radon and thoron comes up from the earth, and in large cities during calm weather. (2) Such cases seem only occasionally to have been investigated. It would probably be worth while to make more systematic studies in this field. At present, these sources of natural radiation are too little known to be treated in this survey and will therefore be disregarded, although it is possible that they are of significance for the irradiation of the pulmonary system of a comparatively great number of individuals living in certain areas.

Variation with time.—The average annual dose to human beings due to natural sources has probably been of roughly the same magnitude during the present geological period. A slight decrease in the radiation occurred when man learned to use wood for building houses, and stopped living in earthen huts or in rocks where the amount of radon in the air was sometimes probably quite high. An increase subsequently took place again with the use of bricks and concrete as building materials, and when people moved to cities where material containing minerals more frequently comprise the surrounding material.

Another factor which may have caused a reduction in the environmental γ radiation for some populations may have occurred during the ice periods followed by certain areas covered by ice and snow during a greater part of the year than today. As already shown the snow causes an absorption of the γ radiation from the ground which appreciably reduces the irradiation out of doors, and produces a seasonal variation (see Fig. 6) in the irradiation of large population groups, especially those living outside the cities.

Variation with site.—As a rule, the difference in the level of natural γ radiation in different parts of the world is probably not very large. Even over areas containing rich uranium or thorium ores, the γ doses to the inhabitants only in rare cases exceed a few times the normal level. This is because the ores are generally very unevenly distributed both in rocks and sands, and are often covered or surrounded by material of normal radioactivity. The inhabitants moving over the area in question might thus, on an average, be exposed to doses which are much lower than could be conceived. This experience based on observations in Sweden needs further verification, but will probably be found to apply to most population groups throughout the world.

The doses of γ radiation to persons living in places more or less permanently covered by deep ice or snow, and those spending most of their time at sea, are generally very small. Here, the amount of γ radiation from the earth is often so minute that it can be entirely disregarded in comparison with the radiation from other natural sources. Recent investigations of the radiation level on wooden and iron vessels of different size have shown that the γ radiation already at a few metres distance from a granite quai is entirely negligible.

In Table IV some observations of natural γ radiation in Sweden are compared with the results of similar investigations in the UK and USA.

In view of the statements in the aforegoing with respect to the average doses received by individuals, it would be of interest to carry out long term measurements by means of personal monitoring, in order to arrive at reliable data regarding the doses actually received.

The natural content of radioactive elements in man

In areas where the radium content of drinking water and food is not exceptionally high, the potassium content of human tissue is the main source of internal irradiation (see Fig. 7). The doses in rad due to the amount of potassium 40 (0.012% in natural potassium) in some human organs is shown in Table V. With respect to some tissues, particularly bone, the data of different authors vary considerably.

The content of carbon 14 and radon contributes about 5 and 10%, respectively, of the average potassium radiation.

According to measurements by Hursh & Gates (3) and recently by Sievert & Hultqvist (4) (Fig. 8), the radium content of the ekeleton is probably less than $0.3 \cdot 10^{-9}$ g, in areas with a radium content in the water of less than 0.2 $\mu\mu$ C per litre. According to Spiers (MRC), the mean dose to the osteocytes is about 6 rem per 30 years for $0.5 \cdot 10^{-9}$ g total radium body burden. The radium amount is, however, very unevenly distributed in the skeleton, and the dose significant for the production of osteosarcoma therefore seems to be extremely difficult to assess.

The *variation with time and site* in the natural internal irradiation is mainly a question of the variations in the radium content of water and food and of the radon in the air. Referring to what has already been said, it may be stated that there are not at present sufficient data available to give any reliable figures for different areas in the world. This also applies to the problem of the natural radioactive elements taken up in the pulmonary system. With respect to these matters, references can be made to a recent publication by Hultqvist (loc. cit.) in which an extensive bibliography is given.

The common limits of the doses to large population groups (>100,000) and to individuals from natural radiations are collected in Table VI.

MAN-MADE SOURCES OF IONIZING RADIATIONS

Radioactive material and technical arrangements producing ionizing radiation used under such circumstances that the user is generally aware of the presence of the radiation

Here, *occupational exposure* and *exposure of patients* undergoing treatment or investigation in radiology are the two matters to be considered.

The doses received by those carrying out work with ionizing radiation in education, science, medicine, technics, and industry are in most cases small, as the personnel can generally be adequately well protected. Furthermore, in all work where patients are not involved, there is no reason to permit irradiation which can in any way cause ill-effects. Here, the maximum permissible levels for individuals and large population groups are exceeded in rare cases only.

In radiology, especially some procedures in γ-ray therapy and in examinations using X rays, circumstances do not always permit entirely satisfactory protection of doctors and personnel. Here, the individual dose will sometimes be close to the maximum permissible levels, or even occasionally exceed them.

The occupational doses contribute to the radiation per capita of whole populations with an amount which in the UK (MRC) has been estimated at about 2.5 r per year as an average for about 14,000 people in research, medical and industrial work, and at about 0.4 r per year for about 7000 people in atomic energy work. Altogether, the average gonad dose per capita due to occupational exposure is estimated at 0.0016 r per year or, if 10 years is supposed to be the average period of work before reproduction, the relevant average gonad dose for the whole population may be less than 0.02 r before 30 years of age. An estimate of the corresponding figure for Sweden has given a considerably lower figure.

The occupational dose is apparently over the whole world attributed mainly to medical radiology, but is presumably very uncertain. It seems, however, that occupational irradiation does not at present contribute to the gonad dose of whole populations with any appreciable amount.

The doses received by patients undergoing treatment and examination by means of ionizing radiations, on the other hand, are of decisive importance, since they contribute by far the largest exposure of the population to man-made sources of radiation. In Germany, France, Sweden, the UK, and the USA, investigations have been carried out in order to ascertain the doses to the patients during various types of radiologic procedures. Numerous publications are available but up to now estimations to find a correct figure for the present average dose to the whole population due to the irradiation of patients have been made only in Sweden, the UK, and the USA. The results are that the average gonad dose per capita to the patients examined seems to be of the magnitude of 1–3 r in 30 years. The reliability of these figures has been much discussed, and it seems advisable to await further investigations based on radiation measurements and some sort of sampling method, before accepting any definite figures. It is nevertheless highly probable that the order of magnitude of the figures quoted is correct, since the estimations were made independently in three different countries.

Sources of radiation used for purposes in which, as a rule, only the specialist is aware of the present of ionizing radiation

At present, we are faced in this field with only a few matters of minor significance. The average gonad dose from luminous compounds in watches is found in the UK (MRC) to contribute to the average gonad dose by 0.001 r per year, and the radiation from television sets to a still more insignificant dose.

In the future development of atomic energy it seems highly probable, however, that the use of radioisotopes for various purposes will change the situation, and constitute a new problem by the distribution in the community of a large number of small radiation sources, each being completely harmless individually, but collectively raising the level of irradiation of the population.

Artificial radioactive elements distributed in nature

WHO and the study group which it has convened are concerned with the peaceful use of atomic energy and the effects of, for instance, radioactive waste disposal from such peaceful uses. However, it is essential to take into consideration the evidence available from atomic weapon tests since the problems in the field of artificial radioactive elements distributed in nature are at the present time mainly related to fallout from these tests. The dose due to the external γ radiation from fallout may at present (December 1956) be disregarded in comparison with the internal dose.

If the fallout in the vicinity of the test area and the effects of radiation during the first few days after the explosion are disregarded, two different effects may be of interest. One is caused by mixed fission products of medium half-life (a few days to less than one year), the other by the fission products of long half life, particularly Sr 90 (28 years) and Cs 137 (33 years).

Fission products of medium half-life are very unevenly distributed over the world after an atomic explosion. Here, meteorological circumstances play a most important role, since a jet stream, a cold or warm front causing turbulence in the atmosphere, and rain or snowfall can lead to a concentration of the radioactive material in some areas even at a great distance (several thousand kilometres) from the explosion.

A typical example of such an effect is given in Fig. 9, showing the γ radiation recorded during about one month in the four northernmost places indicated in Fig. 3b. The increase in the γ radiation occurred about five days after an atomic bomb test. It is obvious from these observations that a comparatively narrow set of stations is required to give an adequate picture of the distribution.

It has been shown by recent measurements of the γ radiation from large samples of foodstuffs in Sweden that most of our food today (milk, beef, corn and vegetables) now contains artificial radioactive elements, in many cases greatly exceeding the K 40 radiation level of animals and plants. As an example, a decay curve obtained from powdered milk is shown in Fig. 10.

After some bomb tests J 131 is easily detectable in the thyroid of growing cattle. The content of this element in Swedish cattle during September–October 1956 is shown in Fig. 11. The maximum dose per week was here 0.04 rad, or about 20 times the dose due to the average natural radiation, which can be considered to be about 0.002 rad per week. It is to be noted that the effects demonstrated in Figs. 10 and 11 are due mainly to atomic bomb tests in August and September 1956, but that even before that time the foodstuffs were contaminated to an easily detectable extent, partly owing to medium half-life elements.

It is impossible to estimate today what doses have been received by populations in different parts of the world from mixed fission products. In comparison with the doses from the fallout of Sr 90 and Cs 137, the mixed fission products may in many cases give smaller doses calculated over a long period. It must, however, be borne in mind, that many biological effects are dependent—perhaps more than we know at present—on the intensity of the radiation. Often there is a threshold to be exceeded before a biological effect is obtained. Our knowledge of the effects of small doses over long periods is very scanty and we cannot as yet be sure that the time-intensity factor can be disregarded, even with respect to genetic effects.

The fallout of Sr 90 and Cc 37 has been carefully studied during the past years. These elements are probably comparatively evenly distributed over the whole world (with the possible exception of the polar regions). Large amounts of these elements remain in the upper atmosphere and will successively contribute to an increase in their present abundance on the earth surface by a factor of 3–5, even if the firing of atom bombs is stopped. The incorporation of Sr 90 into the skeleton may, in places where the calcium content of the soil is small, be regarded as important.

It does not yet seem possible to estimate the doses to human tissue due to fall-out, nor their distribution in time, which are necessary data for judging its possible biologic significance. Experience during the past year is, however, likely to raise doubts as to the lack of biologic importance to the tests of nuclear weapons, at any rate if they are continued on the present scale.

It is extremely difficult to predict what will in the future be the most important sources of radiation caused by artificial radioactive elements distributed in nature. There is reason to believe that the problems of disposal of radioactive wastes will be satisfactorily solved, and that precautions in the handling and use of radioactive material will be adequate, but accidents and unforeseen events may gradually spread radioactive substances of medium and long half-life beyond control. These radioactive materials will follow unknown paths, and may be harmful to mankind in ways that will become known to us only after long experience.

REFERENCES

1. Hultqvist, B. (1956) Studies on naturally occurring ionizing radiations, *Kungl. Svenska Vetenskapsakademiens Handlingar*, Ser. 4, Vol. 6, No. 3
2. Anderson, W., Mayneord, W. V. & Turner, R. C. (1954) *Nature, 174*, 424
3. Hursh, J. B. & Gates, A. (1950) *Nucleonics*, 7, 46
4. Sievert, R. & Hultqvist, B. (1956) *Brit. J. Radiol.* Suppl. 7, 1

TABLE I.—*Roughly estimated doses in soft tissue by cosmic radiation in rad per 30 years (figures in brackets are estimated values in rem considering a rbe of 10)*

Geomagnetic latitude	Dose in rad at an altitude above sea level in metros at						Hours per week to accumulate a dose of 50 rem during 10 years at 18 000 m.
	0[1]	2000[1]	4000[1]	6000[2]	12 000[2]	18 000[2]	
0°	0.7	1.1	2.0	8	35	40 (400)	63 h
40°	0.8	1.3	2.5	12	70	110 (1100)	25 h
>60°	0.8	1.4	2.7	14	85	150 (1500)	17 h

[1] Calculated from measurements of A. H. Compton and co-workers. (See D. Halliday, 1950: Introductory nuclear physics. New York, p. 461.)
[2] Calculated from measurements of R. A. Millikan and co-workers. (See H. J. Schaefer, Oct. 1950, Aviation Medicine, p. 383.)

TABLE II.—*Calculated gonad doses above various minerals*

Mineral	Ionization (ion pairs/cm³/sec) due to content of			Gonad dose (excluding cosmic radiation) r per 30 years
	Ra	Th	K	
Igneous rocks:				
Average	1.6	2.5	2.4	1.9
Granites			3.2	
North America, Greenland	2.0	1.7		2.0
Finland	5.9	5.9		4.1
Alps	5.5	6.9		4.3
Basalts			1.2	
North America, Greenland	1.2	2.1		1.2
England, Germany, France, and Hungary	1.6	1.9		1.2
Sedimentary rocks:				
Sandstone	0.4	1.0	0.9	0.8
Limestone	1.3	0.2	0.3	0.6
Alum shales in Sweden	75	0.3	3.2	21.0
Ore containing:[1]				
1% U				1,000
1% Th				500
0.01–0.001% Th[2]				0.5–5.0

[1] The uranium and thorium are in most cases very unevenly distributed and therefore the figures given here may be of limited practical value. According to a personal communication from Professor Z. M. Bacq, University of Liège, the background radiation in Katanga, Belgian Congo, will reach 100–150 times the normal background.
[2] Travancore sand, containing monazite, according to a personal communication by J. Eklund, Geological Survey of Sweden.

TABLE III.—*Summary of the results of gamma radiation measurements in Swedish dwellings (calculated from Hultqvist's*[1] *figures; cosmic radiation excluded)*

Building material in outer walls	Mean gonad dose in r per 30 years		
	Middle of room	Highest value recorded	Lowest value recorded
Wood	1.0	1.1	0.95
Brick	2.0	2.2	1.9
Light-weight concrete containing alum shale	3.2	3.8	3.0

[1] Hultqvist, B. (1956) *Kungl. Svenska Vetenskapsakademiens Handlingar*, Ser. 4, Vol. 6, No. 3.

TABLE IV.—*Average values for the irradiation of large population groups due to natural sources*

Sweden	2–5 rem/30 years.
UK	2–3 rem/30 years.
USA	4.3 rem/30 years.

TABLE V.—*Potassium content in adult human subjects according to Shol*[1] *(A) and Forbes & Lewis*[2] *(B & C) and the dose due to K 40*

Organ	Weight in percent of whole body			Percent K 39			Dose in organ r in 30 years mean (B and C)
	A	B	C	A	B	C	
	kg	*kg*	*kg*				
Skin	7.3	6.4	6.5	0.09	0.15	0.16	0.30
Skeleton	17.5	17.5	14.7	0.055	0.10	0.11	0.20
Tibia	-------	1.4	-------	-------	-------	0.05	-------
Muscle	43.0	39.5	39.6	0.42	0.33	0.30	0.62
Nerve	-------	3.0	2.1	-------	0.28	0.29	0.56
Liver	2.7	2.3	2.3	0.17	0.27	0.22	0.49
Heart	0.5	0.5	0.6	0.13	0.22	0.19	0.40
Lungs	1.5	3.3	2.2	0.15	0.24	0.26	0.50
Kidneys	0.5	0.5	0.4	0.17	0.16	0.22	0.38
Gl. tract	-------	1.8	1.5	-------	0.13	0.13	0.26
Adipose	-------	11.3	21.4	-------	0.08	0.06	0.14
Remainder	-------	11.3	6.4	-------	0.18	0.17	0.34
Weight loss on dissection	-------	2.6	2.2	-------	-------	-------	
	70	53.8	73.5	0.205	0.212	0.190	0.40

[1] Shol, A. T. (1939) *Mineral Metabolism*, New York, Reinhold Publishing Corp., 19.
[2] Forbes, G. B. & Lewis, A. M. (1956) *J. clin. Invest.* 35, 596.

TABLE VI.—*Estimated values for the irradiation of the gonads of the population due to natural sources in rem per 30 years*

	For large population groups			For individuals	
	Minimum	Maximum	Average	Minimum	Maximum
Cosmic radiation	0.7, including screening in dwellings.	3?, about 4,000 m above sea level.	1?	0.5? (for some miners).	5? (50?) 3% of 30 years 18 000 m above sea level.
Natural radiation:					
¼ of 30 years out of doors.	<0.1 above water (in boats), snow and ice.	1 above igneous rocks.	0.5	0	15 (20?).
¾ of 30 years indoors.	0.9, in wooden houses.	3, in some types of brick and concrete houses.	2		
Radon in air	0.03, out of doors and in wooden houses with good ventilation. (3×10^{-13} c/l)	0.8, in cellar and in stone houses with poor ventilation. (50×10^{-13} c/l)	0.2	<0.01 (<10^{-13} c/l).	2.0 (10^{-11} c/l).
K 40 in body (+0.03 for C 14):					
Fat	0.2	0.2	0.2	0.2	0.2.
Muscles	0.7	0.7	0.7	0.7	0.7.
Gonads	0.5	0.5	0.5	0.5	0.5.
Approximate sum for gonads.	2	6 (8?)	4	1	20 (>50?).

INDEX TO FIGURES

Figure 1: Cosmic radiation at various altitudes according to *Compton* and coworkers.
Figure 2: Cosmic radiation at high altitudes according to *Millikan* and coworkers.
Figure 3a: Variation in cosmic radiation at 50–500 m above sea level in February 1956 recorded in the four places I–IV as shown in Fig. 3b.
Figure 3b: The site of Swedish background recording stations.
Figure 4: Distribution of the average radiation in Swedish dwellings of three types.
Figure 5: Decrease in γ radiation with depth of snow cover for three different snow densities.
Figure 6: The seasonal variation in γ radiation recorded in four places (I–IV in Fig. 3b) and the corresponding snow cover plotted as negative values.
Figure 7: The variations in the γ radiation from human subjects of various age, sex and body weight. The radiation is to more than 95% due to the potassium content.
Figure 8: Comparison of the whole body γ radiation of persons living in a city with a radium content of about 0.2 uug Ra per litre and 1–2 uug Ra per litre (dotted curves) in the pipe water.
Figure 9: The γ radiation five days after a distant hydrogen bomb test, observed in the places I—IV in Fig. 3b.
Figure 10: The decay curve for γ radiation from mixed fission products in powdered milk in a sample taken in September 1956 measured by means of pressure ion chambers.
Figure 11: The γ radiation from J 131 in thyroids from grazing cattle. Measurements by means of a pressure ion chamber. 1000-g samples (50 thyroids). The dotted parts of the curves indicate the extrapolation of the decay curves observed back to the last day on which the majority of the cattle presumably grazed. A–A corresponds to the MPL for large human populations.

FIG. 1

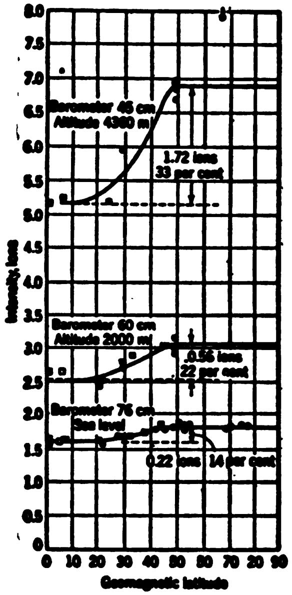

Barometer 45 cm
Altitude 4360 m
1.72 ions
33 per cent
Barometer 60 cm
Altitude 2000 m
0.56 ions
22 per cent
Barometer 76 cm
Sea level
0.22 ions
14 per cent
Intensity, ions
Geomagnetic latitude

FIG. 2

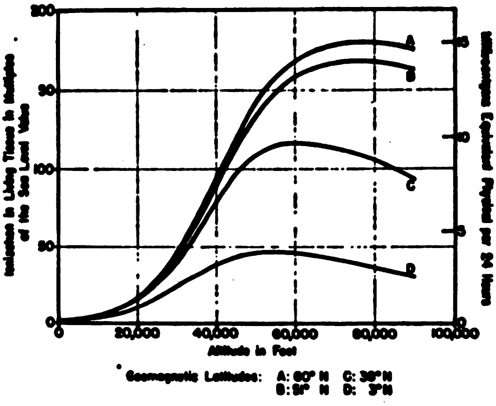

A
B
C
D
0
20,000
40,000
60,000
80,000
100,000
Altitude in Feet

FIG. 3a

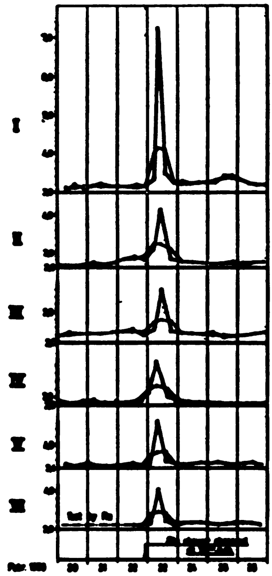
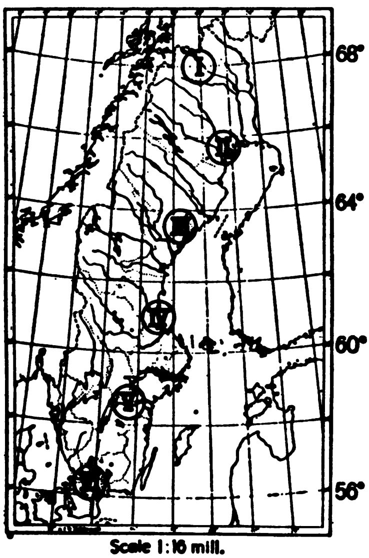


FIG. 3b



Scale 1:16 mill.

FIG. 4

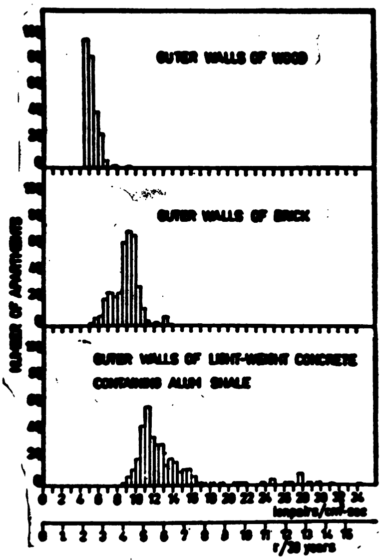

NUMBER OF APARTMENTS
OUTER WALLS OF WOOD
OUTER WALLS OF BRICK
OUTER WALLS OF LIGHT-WEIGHT CONCRETE
CONTAINING ALUM SHALE
0 2 4 6 8 10 12 14 16 18 20 22 24 26 28 30 32 34
ionpairs/cm³-sec
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15
r/30 years

FIG. 5

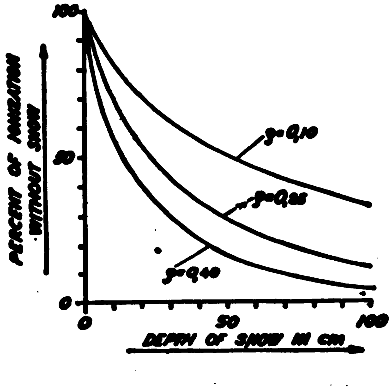

PERCENT OF IONIZATION WITHOUT SNOW
100
50
0
g=0,10
g=0,25
g=0,40
0
50
100
DEPTH OF SNOW IN CM

FIG. 6

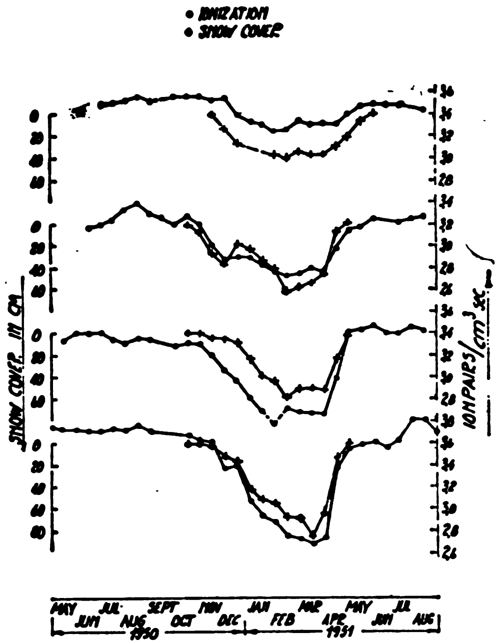

IONIZATION
SNOW COVER
SNOW COVER IN CM
ION PAIRS/cm³ SEC
MAY JUN JUL AUG SEPT OCT NOV DEC JAN FEB MAR APR MAY JUN JUL AUG
1950
1951

FIG. 7

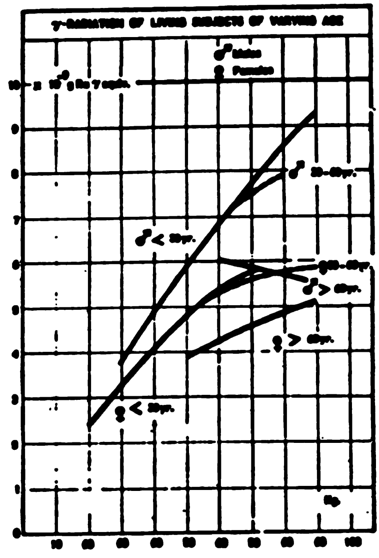


FIG. 8

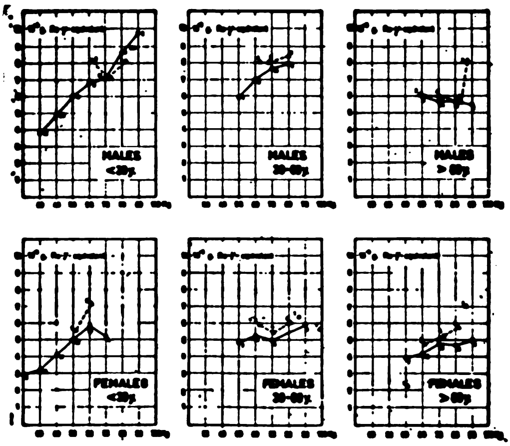

MALES
<20y.
MALES
20-60y.
MALES
>60y.
FEMALES
<20y.
FEMALES
20-60y.
FEMALES
>60y.

FIG. 9

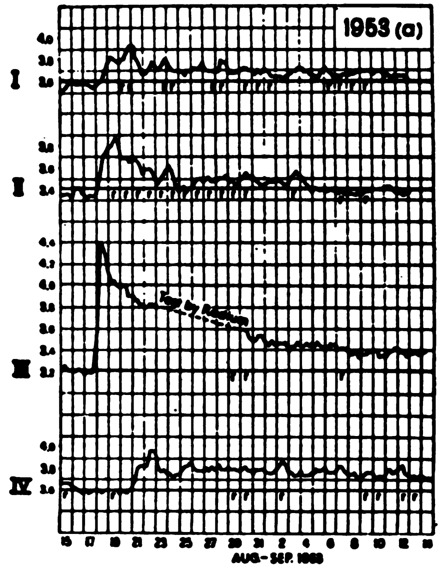

1953 (a)
I
II
III
IV
AUG.-SEP. 1953

FIG. 10

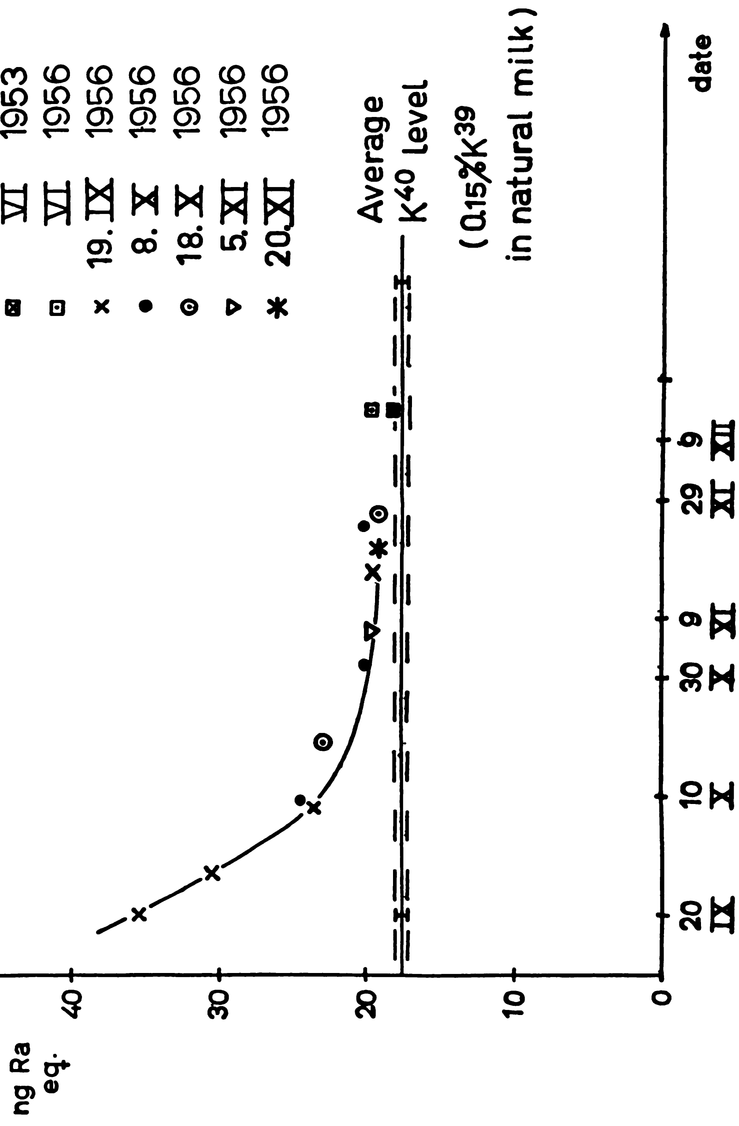
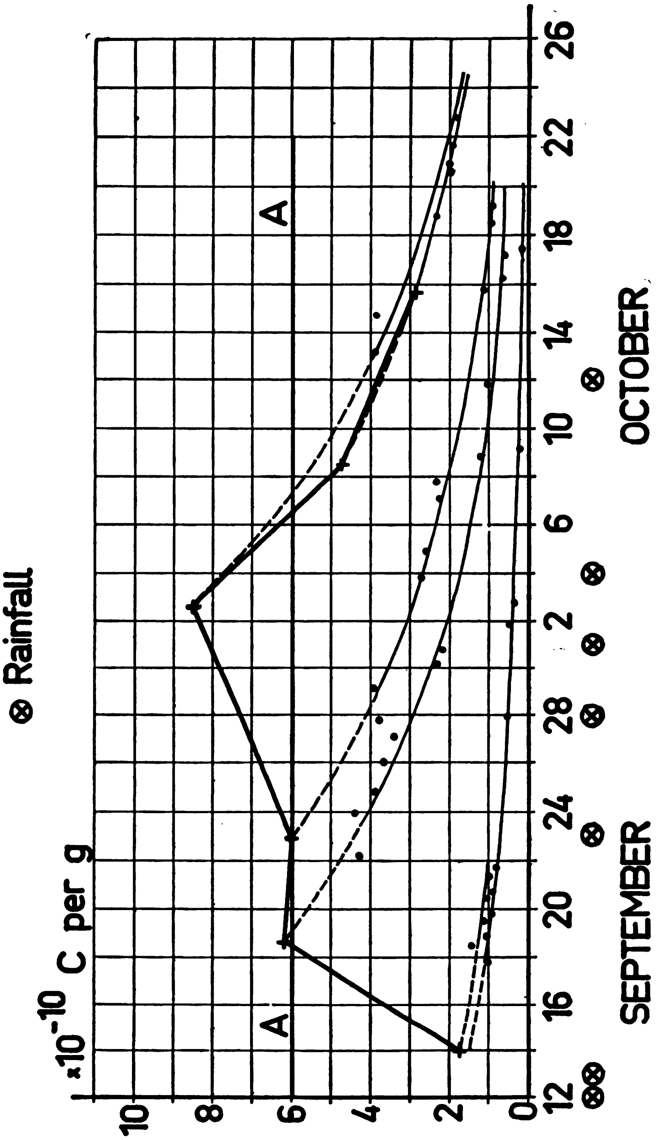

VII 1953
VII 1956
19. IX 1956
8. X 1956
18. X 1956
5. XI 1956
20. XI 1956
Average K^{40} level
(0.15% K^{39} in natural milk)
ng Ra eq.
40
30
20
10
0
20 IX
10 X
30 X
9 XI
29 XI
9 XII
date

FIG. 11

^{131}J, PER G OF THYROID IN GRAZING CATTLE



ANNEX 5

DETECTION OF INDUCED MUTATIONS IN OFFSPRING OF IRRADIATED PARENTS [1]

THE PROBLEM OF THE GONAD DOSE

Among the various sources of ionizing radiation, apparatus for radiodiagnosis and radiotherapy today represents the main contribution of man to the increase in background radiation.

Radiodiagnosis, which is being carried out more and more frequently every year in all the developed countries, at present plays a dominant role in this connexion. Ranging as it does from the radiographs and radioscopies called for by some pathological condition, to the periodic radiographs of the whole skeleton used in studying the growth of normal children, the field of exploratory radiology now covers an extremely large proportion of the population; for example, in France, all children of school age are submitted yearly to systematic radiographic examination.

While there can be no doubt that most radiodiagnostic examinations affect the gonads only very slightly, all examinations of this type which involve the pelvic region may have genetic effects (for example, gynaecological and obstetrical examinations).

One has only to consult the appropriate tables, such as the one drawn up by Plough,[2] to see that radioscopy may involve very high doses; for example, 10 to 20 r per minute for a gastro-intestinal radioscopy. While techniques such as radiocinematography, which, happily, are not used very extensively, are much more harmful, since they involve even higher doses. Several authors have attempted to calculate the mean gonad dose, on the basis of isodose curves, for whole populations. The estimates vary according to the author, but, in general, the gonad dose may be taken as being of the order of one roentgen; in other words, it represents a very considerable fraction of the quantity of natural radiation to which we are inevitably exposed.

Be this as it may, these estimates are based on the assumption that the radiation emitted by such apparatus is of known quality and that the methods employed in its application are standardized, which is very far from being the case. In fact, considering that radioscopic apparatus, often poorly shielded, is to be found more and more frequently in the consulting-rooms of general practitioners, one is justified in concluding that a very high proportion of radioscopic examinations are being carried out by physicians with no formal training in radiology.

In the opinion of the writer and his colleague, Professor R. Turpin, it would be desirable to obtain, by means of surveys among general practitioners as well as in hospitals, an experimental estimate of the gonad dose involved in any given radiological examination, under the actual conditions in which the said examination is carried out. Such surveys would make it possible to check the validity of the extrapolations at present in use.

A simple method would be to place micro-counters—or, perhaps even better, small films of the type used in atomic plants—in contact with the genital organs of all persons examined.

Even if such surveys were to result only in an increase in the precautions taken by those using X-ray apparatus, they would have partially fulfilled their purpose.

Radiotherapy certainly represents a much smaller risk from the genetic point of view, on the one hand because it is carried out by specialists, and on the other because in most cases the people concerned are elderly. Nevertheless, on examining the statistics of the radiotherapeutic services of the Paris hospitals, Turpin, Lejeune & Rethore [3] found that among 238,800 case-histories there were 4.428 cases of pelvic irradiation of adults under 35 years of age. The proportion of the total—about 2%—is, of course, low, but it does represent cases in which the radiation has impinged directly on the gonads of subjects who are still young enough to reproduce.

Although the calculation of the gonad dose is much more reliable in radiotherapy than in radiodiagnosis, direct determination during all forms of treat-

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[1] Submitted by Dr. J. Lejeune, Chargé de Recherche au Centre National de la Recherche scientifique, Paris, France.

[2] Plough (1952) Nucleonics, 10, 17.

[3] Turpin, R., Lejeune, J. & Rethore, M. O. (1956), Etude de la descendance de sujets traités par radiothérapie pelvienne. Note préliminaire (paper presented at the First International Congress of Human Genetics, Copenhagen; unpublished).

ment (even extra-pelvic), by means of films or micro-counters placed in contact with the scrotum or in the vaginal fornix, would also be very desirable.

It follows from the foregoing considerations that the accuracy of an estimate of the mean gonad dose received in 30 years by nubile subjects should be checked by random sampling to determine the dose actually received under various examination conditions. It should be noted, however, that such work will be useless if it is not accompanied by systematic recording of the doses received by every individual in the population.

One method of obtaining systematic records, though perhaps a little startling, has already proved its worth. It entails treating the administration of X-rays in the same way as that of morphine for example, by providing all owners of X-ray apparatus with a counterfoil book (of the type used for prescribing narcotics), in which every radiological operation should be recorded and the following particulars given: name, age, and address of patients; reason for the examination, area examined; and details required for calculation of the dose (e. g., kilovolts, millicuries, type of filter, size of beam, etc.).

In practice, this form of registration would probably involve very considerable difficulties, but the increasing socialization of medicine would probably make it feasible for a known fraction of all radiological operations and would certainly draw the attention of the medical profession to a danger of which it is but little aware at the present time.

METHODS FOR DETECTION OF MUTATIONS INDUCED IN OFFSPRING OF IRRADIATED SUBJECTS

Since human geneticists cannot employ methods such as those used for *Drosophila,* they have to resort to statistical comparison of two populations of children supposedly identical in all respects save the dose of roentgens received by the gonads of their parents.

(a) *The study of abnormalities and malformations* may reveal an increase in frequency in the progeny of the irradiated group. This increase could be considered to be linked with the appearance of unfavourable dominant mutations. Although probably reliable in the case of definite genetic syndromes such as achondroplasia, this method is much less precise in respect of congenital abnormalities as a whole, since the latter are affected by extremely varied factors (age of mother, parity, etc.). However, if sufficient data are to be obtained to make it possible to draw conclusions, all abnormalities must be considered.

(b) *The study of the frequency of sex-linked recessive diseases* in the sons of irradiated mothers, although theoretically possible, necessitates such a large number of observations that it has not been undertaken.

(c) *The production of lethal genes* can be more easily detected. The most serious effect, sterility, is the one most generally known, but measurement of sterility or even of subfertility in man is extremely difficult. As has been remarked elsewhere (Turpin & Lejeune),[4] the actual fertility of civilized populations is hardly a third of the potential fertility of non-malthusian societies, which greatly diminishes the possibility of demonstrating the effects of sterility.

On the other hand, it is logical to expect the production of dominant lethals to bring about an increase in the frequency of miscarriages, which is difficult to establish, and of stillbirths, which can be obtained with much greater accuracy.

It is, however, essentially in the X-chromosome that the lethal genes can be detected, through a study of the sex ratio. Owing to the chromosomal structure of sex, the X-linked lethal mutations appear in different forms according to the sex of the irradiated parent. Thus, in the offspring of a woman exposed to radiation, dominant lethal mutations linked to the sex chromosome have no effect on the sex ratio, whereas sex-linked recessives bring about a deficit of boys. The contrary is true of men, in whose progeny only the dominant lethals manifest themselves by bringing about a deficit of girls.

If we call "n" the average number of dominant lethals linked to the X-chromosome in the offspring of men who have received a given dose of roentgens, then the following simple equation should apply:

$$\text{frequency of surviving daughters} = \frac{\quad}{\text{number expected}} \times e^{-n},$$

[4] Turpin, R. & Lejeune, J. (1955), Bull. Acad. nat. Méd. (Paris), No. 5/6, p. 104.

since the number of mutable loci in the X-chromosome should be large enough for the distribution to be of the Poisson type. Moreover, since it may be taken as a first approximation that this average number, n, should be identical for all the chromosomes, the autosomally viable zygotes represent a fraction of the total fertilized ova roughly equal to $(e^{-n})^{m}$, and it is in this fraction alone that it will be possible to observe a disturbance of the sex ratio.

Similar reasoning can be applied to the case of the offspring of irradiated women, bearing in mind the fact of that there is a relationship between the frequency of the dominant and the recessive lethals.

Since, in theory, the parameter n bears a linear relationship to the roentgen dose received, and since our estimates of the gonad dose are very approximate, there is probably a fairly strong correlation between the actual fertility of the parents after irradiation and the deviation of the sex ratio observed in their progeny. In other words, the most pronounced variations in the sex ratio would be shown by the offspring of parents who are almost sterile owing to the irradiation of one of them (for example, couples who have only one child).

It follows from the above that, in the absence of an accurate estimate of the gonad dose, an overall study of the sex ratio of all children born of an irradiated father or mother may, if it does not take into account the number of siblings born after treatment, result in the masking of the phenomenon by a "dilution effect", caused by the very numerous siblings who are the issue of a parent relatively slightly affected.

Further, the problem of control samples can only be correctly solved by comparing children born before and after treatment of the same irradiated parent and thus eliminating any genetic factor due to the couple itself, as well as the possible influence of siblings of one sex only.

Finally, and bearing in mind the above limitations, it would seem that the sex ratio is the most sensitive touchstone for detecting the production of lethal mutations in the first generation of children born of irradiated parents.

INFORMATION AVAILABLE AT PRESENT

Relatively few direct investigations of the influence of irradiation have been carried out and the writer will mention them in succession here, under the headings of the three main characteristics referred to earlier: frequency of abnormalities, frequency of miscarriages and stillbirths, and variations in the sex ratio.

(a) *Frequency of abnormalities*

Murphy & Goldstein[5] and Maurer[6] have published statistics on the offspring of women treated with X-rays or radium in the pelvic region. Unfortunately, neither of these papers can be considered very satisfactory, owing to the lack of detail concerning the families, on the one hand, and the absence of any controls, on the other.

Two recent papers cast more light on this question. In 1953, Neel et al.,[7] on studying the offspring of the survivors of the atomic bombing of Nagasaki and Hiroshima, did not find any increase in the frequency of serious abnormalities. While Macht & Lawrence[8] on comparing the children of fathers who were radiologists with those whose fathers were medical specialists not exposed to ionization risks, found a significant overall increase in abnormalities among the progeny of the radiologists. Unfortunately, the latter authors on the one hand included as abnormalities syndromes of a very varied and sometimes quite unsuitable nature (foetal erythroblastosis, for example) and on the other accepted the diagnosis made by the parents themselves, who, though admittedly physicians, clearly lacked the necessary objectivity. These considerations greatly limit the significance of the conclusions drawn by Macht & Lawrence, but it is only fair to stress that if, from the authors' data, those relating only to congenital forms of heart disease are selected, the increase pointed out among the progeny of the radiologists remains statistically significant.

(b) *Frequency of miscarriages and stillbirths*

Macht & Lawrence[8] mention a non-significant increase in the overall frequency of stillbirths plus miscarriages, while Neel et al.[7] report a non-significant

[5] Murphy, D. P., and Goldstein, L. (1929), Amer. J. Roentgenol. 22, 207.
[6] Maurer (1933), Zbl. Gynäk. p. 819.
[7] Neel, J. V. et al. (1953), Science, 118, 537.
[8] Macht, S. H., and Lawrence, P. S. (1955), Amer. J. Roentgenol. 73, 442.

increase in the frequency of stillbirths. Finally, Crow,[9] who studied the offspring of American radiologists through a survey, by questionnaire, which was carried out along lines similar to those followed by Macht & Lawrence, has also reported a slight and non-significant increase in foetal mortality among the progeny of irradiated fathers.

All in all, although the published data agree fairly well on this point, they cannot strictly be regarded as conclusive.

(c) *Variations in the sex ratio*

Of the publications already mentioned, only that of Neel et al.[7] supplies any usable material. For example, neither Murphy & Goldstein,[5] nor Maurer[6] nor Crow[9] indicate the sex of the children; and although Macht & Lawrence[8] give some figures, they do not specify the sex of about 10% of the children, so that one can hardly rely on their statistics.

In their preliminary report, Neel et al.[7] showed that among the offspring of the survivors in Nagasaki there was a statistically significant deviation of the sex ratio, an increase being observed among the children of irradiated fathers and a decrease among those of irradiated mothers. On the other hand, such variations were small or non-existent in the more numerous offspring of the Hiroshima survivors.

At the First International Congress of Human Genetics, held in Copenhagen in August 1956, Dr. J. V. Neel presented some further statistics on the subjects mentioned above, including all the births which had occurred in these families since 1953. In this larger sample, the deviations observed in 1953 are no longer discernible.

In Paris, a survey has been carried out on the offspring of subjects given pelvic radiotherapy in all the hospitals in the city and the surrounding districts (Turpin, Lejeune & Rethore).[8] The initial findings, which are concerned exclusively with the sex ratio, were presented by Professor Turpin at the First International Congress of Human Genetics; they are briefly summarized in the table below.

Offspring of various subjects before and after pelvic radiotherapy

Subjects and reason for treatment	Number of children		Sex ratio
	♂	♀	
Before treatment:			
Men (138); various reasons	116	115	0.502±0.034
Men (284); sciatica	242	223	0.520±0.024
Women (154)	131	106	0.553±0.034
After treatment:			
Men (95); various reasons ($\bar{r}$* = 1461 r)	68	62	0.523±0.048
Men (194); sciatica ($\bar{r}$ = 1295 r)	157	118	0.571±0.030
Women (97); ($\bar{r}$ = 1360 r)	63	73	0.463±0.044

*$\bar{r}$ = average skin dose, *not* gonad dose.

The figures given in the table show that, before treatment, the sex ratio of the children was statistically comparable in the two groups, i. e., the male and female subjects. After treatment of one of the parents, however, the sex ratio increased in the offspring of the treated fathers and decreased in those of the treated mothers, this heterogeneity being statistically significant.

Conclusions

In concluding this very rapid review of the few usable data at present available, the writer would like to stress the following two points:

1. The gonad dose per 30 years, in the form in which it has already been established,[10, 11] probably gives an acceptable approximation of the risk resulting from artificial ionizing radiation. Nevertheless, an accurate evaluation can be arrived at only by systematic recording of all individual exposures. Moreover,

[9] Crow, J. F. (1955), Amer. J. Roentgenol. 73, 467.

[10] Great Britain, Medical Research Council (1956), The hazards to man of nuclear and allied radiations, London.

[11] United States of America, National Academy of Sciences (1956), Biological effects of atomic radiation.

it is essential that the gonad dose actually received during irradiation under the conditions obtaining in practice should be checked experimentally. The first—and perhaps the most valuable—result of such investigations would probably be a substantial decrease in the degree of exposure of the gonads.

2. From an analysis of the observations already made, it appears that with the doses used in radiotherapy, it would probably be possible to detect some effect in the first generation. The urgency of research in this connection need hardly be stressed. It is only when a list of the mutations which are at present detectable has been drawn up that it will be possible to extrapolate and obtain an estimate of the over-all genetic damage.

Annex 6

Gonad Doses From Diagnostic and Therapeutic Radiology [1]

DIAGNOSTIC RADIOLOGY

Several estimates have been made recently of the gonad dose to the population in excess of that from natural background radiation, resulting from diagnostic radiology. These range in value from approximately 10% of the background radiation (Martin [2]) to approximately 58% (Clark [3]). Intermediate between these two is the estimate of Osborn & Smith [4] of at least 22%. There appears to be some agreement, on present knowledge, that the "doubling dose" for many may lie between 30 and 80 r in a period of 30 years. The concept of the "doubling dose" is, however, an over-simplification of the situation, as there most likely exists a spectrum of gene sensitivity. It may well be that already we are in the position that the mutation rate for some genes is significantly raised.

The most comprehensive analysis yet made of the gonad dose from diagnostic radiology is that of Osborn & Smith.[4] These authors make a number of points of considerable importance. First, they draw attention to the rapid expansion in the use of diagnostic X-ray procedures, adducing evidence that in England and Wales the number of X-ray examinations may, at the present time, be increasing by about 12% per year, and that in 1954 between 17 and 18 million examinations were made. They point out, however, that the adverse effect of this expansion, so far as the gonad dose is concerned, is offset to some extent by technical advances which have reduced the amount of necessary radiation exposure. Secondly, they draw attention to the important fact that only a small number of examinations, amounting to about 7% of the total, contribute the major portion (about 75%) of the gonad dose. These examinations are those of the hips, the lumbo-sacral spine, the pelvis, the urinary tract (intravenous and retrograde pyelography) and pelvimetry. By and large these findings are comparable to those of Martin.[2]

Of serious import is the widespread use of pelvimetry and other X-ray obstetrical examinations of the abdomen, not to speak of examination of the maternal abdomen for other than obstetrical purposes. According to Osborn & Smith, *at least* 26,000 pelvimetries are carried out annually in England and Wales and 86,000 other obstetrical X-ray examinations. These authors calculate that the maternal gonad dose from pelvimetry alone amounts to 3% of the gonad dose to the total population of both sexes, and the foetal dose to as much as 15.6%.

The real criticism of the work of Osborn & Smith lies in the fact that their results are based on a sample of only five hospitals—two teaching and two non-teaching hospitals and one children's hospital. It is doubtful whether this is an adequately representative sample of the hospitals of England and Wales, particularly as there is some evidence, especially from a study of pelvimetry, of considerable variations in radiographic techniques.

THERAPEUTIC RADIOLOGY

The great majority of patients treated in radiotherapy centres are suffering from malignant disease and are either actually or effectively past the reproduc-

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[1] Submitted by Dr. W. M. Court Brown, Director, Group for Research into the General Effects of Radiation (Medical Research Council of Great Britain), Radiotherapy Department, Western General Hospital, Edinburgh, Scotland.

[2] Martin, J. H. (1955), Med. J. Aust. 2, 806.

[3] Clark, S. H. (1956), Bull. atom. Scient. 12, 14.

[4] Osborn, S. B. & Smith, E. E. (1956), Lancet, 1, 949.

tive age. However, a proportion of young persons are treated for a variety of nonmalignant conditions. These conditions are grouped into those treated during childhood and those treated during early adult life. In the former group are haemangiomata, keloids, hypertrophic tonsillar tissue, bone cysts, etc. The conditions treated during early adult life are mainly ankylosing spondylitis, skin diseases, keloids, some menstrual disturbances and, occasionally, bone cysts.

The childhood conditions chiefly occur on the upper half of the trunk and on the limbs, and, on the whole, are treated with low-voltage radiation given on small localized fields. It is unlikely that these treatments contribute to the gonad dose to any great extent.

Of the conditions irradiated in early adult life, so far as experience in Great Britain is concerned, the treatment of ankylosing spondylitis is likely to contribute an appreciable fraction to the gonad dose. Some tentative estimates which have been made of the size of this fraction are given below. No information is available on the contribution from the treatment of skin diseases, but there is every reason for believing that it may also be appreciable.

ANKYLOSING SPONDYLITIS

During the past year an epidemiological survey has been carried out to determine the incidence of leukaemia among patients treated with X-rays for ankylosing spondylitis. This survey covered all the radiotherapy departments of England and Wales, and of Scotland, at present operating under the respective National Health Services. In the course of the survey, data were recorded concerning 13,352 patients, who were treated between the years 1935 and 1954, inclusive—presumably the majority, if not the great majority, of the patients treated for this disease in Great Britain during the period in question.

It is possible to make some rough and very preliminary estimates of the minimum contribution to the gonad dose as the result of the treatment of ankylosing spondylitis in Great Britain, on the assumption that the testes are not shielded during treatment.

The beneficial effects of X-ray treatment for this disease were only widely recognized during the Second World War, and there was a steady annual increase in the number of new patients irradiated up to 1950, since when there has been a tendency for the number to diminish slightly. For the period 1949 to 1954 the average number of new cases per year was 1336, of which 1109 (83%) were males. Of these 53% were under the age of 35. Of the females, 43% were less than 35 years of age.

The average standard first course of treatment for a male patient has been taken as follows (based on dosage information from a random sample of approximately one in six of the whole population):

(a) A single 15 cm x 10 cm posterior field centred over the sacro-iliac joints, the large axis of the field being horizontal.

(b) A series of fields irradiating the whole length of the spine and extending from the upper margin of the sacro-iliac field to the upper part of the neck. On an average, the breadth of these fields is 7.5 cm.

(c) A total skin dose to each field of 1500 r (half value layer 1.6 mm).

From measurements made in a phantom man it appears that a dose of about 45 r is received in the male gonads during such a course of treatment.

Measurements have not been made of the gonad dose received by females. In some centres the female sacro-iliac area was treated similarly to the male area; but in many others definite attempts were made to avoid the ovaries. The direct irradiation of the sacro-iliac region to 1500 r is almost certain to induce permanent sterility. For present purposes it is assumed that the ovarian dose received is on an average 45 r.

The contributions to the gonad doses every year from the treatment of ankylosing spondylitis have been calculated (see table below) on the basis of the following *de facto* populations for England, Wales and Scotland (1952):

	15–34 years	All ages
Males	6.6×10^6	2.37×10^7
Females	6.8×10^6	2.56×10^7
Total	1.34×10^7	4.93×10^7

Gonad dose (in milliroentgens) per head of population per year in England, Wales, and Scotland

	15-34 years	All ages
Males	4.0	2.1
Females	0.6	0.4
Both sexes	2.3	1.2

These estimates can be considered to be minimal ones. No less than 46% of the males and 44% of the females were given more than one course of treatment. Many of these additional courses were given to the lumbo-sacral region and to the hip joints. However, two other factors which tend to diminish either the size or the effectiveness of the gonad dose must be taken into consideration. First in some radiotherapy centres it was standard practice to provide some form of lead shielding for the testes; this was by no means a universal habit, however, and a number of instances of male infertility, some of which could have been the result of X-ray exposure, were discovered. Secondly, it may well be that sufferers from spondylitis are sub-fertile. The disease is a crippling one and is frequently accompanied by pulmonary tuberculosis and ulcerative colitis. There is, however, no published evidence on this point.

To sum up, the radiotherapy of patients suffering from ankylosing spondylitis in Great Britain will give a gonad dose per year of at least 1% of the natural background, and possibly appreciably higher. A more rigorous examination of the contribution from this source, and an analysis of the contribution from other types of radiotherapy, including the use of radioactive isotopes, could well bring the total contribution from radiotherapy up to the level of about 8% suggested by Clark[3] for the USA, or perhaps to an even higher level.

DISCUSSION

So far as the writer is aware there is no direct evidence of a steady upward trend in the incidence of any of the undesirable traits that might be expected with any increase in the mutation rate due to the steady expansion of medical radiology. The direct epidemiological approach to this problem is clearly beset with difficulties, not the least being the very large population that would have to be kept under observation and the inaccuracy and inadequacy of death certification.

There is, however, some evidence from which it could be argued indirectly that an increase in undesirable traits may already be taking place. For example, recent work has demonstrated a significant increase in the mortality from leukaemia among persons treated with X-rays for ankylosing spondylitis, and it has also been possible to demonstrate the existence of a relationship between the annual incidence of leukaemia in these patients and the radiation dose to the bone-marrow. Some preliminary data have been published (Court Brown & Doll[5]). On the evidence as it stands it seems possible that radiation leukaemogenesis in man is a non-threshold effect, and that over the range of dose met with in ordinary civilian life the dose-response relationship is a simple proportional one, analogous to that for the induction of mutations. If this were finally shown to be the case, two deductions would become valid. First, that a proportion of naturally occurring cases of leukaemia are probably due to natural background radiation; and, secondly, that any increase in the background radiation from artificial sources will be associated with an increased mortality from leukaemia.

The mortality from leukaemia is known to be rising. Thus, the annual crude death-rate for both sexes rose in England and Wales from 26 in 1940 to 49 in 1954. The corresponding figures for Denmark are 48 and 71, for Canada 30 and 51, and for the USA 39 and 63 (in 1953). Undoubtedly part of this increase is due to changes in diagnostic criteria and improvements in diagnostic techniques. There is, however, a general feeling that it may in part be real

[5] Court Brown, W. M. & Doll, R. (1956), Appendix B: Leukaemia and aplastic anaemia in patients treated with X-rays for ankylosing spondylitis. In: Great Britain, Medical Research Council, The hazards to man of nuclear and allied radiations, London, p. 87.

and absolute. If this be the case, and if the dose-response relationship over the relevant range of dose be linear, then part of the increased mortality from leukaemia may well be due to the expanding use of radiations, particularly diagnostic radiology. If the incidents of such traits as haemophilia, muscular dystrophy, and achondroplasia could be examined in the same way, it is more than probable that at least a qualitatively similar upward trend would be found.

ANNEX 7

MUTATION IN MAN [1]

1. INTRODUCTION

The study of gene mutation in man has two aspects. The first concerns the ascertainment of spontaneous mutation rates at specified loci. This gives information about human evolution in general as well as about the causation of certain rare diseases and defects. The second aspect, which has only recently become significant, concerns artificially produced mutagens and, in particular, the genetical effects of ionizing radiation. In order to estimate the magnitude of these effects a knowledge of spontaneous mutation rates at given loci is required and the sensitivity of these loci to radiation needs to be ascertained.

2. MEASUREMENT OF SPONTANEOUS MUTATION RATE

Estimation of mutation rate in man, in relation to any given hereditary trait, depends upon ascertaining three things, the incidence of the trait in the general population, the nature of the genetical contribution to the cause of the trait, and the fitness of the genotypes concerned. These phenomena are not necessarily constant. As seen in the population at the present time they may not represent the true picture over a long series of generations, during which natural selection has been acting. They only give us the first clue to conditions which govern genic equilibrium in human populations.

There are two standard methods of approach, the direct and the indirect.

(i) Direct observation

The most favorable case for estimating mutation rate directly occurs when the gene studied is detectable with certainty or regularity in heterozygotes. Instances of fresh mutation can then be observed in families where a gene appears in an offspring although it was not present in the parents. The ideal kind of regular dominance required for this is rarely (perhaps never) found in human genetics. Man is a wild species, under natural selection, unlike laboratory stocks, and consequently most single gene effects, especially those shown in heterozygotes, are subject to modification. Even with the most reliable characters, such as blood group antigens, suppression is possible by gene interaction (Levine et al. 1955); such events could easily be misinterpreted as evidence of mutation by the unwary.

The situation for sex-linked genes is quite favourable, theoretically, for direct observation of fresh mutation because modification of a character shown in homozygous males is usually slight. Occasional families will be observed in which the probability is very great that the disease in the propositus is due to fresh mutation. The proportion of sporadic cases can also be inferred if the sibships show an excess of sporadic propositi.

For recessive traits the problem is much more difficult because heterozygous carriers are not detectable in ordinary circumstances. In cases where special techniques have been developed for identification of carriers the problem is resolved into one of detection of mutation for a dominant condition, as demonstrated by Vanderpitts et al. (1955) for sickle cell trait. Direct observation of cases of recessive diseases due to fresh mutation is very unlikely to be possible because only a very small proportion of cases of a recessive trait in a given generation can be attributed to fresh mutation in a parent. For diseases in which a single gene is only a part cause and in which environment has a great effect upon manifestation, the contribution of spontaneous mutation is likely to be even less significant. The same applies to conditions due to the interaction of many genes. For none of them can mutation rates be directly determined.

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[1] Submitted by Dr. L. S. Penrose, Galton Professor of Eugenics, University College, London, England.

(ii) The indirect argument

When the total effects of a gene are very disadvantageous an indirect line of argument can be used for estimating mutation rate, even though the gene may not be manifest in the heterozygous state. Principles on which the indirect estimation of mutation rates can be based were laid down by Haldane (1932). The assumption can be made that the human population is in a state approaching genetical equilibrium under natural selection. It is supposed that disadvantageous genes could not persist in the population unless their extinction by selective mortality were completely balanced by the resurrence of mutation.

In the case of dominant or sex-linked characters associated with very high mortality, the direct measurement of mutation rate can be supplemented, and its plausibility greatly strengthened, by the indirect argument. The best situation for this combination occurs in the case of a very deleterious dominant trait. This is a rare circumstance. If the disease is not very lethal, there will be difficulty in measuring the unfitness conferred by the gene: if it is very lethal, there is difficulty in providing the dominant mode of inheritance, as it will seldom last even for two generations. Sometimes the problem might be solved for a locus, which had several different known alleles, some producing milder and others severer types of disease. Then, in each of the severest cases, mutation of a lethal allele will be observable. This possibly occurs in both epiloia and chondrodystrophy. For mild alleles, which last for several generations, the proportion of cases due to fresh mutation is correspondingly smaller.

Estimates which are entirely indirect, are untrustworthy but they have actually been made for a variety of genes recognized only by their recessive effects. One cause of uncertainty with recessive traits is that allowance has to be made for the results of inbreeding. Another likely source of error is that genetical equilibrium can be maintained not only by mutation but also by slightly advantageous effects in heterozygotes. That is to say, on the balance, the total effect of a gene may be much less bad than appears from studying abnormal homozygotes and then an indirect estimate of mutation rate will give much too high a value.

3. SOME STANDARD ESTIMATES OF HUMAN MUTATION RATES

Mutation rates have been calculated for quite a large number of genes in man. It is preferable to express them in terms of loci per generation, if we wish to avoid controversy, because slightly different forms of the diseases concerned can be accounted for by the same allele or by different alleles. If there are several very closely linked loci giving rise to a pseudoallelic system, the real mutation rate for each separate element is lowered by a factor depending upon the number of elements in the complex.

(i) Dominants

The most exact estimates for supposedly single loci are probably those for very deleterious dominant traits (see Table 1). Allowing for the probability that more than one disease entity may be classified under each heading, they are maximal values. The average value for six conditions is about 14×10^{-6}.

Owing to the classification of more than one type of chondrodystrophy under the same heading the rate given is likely to be considerably too high. According to Grebe (1955) there are several clinical types; and some cases may be due to recessive genes. Furthermore, these different types may have different mutation rates.

Another, dominant, condition, which apparently has a relatively high mutation rate, namely, retinoblastoma, occurs perhaps not infrequently as a phenocopy (Vogel 1954) not transmissible to the next generation. The same idea could be applied also to other conditions listed in Table 1, such as microphthalmos.

The indirect argument, which supports all these estimations, can only be used when there is a strong selection against the gene studied. Theoretically it should be possible to obtain mutation figures for several blood antigens, e. g. *ABO* or *MNS*, but selection against any of these genes is too slight and indefinite to be used as indirect support for the mutation hypothesis. On the other hand, the indirect argument can be extended to cover certain cases in which the combination of several genes at different loci is lethal or very deleterious. Thus a lethal condition, caused by the simultaneous presence of two heterozygous genes, will imply that each of the genes concerned mutates frequently enough

to make good the loss occasioned when it occurs in conjunction with the other.

Taking all these considerations together we can reasonably assume that the mutation rates for loci giving rise to dominant genes, though somewhat too high, are of the right order of magnitude. It seems that, for most of these dominant diseases, the rate should be considered to be about 5×10^{-6}.

(*ii*) *Sex-linked loci*

The prevalence in man of sex-linked diseases which are very lethal is difficult to explain except on a mutation hypothesis. Direct evidence based upon observed low incidence of haemophilia in sibships and in maternal collateral relative also supports this explanation. The matter has been repeatedly investigated by Haldane (1946, 1955) and there seems to be some evidence that mutation more commonly occurs in males than in females. The two sex-linked diseases which have given information about human mutation rate are haemophilia and pseudohypertrophic muscular dystrophy. In both cases there are many types of illness easily confused with one another clinically. Sex-linked types are identified by pedigree studies and by their occurrence in males only but, by this process, some autosomal cases may occasionally be incorrectly included. A characteristic difficulty is the exclusion of autosomal sex-limited conditions.

In the standard examples of haemophilia and sex-linked muscular dystrophy, mutation rates have been estimated several times but always on the assumption that, in each disease, there is only one locus involved. These rates, as shown in Table 1, are considerably higher than the direct estimates for autosomal dominants. Perhaps the X-chromosome is peculiar in that it has many complex loci or distinct loci with similar effects.

(*iii*) *Recessive traits*

A recessive trait in man can be defined as one which depends upon a gene in homozygous form. There may be mild manifestations detectable in heterozygotes (e. g. thalassaemia, galactosaemia, cystinuria) but the disease in the homozygote is the effect with which we are concerned. The indirect estimates of mutation rates for recessive diseases, shown in Table 2, assume that the heterozygote is neutral in its effect upon fitness. If the heterozygotes were deleterious, as suggested by Böök (1953) for schizophrenia, the values would have to be increased. Conversely if heterozygotes were slightly favourable, the values would have to be reduced.

TABLE 1.—*Estimates of spontaneous mutation rates of some human genes: (a) Dominant inheritance; (b) sex-linked inheritance*

Trait	Mutation rate per million loci per generation	Region	Source [1]	Date
Epiloia (a)	8	England	Gunther & Penrose	1935
Chondrodystrophy (a)	45	Denmark	Mørch	1941
Do	70	Sweden	Böök	1952
Aniridia (a)	[2] 5	Denmark	Møllenback	1947
Microphthalmos without mental defect (a)	5	Sweden	Sjögren & Larsson	1949
Retinoblastoma (a)	15	England	Philip & Sorsby	1947
Do	23	USA	Neel & Falls	1951
Do	4	Germany	Vogel	1954
Partial albinism & deafness (a)	4	Holland	Waardenburg	1951
Haemophilia (b)	20	England	Haldane	1953
Do	32	Denmark	Andreassen	1943
Do	27	Switzerland and Denmark.	Vogel	1955
Pseudohypertrophic muscular dystrophy (b).	95	USA	Stephens & Tyler	1951
Do	45	Northern Ireland	Stevenson	1953
Do	43	England	Walton	1955

[1] See Penrose (1956a).
[2] This estimate differs by a factor of 2 from that given by the author but it is based on his material.

TABLE 2.—*Indirect estimates of spontaneous mutation rates on the assumption of recessive inheritance*

Trait	Mutation rate per million loci per generation	Region	Source	Date
Juvenile amaurotic idiocy	38	Sweden	Haldane	1939
Albinism	28	Japan	Neel et al.[1]	1949
Icthyosis congenita	11	Japan	Neel et al.[1]	1949
Total colour blindness	28	Japan	Neel et al.[1]	1949
Infantile amaurotic idiocy	11	Japan	Neel et al.[1]	1949
Amyotonia congenita	20	Sweden	Böök [1]	1952
Epidermolysis bullosa	50	Sweden	Böök [1]	1952
Cystic fibrosis of pancreas	700	USA	Goodman & Read	1951
Sickle cell anaemia	10,000	USA	Neel	1951
Thalassaemia	400	USA	Neel	1951
Spastic diplegia	2,000	Sweden	Böök	1953
Microcephaly	49	Japan	Komai et al.[1]	1955
Phenylketonuria	25	England	Penrose [1]	1956
Schizophrenia	500	England	Penrose [1]	1956

[1] See Penrose (1956a).

A very slight amount of heterozygous advantage is sufficient to keep a rare recessive lethal in stable genic equilibrium in the absence of mutation so that the calculation of mutation rate is very easily invalidated. This is an extremely important principle and is worthy of detailed consideration.

Most well known recessive traits cannot easily be supposed to have arrived at their existing levels of gene frequency (e. g. 1/100 for phenylketonuria) by chance or by "drift". The situation for commoner genes is even more striking. For tnalassaemia and sickle cell trait (Neel 1951), cystic fibrosis (Goodman & Reed 1952), spastic diplegia (Böök 1953) and schizophrenia (Penrose 1956a) improbably high mutation rates have to be postulated. Indeed the maximum rate for sickle cell trait, derived from direct observation on heterozygotes, is much lower than that calculated indirectly (Vanderpitte et al. 1955). These common traits could not have easily established themselves unless the heterozygotes had some advantage. The advantages may have been local ones in the distant past, for example, ability to withstand infections, plagues, famines, abnormal climates and so on.

It is not necessary to postulate any virtue in the heterozygote as such. It could be sufficient if the mutant alleles were favourable at one epoch and unfavourable at another epoch, in different circumstances or at different stages of the same life cycle. The principle of genetical stability produced by heterozygous advantage or, more accurately, homozygous disadvantage, is one which has been understood for a long time (Fisher 1930) but only recently taken seriously. In human genetics it is exhibited by such a system as may be present in relation to the sickle cell trait in Africans. The disadvantage of one homozygote, SS, which suffers from anaemia, is balanced to some extent by disadvantage of the homozygote, *AA*, which is especially susceptible to malaria caused by *P. Falciparum.* Balanced human genetical systems are shown in metrical traits because the extreme types, which tend to be homozygous, are relatively unfavourable. Examples are stature, birth weight and intelligence level. For intelligence, in particular, there is a marked fertility differential in one direction and a viability differential in the other. That is, low intelligence levels are associated with low viability and high levels with low fertility.

In all such cases of balanced polymorphism the variation, which is apparently, reduced each generation by loss of extreme types, is not maintained by fresh mutation. It is maintained simply because the heterozygotes, who tend to have medium metrical value, are the parents of most children in each successive generation. In these circumstances it is quite useless to attempt to estimate the mutation rates of component genes: any indirect estimate will be far too high.

It has been suggested by Haldane (1939) that the converse may be true, namely that mutation rate estimates for recessive traits are often too low. The argument used is that the true incidence, which recurrent mutation would theoretically balance, has in the past been much greater than it is at the present time. This is likely because inbreeding, which facilitates the appearance of recessive

diseases, has been gradually diminishing for many decades in all civilized communities. I believe this argument to be unsound because the incidence of rare recessive traits in man is extremely irregularly distributed. Tay-Sachs disease is almost confined to Jewish communities as also is pentosuria, Cooley's disease has centre in the Po delta. Phenylketonuria, on the other hand, does not occur among Jews. Sickle cell anaemia is common in Africans. Juvenile amauratic idiocy is commonest in Sweden and acatalazaemia has only been found in Japan. These facts suggest that recessive mutations are very rare but that occasionally they have spread for unknown reasons probably connected with heterozygous advantage at one epoch or another. If mutations were not very rare the same set of recessive diseases would appear in all communities or at least in all inbred communities throughout the world.

To sum up the discussion on spontaneous mutation rate, my view is that, for a variety of reasons, most mutation rates already calculated are too high; points to be stressed are, first, that mutation may be mimicked by suppression of even the most regular kinds of dominant inheritance, secondly that different conditions are grouped under single clinical headings, and, thirdly, that heterozygotes of established recessive lethal traits are likely to have carried slight advantages in the past even if they do not at the present time.

4. EFFECT OF INDUCED MUTATIONS

The immediate effect of an increase above spontaneous mutation rate is most easily calculated when the gene is dominant. The rule, however, is quite general. The increase of incidence of any trait in the first generation, due to induced mutation, depends upon the proportion of cases due to fresh mutation in ordinary circumstances. For lethal dominants and sex-linked traits this proportion is large but in lethal recessives it is very small. It is also small for dominants which are very imperfectly manifested as with those contributing to multifactorial traits. The rule refers to the effect in the first generation or in closely succeeding generations, which especially interest people now living. The total quantitative effect on the population of altered mutation rate is theoretically the same whatever the manner of inheritance but, in the case of recessive or heavily modified dominants, a slight effect is maintained over such an enormous length of time, many thousands of years.

The proportion of cases of a lethal condition due to fresh mutation in any given generation can be estimated on the basis of the indirect argument. If the mutation rate, μ, is expressed as a function of the gene frequency thus,

$$\mu=f(q)$$

it follows formally that the proportion of cases in any given generation due to fresh mutation, a quantity which can be called M, is given by the approximation,

$$M=d\mu/dq.$$

For example, for a recessive lethal trait,

$$\mu=q^2, \text{ and so } M=2q.$$

Substituting 1/40 000 for q^2, the frequency of juvenile amaurotic idiocy as estimated by Sjörgren (1931), we get M=1/100. In view of what has been said about the use of the indirect method this is probably an upper limit but it shows how little effect a change in spontaneous mutation rate would have upon the incidence in the next generation after it had occurred or, indeed, in any subsequent generation. Doubling the mutation rate would only increase the incidence by one or two per cent. in the first subsequent generation.

5. SENSITIVITY OF HUMAN LOCI TO RADIATION

Much has been written about the probable sensitivity of human loci to radiation using experimental data on lower animals as a basis for comparison. Direct observations on man, however, are essential and three sources of information are at present available.

(i) *The comparison of offspring of selected parents exposed to different quantities of radiation*

This is the method attempted in several comparative studies. Children of radiologists have been examined by Crow (1955) and also by Macht & Lawrence

(1955) and the exposed Japanese population by Neel and his colleagues (Neel & Schull 1954). A development of the same idea is implied in two other proposed types of investigation. One of these is the special examination of children of patients who have received large therapeutic doses of radiation before conception, as may be the case in sufferers from spondylitis. The other suggested method is to examine the incidence of mutations in areas where the natural background radiation is high. Each of these methods, though theoretically possible, has its own special technical difficulties. There is a general objection to all of them, however. Fresh mutation is a phenomenon which can only very rarely be observed even though it may be occurring all the time. To search for slight increases in incidence of traits which, in the case of known recessives, will not exceed one per cent. requires the collection of enormous quantities of data and results are likely to be inconclusive. These methods are, in fact, rather inefficient even after allowances have been made for sources of error peculiar to each type of enquiry.

(ii) The examination of parental history in known instances of mutation

An alternative and more efficient method, which has received scant attention hitherto, is the careful examination of the personal histories of parents and, in certain instances, of grandparents, for groups of cases where fresh mutation is suspected of having played a part in causing disease in the offspring. This method has already produced valuable results by using the simple test of parental age.

Clearly, the older the parent the more likely he is to have been subjected to mutagenic influences. If the influence is background radiation, at the age of 40 the dose will have been twice that received at the age of 20. The net effect on parental age distribution of diseases in the offspring caused by background radiation alone, though definite, would be slight. The expected average increase would be scarcely more than one year above normal parental age (Penrose 1955a). Marked effects confined to one or other parent have, however, been observed in several malformations. Marked increase of father's age has been found in chondrodystrophy and acrocephalosyndactyly, as shown in Table 3. On the other hand, the incidence of mongolism is associated solely with advancing age of the mother. It would appear, thus, that, in so far as these traits may have their origin in fresh mutations, the causes must be different. In particular, a marked increase in paternal age strongly suggests some process connected with cell division in the spermatogonial stage which might be chemical in origin. The effect does not appear in other traits thought to be often caused by fresh mutation, such as epiloia, neurofibromatosis and retinoblastoma, where only slight and statistically insignificant parental age increases have been registered. Mongolism would, by the same test, appear to have an entirely different cause.

TABLE 3.—*Mean parental ages in sporadic cases of diseases attributed to fresh mutation compared with control mean ages*

Disease	Source [1]	Control mean [2]	Number of cases	Excess over control mean (years)	
				Father's mean age	Mother's mean age
Chondrodystrophy	Mørch (1942)	D	97	+5.4	+3.5
	Krooth (1952)	E	16 } 176	+6.8 } +5.1	+5.7 } +3.6
	Grebe (1955)	G	63	+4.3	+3.1
Acrocephalosyndactyly	Grebe (1944)	G	7	+5.5	+3.5
Epiloia	Gunther & Penrose (1935)	E	12 } 33	+0.8 } +0.5	+0.3 } +0.4
	Borberg (1951)	D	21	+0.4	+0.5
Neurofibromatosis	Borberg (1951)	D	49	+0.9	+0.8
Retinoblastoma	Neel & Falls (1954)	M	64	+0.5	+0.7
Mongolism	Schulz (1931)	G	80	+5.3	+7.7
	Øster (1953)	D	369 } 664	+5.3 } +5.8	+6.5 } +6.9
	Penrose (1955)	E	215	+6.8	+7.4

[1] See Penrose (1956b).
[2] E, England: father 30.9; mother 28.6. D, Denmark: father 33.3; mother 28.6. G, Germany: father 32.6; mother 28.9. M, Michigan, USA: father 30.5; mother 26.4.

The investigation of parental age is only one part of the problem. The history of parental exposure to X-rays and other kinds of radiation needs to be recorded;

occupational risks and possible exposure to chemical mutagens from external sources could also be made the subject of enquiry.

(*iii*) *Observations on somatic cells*

It has been suggested that a tissue culture treated by exposure to a known dose of radiation could serve to investigate the sensitivity of human cells. Techniques of this purpose will no doubt be developed in time though such experiments may never be critical because germ cells could have different sensitivity from that of somatic cells. This objection may be for the moment left on one side, however, while we search for existing data which might give clues to mutation rate in somatic cells. The obvious source of information is observations concerning inductions of tumours by radiation.

It has until recently usually been assumed that very small amounts of ionizing radiation have no effect on the induction of leukaemia. This is now doubted and the relation between bone marrow dose and incidence of leukaemia is thought to be not unlike the linear effect observed in the induction of X-chromosome lethals in *Drosophila*. Some idea of the dosage to bone marrow required to double the spontaneous leukaemia rate can be obtained from unpublished figures (Court, Brown & Doll 1956) and it is in the region of 30 to 50 roentgen units.

This line of thought leads to another interesting idea. The suggestion has been made that many sporadic cases of retinoblastoma arise as phenocopies. Is it not possible that these phenocopies are simply somatic mutations of the same gene which sometimes is carried in the germ track causing a dominant type of inheritance?

6. THE "LOAD" OF ABNORMAL GENES IN MAN

Finally, I would like to mention one or two points about the total effect of mutation on man since this has been so much discussed recently. Consider the total number of zygotes formed in a generation. We have no idea how many fail to pass through the first few divisions and never develop into embryos. Indeed it is impossible to estimate how many embryos are lost in the first six weeks after fertilization. According to Yerushalmy (1945) 15 per cent. of human pregnancies are known to terminate in miscarriages or abortions. Beyond this, three per cent. are stillborn and two per cent. are neonatal deaths. In addition, early mortality after the first month amounts to three per cent. These are figures for European and North American communities, where infectious diseases and malnutrition are under efficient control. In many parts of the world they would be gross underestimates. Among those who survive to adult status, 20 per cent. are unmarried and of those who do marry some 10 per cent. are infertile. How much of this continuous loss of zygotes, which may amount to about 50 per cent., is genetic is not known; by analogy with results obtained on ordinary metrical traits such as stature and intelligence, about half of this loss of zygotes might me directly hereditary. Perhaps the main factors are recessive lethals. If this were so, the indirect argument would lead to the conclusion that about a quarter of zygotes are lost each generation and that the genes which are thereby eliminated are replaced by fresh mutations. This points to the further conclusion, that a large increase in mutation rate, say permanent doubling, would eventually increase this lethal lead to a half and would greatly reduce human fitness, though the immediate effects would be small. However, for reasons given earlier, I do not suppose this picture to be an accurate one. Much of the permanent lethality which we experience is likely to be due to balanced genetical mechanism which do not require the assumption of appreciable amounts of mutation to maintain them. As I have previously pointed out (Penrose 1955b) improved living conditions are likely to reduce the frequencies of recessive genes whose prevalence is due to heterozygous advantage. Thus genetic damage which may be done by increase of mutation rate, due to industrial and medical uses of radiation, may be offset in the future by the improvements in hygiene which are taking place at the present time all over the world.

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Annex 8

Possible Areas With Sufficiently Different Background-Radiation Levels To Permit Detection of Differences in Mutation Rates of "Marker" Genes [1]

The impressive developments in atomic research in recent years and the increasing application of nuclear energy in many fields of human endeavour have brought to the forefront serious problems concerning the long-term effects of radiation on man and his environment. In order to obtain a better appreciation of the totality of effects on human populations, it is necessary first to take into account the magnitude of the various components of natural background radiation as well as the radioactive elements normally present in the body and environment. The magnitude of the contributions of cosmic rays, radioactive radiations from the earth's surface and radioactive elements in the body is shown in Table I.

In his paper (see p. 1761), Professor R. M. Sievert has dealt with the naturally occurring sources of radiation and measurements of low-level radioactivity. No attempt will therefore be made here to traverse this ground again.

MONAZITE AREAS

In this paper, the writer will endeavour to show that conditions in certain parts of India are particularly favourable for studies of the differences in muta-

[1] Submitted by Dr. A. R. Gopal-Ayengar, Head, Biology Division, Department of Atomic Energy, Indian Cancer Research Centre, Bombay, India.

tion rates due to differences in background radiation. But before doing so, it may perhaps not be out of place to say a few words about thorium ores and monazite, since it is these constituents on the earth's surface that contribute to the buildup of natural background levels in certain areas.

Like uranium, thorium is a derivative of acid rocks and is for the most part concentrated in granites, syenites and their corresponding pegmatites. There is, however, a basic geochemical difference between the two radioactive elements, in that while the weathering of thorium ores supervenes by and large a process of physical comminution, that of uranium ores is acted upon by chemical processes. In consequence, thorium becomes incorporated into sediments as discrete, detrital grains of primary mineral, whereas secondary uranium in sedimentary formation occurs as a diffuse, chemically absorbed entity on carbonaceous matter, phosphates and clays. The usual ore minerals of thorium are monazite, thorite and thorianite. Of these, mention will be made only of monazite, the principal ore.

Without entering into any detailed description of the mode of formation of monazite or the manner in which it finds its way into the sea—aspects which legitimately belong to the province of geology—the writer will pass on to the stage where it is found to accumulate as beach-sand deposits in different parts of the world.

As to geographical areas, menazite deposits have been found admixed with ilmenite, rutile, zircon and other rare-earth elements in patches along the coastline of India. The monazite has accumulated along the sea-shores by a process of natural concentration out of the products of rock decay in the course of long geological ages. The heaviest deposits occur in Travancore-Cochin State, in the south-west part of India. Here, over a stretch of about 100 miles (160 km), the coastline is characterized by patches of this radioactive sand. The most concentrated distribution is to be found over a 12-mile range from Neendakara to Kayankulam and in another stretch, a mile long, at Manavala-Kurichi. Although the monazite constitutes only 1% of the beach sand, the thorium content in it is one of the highest in the world, amounting to about 10.5%. Small pockets of the littoral belt also contain even higher amounts (33%).

Other areas of the world worth mentioning in this context are: Brazil, Ceylon, Indonesia, Australia, the Belgian Congo, parts of the USSR, South Africa, Madagascar, Korea, Spain and the USA. According to Davidson,[8] the beach-sand deposits of Brazil are situated in the States of Rio de Janeiro, Espirito Santo, Bahia, Parahyba and Rio Grande do Norte, extending along a coastline of more than 1000 miles, the largest accumulation being at Comaxatiba and Guaratiba in Bahia, Guarapary in Espirito Santo, Barra do Itabopoana in Rio de Janeiro and the so-called "fossil bar". Although the ratio of monazite to the other minerals is higher than in the Indian and most other sands, the Brazilian concentrate apparently has only 5–6% of thoria, as compared with 10% in Travancore.

MEASUREMENT

A preliminary sample survey of the monazite areas of Travancore-Cochin was carried out recently by the Health Physics and Air Monitoring Divisions of the Department of Atomic Energy, Government of India, to estimate the internal and external radiation exposures of the population living in the coastal areas. A more extended series of measurements is under way to obtain detailed information on the levels of activity in the areas of highest background radiation, where there is also a high population density. Measurements have also been taken in and around the houses at the active area.

The external radiation exposure in the region is caused by:

(a) β–γ radiation from natural uranium and thorium contained in the monazite, and

(b) β–γ radiation from radon, thoron and their decay products in the air (Table II).

The external exposure to alpha radiation is not important because of the small range of the particles.

Measurements were made with a thin window β–γ, Geiger-Müller counter with a thickness of 20–30 mg/cm.^{2}. The measuring instrument was calibrated with a thin walled ionization chamber. Measurements of this nature have also been carried out in Sweden by Professor Sievert who, as we have seen has been interested mainly in the measurement of low levels of activity, particularly

[8] Davidson, C. F. (1956) Mining Mag. 94, 197.

the γ radiation from living subjects. On the basis of his painstaking studies he has built up what is probably the most complete body of knowledge obtained so far on radiation exposure in human material of all age groups.

The internal exposure to the population on the monazite sands is caused by the intake of radioactive substances through air, water and food. Moreover, radon and thoron emanating from monazite will add to the contamination of the air in the vicinity. However, these gases decay in the air, and their decay products get attached to fine dust particles in the air from whence they settle down on the soil or on the population. When the air is breathed, a considerable portion of the active dust is retained in the respiratory system, where it undoubtedly acts on the epithelium. The intake of soluble compounds of uranium and thorium through food and water would increase the body burden of uranium and thorium through ingestion and become a permanent source of internal irradiation. The accompanying schematic diagram gives an idea of the disintegration of thorium and its decay products.

Representative series of measurements taken on the beach, at the surface and in the air, as well as those in the houses, are given in Tables III to VI. These relate to Neendakara Chavara (Table III), Shakthikulangara (Table IV), Panderathuruthu (Table V) and parts of Midalum (Table VI).

It will be seen that there is considerable variation in the intensities of the radiation at different points in the measured areas. While the actual amount of radiation that the population receives must remain speculative to a certain extent at this stage, the balance of evidence seems to point to the fact that the population is subjected to fairly high doses. The estimated values in terms of γ doses for different regions range from 200 mr/yr to about 2.6 r/yr. It is further estimated that the population would be exposed to a total γ dose of about 10.20 r over a reproductive span of 30 years. It may be mentioned in this connection that Travancore State has the highest density of population in India; the estimated number of inhabitants in the monazite area is of the order of 100,000. It should be stressed that the total β–γ dose in all cases was 3.5 times higher than that due to γ alone. Although in normal circumstances the β dose could be considered to have a negligible effect, the fact that the decay products of thorium, mesothorium 2 (2.1 Me.) and thorium C (2.25 Me.) are high β-energy emitters should not be lost sight of, especially when it is considered that the people come into close contact with the surface of the soil every time they sit or sleep on it. A correction factor would therefore have to be applied to the γ doses in order to estimate the total dose to the hole body as well as to the gonads.

POSSIBILITIES OF DETECTING DIFFERENCES IN SPONTANEOUS MUTATION RATES

It has become all too clear that the compilation of exact genetical data of man is beset with numerous difficulties: for one thing, there are no pure strains to work with; for another, the generation times are inordinately long. There is also the probability that many the radiation-induced genetic changes would be lethal. A considerable number of recessive mutations are passed through successive generations and may express themselves as physiological aberrations that weaken but do not necessarily kill the individual. In such cases, distinction from incidental disease processes may be difficult if not impossible, and may therefore never be resolved. Many mutations induced by radiation may be expected to affect the fertilized ovum, and hence abortions, foetal deaths, stillbirths, infant mortality, malformations, sex ratios, viability and fertility, etc., are the genetic changes most readily seen and analysed statistically. But what we need to look into also are the possibilities of detecting differences in the mutation rates of "marker" genes of populations exposed to radiation of the order present in the monazite area of Travancore. A careful study of the population structure in this area should furnish information concerning gene frequencies and their distribution in time and space, as well as data on mutation rates. Several years ago, Muller[8] raised the question of the possibility that genic erosion will result in a piling up of deleterious mutations following ameliorative medical practices. Now, in the monazite belt of Travancore we have an almost unique situation, where there would appear to be no relaxation of the forces of selection on the population, since the alleviating action of modern medical services has not found its expression to any appreciable degree. The

[8] Muller, H. J. (1950) Amer. J. hum. Genet. 2, 111.

population has been more or less stationary for generations and might be expected to show differences in mutation rates for particular traits—autosomal dominants or sex-linked recessives of the type already discussed by Professor Stevenson in his paper (see page 5 Annex 9). A control population of comparable dimensions, with similar demographic conditions and normal background radiation exists in the nearby areas.

An inquiry of such a nature would obviously be a long-range one, but would be well worth doing in view of the possibility of thus obtaining some direct evidence on the genetic consequences of naturally occurring high background radiation. The investigation would also be likely to shed light on the dosage relationships for doubling the spontaneous mutation rate and other cognate problems. Moreover, it might also reveal interesting somatic effects, such as the incidence of leukaemia, cancer and other conditions.

TABLE I.—*Level of exposure of human body to background radiation*

[Dose Unit: Milliroentgens per year]

I. RADIOACTIVE ELEMENTS IN THE BODY

Radioactive carbon (15 disintegrations per minute per gram of carbon)	2
Radioactive potassium (1980 disintegrations per minute per gram of potassium)	19
Radium (3.7×10^{10} disintegrations per second per gram of radium	(7?)

II. COSMIC RAYS

	Equator	High latitudes
Sea level	33	37
5,000 feet	40	60
10,000 feet	80	120
15,000 feet	160	240
20,000 feet	300	450

III. RADIOACTIVE RADIATIONS FROM EARTH'S SURFACE		TOTAL: I+II+III AT EQUATOR (SEA LEVEL)
Granite rock abundance in parts per million (Typical): U Th K 4 13 3×10^4	90	21+33+90=144
Sedimentary rock U, Th and K about one quarter as abundant as in granite	23	21+33+23=77
Ocean Abundance in parts per million U Th K 2×10^{-3} 10^{-5} 4×10^2	0	21+33+0=54
Uranium (ore content, 0.1% U) rock		
rock surface	2800	
inside mine (2 x 2800)	5600	
Phosphate (fertilizer) rock: 280–700 (U content, about 0.01–0.025%) rock surface		

TABLE II.—*The thorium series*

Name	Symbol	Half-life	Energy of radiation (Mev.)		
			α	β	γ
Thorium	$_{90}Th^{232}$	1.39×10^{10} years	4.03		0.05
Mesothorium 1	$_{88}Ra^{228}$ ($MsTh_1$)	6.7 years		0.02	0.03
Mesothorium 2	$_{89}Ac^{228}$ ($MsTh_2$)	6.13 hours		2.1, 1.7, 1.0	0.06, 0.97
Radiothorium	$_{90}Th^{228}$ (RdTh)	1.90 years	5.42, 5.34		0.084, 0.087
Thorium X	$_{88}Ra^{224}$ (ThX)	3.64 days	5.68, 5.45, 5.19		0.24, 0.05
Thoron	$_{86}Pa^{220}$ (Th)	54.5 seconds	6.28		
Thorium A	$_{84}Po^{216}$ (ThA)	0.158 seconds	6.77		
Thorium B	$_{82}Pb^{212}$ (ThB)	10.6 hours		0.33, 0.57	0.24, 0.30, 0.11, 0.25
Thorium C	$_{83}Bi^{212}$ (ThC)	60.5 minutes	6.05, 6.09	2.25	0.04, 2.2
Thorium C^{I}	$_{84}Po^{212}$ (ThC^{I})	3×10^{-7} seconds	8.78		
Thorium C^{II}	$_{81}Tl^{208}$ (ThC^{II})	3.1 minutes		1.79	2.65, 0.58, 0.51, 0.23, 0.86
Thorium D	$_{82}Pb^{208}$ (ThD)	Stable			

TABLE III. INVESTIGATIONS AT NEENDAKARA-CHAVARA BEACH*

Beach				Road
62 52 48	82 18 16	5.4 5 4.6	14 12 12	2.2 2 2
62 54 50	44 36 32	hut 10 9	6.5 6 6	10 8 7
72 62 56	34 30 26	16 12 10	7.5 7 7	14 12 10
64 56 46	44 38 32	12 10 9	hut .	28 24 20
30 24 22	12 10 8.5	. 10 8.4	4.4 4 3	6.5 6 5.8
36 30 28	12 10 8	8.4 8.4 7.5	hut	16 12 12
22 18 14	10 9 8.5	hut . .	32 26 20	24 22 20
32 28 24	22 18 16	5.4 5 5	cowshed	2.2 2 2
22 18 16	12 10 8	hut . .	8.5 8 7.8	7.8 7.4 7
10 8 7.5	8.5 8 7.5	6 7.6 5.2	4.2 4 3.8	16 14 12

* An area of one mile by about 500 yards was scanned by taking ten points along the length and five across the width.

Reading from left to right, the three figures in each square refer to actual counts of surface $\beta+\gamma$, surface γ, and air γ, respectively.

For $\beta+\gamma$, 100 counts = 10.15 r per year

For γ, 100 counts = 2.86 r per year

TABLE IV. INVESTIGATIONS AT SHAKTHIKULANGARA (SOUTH OF NEENDAKARA-CHAVARA BEACH GRIDLINES)*

← 100 yards →

↕ 30–50 yards

56 44 40	36 28 24	18 14 12	14 10 8.6
72 58 50	12 9 7.5	14 10 8 A	12 9 7.4
72 58 48	22 18 14	12 10 8	12 10 8.2
44 36 30	20 14 12 B	72 60 48	12 10 8.4
20 16 14	28 20 16	32 22 18	16 12 10
16 12 10	28 20 16	32 24 20	14 12 C 10
8 7.5 7	32 24 22	22 18 16	22 18 14

* Most of the area is covered with coconut plantations, the coconut pits being filled with the sand from the beach. Cross-road activity, one furlong from beach, is 7.

Reading from left to right, the three figures in each square refer to actual counts of surface $\beta+\gamma$, surface γ, and air γ, respectively

For $\beta+\gamma$, 100 counts = 10.15 r per year

For γ, 100 counts = 2.86 r per year

A:	hut entrance	8	B:	hut entrance	24	C:	hut entrance	22
	yard (black sand)	30		floor	12		floor	12
	floor	7.5		wall	10		wall	10
	wall	6.4						

TABLE V. INVESTIGATIONS AT PANDARATHURUTHU*

Beach				Canal
18 16 14.2	12 9.4 8.8	16 14 12	10.4 9 8.4	8 7.5 7
9.2 8 7.6	16 14 12	9 8.2 7.8	12 10 10	7.6 7 6.4
16 14 12	12 9.6 9	hut	10 8.8 8	7.6 7 6.6
22 18 14	10 8.6 8	14 12 10	9.6 9 8.6	8.4 8 7.6

* An area of 100 yards by 50 yards, one mile down towards Chavara from Cheriaakhiakal, was scanned by taking four points along the beach and five across it.

Reading from left to right, the three figures in each square refer to actual counts of surface $\beta+\gamma$, surface γ, and air γ, respectively.

For $\beta+\gamma$, 100 counts = 10.15 r per year

For γ, 100 counts = 2.86 r per year

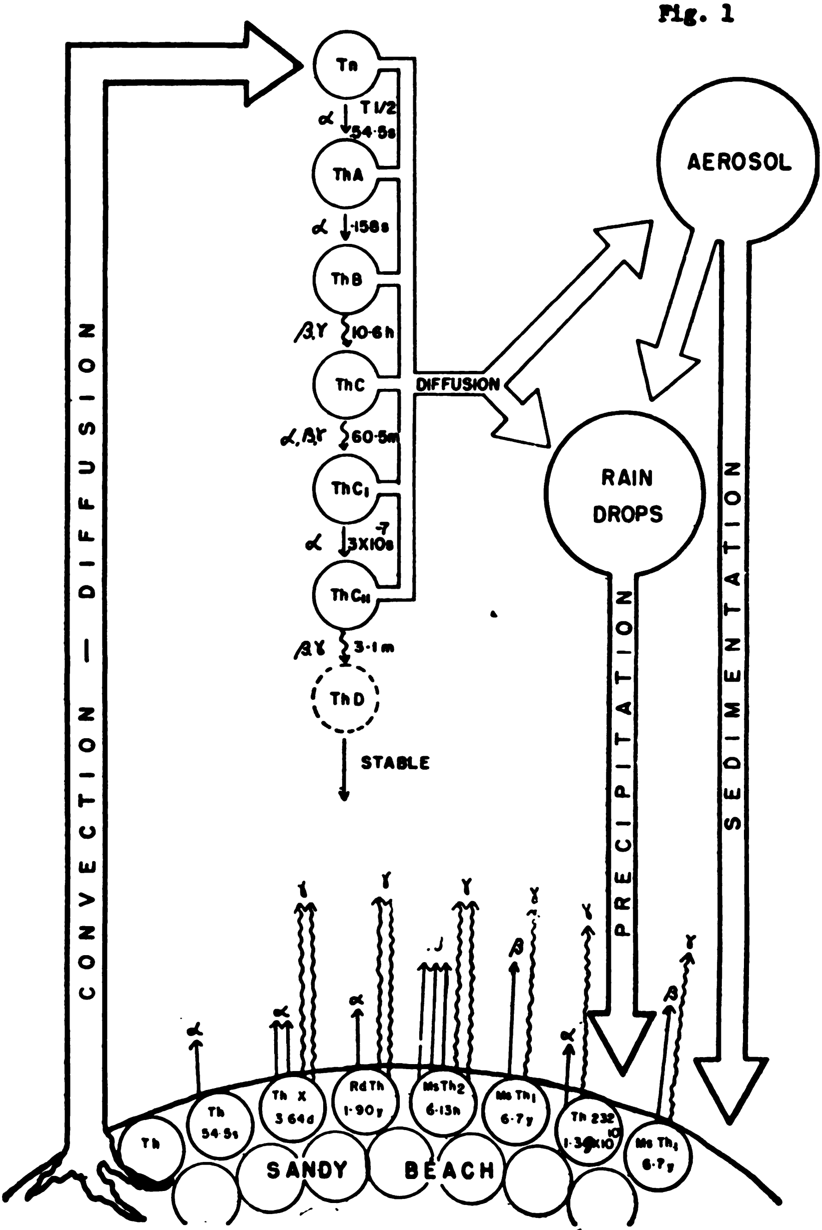
Hut:				
	entrance	10	8.6	8
	floor	10	8.4	8
	wall		8.0	

TABLE VI.—*Investigations at Midalum (Midalum teri sandhill)*

Location	Surface $\beta+\gamma$	Surface γ	Air γ
Black sandy patch	18	14	12
Black sandy stream, 1,000 yards from the sea, 500 yards from sandhill	76	70	60
In centre of six huts	24	22	20
Black and yellow spots on another stream	-------	90	80
Black and yellow spots on another stream [1]	90	80	80
Nearby garden, 100 yards upstream	18	14	12
Inside the hut:			
Entrance	9.5	8.8	8
Floor	13	12	12
Wall	-------	12	-------
Centre of 20 huts in locality	-------	8	7.5
Market	14	12	11.5
In centre of locality on wet ground	-------	12	-------
Hotel floor	12	10	9.5

[1] An underground measurement made here gave a γ count of >100.

Fig. 1



Tn
T 1/2
54·5s
ThA
·158s
ThB
10·6h
ThC
DIFFUSION
60·5m
ThC$_1$
3X10^{-7}s
ThC$_{11}$
3·1m
ThD
STABLE
AEROSOL
RAIN DROPS
PRECIPITATION
SEDIMENTATION
CONVECTION — DIFFUSION
Th
Th 54·5s
Th X 3·64d
Rd Th 1·90y
MsTh$_2$ 6·13h
Ms Th$_1$ 6·7y
Th 232 1·39X10^{10}
Ms Th$_1$ 6·7y
SANDY BEACH

Annex 9

Comparisons of Mutation Rates at Single Loci in Man [1]

This note is concerned with the practical problems that arise in attempting to compare phenotype frequency, gene frequency and mutation rates between different communities. Such a comparison may be desirable for a number of reasons and, indeed, it is always something which is inherently of great interest in human population genetics. In one context, however, the comparisons which have been suggested as particularly desirable are those between communities known to have been exposed to widely different total radiation.

It seems most unlikely that it *would* be feasible to detect differences of the small order of magnitude which would be expected in such communities, or if they were detected, to attribute them with confidence to differences in background radiation alone. It would appear necessary to elaborate these and other points of criticism, however, because the suggestion has been made so frequently that it is perhaps better not to ignore, but rather to analyse, the difficulties inherent in such comparisons. This must be the writer's excuse if what follows appears largely destructive.

OUTLINE OF PROBLEMS INVOLVED

The difficulties may be summarized as follows:

1. There are the statis tical problems inherent in attempting to detect small differences in very low frequencies based on small numbers, such as mutation rates in man. Suppose in two populations of about 3,000,000 each it was desired to compare the frequency of a dominant or a sex-linked trait with an approximate expected frequency of 1/30,000. Suppose, also, that a background radiation of 3 r per generation to the gonads was expected to cause 10% of all mutations. If the two areas compared had a difference of 4 r in radiation exposure, then it would be necessary to interpret a difference of some 10–20 in the number of affected individuals in the two populations.

2. There are the hazards of assuming that any differences detected in mutation rates between two areas are, in fact, caused by different exposure to radiation. The proportion of mutations attributable to radiation is not known, and there are other factors—racial, dietary and demographic—each of which separately may determine more variation in rates than that determined by background radiation. The evidence of a close relationship between mutation and parental age is very strong for several of the genes whose expression would be suitable for comparative purposes, so that conventions of age at marriage and of the age interval between the men and women might well greatly influence the mutation rates. There are used in medicine and industry naturally occurring and synthetic substances known to be mutagenic in some lower organisms. Their effects on mammals are unknown.

3. In calculating mutation rates by the indirect method the value adopted for relative fertility of the specific phenotype could be rather critical. Yet in different populations the actual fertility of the phenotype may vary for social reasons, and because of inadequate or differing sources of demographic information it might be that only a very unsatisfactory method could be used in the comparison of two communities. For example in Denmark (Mørch [2]) a high proportion of adult achondroplastics have had offspring, while in Northern Ireland at present only one living achondroplastic is known to have had children (Stevenson [3]). In the State of Michigan, USA, the numbers of offspring born to people of both sexes at different ages can be compared (Falls & Neel [4]) whereas in England and Wales this information is available in respect of females only, and in Northern Ireland there are no national statistics of this kind. Hence the available background demographic information may not be comparable in areas otherwise suitable.

4. An essential for adequate comparison of mutation rates in two communities is that the complete patterns—not the truncated patterns which result from different degrees of ascertainment—should be available. With the best will in the world it is impossible to arrive at complete ascertainment unless the medical services are reasonably well organized, records are good and available, and

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[2] Mørch, E. T. (1941) Op. dom. Biol. hered. hum. (Kbh.), 3.

[3] Stevenson, A. C. (1956). (In press.)

[4] Falls, H. F., and Neel, J. V. (1951) Arch. Opthal. (Chicago), *46*, 367.

co-operation is readily given by medical and other authorities To take a specific example, it is extremely difficult to find the older surviving sporadic boys with Duchenne-type sex-linked muscular dystrophy. If they have not had medical attention for many years, if they only attended a long time previously a hospital which kept poor records, or if they live in an area where the education authorities tend to ignore children who do not attend school, they may be well nigh impossible to find. Any comparisons between communities would therefore need to be supervised by physicians or other persons with plenty of practical experience of the difficulties and of the devices for checking and cross-checking the efficiency of ascertainment.

5. Lastly, there are the dual problems of diagnosis and of the complications introduced when it is appreciated how diverse the apparently simple relationship between gene and trait usually is. As might be expected from animal work, experience is constantly showing that human syndromes can be determined by genes at various loci or by different alleles. In addition there is, of course, the complication of the occurrence of phenocopies. The more carefully single-gene traits in man are studied, the more complex the picture becomes, so that there is hardly a trait known where there is not either some evidenec or a suspicion that the trait may be determined by various mechanisms. This does not by any means depend only on the text-book descriptions of many traits described as being caused sometimes by dominant, sometimes by recessive and sometimes by sex-linked genes. Many of these may well have arisen from the misinterpretation of pedigrees or from the selective publication of the data of remarkable families. However, clinical and biochemical separation of traits and the increasing practice of studying all families with affected members in a community constantly seem to suggest such probabilities. Perhaps more will come to light when more data are available for the analysis of various measurable characteristics of traits, such as measurements between sibs and between all the phenotypes. Perhaps, too, we shall get help in the future from some further knowledge of the morbid anatomy and histology of genetical conditions. The most remarkable example of all is probably hereditary deaf-mutism, where the evidence is strong that many recessive genes and a few dominant ones can each determine deafness which cannot be separated clinically. It is interesting to see that in mice, where similar evidence is available, even histology fails to reveal differences in lesions produced by different genes In deaf-mutism, too, the so-called "congenital" cases or phenocopies seem to constitute as many as one-third of all cases (Stevenson & Cheeseman [5]).

Such reflections make it difficult to suggest which traits would be suitable for use as markers for comparisons of the kind suggested, and indeed they leave a nasty suspicion in one's mind that certain traits might be chosen as satisfactory largely because not enough is know about them.

TYPES OF TRAIT SUITABLE FOR USE AS MARKERS

It seems worth while here to review briefly the kinds of trait which might be used as markers for mutation rates at specific loci and to point out the practical problems just mentioned as they arise. It will be generally accepted that autosomal-recessive gene traits are quite unsuitable for the purpose. In the first place, they are individually rare. Secondly, variations in inbreeding ratios, difficult to detect except in the crudest form of full-cousin rates in man, together with the fact that if small isolates with close inbreeding produce a number of cases of a rare trait the total rate will be very markedly affected, render it very hazardous to estimate a gene frequency as a preliminary to calculating a mutation rate.

In contrast, autosomal-dominant and sex-linked gene traits offer some opportunities for direct estimations and, being less dependent on vagaries from random mating, offer the chance of indirect calculation of mutation rates based on the theoretical distribution of genotypes at equilibrium developed by Haldane.[6]

It may be presumed with some confidence that the genes for which mutation rates have already been estimated are those whose characteristics are most likely to be suitable for comparative purposes. As will be seen from Tables I and II in which mutation rates for some dominant and sex-linked gene traits are given, the number of adequate calculations made is unfortunately very small and the

[5] Stevenson, A. C., and Cheeseman, E. A. 1956) Ann hum. Genet. *20,* 177.
[6] Haldane, J. B. S. (1935) J. Genet. *31,* 317.

data from which they are derived are frequently rather meagre or dependent on indirect estimates rather than on actual counts. This is intended not so much as a criticism as an indication of the difficulty—and, at times, impossibility—of assembling data which are sufficient in quantity and quality.

AUTOSOMAL DOMINANT GENES

Autosomal dominant genes would seem to offer the best opportunity for collecting data for comparative purposes, and the ideal trait would be one with the following charatceristics:

(b) that only one gene can determine the trait;
phthaimus, heart defect and rudimentary tail, and two had cleft palate, poly-
errors in estimating gene frequency and in identifying new mutant phenotypes);

(c) that the phenotype cannot be mimicked by a phenocopy;

(d) that the condition is recognizable at birth or in early life but that its possessors do not die too young (this is important, in that otherwise defferential mortality experience makes estimates of frequency difficult);

(e) that the frequency of the condition is reasonably high (the lower the frequency, the larger the population which will be needed to detect differences in frequency and, hence, mutation rates):

(f) that selection against the phenotype is marked. (If it is not, direct estimates of mutation rates would be well nigh impossible to obtain as new mutant phenotypes would very seldom be observed. Indirect estimates of the mutation rate would also be less reliable, as unless the dimunition of the effective fertility is big enough to be recognized and measured, such estimates could not be made. In a low frequency trait, with little negative selection, a small number of mutations a few generations previously could result in great differences in phenotype frequency.)

Looking at the traits in Table I and the mutation rates calculated, it is unfortunately easy to point out the deficiencies of each for comparative purposes. Perhaps it may be worth just mentioning them for the benefit of non-medical readers, who may not be familiar with the clinical aspects.

Achondroplasia

There are three certain objections, and another possible one, to the use of achondroplasia as a marker. In the first place, it is fairly clear that achondroplasia as commonly recognized at birth as "different" from achondroplasia as commonly seen in older subject. Pooling the obstetric history and the foetal condition of Mørch's eight and the writer's nine cases of achondroplasia recognized at birth and born to normal parents (Stevenson, 1956), it appears that of the seventeen (six males and eleven females), six were stillborn; eight died shortly after birth; one lived for one year and died of pneumonia; one lived for eighteen months, but never walked or had any teeth, and died of pneumonia; and one lived to twenty-nine years of age. In this last case (one of Mørch's), however, the condition of the father was not known.

Further in this combined series of cases, six of the mothers had hydramnios in pregnancy and three of the babies had other gross anomalies: one had micro-
(a) 100% manifestation in the appropriate genotype (this would obviate
dactyly and syndactyly.

In Northern Ireland a complete ascertainment has been made of 37 subjects presumed to have received fresh mutations, only three of whom were recognized at birth. All this suggests that the cases recognized in hospital at birth have the type of maternal history and foetal appearance which we usually associate with congenital, but not necessarily hereditary, anomalies. It further raises the question whether the possible survival of mild cases of this type may not complicate the gene frequency and fertility issues in the living. It may also be noted here that it would be hazardous to compare the incidence of this condition in births in various hospitals, as the amount and quality of ante-natal care may alter the incidence. For example, of nine cases born in the Royal Maternity Hospital, Belfast, from 1 January 1938 to 30 June 1956, six mothers were admitted either because of hydramnios or because of pre-natal X-ray diagnosis (four cases).

Secondly, from time to time cases have been reported of two achondroplastic subjects being born to normal parents. Helwig-Larson & Mørch[7] and Grebe[8]

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[7] Helwig-Larson, H. G., and Mørch, E. G. (1950) Nord. Med. *43*, 180.
[8] Grebe, H. (1952) Z. Kinderpsychiat. *71*, 437.

have reported such instances, in one of which the parents were cousins. There are two such families in Northern Ireland, and again in one case the parents are full cousins. This suggests that there may be a recessive gene or allele, and introduces another complication.

Thirdly, there are the difficulties of diagnosis. The taller achondroplastic subjects are usually discovered only as the parents of affected children. Several cases about 5 foot in height have been reported. The writer has seen a man 5 foot 1 inch in height, and neither he nor his colleagues can decide whether this man is affected. If he had an affected relative in an appropriate relationship, the issue would probably be beyond doubt.

The separation of achondroplasia from Morquiö's syndrome is perhaps not as easy as is commonly assumed. For example, some cases of achondroplasia seem typical as far as limb-strength, shape of head and hands are concerned, yet radiographs of the spine show vertebral changes commonly assumed to be characteristic of Morquiö's disease.

Finally, as already mentioned, in the indirect estimation of the frequency of mutation, the value taken for the relative fertility may greatly alter the figure calculated. In Denmark a very high proportion of subjects have had offspring, mostly illegitimate, whereas only one subject in Northern Ireland is known to have had any children. Thus, variation in social standards would interfere with comparisons.

Epiloia

Epiloia must, it would seem, be ruled out as a marker. The total frequency of the trait as measured is low. Gunther & Penrose[9] estimate 1/120,000 and the nine living cases in Northern Ireland represent essentially the same frequency (Stevenson & Fisher[10]). It is possible to estimate that the real frequency of the genotype is perhaps three times as great, but such speculations, although no doubt valid in some contexts, are hardly satisfactory when attempting to compare two frequencies.

The trouble is that the gene may not be manifested at all or may appear only in such mild or uncharacteristic formation that the condition will not be diagnosed unless attention is called to severely affected relatives. In addition, the condition may be impossible to diagnose before the characteristic skin affections appear, and there will almost certainly be some undetectable cases in any large group of young epileptic children. When one adds that subjects suffer a very high mortality, that relatively few survive for thirty years, and that many cases of tuberose sclerosis are only discovered at post-mortem examination, the difficulties are even more obvious.

Retinoblastoma

This would seem to be a trait more suitable in many ways for the purpose of comparing frequencies, provided that there are good ophthalmological services in the areas observed. Children with eye symptoms rapidly come to the attention of the doctor, and those with the kind of symptoms and signs likely to be caused by retinoblastoma would be referred quickly to an ophthalmologist and the diagnosis would be made, if not immediately, then soon afterwards. In a very high proportion of cases the eyes are enucleated, and histological examination is available to confirm the clinical diagnosis. However, as it seems likely that as many as one quarter of eyes which are enucleated as a result of retinoblastoma are otherwise affected, biopsy examination is essential.

Falls & Neel,[4] who have made the most complete study as yet carried out are not willing to exclude the possibility that more than one gene can cause the condition and that some cases, particularly the uni-ocular ones, represent phenocopies. Further, they raise the question of racial differences in frequency by pointing to the apparently low frequency in people of African as opposed to those of European origin. Finally, they were not satisfied that they had made a complete ascertainment.

Waardenburg's syndrome

Waardenburg[11] estimated that the interesting syndrome described by him (hair pigment, eye and hearing defects) had a frequency in the Netherlands of

[9] Gunther, M., and Penrose, L. S. (1935) J. Genet. *31*, 413.
[10] Stevenson, A. C., and Fisher, O. D. (1956) Brit. J. soc. Med. *10*, 134.
[11] Waardenburg, P. J. (1951) Amer. J. hum. Genet. *3*, 195.

about 1/42,000, but he had to make allowance for an estimated proportion of undiscovered cases. Indeed, a direct estimate would involve examining the whole population for minor signs. It is clear from Waardenburg's account that, in a given subject, only one of the triad of hair anomaly, deafness and eye signs may be present, and it would seem impossible on clinical grounds and in terms of the effort required to examine sufficient people to make a direct estimate of the frequency of the trait. For example, in Northern Ireland only one case has been discovered, and this was a sporadic case which turned up in work on hereditary deafness.

Pelger's leucocyte nuclear anomaly

This appears to be an uncommon trait even in continental Europe, although it is much commoner there than in North America or in the United Kingdom.[12] Indeed, until controlled studies determine whether these apparent differences are in fact real, and if so whether they are racial or geographic, it would appear rather hazardous to suggest that the trait might be used for the purpose of comparing mutation rates. Further, in the absence of easily recognizable external characteristics it would require examination of perhaps 500,000 samples of blood to find a reasonable number of cases.

Aniridia

The syndrome of aniridia and mental deficiency is estimated by Møllenbach[13] to have a frequency of about 1/100 000. The condition appears to be inherited as a dominant, but with considerable variation down to complete failure of manifestation. The history of some families also suggests that there is a recessive form, and the pattern of aniridia and other associated eye anomalies described within and between families suggests that we may be observing the effects of several different genes or of alternative alleles. It would seem that further studies of this condition are needed before it may be considered for use for our purpose.

Multiple polyposis of colon

A mutation rate for multiple polyposis of the colon was calculated by a most ingenious method by Reed & Neel,[14] but the incidence of the condition could hardly be counted directly. The condition is one of multiple small benign tumours of the colon and rectum, and cases only come to attention (a) when one of the benign tumours undergoes malignant change and causes symptoms, (b) when there is accidental bleeding from the tumours, (c) when the colon is examined by sigmoidoscopy for some other purpose, and (d) when found by chance at autopsy.

These would have to be the starting-points for all cases of ascertainment of sporadic cases and for investigations of families in familiar cases. Short of passing protoscopes and sigmoidoscopes on perhaps ten thousand people and chasing relatives with such instruments, it seems unlikely that this condition could be used as a marker!

Dystrophia myotonica

Lynas[15] in the Department of Social and Preventive Medicines, Queen's University of Belfast, has made the only complete ascertainment of dystrophia myotonica that the writer has been able to discover. The greatest single difficulty in this condition is again failure or partial failure of manifestation of the gene and variation in age of onset, so that the mildly affected mutant phenotype or the mildly affected members of the present generation of a family could hardly be ascertained. Possibly, very careful assessment of the neurological condition of persons presenting with pre-senile cataract would make it possible to find more cases but there would still be an element of doubt about many cases.

Marphan's syndrome

Lynas (unpublished data, 1956) has also made a complete ascertainment of Marphan's syndrome. Here again, all the difficulties arise which are inherent in dealing with a trait which is the variable manifestation of an irregular dominant gene. Precisely parallel difficulties to those mentioned for dystrophia myotonica are encountered: diagnostic doubts in mild cases, the impossibility

[12] Patau, K., and Nachtsheim, N. (1940) Schr. Naturforsch. *1*, 345.
[13] Møllenbach, C. J. (1947) Op. dom. Biol. hered. hum. (Kbh.), *15*.
[14] Reed, T. E., and Neel, J. V. (1955), Amer. J. Hum. Genet. 7, 236.
[15] Lynas, M. A. (1956) Ann. hum. Genet. *21* (in press).

of ascertaining mild cases unless there are more severe cases in the family, and so on.

SEX-LINKED GENES

Of the sex-linked gene traits, only haemophilia and Duchenne-type muscular dystrophy appear to be sufficiently frequent and well-defined for possible use as markers. The question of differential mutation rates in males and females—raised by Haldane[16, 17] for both these conditions—must be regarded meanwhile as unproven. It should be remembered that we must rely on indirect estimates involving an estimate of relative fertility in calculating rates for sex-linked recessive genes.

Haemophilia

This seems to be a reasonably suitable condition for the purpose, provided that there are adequate clinical, pathological facilities for differentiating between haemophilia and allied disorders. Curiously enough, apart from Andreasson's work in Denmark[18] and possibly Fonio's inquiry in Switzerland,[19] no one has made a complete ascertainment of the condition, and with new techniques presenting opportunities of separating out different haemoglobins, such work seems overdue. The increasing life-span and fertility of haemophiliacs make for some difficulty in assessing mutation rates, but these do not seem insurmountable.

Duchenne-type muscular dystrophy

Three complete ascertainments of Duchenne muscular dystrophy have been reported. Stephens & Tyler's[20] and Stevenson's[21 22] data are strictly comparable clinically but Walton's[23] include two females, and one male who lived to 40 years of age, who would certainly not be accepted by the other authors. However, the clinical details given by Walton make it possible for the data to be equated, and there is reasonable agreement between the three on gene frequency and mutation rate, though perhaps Walton's ascertainment would seem to be less complete on internal evidence.

CONCLUSIONS

To sum up, it would appear unlikely that communities of sufficient size could be found which would have sufficiently different exposures to background radiation to permit detection, far less measurement, of differences in mutation rates.

The basic problem is likely to be statistical. In addition, however, problems in ascertainment, in clinical diagnosis and in the complexity of the underlying genetical mechanisms would add further to the difficulty in using the "single gene traits" which have been suggested as markers.

Finally, since it seems wise not to end on too pessimistic a note, the following points may be worth considering:

1. Suppose the proportion of mutations due to a background radiation of 3 r is not 10% but, say, 20%, the upper limit suggested in the report of the Medical Research Council of Great Britain.[24] Then given a population of 3,000,000 and a dominant trait with a frequency of about 1/30,000 as before, a difference of just under 5 r near the 3 r level would seem theoretically to give expected differences of the trait of about 30 cases, which might be interpreted as significant. If only 10% of the mutation rate is determined by radiation, then the same numerical difference in cases would require about 9 r difference in background radiation.

2. If, in spite of the difficulties outlined, mutation-rate comparisons are thought to be fundamental, then another type of planned observation than straight comparison between two areas might be more satisfactory. For example, serial comparisons of a number of defined areas, for several traits with carefully planned control of diagnostic standards and ascertainment, and the simultaneous collection of background radiation information would perhaps be more valuable than a comparison between two areas.

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16 Haldane, J. B. S. (1947) Ann. hum. Genet. *13*, 267.
17 Haldane, J. B. S. (1956) Ann. hum. Genet. *20*, 344.
18 Andreasson, M. (1943) Op. dom. Biol. Hered. Hum. (Kbh.), *6*.
19 Fonio, A. (1954) Die erblichen und sporadichen Bluterstamme der Schweiz, Basel.
20 Stephens, F. E., and Tyler, F. H. (1951) Amer. J. Hum. Genet. *3*, 111.
21 Stevenson, A. C. (1953) Ann. Eugen. (Camb), *18*, 50.
22 Stevenson, A. C. (1955) Ann. hum. Genet. *19*, 159.
23 Walton, J. N. (1955) Ann. hum. Genet. *20*, 1.
24 Great Britain, Medical Research Council (1956) The hazards to man of nuclear and allied radiations, London.

TABLE I.—*Estimations of mutation rates of autosomal dominant gene traits*

Trait	Basis of estimation of mutation rate	Estimated rate per million	Source
Achondroplasia	*Direct:* 8 sporadic cases in 94 073 hospital births.	43	Mørch [1]
	Indirect: $\mu=\frac{1}{2}(1-f)x=\frac{1}{2}(1-0.098)\frac{86}{3\ 793\ 000}$ (Denmark)	10	
	Direct: 6 sporadic cases in 44 109 hospital births. (South Sweden)	68	Böök [2]
	Direct: 9 sporadic cases in 31 753 hospital births.	142	Stevenson [3]
	Direct: 37 sporadic cases in 1 387 000 living subjects.	13	
	Indirect: $\mu=\frac{1}{2}(1-f)x=\frac{1}{2}(1-0.09)\frac{39}{1\ 387\ 000}$ (Northern Ireland)	14	
Epiloia	*Direct:* Estimated frequency $\frac{1}{30\ 000}$, one quarter of the cases being sporadic. (South-East England)	8-12	Gunther & Penrose [4]
Retinoblastoma	*Direct:* 51 sporadic cases from an established number of about 1 500 000 births. (London)	17	Philip & Sorsby (unpublished data, 1947) [5]
Retinoblastoma	*Direct:* 49 sporadic cases in 1 054 985 births (State of Michigan, USA)	23	Falls & Neel.[6]
	Direct: 47 sporadic cases in 1 376 000 births (Germany)	17	Vogel.[7]
Waardenburg's syndrome (hair pigment, eye and hearing defects).	Based on proportion of cases observed in deaf mutes, an estimate of "penetrance" and the frequency of deaf mutism (Netherlands)	(4) 4	Waardenburg.[8]
Multiple polyposis of colon.	Based on frequency of condition at autopsy and proportion of cancer of colon autopsies showing some polyposis (State of Michigan, USA)	13	Reed & Neel.[9]
Dystrophia myotonica	$\mu=\frac{1}{2}(1-f)x=\frac{1}{2}(1-\frac{1}{2})\frac{33}{1\ 370\ 921}$ (Northern Ireland)	8	Lynas.[10]
Marphan's syndrome	$\mu=\frac{1}{2}(1-f)x=\frac{1}{2}(1-\frac{1}{2})\frac{36}{1\ 370\ 921}$	5	Lynas (unpublished data, 1956)
Aniridia	28 sporadic cases (1875-1944) and 13 isolated cases in 1944 in population of 3,844,000. Estimated frequency $\frac{1}{200,000}$.	5	Møllenbach.[11]

[1] Mørch, E. T. (1941), Op. dom. Biol. hered. hum. (Kbh.), *3*.
[2] Böök, J. A. (1952), J. Genet. Hum. *1*, 24.
[3] Stevenson, A. C. (1956) (in press).
[4] Gunther, M., and Penrose, L. S. (1935), J. Genet. 31, 413.
[5] Based on data of Griffith and Sorsby (Griffith, A. D., and Sorsby, A. (1944), Brit. J. Ophthal. *28*, 279).
[6] Falls, H. F., and Neel, J. V. (1951), Arch. Opthal. (Chicago), 46, 367.
[7] Vogel, F. (1954), Z. KonstLehre, *32*, 308.
[8] Waardenburg, P. J. (1951), Amer. J. Hum. Genet. 3, 195.
[9] Reed, T. E., and Neel, J. V. (1955), Amer. J. Hum. Genet. 7, 236.
[10] Lynas, M. A. (1956), Ann. hum. Genet. *21* (in press).
[11] Møllenbach, C. J. (1947), Op. dom. Biol. hered. hum. (Kbh.), *15*.

TABLE II.—*Estimations of mutation rates of sex-linked recessive gene traits*

Trait	Basis of estimation of mutation rate	Estimated rate per million	Source
Haemophilia	Estimates frequency in London as between 35 and 175 per million births and relative fertility of affected male subjects as 0.25. (London)	50	Haldane.[1]
	$\mu=1/3(1-f)x=1/3(1-0.286)\times 1.33\times 10^{-4}$	32	Andreasson [2] modified by Haldane.[1]
	$\mu=1/3(1-f)x=1/3(1-0.333)\times\frac{3.163}{4\ 092\ 025}$ Based on data of Fonio [4] and Andreasson.[1] (Switzerland and Denmark)	27	Vogel.[3]
Duchenne-type muscular dystrophy.	18 cases in 67 000 male live-births $\mu=1/3(1-f)x=1/3\times1\times\frac{18}{67\ 000}$ (State of Utah, USA)	95	Stephens & Tyler.[4]
	36 cases in 162 488 male live-births $\mu=1/3(1-f)x=1/3\times1\times\frac{36}{162\ 448}$ (Northern Ireland)	74	Stevenson [5] also unpublished data 1956.
	16 cases in 138 403 male live-births $\mu=1/3(1-f)x=1/3\times1\times\frac{16}{138\ 403}$ (England)	39	Walton.[6]

[1] Haldane, J. B. S. (1935), J. Genet. *31*, 317.
[2] Andreasson, M. (1943), Op. dom. Biol. Hered. Hum. (Kbh.), 6.
[3] Vogel, F. (1955) Z. ges. Blutforsch. *1*, 91.
[4] Stephens, F. E., and Tyler, F. H. (1951) Amer. J. Hum. Genet. *3*, 111.
[5] Stevenson, A. C. (1955) Ann. hum Genet. *19*, 159.
[6] Walton, J. N. (1956), Ann. hum. Genet. *20*, 1.

ANNEX 10

SOME PROBLEMS IN THE ESTIMATION OF SPONTANEOUS MUTATION RATES IN ANIMALS AND MAN [1]

In view of the known species differences both in the genetic structure of populations and in the apparent genetic responses to irradiation, when considering the genetic impact of increased exposure to ionizing radiation we should prefer not to attempt to extrapolate from other species to man, but rather base our thinking entirely on human data. Unfortunately, as has already become abundantly clear, the necessary data on man are not yet at hand, nor is it likely that they will be for some time to come. Under the circumstances, our thinking must for the present be guided to a large extent by what we know about the genetics of other species.

Attempts to quantitate the effects of radiation on human populations have usually been based on five factors. These are:

(1) The spontaneous mutation rate/locus/generation.

(2) The induced mutation rate/locus/*r*.

(3) The total gene number.[2]

(4) The 'accumulation factor', i. e., the ratio of nominally recessive genes already present in the population to those arising spontaneously each generation through mutation.

(5) The manner in which selection operates on the total gene complex.

Although I was asked to speak on "extrapolation from animals to man: the problem", this is so very broad an assignment that rather than utter a few

[1] Submitted by Dr. J. V. Neel, Associate Geneticist, Heredity Clinic, Institute of Human Biology, University of Michigan, Ann Arbor, Mich., USA. The material presented in this paper is drawn in large part from Chapter XV of a forthcoming monograph by J. V. Neel and W. J. Schull, entitled "The Effect of Exposure to the Atomic Bombs on Pregnancy Termination in Hiroshima and Nagasaki", and is reproduced by permission of the publishers, the National Academy of Sciences-National Research Council (USA).

[2] The product of (1)x(3), or (2)x(3), is the rate of mutuation pergamete, spontaneous or induced, as the case may be. It is possible in suitably designed experiments to estimate this directly (cf. Muller, 1955), and so decrease the number of variables involved in the calculation.

generalities about each of the factors just mentioned, and how they are manipulated to give estimates of the quantitative risks of radiation, I would like to examine in some detail the present state of knowledge as regards just one of these. The factor to be singled out for special consideration is 'spontaneous mutation rates'. There is no deep significance in the choice of this topic, i. e., any one of the four other factors of importance to attempts at extrapolation might almost equally well have been chosen for detailed consideration. In what follows, attention will repeatedly be drawn to gaps in knowledge. This should in no way detract from the past accomplishments of investigators in the field, but is submitted in the belief that the primary purpose of this meeting is to discuss what remains to be done if we are to place in proper perspective the genetic risks of ionizing radiation to human populations.

Current thinking concerning the rate of mutation of mammalian genes is for obvious reasons strongly influenced by what is known concerning drosophila rates. No less pertinent, but more difficult to fit into our conceptual framework for man at present, is the extremely important data emerging from the study of other inframammalian forms, such as the recent work on bacteriophage of Benzer (1955). We will accordingly first consider briefly what seem to us to be some of the more pertinent data concerning drosophila. For methodological reasons it is customary to distinguish on the basis of their physiological effects between three categories of mutations, namely, those associated with visible effects, those associated with lethal effects, and those which express themselves through a reduction of viability in the absence of detectable somatic effects, the so-called semi-lethal mutations (1–10 per cent. viability) and deleterious mutations (over 10 but less than 100 per cent. viability). Terminology in this field leaves something to be desired. Thus, the 'deleterious' mutations must have an organic basis, so that many of them would be found on careful study to be also 'visibles'. By the same token, most 'visibles' are also 'deleterious'. Finally, the dividing line between 'lethals' and 'semilethals' may be altered by culture conditions. Be that as it may, the division into these three categories has an operational usefulness, as we shall now see.

Beginning with the pioneer attempts of Muller (1934; see also Kerkis, 1935) and Timoféeff-Ressovsky (1935) a number of efforts have been made to establish the relative frequencies with which these types of mutations are represented among all mutations. These attempts have involved radiation-induced rather than spontaneous mutations because of the much more laborious nature of the problem if attacked through the study of spontaneously occurring mutations. In view of the possibility that the relative frequency of lethals is higher among the radiation-induced mutations because of the increased proportion of minute deletions, the estimate of the ratio, (semi-lethals + deleterious)/lethals, may be a minimum estimate. Muller (1954; see also Falk, 1955) places this ratio at 3–5 to 1. This same author goes on to state that ". . . the ratio may indeed be considerably higher than this, since the technique was hardly refined enough for the detection of detrimentals with a viability greater than some 85 per cent. of normal. Other studies have shown that 'invisible' mutants causing sterility or lowered fertility of some degree also form a very large group. This group, however, overlaps, to an extent not yet well investigated, that of the detrimental mutations (p. 396).

The significance of information concerning the relative frequency of mutants with viability in the 85–99 per cent. range in attempts to quantitate the genetic risks of ratiation is of course enormous. A related problem concerns the frequency of mutations for which the organism *at the time* is able to compensate completely, the undetectable mutations. Lately considerable attention has been directed towards the genetic basis and evolutionary implications of physiological homeostasis (refs. in Lerner, 1955). The possibility cannot be ruled out that the principle of homeostasis enables some organisms to compensate entirely, under particular sets of circumstances, for the effects of certain mutations.

It may be argued that there is no reason to be concerned about the relative frequency of mutants with undetectable effects in a consideration of the deleterious effects of radiation. However, these mutations are undetectable only under the conditions set by the observer. Under other conditions, set by nature and not by man, they might have decided effects. It is not at all difficult to argue that the mutants with over 85 per cent. viability which cannot now be studied in drosophila may in evolutionary importance far outweigh the visibles.

Muller (1950), in a discussion of the question of the numerical relationship between lethals, on the one hand, and semi-lethals and deleterious mutations, on the other hand, has made the following statement: "However, studies carried

on in drosophila during the past year by Meyer, Edmondson, and the writer indicate that in this organism the assumption of an equal distribution of detrimental mutations throughout all i_{ho} values[8] (when represented on an arithmetic scale) does not hold. Instead, it appears that, following the high but descending peak formed by complete lethals (i_{ho}=100 per cent.) and nearly complete lethals (i_{ho} = between 98 per cent. and 100 per cent), there is a marked drop in the frequency of mutations. The mutations studied were induced in an autosome (the second chromosome) by ultraviolet light acting on an interphase stage (in the polar cap). Along with 208 complete lethals there were 20 mutants found in the range of i_{ho} between 98 per cent. and 100 per cent., and again only 20 in the range of i_{ho} between 90 per cent. and and 98 per cent., although this range is four times as wide as the preceding one. If the rest of the distribution, as far as i_{ho}=10 per cent. had only the same frequency of mutations as in the range between 90 per cent. and 98 per cent. there would have been only 240 detrimentals in the entire interval between 100 per cent. and 10 per cent. to set against the 208 complete lethals found. But since we know from other work, previously cited, that the detrimentals in this interval are in reality several (about five) times as numerous as the complete lethals, it is evident that their frequency must, at lower degrees of detriment (lower i_{ho}), rise very much above that existing in the 90 per cent. to 98 per cent. range. The distribution of frequencies of i_{ho} therefore forms a bimodal curve with one peak at the left origin, lethality (i_{ho}=100 per cent.), and another peak somewhere to the right.

"Little more than this is yet known definitely about the shape of the curve in question, important though this genetic question is. However, there are grounds, both theoretical and observational, for regarding it as very unlikely that the second peak is near the first or that the rise towards it is sharp. Hence it is probable that detrimental mutations, instead of having an even distribution with respect to values of i_{ho}, form a curve which, except for its peak of near-lethals at the left end, is massively skewed towards the right, with its mean at a value of i_{ho} significantly beyond the middle (0.5)." (pp. 140–141).

If we consider these remarks of Muller in conjunction with the possibility of 'invisible' mutants discussed earlier, then the problem of estimating the relative frequency of lethal mutants versus those viable to some degree assumes new complexity. Fig. 1 attempts to present some of this complexity graphically. The abscissa of this figure represents viability of the homzygous genotype in some arbitrary environment. In this connexion, it is apparent that the term 'lethal' is relative, some lethal mutations having effects under no known circumstances compatible with life, other lethal mutations having far lesser effects. Likewise, the term 'normal' as applied to viability is relative, some normals being more normal than others, with the differences brought out only under unusual circumstances. Thus far, observations have been limited to the range of lethality and 1–85 per cent viability. As Muller has pointed out in the statement quoted above, there is great doubt concerning the shape of the curve of numerical relationships within this range. We have indicated two of the principal alternatives. Curve A assumes a mode at 60–70 per cent. viability, from which it would seem likely that the proportion of mutations in the 85–100 per cent. and normal viability range is small. Curve B assumes that the mode is farther to the right with the corollary that there is a considerable group of mutations not now being detected. How large that group is depends of course on the shape of the curve.

The question of the relative frequency of lethal mutations as contrasted to visibles, is on somewhat more secure footing than the question of the ratio of lethal mutations to mutations reducing viability to a lesser degree. In tabulating the results of radiation experiments by five different workers, Schultz (1936) found this ratio to be 7.4:1. In view of the well-recognized differences in the ability of individuals to recognize mutant phenotypes, the true ratio is probably somewhat lower. We refer, for instance, the ratio of 5.2:1 which obtained in the extensive and meticulous experiments of Spencer & Stern (1948). Even this ratio may be too high. Thus, in the control cultures, Spencer & Stern obtained a ratio of sex-linked lethals to visibles of 4.3:1 and in the irradiated cultures, a ratio of 5:3:1. In a study on spontaneous mutations in a 'high mutation rate' line, the ratio of sex-linked lethals to visibles was 3.6:1 (Neel, 1942). The ratio of visibles :lethals :semi-lethals and deleterious mutations may, as an approximation, be said to be someplace between 1:4:16 and 1:6:30, with, as noted above, the most uncertainty centring about the magnitude of the third figure in the ratio.

[8] i_{ho} = the amount of impairment produced by a gene when homozygous.

The important question of the mutation spectrum at individual loci remains in its early stages because of the amount of labour involved in securing reliable data. The effort involved in studying this problem through the use of spontaneously occurring mutations appears almost prohibitive. Attempts to study the problem using induced mutations again encounter the question of how precisely the mutational spectrum obtained with mutagenic agents parallels that derived from the study of spontaneous mutations. However, there is some preliminary evidence that the ratios just given may vary significantly from locus to locus. Thus, although there are many instances of lethal and visible mutations arising at the same locus, there are also some few cases in which a locus does not appear to be essential to life, in the sense that flies with a deficiency for this locus may live although they are of reduced viability (e. g., yellow and achaete, Muller, 1935). These loci, then, would not produce at least one type of lethal mutation. Finally, for methodological reasons, localizing 'deleterious' mutations to specific loci is extremely difficult, so that studies relating these to the loci producing lethals and visibles are in an early stage.

It should also be pointed out that the question of the total relative frequency of mutation at different loci is in a very unsettled state. Although there seems no doubt that the rate of recovery of mutations differs from locus to locus, care must be exercised in reasoning as to the magnitude of the true differences (cf. Neel & Schull, 1954). In the following discussion of mutation rates at specific loci, the fact that these are *selected* loci must constantly be borne in mind.

With respect to the rate of occurrence of spontaneous 'visible mutations' at specific loci in drosophila, data are available from five extensive series of observations. These are summarized in Table 1. Time does not permit us to give this table the detailed attention it deserves. In most of these studies, some 'special circumstance' occurred that requires at least very brief mention. Thus, Muller, Valencia, & Valencia (1950) observed in other experiments with the same strain used for their 'visible mutation' series that the rate of occurrence of sex-linked lethal mutations in this strain was 0.7 per cent., a rate some fourfold greater than usual. From this they argue that "the frequency of gene mutations at the nine loci would *ordinarily* average between 10^{-5} and 7×10^{-6} per locus in females" (p. 125). However, in view of the possibility that these 'high mutation rate' lines contribute significantly in nature to the total of spontaneous mutation (Ives, 1950; see also Neel, 1942), it seems appropriate simply to average this finding with the others. From the data of Glass & Ritterhoff (1956), it would appear that the mutation rates of males are higher than females, but this is scarcely substantiated by the difference in the findings of Alexander (1954) and Muller, Valencia, & Valencia (1950). Accordingly, we have simply averaged all the findings without regard to sex. No attempt has been made to take into account the effect on the observed results of possible differences in the age of the flies tested. The paper of Glass & Ritterhoff contains additional data on mutation rates emerging incidentally to their study—it has seemed preferable in this summary to utilize only data on loci 'pre-selected' for mutation rate estimates. It would appear that Muller et al., and Schalet, did not score as mutants flies with mutant phenotypes which were infertile, whereas Glass & Ritterhoff did. In any mutation rate study, a considerable proportion of apparent mutants prove sterile. Schalet encountered one 'cluster' of ten 'cut' mutants, presumably due to a mutation occurring in a spermatogonium. One can argue as to whether this should be scored as 1 or 10 mutations in the context of the present discussion. Finally, in all these studies where mutation gives rise to one mutant in a culture of wild type, there is the question, probably not so serious in studies on man, of the extent to which the less vigorous mutants are eliminated prior to the inspection of the culture, and the further problem of the human factor in recognizing the mutant.

While, then, it is possible to 'correct' the data of Table 1 in several ways, I have made no attempt to do so. In a total of 4 625 945 locus tests, at least 41 mutations were recovered, a rate of 0.9×10^{-5}. However, *this applies only to 'visible mutations', or to lethal and deleterious mutations which in combination with a mutant allele have visible effects.* If other lethals, semi-lethals, and deleterious mutations are arising at these same loci which are without detectable visible effects with the test-crosses employed, the mutation rate must be higher. There is no way at present to estimate the amount of mutation not detected with current specific locus techniques, but if, for instance, the ratio of visibles/

(undetected lethal+semi-lethal+deleterious+sterile mutations) at these loci were as high as 1:4, the mutation rate per locus becomes 4×10^{-5}. In other words, there are some grounds for feeling that the commonly quoted mutation rates for specific loci for drosophila are conservative. If, on the other hand, the mutation spectrum at specific loci is restricted, as some evidence suggests it to be in the sense that some loci give rise predominantly to 'visibles' and others to 'deleterious' mutations, then the 'true' mutation rate may be closer to the 1×10^{-5} which emerges from specific locus studies than the 4×10^{-5} just suggested.

Two other studies involving individual loci should be quoted. Lefevre (1955), in a paper which contains an excellent discussion of the problem of estimating spontaneous mutation rates, reports that the rate of appearance of mutants with visible or lethal effects at the *y* locus is "about 1 per 75,000" (p. 379). On the other hand, Bonnier & Lüning (1949), in a paper criticized by Muller (1954) because the rate of recovery of spontaneous mutations appeared to be too low in comparison with certain other findings, observed only one mutation at the white and forked loci among 153 579 flies tested for visible mutations, a rate of 3×10^{-5}. Again, both of these estimates do not take into account the semi-lethal and deleterious mutations which may not be detected by the techniques being employed.

Utilizing a somewhat different approach, Dobzhanzky, Spassky, & Spassky (1952) have estimated the average rate of mutation to lethals, semi-lethals (1–20 per cent. viability), and visibles *per lethal producing locus* in different species. These estimates, which they felt are more likely to be overestimates than underestimates, are reproduced in Table 2. Again, the estimate does not include the deleterious mutants.

In summary, then, it would appear that depending on one's view of the representativeness of the loci studied, and the problem of the relative frequency of mutations not detected by current techniques, there is room for a divergence of opinion concerning the average rate of mutation of drosophila genes, with the range of possibilities perhaps extending from 0.5×10^{-5} to 5×10^{-5}. In our opinion, even this range of estimates must be applied with great caution to human problems.

Turning now to mammals, we find that significant studies are available for only two species, the house mouse and man himself. The figures for the house mouse were derived in much the same fashion as the figures quoted for drosophila, namely, through a search for mutant individuals among animals simultaneously heterozygous at multiple loci. Russell (1954), in connexion with his important observations on radiation-induced mutations in the house mouse, has found that in his control material the rate of appearance of *visible* mutations in 265 076 locus tests was 0.8×10^{-5}. The observational error is of course large. Again it must be recognized that these tests very probably detect only a fraction of the mutations occurring at these loci.

There seems no reason to labour further the point that our knowledge of spontaneous mutation rates at specific loci is poor for any species. In some attempts to extrapolate from non-human material to man, the additional problem arises of the greater life span of man than of laboratory material, as well as the question of the type of species which man represents in terms of mutation rate. While I would be the first to defend the animal work as providing the best estimates available at the present time, it is my opinion that in the final analysis, we must have figures for man himself.

The available estimates for the frequency of occurrence in man of certain mutations with visible effects have already been summarized by Professor Stevenson and Professor Penrose. Many of the problems involved in estimating human mutation rates have been discussed by these two authors as well as previously (Haldane, 1948, 1949; Neel (1952) Neel & Schull, 1954; Nachtsheim, 1954) and will not be re-examined here. The average of the available estimates of the rate of mutation for the autosomal dominant and recessive sex-linked mutations thus far investigated in man is in the neighborhood of 2×10^{-5}/locus/generation. These estimates, now, are entirely limited to mutations with visible effects. Because of the nature of the design of observations on human mutation rates, it seems possible that a higher proportion of the mutations at the specific loci being studied is going undetected in man than in Drosophila. There is no way to estimate the magnitude of this difference at present, but a total mutation rate as high as 1×10^{-4} at these loci is a possibility. Although the

apparent correspondence between human and *Drosophila melanogaster* rates is noteworthy, because of the differences in the way the rates are obtained one must be cautious in the emphasis placed on the similarity. However, if the correspondence were indeed valid, this has interesting implications concerning the importance of 'aging' and failures in the 'copying process' at mitosis, and the role of background radiation, in spontaneous mutation rates.

The representativeness of these estimates for man has been repeatedly challenged. There can be no doubt that there is definite selection in the loci studied. How this influences our estimates is not at all clear. As we have pointed out elsewhere (Neel & Schull, 1954), mutation at any particular locus may be thought of in terms of these aspects: (1) the frequency of mutation at that locus; (2) the number of alternative forms of the gene which may occur at any locus, i. e., the number of multiple alleles; and (3) the ease with which the effect associated with each of these multiple alleles can be detected. We assume that some loci are more mutable than others because we detect the results of mutation more frequently at these loci. However, making allowance for 'unstable loci', the hypothesis has not been disproven that the inherent instability of all genes is, by virtue of their biochemical complexity, very similar, but that the results of mutation are more readily detected at some loci than at others because of the role of that particular locus in the animal's physiology. It is entirely conceivable that the loci[4] thus far selected for study in man are those at which a high proportion of all possible alleles at that locus results in readily detectable effects, but at which the per locus mutation rate is fairly representative of the human species.

For purposes of calculation, estimates of the rate of mutation of human genes have included 10^{-4} (Evans, 1949), 10^{-7} (Wright, 1950), and 2×10^{-5} (Muller, 1950; Slatis, 1955). In the current state of our knowledge, students of the problem can select and justify estimates differing from one another by a factor of 100.

If time permitted, we would do well to submit to the same kind of scrutiny our knowledge concerning the other factors that enter into quantitative treatments of the risks of irradiation, namely, (1) induced mutation rates at specific loci, (2) gene number (or, alternatively, gamete mutation rates), (3) the accumulation factor, and (4) the manner of action of selection. This will obviously be impossible. However, in closing I would like to say just a few words about the nature of selection in human populations. To begin with, there would seem to be little problem in extrapolating from animals to man, since there is practically nothing known concerning the detailed action of selection in animal populations on which to base an extrapoliation. For all our allegiance to the principal of natural selection, it is amazing how little we know of its actual detailed workings. True, it is easily demonstrated in experimental populations that grossly defective individuals seldom reproduce. But the problem of how the population as a whole maintains its fitness is virtually untouched. To mention only one important point, to what extent does the stability and adaptability of the species rest on the mechanism of balanced polymorphism, a mechanism not readily disturbed by an increase in mutation rates?

There is one final point I should like to emphasize. In our attempts to evaluate the genetic risks of increased radiation to the human species, I am a strong proponent of extensive animal experimentation. Through such work, possibilities can be explored which would either involve prohibitive amounts of time or be impossible for man. But when differences do appear between two animal species, as is already the case, only work on man will tell which of the species he resembles more closely. It is of tremendous significance to the practice of medicine and the development of atomic energy whether the 'permissible' population dose above background is 3 *r* or 30 *r* per generation. In reaching any final conclusions concerning permissible radiation doses in man, regardless of what is learned concerning other animal species, we must have accumulated far, far more data on man himself than are now available.

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[4] In point of accuracy, we do not know but what any particular mutation rate study in man is detecting mutation at several loci.

TABLE 1.—*Frequency of occurrence of spontaneous "visible mutations" in various species*

Author	Chromesome	Number of organisms	Number of loci	Total locus tests	Mutations	μ
Drosophila:						
Muller, Valencia, & Valencia, 1950.	X (females)	±60,000	9	540,000	15	2.8×10^{-5}
Altenburg, after Muller et al., 1950.	X (females)	±50,000	8	400,000	0	0
Alexander, 1954	III (males)	45,504	8	364,032	0	0
Glass & Ritterhoff, 1956	Multiple (females)	100,414	4	401,656	1	2.5×10^{-6}
	Multiple (males)	102,759	[1] 4	359,657	17	4.7×10^{-5}
Schalet, unpublished	X (males)	111,600	14	1,562,400	[2] 6	0.4×10^{-5}
	X (females)	71,300	14	998,200	2	0.2×10^{-5}
				4,625,945	41	0.9×10^{-5}
House mouse: Russell, 1954	Several	37,868	7	265,076	2	0.8×10^{-5}

[1] 1 sex-linked.
[2] One of these mutants appeared as a "cluster" of 10 flies. If one is concerned only with the rate of recovery of mutant phenotypes, then the entry here should be 15, and the average of the five studies quoted becomes 1.1×10^{-5}.

TABLE 2.—*Estimated average mutation rates per lethal-producing locus in several drosophila species, after Dobzhansky, Spassky, & Spassky (1952). These estimates are felt by the investigators to be more likely overestimates than underestimates*

Species	Second Chromosome	Third Chromosome
D. Melanogaster	1.1×10^{-5}	
D. pseudo-obscura		1.1×10^{-5}
D. willistoni	2.2×10^{-5}	3.0×10^{-5}
D. prosaltans	1.1×10^{-5}	2.1×10^{-5}

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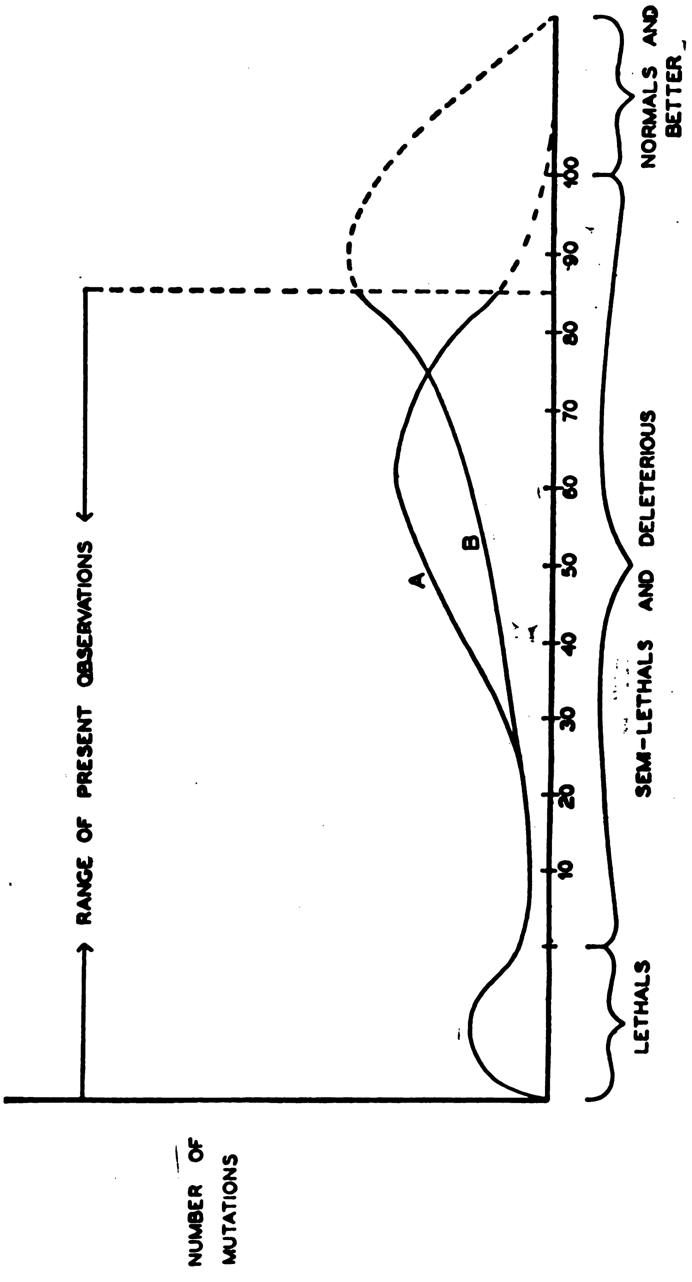

Figure 1

A schematic representation of two possible "mutation spectra" with reference to the degree of viability of the mutant, both compatible with the present data.

ANNEX 11

EFFECT OF INBREEDING LEVELS OF POPULATIONS OF INCIDENCE OF HEREDITARY TRAITS DUE TO INDUCED RECESSIVE MUTATIONS [1]

It is well known that the incidence of hereditary traits in populations depends not only on the respective gene frequencies, but also on the breeding structure of the populations. One of the important aspects of the prevailing mating pattern can be measured through the use of the coefficient of inbreeding, a population parameter practically impossible to evaluate in natural populations of animal species, but very easily determined in man.

INBREEDING IN BRAZIL

During the past six years, the writer has become very interested in this particular problem in human genetics and has tried to discover the inbreeding levels under which the present-day Brazilian populations live, as well as the magnitude of the same parameter during the last 150 years. The study of this problem in Brazil is greatly facilitated by the fact that much data can be obtained through the analysis of Catholic marriage records, a source of information which is available for the great majority of the population. The calculation of the coefficients of inbreeding has been done on the basis of data on the frequencies of marriages between uncles and nieces, aunts and nephews, first cousins, first cousins once removed, and second cousins. A few of the results obtained have already been published (Freire-Maia [2,3,4]), but the great bulk of the data will be presented in a paper now in preparation.

The following features of the breeding pattern of Brazilian populations have become apparent:

1. The degrees of inbreeding vary greatly in different regions: the rates are relatively low in the south and in parts of the east (a mean level of about 1% of first-cousin marriages has been found); are very high in large regions of the east and north-east (a level as high as 10% for first-cousin marriages has been detected); and are intermediate, but highly variable, in other regions.
2. There is, in general, a clear trend towards decreasing inbreeding rates, although a few reversals of this trend have been noted.
3. Geographic inbreeding gradients have been detected in some zones, with increasing inbreeding from the coast to the hinterland.
4. Although some of the Brazilian rates of inbreeding are higher than the highest so far found in other countries, the mean Brazilian coefficient of inbreeding (0.002) is relatively low because about one-third of the population lives at the level of 1% of first-cousin marriages.

An analysis of the factors probably responsible for the different degrees of inbreeding found in Brazil revealed that cultural pattern, economic level, migration, population density and degree of ruralization seem to be the most important.

EFFECT OF INBREEDING ON POPULATION STRUCTURE

It is known from theoretical analysis that inbreeding coefficients as high as 0.01 and 0.02, such as are found in some localities, produce negligible effects on common recessive traits, but have a considerable influence on the rare ones (Table I). Suffice it to say, for instance, that characteristics with a gene frequency of 1% will, under the action of a coefficient of inbreeding of 0.01 (which prevails in large populations in Brazil), have phenotype frequencies 99% greater than those expected in a model assuming no inbreeding (Table I) and 90% greater than those expected in populations with a coefficient of inbreeding of 0.001 (Table II). These almost 100% increases could hardly be detected in a direct way, through the analysis of the phenotype distribution in the populations, but may be appreciated through the incidence of the so-called "recessive" diseases in the offspring of consanguineous marriages, as well as through the incidence of consanguineous marriages among the parents of people presenting the same kind of genetic traits. Some data obtained on deaf-mutism show, for instance, a really very high inbreeding effect. In a population where the frequency of

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[1] Submitted by Dr. N. Freire-Maia, Director, Laboratory of Genetics, University of Paraná, Curitiba, Paraná, Brazil.
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[3] Freire-Maia, N. (1953) Ciencia (Méx.), *1*, 26.
[4] Freire-Maia, N. (1954) Carologia, *6*, Suppl., p. 923.

first-cousin marriages has been estimated as 3.5%, there has been shown to be about 21% of first-cousin marriages among the parents of deaf-mute children (Aguiar & Freire-Maia[5] and unpublished data). Inasmuch as some of these children undoubtedly owe their defect to extrinsic factors, the frequency of consanguinity among the parents of children with genetically determined defect is of course even higher. This problem will, however, be discussed in detail elsewhere.

EFFECT OF INBREEDING UNDER INCREASED MUTATION RATE

In recent times one of the most important human genetics problems is the evaluation, on a quantitative basis, of the effects of increasing radiation levels on the genetic composition of populations. Unfortunately, no accurate mathematical treatment could be given to this subject up to the present, as no precise information has yet been collected on some basis phenomena—namely, the spontaneous mutation rates, the induced mutation rate per gene per roentgen, the total number of loci, etc. (see discussion in Neel & Schull[6]). Thus the treat ment to be presented below is *not* intended to show what happens under increasing radiation, but what could happen under increasing inbreeding. The emphasis will be put *not* on the rates of mutation frequency increment but rather on the fact that, assuming a given increment, the action of different inbreeding rates will produce different quantitative effects.

For instance, assuming that the probability of induced mutation per gene per roentgen in man is of the same order (2.5×10^{-7}) as that found in the mouse (Russell[7]), doses of 100 r would increase this probability to 2.5×10^{-5}. Thus, five different populations (A, B, C, D, and E), differing only in the intensity of inbreeding, with two given recessive genes at frequencies of 0.007 and 0.003, will have the frequencies of these genes increased respectively to 0.007025 and 0.003025. However, the increase of the frequencies of the recessive genotypes will depend on the inbreeding level of each population (Table III). Five inbreeding coefficients have been chosen for comparison: 0.001, 0.003, 0.006, 0.009 and 0.011. The first one, representative of European populations according to Haldane,[8] also holds true for southern Brazil; the third has been assumed to characterize the highly inbred Japanese populations as a whole (Neel et al.[9]); the fourth is the highest detected Brazilian coefficient for a large zone (the centre of the north-eastern region); the fifth is probably the coefficient now prevalent in the populations of this particular zone; and the second has been selected to represent an intermediate step between the "low" European level (0.001) and the "high" Japanese one (0.006). Table III shows how much different coefficients of inbreeding may change the phenotype composition of populations subject to the same radiation impact. With the initial frequency of 0.003, it is seen that the Brazilian coefficients 0.009 and 0.011 produce total increments (0.01125 and 0.01275) more than twice as great as that produced by the European and southern Brazilian coefficient of 0.001. With the initial frequency of 0.007, the effect is a little smaller. The action of the Japanese mean coefficient is somewhat intermediate, as expected. Other things being equal, then, it is to be expected that induced recessive mutations manifest their effects with much higher frequencies in populations with inbreeding rates like those found in some Brazilian regions than in some European or North American populations.

PROSPECTIVE GENETIC RESEARCH ON INBREEDING

Now that we possess the basic information on inbreeding rates in Brazil, it has been possible to discover some "modern" populations living at inbreeding levels probably comparable to those of European communities in the Middle Ages. In the focus of the highest inbreeding levels in Brazil, for instance, some localities have been found where as many as 1 out of 6 and 1 out of 5 of the marriages are contracted between first cousins, and 1 out of every 3 marriages is consanguineous up to and including second cousins. This situation would seem

[5] Aguiar, W. C., and Freire-Maia, N. (1953) Ciênc. e Cult. *5*, 203.
[6] Neel, J. V., and Schull, W. J. (1956) The effect of exposure to the atomic bombs on pregnancy termination in Hiroshima and Nagasaki (in press).
[7] Russell, W. L. (1951) Cold Spr. Harb. Symp. quant. Biol. *16*, 327.
[8] Haldane, J. B. S. (1947) Ann. Eugen. (Camb.), *14*, 35.
[9] Neel, J. V. et al. (1949) Amer. J. hum. Genet. *1*, 156.

to be of great potential usefulness in the study of the general effect of consanguineous marriages on the genetic make-up of populations and the discovery of the mean number of deleterious recessive genes per individual. Unfortunately, in the zones in Brazil where the very high inbreeding rates prevail, no analysis regarding the incidence of specific hereditary anomalies seems feasible because the level of medical practice there is lower than in the larger cities. Nevertheless, in these regions a study can be made of such population characteristics as the frequencies of abortion, miscarriage, stillbirth, infant mortality, malformations as a whole, etc., and should afford some interesting results. Furthermore, in Rio de Janeiro, Sao Paulo, and some other cities, where the inbreeding rates are probably from ten to twenty times higher than those prevailing in similar or even smaller cities in the USA (Glass;[10] Herndon & Kerley;[11] Steinberg (personal communication, 1956); Woolf et al.[12]), and where very good hospitals exist, a complete and detailed analysis is possible.

ACKNOWLEDGMENT

The writer is indebted to Dr J. V. Neel and Dr J. N. Spuhler for discussions in connexion with the preparation of this manuscript.

TABLE I.—*Effect of two coefficients of inbreeding (0.01 and 0.02) on incidence of recessive traits with different gene frequencies (q)*

q%	q^2%	$\alpha=0.01$			$\alpha=0.02$		
		αpq%	$q^2+\alpha q$(%)	$\frac{\alpha pq(\%)}{q^2}$	αpq%	$q^2+\alpha pq$(%)	$\frac{\alpha pq(\%)}{q^2}$
50	25	0.25	25.25	1	0.5	25.5	2
20	4	0.16	4.16	4	0.3	4.32	8
10	1	0.09	1.09	9	0.18	1.18	18
5	0.25	0.0475	0.2975	19	0.095	0.345	38
1	0.01	0.0099	0.0199	99	0.0198	0.0298	198
0.5	0.0025	0.004975	0.007475	199	0.00995	0.01245	398
0.1	0.0001	0.000999	0.001099	999	0.001998	0.002098	1 998

TABLE II.—*Effect of two coefficients of inbreeding (0.001 and 0.01) on incidence of recessive traits due to genes with frequency of 1%*

Incidence of the traits		Increment
$\alpha=0.001$	$\alpha^1=0.001$	$\frac{\alpha^1 pq}{q^2+\alpha pq}$
0.01099%	0.0199%	90%

[10] Glass, B. (1950) Cold Spr. Harb. Symp. quant. Biol. *15*, 22.
[11] Herndon, C. N., and Kerley, E. R. (1952) Cousin marriage rates in Western North Carolina (Paper presented at the annual meeting of the American Society of Human Geneticists, Ithaca, N. Y.; unpublished).
[12] Woolf, C. M. et al. (1956) An investigation on the frequency of consanguineous marriages among the Mormons and their relatives in the United States. (In press.)

TABLE III.—*Effect of inbreeding level of populations on frequency of recessive traits under increased mutation pressure, according to Haldane's formulae* [1,2]

Population	Coefficient of inbreeding (α)	Initial frequency of recessives $q^2+\alpha q(1-q)$ (A)	Increment of recessives $\Delta q(\alpha+2q)$ (B)	Increased frequency of recessives (A+B)	"Total" increment [3] (Bx30 000)
$q=0.003$ $q+\Delta q=0.003025$					
A	0.001	0.000011991	0.000000175	0.000012166	0.00525
B	0.003	0.000014973	0.000000225	0.000015198	0.00675
C	0.006	0.000026946	0.000000300	0.000027246	0.00900
D	0.009	0.000035916	0.000000375	0.000036291	0.01125
E	0.011	0.000041901	0.000000425	0.000042326	0.01275
$q=0.007$ $q+\Delta q=0.007025$					
A	0.001	0.000055951	0.000000375	0.000056326	0.01125
B	0.003	0.000069853	0.000000425	0.000070278	0.01275
C	0.006	0.000090706	0.000000500	0.000091206	0.01500
D	0.009	0.000111559	0.000000575	0.000112134	0.01725
E	0.011	0.000125461	0 000000625	0.000126086	0.01875

[1] Haldane, J. B. S. (1947), Ann. Eugen. (Camb.), 14, 35.
[2] For details, see Neel et al., footnote 9 in text.
[3] Assuming an identical behaviour of 30 000 loci in gametes (Spuhler, J. N. (1948) Science, 108, 279).

ANNEX 12

DETECTION OF GENETIC TRENDS IN PUBLIC HEALTH [1]

To assess the practical genetic consequences of irradiating human populations, one must either: (a) extrapolate from mutation-rate studies in exposed animals, or men, to the effects of the additional mutations on human health and fitness; (b) extrapolate from fitness studies in animals to health and fitness in man; or (c) measure the important changes in the genetic component of health and fitness directly in the human populations which are exposed to a rising background of radiation.

The first two of these approaches have received considerable emphasis because the experimental procedures are relatively straightforward, and because predictions are needed, however tentative they may be. Unfortunately, however, it is extremely difficult to extrapolate from increases in mutation rates to the magnitude of the resulting increases in amount of general ill health, or from the fitness of animal populations to the fitness of human populations. In fact, there is reason to doubt whether the extent of the effect of a given increase in background radiation can ever be adequately anticipated.

The logical complement to prediction lies in the development of some sensitive means of detecting important genetic trends before they have gone too far. Ultimately, of course, this detection is the only way our predictions can be tested.

The main deterrents to setting up a continuing survey aimed specifically at the detection of important long-term genetic trends are the absence of any certainty as to how sensitive a method can be devised, and the very considerable financial and organizational difficulties. However, since we lack the assurance that "prediction" alone will fill our needs, it seems important that the feasibility of "early detection" receive much fuller consideration than it has in the past.

The present account deals with the application of certain of the methods of vital statistics to such detection. It seems essential to make it quite clear that the suggestions which follow are not at present recommended lines of action for general adoption by central vital statistical or record departments. These suggestions have been made earlier with Canada, solely as personal recommendations, with a view to studying their feasibility. As presented here, however, the remarks are designed simply to show the possibility of collecting specific data on human variation which could not possibly be assembled on a comparable scale by the conventional *ad hoc* field inquires used in population genetics. The methodology is discussed in this paper in the hope of getting constructive criticism before we embark on research on such a large scale.

[1] Submitted by Dr. H. B. Newcombe, Biology Branch, Atomic Energy of Canada Limited, Chalk River, Ont., Canada.

Sources of Information

In general, to discriminate between genetic and environmental causes (either in genetic conditions of individuals or in population trends) information is needed concerning the number of affected and unaffected individuals, their family relationships, and the environments to which they have been exposed. Considerable information of all three kinds exists in the routine registrations of births, deaths and marriages. The handling procedures are not at present designed to discriminate between the genetic and environmental contributions to the diseases which are reported on the death registrations, but if maximum use were made of all three kinds of information we could presumably make such a distinction, with at least some degree of success.

Since routine vital statistics are a recognized measure of the health of a population, one approach to the detection of genetic trends would seem to lie in supplementing the basic information where necessary, and in designing handling procedures to distinguish the genetic from the environmental causes of ill health.

The approach is limited at present with regard to the health information, which relates solely to causes of death and still-birth, but other routine sources could be tapped. For example, one Canadian province (British Columbia) has made use of the "Physician's Notices of Births and Stillbirths", which are quite separate from the birth registrations, to obtain details of still-births and congenital abnormalities. The problem of adequate ascertainment is undoubtedly soluble.

The chief advantage in the use of registrations of births, deaths and marriages, however, is that these contain, in raw form, the most reliable and complete information on the family relationships of the individuals who make up the population. They are in essence a family tree on a very large scale (complete with marriage dates, birth dates, and the dates of all deaths). To extract this family information manually, and to convert it into a usable form, would be a prodigious task, and for this reason we have concentrated much of our thinking on the development of mechanical procedures involving the matching and sorting of punch cards to form a "Family Register Index".

A "Family Register" would contain cards for all marriages, starting from a given year. To these would be added the cards for all births arising out of these marriages, which would be identified and sorted into their respective sibling groups. In addition, all cards for still-births and for the deaths of offspring would be similarly identified and sorted. A further procedure has been devised whereby any marriages between first cousins could be identified, together with the births and deaths of their offspring, without resorting to interviews and without reference to any other kind of record (see Fig. 1 and 2).

Thus, for each disease condition on which information is available, it would be possible to determine the incidence within three groups of individuals: the population as a whole, the offspring of first-cousin marriages, and the siblings of affected individuals. This seemed the most suitable use to make of the family relationship data in an initial study, but the information could be applied in many other ways.

In addition, the registrations contain a considerable amount of routinely recorded information on environment, which would permit a breakdown of any data by the following factors: rural or urban residence; socio-economic class, as derived from father's occupation; age of mother and of father at time of birth; family size and spacing; racial origin; gestation period; legitimacy or illegitimacy; and home *versus* institution birth. This is probably adequate for any initial study, and supplementary information could undoubtedly be obtained.

The "Family Register Index" is the one unique feature of the present proposals. In emphasizing the mechanical methods involved, it should be explained that it was felt that the personal-interview technique of obtaining pedigrees would become too laborious in any study involving both a large population and many of the common diseases. And yet, if the common diseases were not included, or if the study were limited to a few genetically simple "indicator conditions", it would be difficult to relate any trend observed to the general health of the population. And the latter is, of course, our ultimate practical concern.

In making these proposals it has been assumed that the present problem of genetic damage from radiation, and the related problem of the operation of other causes in the production of genetic trends, are of sufficient importance to justify any appropriate changes in the collection and analysis of statistics relating to the health of the whole population. Our prime concern is with broad categories

of ill-health, and methods of obtaining and handling information on the very large numbers of affected individuals and their relatives need to be developed.

Rationale

In general, the greater the complexity of the genetic and the environmental causes of a condition, the more information of the three kinds (i. e., pertaining to health, family and environment) will be required to disentangle the two.

Thus, to look for a trend in the frequency of a simple dominant "indicator" condition, it would only be necessary to observe the proportions of affected individuals in the population. And to detect trends involving a simple recessive gene it would be sufficient to know the proportions of affected individuals in offspring from consanguineous parents. However, if one extends the survey to conditions arising from a recessive gene with incomplete penetrance, it would be necessary, in addition, to know the corresponding proportion of affected individuals in the rest of the population. In this case the gene frequency would be calculated from the ratio of the two, referred to as a "K" value; and since penetrance affects both components of the ratio equally, both "K" and the calculated gene frequency would be essentially independent of penetrance.

The number of diseases can, of course, be extended still further to include those due to dominant genes of unknown penetrance and those due to multiple additive genes, using comparisons between siblings (or other closely related individuals). Formulae for the estimation of gene frequencies from data of this kind (using the ratio of the incidence in close relatives of affected individuals to that in the population as a whole, i. e., a "K" value) have been derived by Penrose[1] and applied to a number of common diseases.

Where our main interest is in the detection of changes in the gene frequencies, rather than in the absolute frequencies, the problem is considerably simplified. Attention centres on trends in the values of "K", and it is not essential to know whether the genes are recessive, dominant, or multiple additive.

Thus, a basic requirement for discriminating between the genetic and the environmental trends affecting public health is a knowledge of the proportions of affected individuals within three groups of people: the offspring of consanguineous unions, the close relatives of affected individuals, and the population from which these were drawn.

Environmental changes, when they affect penetrance uniformly throughout the population, are unlikely to produce spurious trends in the estimates of gene frequencies. This is true also of changes affecting the extent of the ascertainment, and of changes in diagnostic fashion, when they occur uniformly throughout the population. However, there are a number of sources of error, and additional information would be needed in order to detect and evaluate them. Such information would relate mainly to the environment.

Gene frequencies based on consanguinity data would be least subject to bias, the main source of which is the fact that marriages between close relatives tend to be more common in certain sectors of the population—notably, the rural groups. To eliminate errors from this source it would be necessary to obtain independent values of "K" from the various population groups (e. g., breaking the data down by: rural or urban residence; racial origin; socio-economic class; etc.) or, better still, from comparisons with offspring from the brothers and sisters of the individuals who married their cousins.

Gene frequencies based on "K" values for sibling comparisons may be biased in a number of ways. In general, where there are family-to-family differences in environment affecting the expression or penetrance of an hereditary disease, the increased tendency for affected individuals to appear within particular families will increase the value of "K" and bias the estimate of gene frequency downwards. Environmental heterogeneities which might give rise to this kind of bias could be associated with: (a) maternal effects due to the mother's hereditary constitution; (b) maternal effects due to the environment to which the mother has been exposed; and (c) effects due to the child's post-natal environment. It should be possible to detect any bias from these sources, and to estimate its magnitude.

Thus, where there are maternal effects due to the mother's heredity, these should make for a closer resemblance between the children and any of their first cousins by their mother's sisters, than with first cousins of the other three kinds (i. e., offspring of the mother's brothers, of the father's sisters, or of the father's

[1] Penrose, L. S. (1953) Acta genet. (Basel), 4, 257.

brothers). The extent of the discrepancy (allowance having been made for differences in the likelihood of inheriting similar X-chromosomes) should indicate the magnitude of the bias. Important environmental variables other than inherited maternal effects should be strongly correlated with the incidence of the condition in a suitable breakdown by environmental groups.

Environmental differences may operate by altering either the expression of a genetic condition (i. e., the "penetrance" or "expressivity") or the production of non-genetic effects which simulate the genetic condition (i. e., "mimics" or "phenocopies"). Variations in environment might tend to group the affected individuals into families by either mechanism, thus increasing the value of "K" and causing the gene frequency to be underestimated. But variations in the production of mimics could operate in the opposite manner through obscuring the grouping due to genetic causes. Both environmental influences will be observed in an appropriate breakdown of the data, as a correlation between environmental group and incidence of affected individuals.

The two effects will in many cases be distinguishable, however, by observing the "K" values for appropriate environmental groups. Where the influence is on penetrance, the value of "K" for any homogeneous group will tend to be less than that for the mixed population (and the estimates of gene frequency will be less biased). Where the influence is on mimic production, the value of "K" for a genetic condition would tend to be increased in homogeneous favourable groups where mimics are rare, and decreased in the unfavourable groups where they are common. In either case, the most reliable estimates of gene frequency would be obtained from groups living in the most uniformly favourable environments.

Such refinements, using information which is already collected as a matter of routine, would remove many of the sources of bias. Further, since genetic applications were not envisaged in the planning of the present system of vital statistics collection, improvements could undoubtedly be devised after any major attempt to apply the existing information. It is, of course, impossible to predict just how sensitive a means for the detection of genetic trends might eventually be developed; this can only be done as experience is gained in using the information which we already have in a readily available form.

In case the present proposals seem over-optimistic, it is worth noting that at least one serious attempt has already been made to detect a genetic trend in a complex quantitative character (namely, intelligence[8]) which is known to be subject to environmental influences, and that this attempt has made very little use of information on family relationships and environment, and of the refinements which these permit.

DETAILS OF FACILITIES AND PROCEDURES

Microfilms of the registration forms for all births, deaths and marriages occurring in Canada are kept centrally at the Bureau of Statistics. From each of these microfilms, a punch-card, bearing a non-repetitive serial number and containing particulars of the event and of the individuals involved, is prepared as a matter of routine. At present a modification in the punching of these cards is under consideration. The modification is designed to enable each birth card to be identified mechanically with the marriage card of the parents, matching by name of father, maiden name of mother, and parents' initials and birth years; while the death cards would in a similar manner be identified with the individual's birth cards, matching by family name, first name and initials, province and date of birth. A small proportion of apparent discrepancies are known to arise, almost all of which could be matched manually.

The new birth and death cards would have additional blank spaces into which could be transferred the serial number from the corresponding marriage card. This operation would be mechanical, and the serial number from the marriage card would then become a "family number", enabling all three cards to be readily identified into family groups.

The change in the method of punching would not add appreciably to the present costs, which are in the vicinity of $100,000 per year, while the additional matching procedures and the punching of the family number might perhaps double these costs. This estimate refers to a proposed ten-year pilot study, but in a continuing study the handling of an expanding file of cards would involve a further increase in cost which has not as yet been estimated.

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[8] Scottish Council for Research in Education (1949) Report of the * * *, London.

In addition, to identify all marriages between first counsins, the microfilms of the marriage registration forms would be scrutinized, and those in which one of the bride's parents had the same family name as one of the groom's parents would be singled out. In the case of these registrations (which amount to about 1%–2% of all marriages in Canada), the birth records of the bride and groom, and then of the respective parents of similar family name, would be checked for positive identification of the marriages which are in fact between first counsins. One man can scan approximately 1000 marriage registrations a day for this purpose, and searching of birth records is a function which the provinces carry out routinely at a relatively small cost.

When a Family Register Index is created, it would be used in conjunction with an Ill-health Register of all "affected" persons, who will be identifiable by their names and by the dates and places of their births, if they have not already been identified by the "family number". The latter register would include stillbirths, infant deaths, other deaths, congenital malformations, hospital records, other medical records, etc. From the two registers one would derive the sizes of the sibling groups and the numbers of affected individuals in each. Weinberg's propositus method would be used to calculate the probability that a sibling of an affected individual will be similarly affected: $p=\Sigma x(x-1)/\Sigma x(s-1)$, where p is the required probability, x the number of affected persons in the families, and s the number of children of the individual families. The incidence of the condition in offspring of first-counsin marriages, and in the population as a whole, would be obtained directly.

The Family Register Index can be thought of as a major research tool, designed to do away with the need for obtaining pedigrees by personal interview and thus to pave the way for whole-population studies of common diseases. Such studies would seem to be an integral part of any attempt to measure the practical consequences of genetic trends in terms of the general health of the population.

Details of Suggested 10-year Pilot Study

It has been proposed that before embarking on a major continuing programme of an entirely new kind, the design should be tested in a preliminary special study. In the present case there is an additional reason for such a study.

The main programme, if started solely with current registrations of marriages, and of the births and deaths of the children arising out of these, would require approximately four years before any comparisons could be made in brother-sister groups, and about ten years before it would have expanded sufficiently to yield data for a breakdown by cause of death. Only then would it be possible to evaluate the design of the project.

To avoid loss of time a special study could be carried out, essentially similar to the projected continuing study, but using existing records on a "backlog" basis. In drawing up the specifications it was assumed that the special study would cover the ten-year period from 1946 to 1955 inclusive. An attempt has been made to foresee the amount and kind of data which might be expected from the special study. It is estimated that there would be in the vicinity of a million infants born to the marriages under study, and that approximately half of these would have at least one brother or sister with whom comparisons could be made.

The special study would deal mainly with infant deaths, and should indicate the extent to which deaths from various causes tend to be correlated in families and in the offspring of first-counsin marriages. These correlations (i e., the factors by which the various causes are more common in these two groups of individuals than in the population as a whole) are the values which will be expected to change with changes in gene frequencies. The study would show how large these factors are for the various causes of death, together with the confidence limits, and would at the same time indicate any changes which should perhaps be made in the design of the continuing study.

In addition, since the relatives of affected individuals constitute a group in which the frequencies of the predisposing genes are, as it were, artificially increased, the results should give us a better appreciation of the practical consequences of an increase in the incidence of deleterious genes in the population as a whole.

The extensive Family Register Index, developed during the special study, would be used in the continuing study so that current births from much earlier marriages could be included from the start. This would ensure an appreciable annual yield of data without having to wait until the marriages occurring in the first year of the study had yielded two children.

FUTURE FACILITIES AND THEIR POTENTIALITIES

With the present punch-card equipment a storage problem would eventually develop. The family Register Index must contain three cards for each individual who has been born, married and died, and family groups of cards will have to be retained until the last of the brothers and sisters has died. Probably the equilibrium number of cards would be in the vicinity of three for each living member of the population. The obvious solution is a system of miniature cards, capable of being mechanically sorted and matched. If, in addition, these cards had an increased information capacity and could be handled more rapidly, the usefulness of the Family Register Index would be enormously increased.

One such system—the Kodak Minicard, which has been described by Tyler, Myers & Kuipers [8]—is in the process of development. (Details: card size, 32 mm by 16 mm; storage space required, 15 inches by 30 inches by 50 inches per 2 000 000 cards; digital information capacity, between five and six times that of the standard punch-cards, with room for a photographic image of the original registration form as well; handling speeds of 1800 cards per minute for sorting and selecting.) Such a medium could replace both the existing microfilm and the punch-cards, while taking approximately the same space as the microfilm alone.

The most important use for the additional information capacity would be in the identification of relatives more distance than brothers and sisters. The "family identity number" assigned at the time of marriage would be carried forward, not only to the children's birth and death cards, but to the children's marriage cards as well, and so on. The number of steps in this carryover would of course depend ultimately on the amount of digital information space allocated to ancestor-family numbers.

Let us assume, for example, that two generations of ancestor-family numbers are present on all cards (requiring, for 10-digit numbers, 60 out of the 420 spaces which will be available when the card contains a photographic image as well). Causes of death could then be compared in children, parents and grandparents, and in other relatives as remote as second cousins, using a single sorting and matching of death cards.

Another medium which might have applications is magnetic tape. (Information capacity, 100 characters per inch; handling speed for tabulation and other purposes, 15,000 characters per second.) Records from cards which had been suitably sorted in advance could be incorporated each year, together with the accumulated records of previous years, into a single master-tape, which would be revised annually. Assuming that such a tape contained records from 50 000 000 cards with 100 characters per card, it would require approximately 100 hours (not counting the changing of reels) to run the entire tape through a machine in order to tabulate the information in the required form.

In view of the rapid improvement in the designs of such equipment, the bulk of information to be processed, and the complexity of the operations involved, should not constitute more than a temporary limitation on any system of handling which was deemed necessary.

CONCLUSION

In this account it has been assumed that we may not be able to assess adequately the genetic damage occurring in irradiated human populations, either from a knowledge of the changes in mutation rates, or by observing the changes in fitness in similarly irradiated animal populations, or even by observing the prevalence of a number of genetically well-defined "indicator conditions". With each of these observations there remains an uncertainty as to the amount by which the ill-health of the population has been altered, and if the answer cannot be stated in such terms it is of only limited use.

This would seem to lead us to the much more arduous and exacting task of attempting to detect changes in the genetic factors which affect broad categories of human ill-health. The degree of precision which might be achieved is impossible to predict, but it is clear that we could not afford to waste any of the available information relating to the health of the individuals who make up the population, to their family relationships, or to the environments in which they have been reared.

Experience in effectively handling the masses of information of these three kinds which are at present readily available would seem to be one of our imme-

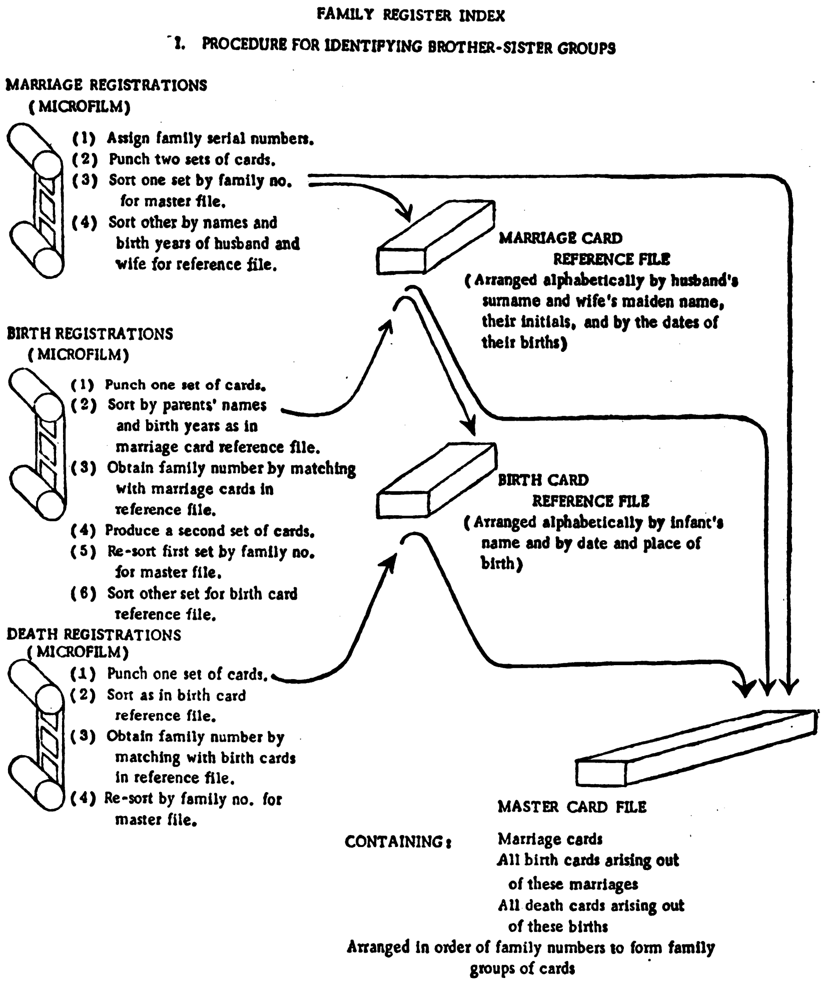
[8] Tyler, A. W., Myers, W. L., and Kuipers, J. W. (1955) Amer. Documentation, 6, 18.

diate needs, while improvement in the routine sources of information is another. It is with the first of these needs that the present paper has been primarily concerned.

ACKNOWLEDGMENTS

While the opinions expressed in this paper are entirely those of the writer, the procedures described are the outcome of numerous discussions with representatives of the Bureau of Statistics and the Department of National Health and Welfare, and with other geneticists. In the latter connexion, the writer would like to thank Mr Fraser Harris, Mr S. J. Axford, Mr Gordon H. Josie, Dr A. P. James and Dr F. Clarke Fraser for their generous co-operation.

FIG. 1. FAMILY REGISTER INDEX - 1



If both the family information and the vital statistics are included on a single card, tabulations from the Master Card File will yield family sizes and numbers of children "affected", from which sibling correlations can be derived. Note that with the use of a miniature card the storage space required for the Master Card File would be about the same as that needed for the microfilms of the corresponding registration forms.

FIG. 2. FAMILY REGISTER INDEX—II

FAMILY REGISTER INDEX

II. PROCEDURES FOR IDENTIFYING FIRST COUSIN MARRIAGES

A. Procedure to be used when family index is first started

(1) Examine all marriage registrations for cases in which a parent of the groom and a parent of the bride have the same surname (or maiden name).

(2) Where this is observed, search the birth records of the bride and groom for the birth years of the parents of similar name.

(3) Then, search the birth records of the parents of similar name. Where the bride and groom are first cousins the names of the common grandparents will be found on both registrations.

(4) Enter consanguinity on the marriage card in the family register index, and on all subsequent birth and death cards arising out of this marriage.

B. A more direct procedure using the family numbers assigned to the marriages in the two preceding generations (the method will be usable after the family register index has been in operation for about 40 years)

(1) Carry forward the following family serial numbers on to all new marriage cards:

(a) From the groom's parents' marriage
(b) From the bride's parents' marriage
(c) From the groom's paternal grandparents' marriage
(d) From the groom's maternal grandparents' marriage
(e) From the bride's paternal grandparents marriage
(f) From the bride's maternal grandparents' marriage

(2) Where the family number for (c) or (d) is the same as that for (e) or (f), the bride and groom are first cousins. Cards in which this is the case will be identified mechanically.

(3) Enter consanguinity on the marriage card, and on all subsequent birth and death cards arising out of the marriage.

The first of these procedures can be used immediately in the case of all provinces where the maiden names of the mothers of both bride and groom appear on the marriage registration form (i. e., all provinces in Canada except Quebec).

The second procedure will enable consanguinity data to be obtained from all of the provinces, after the Family Register Index has been in operation for a sufficient period.

APPENDIX 4

THE BIOLOGICAL EFFECTS OF ATOMIC RADIATION AND EXCERPTS FROM PATHOLOGIC EFFECTS OF ATOMIC RADIATION, STUDIES BY THE NATIONAL ACADEMY OF SCIENCES, NATIONAL RESEARCH COUNCIL

THE BIOLOGICAL EFFECTS OF ATOMIC RADIATION

SUMMARY REPORTS FROM A STUDY BY THE NATIONAL ACADEMY OF SCIENCES

National Academy of Sciences—National Research Council, Washington, 1956

FOREWORD

The reports published in this volume summarize the first technical findings and recommendations of six committees established to carry on a continuing study of the biological effects of atomic radiations from the points of view of

genetics, pathology, meteorology, oceanography and fisheries, agriculture and food supplies, and the disposal and dispersal of radioactive wastes.

The members of these committees, numbering more than 100, are among the most distinguished scientists in their fields in the United States. They have given generously of their time and talents in making this analysis during the past several months because they are convinced that their fellow citizens should have the facts about the biological effects of atomic radiations based on all existing knowledge available to us. The members of the committees served as individuals, contributing their knowledge and their judgment as scientists and as citizens, not as representatives of the institutions, companies, or Government agencies with which they are associated.

The use of atomic energy is perhaps one of the few major technological developments of the past 50 years in which careful consideration of the relationship of a new technology to the needs and welfare of human beings has kept pace with its development. Almost from the very beginning of the days of the Manhattan Project careful attention has been given to the biological and medical aspects of the subject. By contrast, the automobile revolutionized our pattern of living and working, but we are only now beginning to appreciate the problems of safety, urban congestion, nervous tension, and atmospheric pollution which have accompanied its development. In the same way, the development of the aircraft industry outran our knowledge of how to meet the environmental needs of the human beings it intended to transport through the skies.

The reports now completed vary greatly as to the extent of technical detail they contain. The full reports of each committee, including technical appendices where these have been prepared, will be published at a later date by the National Academy of Sciences. Here only the essential facts, arguments. and conclusions as seen today by each Committee are published. As further research provides new facts or further consideration sheds new light on what is now known these conclusions will almost certainly be modified. Moreover as time permits certain specialized aspects of the problem will be studies in more detail by the Committees. The results of these further analyses will be published from time to time as the National Academy of Sciences' study continues.

Douglas M. Whitaker, Vice President of the Rockefeller Institute, has provided coordination and liaison among the study committees with the assistance of Charles I. Campbell of the Academy staff. The study has been greatly assisted by consultations with many authorities in private and Government organizations. Particular mention should be made of the cooperation of the United States Atomic Energy Commission and the Department of Defense. Financial support of the Academy's study of the biological effects of atomic radiations is provided by the Rockefeller Foundation.

DETLEY W. BRONK,
President, National Academy of Sciences.

June 4, 1956.

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REPORT OF THE COMMITTEE ON GENETIC EFFECTS OF ATOMIC RADIATION

FOREWORD

The National Academy of Sciences, with the approval of the top Government authorities, is carrying out an over-all Study of the Biological Effects of Atomic Radiations. One part of that general study is being made by a Genetics Committee, and the present report is a preliminary one from that Committee.

This Genetics Committee has sixteen members, whose names and positions are listed at the beginning of this report. Thirteen of these have been directly and extensively concerned with research in genetics. This number includes specialists on the genetics of lower forms of life, on the genetics of such mammals as mice, on the more mathematical aspects of population genetics, and on human genetics. One member is specially experienced in the general biological effects of radiation, one in radiological physics, and one in pathology.

The problems of the Atomic Age affect every man, woman, and child—in fact, every living thing—in our country, and of course in the whole world as well. Although many of these problems are technical in character, it is nevertheless of importance to our democracy that these matters be as widely understood as possible. Therefore every effort has been made that this report be generally understandable.

This necessitates a certain amount of explanation of technical matters; but this report will use just as few unfamiliar terms as possible, and will define those that are used. It should be understood that many of the statements made in this report would require various qualifications and a lot more detail to attain full technical precision.

The subject is an inherently complicated one, and the reader must be prepared for a certain amount of detailed explanation, some of which is not easy to grasp.

It is felt that the subject is important enough so that many citizens will wish to make the effort which is necessary to a careful reading of this report.

The simplifications and abbreviations which have been adopted in this report in order to achieve a generally understandable presentation will undoubtedly be recognized by, and it is hoped will not disturb, the more technical reader. The later sections of the present report will be supplemented by more detail and factual justification if this is later desired by any of the agencies (as for example, the National Committee on Radiation Protection, the Atomic Energy Commission, governmental and industrial groups concerned with radiation hazards, etc.), which have responsibility for the procedures and standards to which our recommendations apply.

This particular report is preliminary for two reasons. First, we wish later to make a fuller report with more technical detail. Second, the situation is changing at such a rate that there should be a continuing series of reports, each bringing the subject up to date.

The National Academy study is not directed toward the problems posed by wartime use of atomic weapons, nor toward the political aspects of atomic power. The study is only indirectly concerned with the social and economic aspects. In fact, the National Academy study, as its title indicates, is concerned with the *possible biological hazards* due to atomic and other radiations. And the present report, made by the Genetics Committee, is concerned with the *genetic aspects* of the possible biological hazards. As this report is read, it should become progressively clearer what these genetic aspects are.

(I) WHAT ARE WE WORRIED ABOUT?

The coming of the Atomic Age has brought both hopes and fears. The hopes center largely around two aspects: the future availability of vast resources of energy; and the benefits to be gained in biology, medicine, agriculture, and other fields through application of the experimental techniques of atomic physics (isotopes, beams of high-energy particles, etc.).

Gains in both of these areas can be of great benefit to mankind. Advances in medicine and agriculture are obviously desirable. The wide availability of power can also be of great benefit, if we use this power wisely. For not only should there be enough power to meet the more obvious and mechanical demands, there should be enough to affect society in much more far-reaching and advantageous ways, so as to reduce world tensions by raising the economic standards of areas with more limited resources.

On the other hand, the Atomic Age also brings fears. The major fear is that of an unspeakably devastating atomic war. Along with this is another fear, minor as compared with total destruction but nevertheless with grave implications. When atomic bombs are tested, radioactive material is formed and released into the atmosphere, to be carried by the winds and eventually to settle down at distances which may be very great. Since it does finally settle down it has aptly been named "fall-out."

There has been much concern, and a good deal of rather loose public debate, about this fall-out and its possible dangers.

Are we harming ourselves; and are there genetic effects which will harm our children, and their descendants, through this radioactive dust that has been settling down on all of us? Are things going to be still worse when presently we have a lot of atomic power plants, more laboratories experimenting with atomic fission and fusion, and perhaps more and bigger weapons testing? Are there similar risks, due to other sources of radiation, but brought to our attention by these atomic risks?

(II) WHAT COMPLICATIONS ARE MET IN REACHING A DECISION?

Now it is a plain fact, which will be explained in some detail later in this report, that radiations,[1] penetrating the bodies of human beings, are genetically undesirable. Even very small amounts of radiation unquestionably have the power to injure the hereditary materials. Ought we take steps at once to reduce, or at least to limit, the amount of radiation which people receive?

There are two major difficulties that make it very hard to decide what is sensible to do. First, although the science of genetics is as precise and as

[1] Throughout this report the word "radiation" is not used in its broadest sense, but refers to certain kinds of high-energy radiations which are described in Section V.

advanced as any part of biology, it has in general, and particularly in human genetics, not yet advanced far enough so that it is possible to give at this time precise and definite answers to the questions: just *how* undesirable, *how* dangerous are the various levels of radiation; just *what* unfortunate results would occur?

Second, even if the relevant questions concerning radiation genetics could be answered definitively that would be only part of the story. The over-all judgment (how much radiation should we have?) involves a weighing of values and a balance of opposing aims in regard to some of which the techniques of physical and biological science offer little help.

What is involved is not an elimination of all risks, for that is impossible—it is a balance of opposed risks and of different sorts of benefits. And in disturbing and confusing thing is that mankind has to seek to balance the scale, *when the risk on neither side is completely visible.* The scientists cannot say with exact precision just what biological risks are involved in various levels and sorts of radiation exposure (these considerations being on one pan of the risk-scale); nor can anyone precisely evaluate the over-all considerations of national economic strength, of defense, and of international relations (all on the other pan of the scale).

(III) MUST WE THEN MOVE ENTIRELY IN THE DARK?

Does this mean that geneticists have, at the moment, nothing useful to say on this grave subject? Fortunately, this is not the case. We do know something, though not nearly enough to give definite answers to a great many important questions. There is a considerable margin of uncertainty about much of this, and as a result, there are naturally some differences of opinion among geneticists themselves as to exact numerical values, *although no disagreement as to fundamental conclusions.*

Many people, moreover, suppose science to be definite—open or shut. Things are supposed to be so or not so. And, therefore, some persons may, quite mistakenly, conclude that geneticists are unscientific because they do not completely agree on all details.

In relatively simple fields, where both theory and experiment have progressed far, a comforting kind of precision does often obtain. But it is characteristic of the present state of human radiation genetics that one must carefully and painstakingly note a lot of qualifications, of special and sometimes very technical conditions, of cautious reservations. The public should recognize that the attitudes and statements of geneticists about this problem of radiation damage have resulted from deep concern and from attempts to exercise due caution in a situation that is in essence complicaed and is of such great social importance.

It is not surprising that our knowledge of genetics—and especially human radiation genetics—is so fragmentary. What goes on inside cells and the effects of radiations on these processes are extremely complicated and subtle problems. To attack them successfully requires a tremendous lot of time; for the inherent variability of certain of these effects is such that to establish something with certainty one must do not one experiment but many thousands of individual tests and observations. To attack these problems also requires a high degree of special skill—and perhaps most of all, imaginative ideas which can be tested.

Single-celled organisms, as well as fruit flies and corn plants, have been specially rewarding objects of genetic study. In evolutionary terms, however, insects and plants are clearly a long way from man, and we are really just beginning to get genetic information about the effects of radiation on some of the lower mammals, such as mice. Even so, several matters of profound importance have already become clear: bacteria or fruit fly, mouse or man, the chemical nature of the hereditary material is universally the same; the main pattern of hereditary transmission of traits is the same for all forms of life reproducing sexually; and the nature of the effects of high-energy radiations upon the genetic material is likewise universally the same in principle. Hence, when it comes to human genetics, where the impossibilities of ordinary scientific experimentation are clear and only a tantalizing start has been made, we can at least feel certain of the general nature of the effects, and need only to discover ways in which to measure them precisely.

(IV) HOW COULD WE REDUCE RADIATION RISK?

The major ways to reduce our present and future exposures to radiations would be: a) to reduce medical and other use of Xrays as much as is feasible; b) to

set and to observe regulations for the proper construction and the same operation of nuclear power plants and for the methods used to dispose of their radioactive wastes as well as the methods used in mining and processing the fissionable materials; c) to reduce the testing of atomic weapons and hence to reduce radioactive fall-out; d) to place limits on the human exposures involved in certain aspects of experimentation in atomic and nuclear physics.

To carry out the steps just mentioned would, in greater or lesser degree for the various items, reduce radiation risks. Progress with regard to step a) can doubtless be achieved, although to go too far in reducing the medical use of Xrays would of course lead to the risk of poorer diagnosis and less effective treatment of disease. But to carry out steps b), c), and d) would subject us to a different set of risks. We might thereby impede progress in the nuclear field. We might seriously weaken our country's position in the world. We might deny future generations some of the possible benefits of nuclear power and of other atomic discoveries.

(V) RADIOACTIVE MATERIAL AND RADIATIONS

Now that the problem has been posed, and now that we are warned somewhat about the difficulties, we must begin to consider some of the more technical issues involved. What is radioactive material, what are radiations, and what biological effects do they have?

By *radioactive material* is meant those naturally occurring substances such as radium, or those man-produced atoms resulting from atomic experiments, which are inherently unstable. Instead of remaining unchanged like ordinary atoms of familiar substances such as oxygen, gold, etc., the atoms of these radioactive substances act like alarm clocks set by mischievous gremlins for unknown times. Unpredictably (at least in individual instances, but predictably for the average behavior of a large number) these atomic alarm clocks "go off"; that is to say, they disintegrate.

When radio material disintegrates it emits, along with other less penetrating and hence less significant rays, certain high-energy rays known as gamma rays. Some of these rays are entirely similar to a beam of light, except for the important distinction that they readily penetrate human tissue which is nearly opaque to ordinary light. Also the energy of these rays is much higher than that of light, and this enables them to produce chemical and biological changes in the tissue they traverse. Rays of this sort, which transport energy from one point in space to some other point, are in general referred to as *radiation*. We also class as radiations beams of minute particles travelling at high speeds—such as electrons or neutrons which when they hit matter produce effects like those of the radiation mentioned.

As indicated above, gamma rays are emitted by naturally occurring radioactive substances, such as radium. They are also emitted by the radioactive materials which are produced in the nuclear fission which occurs in atomic weapons testing, in nuclear power installations, and in various sorts of experimental installations. These same rays, in dilute amounts, impinge on and penetrate all of us all the time. For radioactive material is, as an inevitable and hence normal procedure, built into the soil, rocks, plants, etc., and for that matter is also built into our own bodies. Similarly, such material exists on the luminous dials of our watches and clocks. The familiar Xrays of the hospitals and tuberculosis clinics, and in the offices of dermatologists and dentists, have properties of penetration and energy which are similar to gamma rays.

Throughout this report, the word "radiation" refers primarily to gamma rays and/or x-rays, sometimes to other sorts of radiations as will be more particularly mentioned later.

Everyone knows what a pound of beefsteak is, or a yard of cloth. We do not have that sort of familiarity with amounts, or units, or dosages of radiation. X or gamma radiation is measured in units called roentgens (abbreviated r; for example, "a dose of 3r"). Dental Xrays *involve* a dose (to the reproductive organs or gonads, that being the important matter from the point of view of genetics) of about 0.005 r; and a general fluoroscopic examination may involve a dose of 2r or even more.

(VI) SOME BASIC FACTS ABOUT GENETICS

Before we ask what effect radiations have on genetic processes, we must review a little basic information about genetics itself.

Every cell of a person's body contains a great collection, passed down from the parents, the parents' parents, and so on back, of diverse hereditary units

called *genes*. These genes singly and in combination control our inherited characteristics.

These genes, as was just stated, exist in every cell of the body. But from the genetic point of view the ordinary "body cells," which make up the body as a whole, are not comparably as important as the "germ cells" which exist in the reproductive organs, and which play the essential roles in the production of children.

The genes are strung together, single-file, to form tiny threads of genetic material called *chromosomes*, which are visible under a microscope. These chromosomes, in ordinary body cells, customarily exist as similar but not identical pairs. Human body cells normally contain 48 chromosomes, these constituting two similar but not identical sets of 24 chromosomes each. One of these sets of 24 chromosomes was inherited from the mother, for the egg cell carries a set of 24 chromosomes; and the other set of 24 chromosomes was inherited from the father, for the sperm cell also carries a set of 24.

All the genes that a person starts out with when the original egg cell is fertilized are in general kept unchanged as the cells divide and the person's body is elaborated and maintained. The process by which the dividing cells duplicate the genes may not always produce perfect copies, but it does so in general. But genes do nevertheless essentially change. They are changed by certain agents, notably by heat, by some chemical, and *by radiation*. It is with the last of these three agents of gene change that we are concerned in this report.

When a gene becomes permanently altered, we say it *mutates*. The gene in its altered form is then duplicated in each subsequent cell division. If the mutant gene is in an ordinary body cell, then it is merely passed along to other body cells; but the mutant gene, under these circumstances, is not passed on to progeny, and the effect of the mutant gene is limited to the person in whom the mutation occurred.

However, it cannot safely be assumed that the effect is a negligible one on the person in whom the mutation occurred, nor can it properly be said that this effect is nongenetic, even though passage to offspring is not involved. For various kinds of cellular abnormalities are known to be perpetuated within an individual through body-cell divisions; so these effects are genetic in the broad sense.

What is involved here is not only mutant genes, but also larger scale disruption of the genetic material, such as breakage of chromosomes.

The quantitative relations are not yet clear, but it is established that certain malignancies such as leukemia, and certain other cellular abnormalities can be induced by ionizing radiations. There is also some evidence that effects of this sort measurably reduce the life expectancy of the individual receiving the radiation. These risks have genetic aspects and therefore should receive mention in this report. Indeed these direct risks to the individuals exposed may well constitute another adequate genetic reason for limiting radiation exposures to the lowest practicable levels.

To return to a consideration of the risks which are passed on to progeny, the mutant gene may exist in a sperm or an egg cell as a result of a mutation having occurred either in that cell or at some earlier cell stage. In this case, a child resulting from this sperm or egg will inherit the mutant gene.

If we were to take the two chromosomes of a similar pair, stretch them out straight, and put them alongside each other, then each gene of one would be opposite a corresponding gene in the other. Thus the genes exist in pairs, as do the chromosomes. The two members of each pair of genes are not always identically the same. That is, in fact, why we call the chromosome pairs *similar* rather than *identical*. The two genes of a corresponding pair play similar roles, in that they both affect or help to determine the same characteristic of the whole organism. But one of the two may have a somewhat different, or a much more powerful effect than the other.

Thus of a certain pair of genes, both might be concerned with hair color. If both genes of this hair-color pair are the sort which favor red hair, then the person has red hair. If both genes are the sort which favor non-red hair (black, brown, or blond) then the person has non-red hair. But suppose that, of this pair of hair-color genes, one favors red hair and the other non-red hair. What happens then?

The answer (husbands and wives will understand this) is that one of the two usually dominates the situation and gets its way, although (and again this seems reasonable) the meeker one of the two usually manages to avoid being completely ignored.

Thus with one non-red gene (this being the powerful and dominant one of the two), and one red gene (this being the meeker one), the hair is ordinarily not red, but the red gene may nevertheless produce some effect, a little red showing in the hair so as to make it faintly rusty or tawny in color.[8]

The powerful type of gene, which gets all or most of its own way in contrast to its companion gene, is very naturally called a *dominant* gene. The less effective type is called a *recessive* gene. In this same terminology, non-red hair color is called a *dominant characteristic*, whereas red hair color is called a *recessive characteristic*. A recessive characteristic actually fully appears only if both of the relevant genes are of the recessive type. Of great importance for our present study is the fact that *mutant genes*—genes which have, for example, been changed by radiations—*are usually of the recessive type.*

It is now easy to see that any organism may have, latent in its genetic constitution, ineffectual or recessive genes that have not had much of a chance to become apparent in its developed external characteristics, since the recessive genes are masked by their dominant companion genes. Yet often, as we have seen, this dominance is incomplete and recessive gene is able to manifest itself partially.

When the two genes of a pair are alike (both recessive or both dominant) then they are called a *homozygous* pair; but when one is recessive and the other dominant, then the pair is called *heterozygous*. Thus a recessive characteristic (like red hair) can be fully expressed only when the corresponding gene pair is homozygous.

(VII) RADIATIONS AND GENETIC MUTATIONS

We are now in a position to indicate why it is that radiations, such as X-rays or gamma rays, can be so serious from the genetic point of view. For although the genes, as described above, normally remain unchanged as they multiply and are passed on from generation to generation, they do very rarely change, or *mutate;* and *radiation*, as we have already mentioned, *can give rise to such changes or mutations in the genes.* The change is presumably an alteration in the complicated chemical nature of the gene, and the energy furnished by the radiation is what produces the chemical change. Mutation ordinarily affects each gene independently; and once changed, an altered gene then persists from generation to generation in its new or mutant form.

Moreover, the mutant genes, in the vast majority of cases, and in all the species so far studied, lead to some kind of harmful effect. In extreme cases the harmful effect is death itself, or loss of the ability to produce offspring, or some other serious abnormality. What in a way is of even greater ultimate importance, since they affect so many more persons, are those cases that involve much smaller handicaps, which might tend to shorten life, reduce number of children, or be otherwise detrimental.

The changed character, due to the mutated gene, seldom appears fully expressed in the first generation of offspring of the person who received the radiation and thus had one of his genes mutated. For these mutant genes are usually recessive. If a child gets from one parent a mutant gene, but from the other parent a normal gene belonging to that pair, then the normal gene is very likely to be at least partially dominant, so that the normal characteristic will appear.

But this is not all of the story. For, like the red-hair gene, the harmful recessive mutant genes are not usually completely masked. Even when paired with a normal and dominant gene, that is to say even when in the heterozygous state, they still have some detrimental effect. This "heterozygous damage" is ordinarily much smaller than the full expression of the mutant when in the homozygous state, and yet there may be a significant shortening of the length of life or reduction of the fertility of the heterozygous carriers of the mutant. And the risk of heterozygous damage *applies to many more individuals*, indeed to every single descendant who receives the gene.

The relations of genes to ordinary traits (not to the most simply determined biochemical traits (are of course much more complex than the previous paragraph would seem to imply. Such gene-determined traits may vary from person to person, due perhaps to environmental differences, and often may not even appear at all. A single gene usually affects several such characters, and char-

[8] The accurate and complete genetic story about red hair is more complicated than has been stated here. There are less familiar characteristics—thalassemia and sickle cell anemia for example—which more strictly conform to the simple pattern here described.

acters are practically always affected by many genes. Also the effect of a gene may depend on what other genes are present, often in a complex way. For example, a mutation tending to increase weight might be harmful to certain persons, but beneficial to others.

Indeed it is likely that a large fraction of the genes that determine normal variability are of this rather ambiguous type that are sometimes deleterious, sometimes not. Mutations within this sort would not necessarily be harmful. Such mutations presumably occur, but geneticists do not know what fraction of all mutations are of this type, for they are not ordinarily detectable. However, the mutations that form the basis of this report are those that are relatively detectable, and these, as mentioned earlier, are almost always harmful.

Individuals bearing harmful mutations are handicapped relative to the rest of the population in the following ways: they tend to have fewer children, or to die earlier. And hence such genes are eventually eliminated—soon if they do great harm, more slowly if only slightly harmful. A mildly deleterious gene may eventually do just as much total damage as a grossly and abruptly harmful one, since the milder mutant persists longer and has a chance to harm more people.

In assessing the harm done to a population by deleterious genes, it is clear that society would ordinarily consider the death of an early embryo to be of much less consequence than that of a child or young adult. Similarly a mutation that decreases the life expectancy by a few months is clearly less to be feared than one that in addition causes its bearer severe pain, unhappiness, or illness throughout his life. Perhaps most obviously tangible are the instances, even though they be relatively uncommon in which a child is born with some tragic handicap of genetic origin.

A discussion of genetic damage necessarily involves, on the one hand, certain tangible and imminent dangers, certain tragedies which might occur to our own children or grandchildren; and on the other hand certain more remote trouble that may be experienced by very large numbers of persons in the far distant future.

No two persons are likely to weigh exactly alike these two sorts of danger. How does one compare the present fact of a seriously handicapped child with the possibility that large numbers of persons may experience much more minor handicaps, a hundred or more generations from now?

There are thoughtful and sensitive persons who think that our present society should try to meet its more immediate problems, and not worry too much about the long-range future. This viewpoint is in some instances supported by the belief that new ways, perhaps unimaginable at the moment, are likely eventually to be found for meeting problems.

There are other thoughtful and conscientious persons who think that we are specifically responsible for guarding, as well as we can now determine, the long future.

Recognizing the inevitability and propriety of both viewpoints, and recognizing that they lead different persons to express their concerns through different examples and with differing emphases, the fact of major importance for this present study is that, travelling by different routes, different geneticists arrive at the same conclusion: *Complexities notwithstanding, the genetic damage done, however felt and however measured, is roughly proportional to the total mutation rate.*

(VIII) MUTANT GENES AND EVOLUTION

Many will be puzzled about the statement that practically all known mutant genes are harmful. For mutations are a necessary part of the process of evolution. How can a good effect—evolution to higher forms of life—result from mutations practically all of which are harmful?

First of all, it is not mutations which, of themselves, produce evolution, but rather the action of natural selection on whatever combinations of genes occur. Much of evolutionary progress probably depends on changes within the range of normal variability, and thus depends on genes of very small effect, and of the type mentioned in the previous section which are favorable or unfavorable depending on what other genes are present. Thus evolution consists of a complex shifting of frequencies of such genes, accompanied by the continuous process of elimination of detrimental mutations and the occasional incorporation into the population of a favorable mutation.

Nature had to be rather ruthless about this process. Many thousands of unfortunate mutations, with their resulting handicaps, were tolerated, just so

long as an advantageous mutation could be utilized, once in a long while, for inching the race up slightly higher to a better adjustment to the existing conditions. The rare creature with an advantageous combination of genes was better fitted to survive and displace his less favored companions, and thus evolution was served, even though there were thousands of tragedies for every success.

The reader may be troubled by a second difficulty. If mutation results in at least some favorable types, and if these are building blocks of evolution, why is an increase in mutation rate regarded as undesirable? Why wouldn't an increase in mutation rate produce a larger total number of the favorable types and so speed up evolution? If the favorable types are normally quite rare, wouldn't it almost seem that increasing the mutation rate would be desirable? The answer to this question lies in the consideration that the bad effects of mutation must be balanced against the good. Some mutation is necessary for evolution, but if the mutation rate is too high, the unfavorable mutations will be so numerous that the species and its future evolution will be handicapped. Under present-day conditions of living and medical care, it seems unlikely that the unfavorable results of mutation are being eliminated nearly as rapidly as was formerly the case. In other words, one of the consequences of the amazing mastery of his environment which man has achieved has been an actual decrease in the severity of natural selection.

Geneticists in fact believe that although favorable mutations are rare compared with unfavorable ones, the human population probably already has, and will continue to have as a result of its present mutation rate and without additional mutations from increased radiation, a large enough total supply of favorable, partially favorable, and potentially favorable mutations. In other words, with our present mutation rate we shall continue to have a degree of genetic variability adequate for further evolution.

(IX) WHAT, THEN, CAN GENETICISTS SAY TO HELP RESOLVE OUR PROBLEM?

With the background furnished by the preceding discussion, we can now state rather concisely certain main points on which geneticists are in substantial agreement. Some of these points will partially repeat statements already made, but they are included here in order that this section be reasonably complete of itself.

(1) Radiations cause mutations.

Mutations affect those hereditary traits which a person passes on to his children and to subsequent generations.

(2) Practically all radiation-induced mutations which have effects large enough to be detected are harmful.

A small but not negligible part of this harm would appear in the first generation of the offspring of the person who received the radiation. Most of the harm, however, would remain unnoticed, for a shorter or longer time, in the genetic constitution of the successive generations of offspring. But the harm would persist, and some of it would be expressed in each generation. On the average, a detrimental mutation, no matter how small its harmful effect, will in the long run tip the scales against some descendant who carries this mutation, causing his premature death or his failure to produce the normal number of offspring.

Although many mutations do disturb normal embryonic growth, it is not correct that all, or even that most mutations, commonly result in monstrosities or freaks. In fact, the commonest mutations are those with the smallest direct effect on any one generation—the slight detrimentals.

(3) Any radiation dose, however small, can induce some mutations. There is no minimum amount of radiation dose, that is, which must be exceeded before any harmful mutations occur.

(4) For every living thing—bacterium, fruit fly, corn plant, mouse, or man—there exist mutations which arise from natural causes (cosmic rays, naturally occurring radiations from radium and similar substances, and also from heat and certain chemicals). These naturally occurring, and hence unavoidable, mutations are usually called "spontaneous mutations."

Like radiation-induced mutations, nearly all spontaneous mutations with detectable effects are harmful. Hence these mutations tend to eliminate themselves from the population through the handicaps or the tragedies which occur because the persons bearing these mutants are not ideally fitted to survive.

We all carry a supply of these spontaneous mutant genes. The size of

this supply represents a balance between the tendency of mutant genes to eliminate themselves, and the tendency of new mutants to be constantly produced through natural causes.

(5) Additional radiation (that is, radiation over and above the irreducible minimum due to natural causes) produces additional mutations (over and above the spontaneous mutations). The probable number of additional induced mutations occurring in an individual over a period of time is by and large proportional to the total dose of extra radiation received, over that period, by the reproductive organs where the germ cells are formed and stored. To the best of our present knowledge, if we increase the radiation by X%, the gene mutations caused by radiation will also be increased by X%.

The *total dose* of radiation is what counts, this statement being based on the fact that the genetic damage done by radiation is *cumulative*.

A larger amount of radiation produces a larger number of mutations. But within the limits of the radiation doses being considered in this report there is every reason to expect that these additional mutants would be of the same general sort as those produced by the natural background radiation. That is to say, mildly larger doses of radiation would produce *more*, but not *worse*, mutants.

(6) From the above five statements a very important conclusion results. It has sometimes been thought that there may be a *rate* (say, so much per week) at which a person can receive radiation with reasonable safety as regards certain types of direct damage to his own person. But the concept of a *safe rate* of radiation simply does not make sense if one is concerned with genetic damage to future generations. What counts, from the point of view of genetic damage, is not the rate; it is the *total accumulated dose to the reproductive cells of the individual from the beginning of his life up to the time the child is conceived.*

What is genetically important to a child is the total radiation dose that child's parents have received from their conception to the conception of the child. Since this report necessarily deals with averages, the significant total dose period should be, at least approximately, the number of years that normally elapses from the conception of a person to the average time at which offspring are conceived. In the United States, based on 1950 data, the average age of fathers at the births of all children is 30.5 years, whereas the average age of both parents is 28.0 years. It therefore seems sensible for us to use the round figure of 30 years, especially since this figure is the one usually chosen to measure a generation. Using this 30-year figure for characterizing the "total reproductive life radiation dose" would have the result that about half of the total offspring would receive the possible effects of a smaller, and about half the possible effects of a larger, radiation dose.

(7) The problems of defining and estimating genetic damage are very difficult ones.

There are at least three different aspects which must be considered. The first aspect places emphasis on the risk to the direct offspring and later descendants of those persons who, from occupational hazard or otherwise, receive a radiation dose substantially greater than the average received by the population as a whole.

The second aspect refers to the effect of the *average* dose on the population as a whole.

The third aspect refers in still broader terms to the possibility that increased and prolonged radiation might so raise the death rate and so lower the birth rate that the population, considered as a whole, would decline and eventually perish. We are at present extremely uncertain as to the level of this fatal threshold for a human population. This is one reason why we must be cautious about increasing the total amount of radiation to which the entire population is exposed.

These three approaches to the problem of genetic damage involve estimating the damage in successive generations and also the total damage in all generations, due to an increase in the amount of mutation. The relative emphasis one places on these three aspects depends in part on whether one thinks primarily in terms of distress to individual persons, or whether one thinks in terms of the population as a whole. Necessarily involved is the contrast between manifest harm to a few, and less evident but no less unreal harm to many. Also involved is the contrast between a more short-term and a more long-range point of view.

One way of thinking about this problem of genetic damage is to assume that all kinds of mutations on the average produce equivalent damage, whether as

a drastic effect on one individual who leaves no descendants because of this damage, or a wider effect on many. Under this view, the total damage is measured by the number of mutations induced by a given increase in radiation, this number to be multiplied in one's mind by the average damage from a typical mutation.

Measuring total damage in terms of the number of mutations does indeed necessarily involve this concept of the average damage from a typical mutation, and some geneticists find this concept difficult and illusive. They would point out that mutations may be grouped in classes that differ, on a subjective scale, many thousand-fold in the amount of damage per mutation. As examples they would cite a mutation which results in very early death of an embryo (which might cause very little social or personal distress), and a mutation which results in severe malformation to a surviving child (which would cause very great personal distress and which clearly involves a social burden).

Rather than utilizing this concept of the average total damage per mutation, some geneticists prefer to start with a consideration of the tangible damage which occurs now, as a result of the current rate of mutation and get an index of damage by multiplying this by the ratio of the expected new mutation rate to the current one. This procedure, however, admittedly deals with only *part* of the total damage; so an alternative difficulty faces those who prefer this procedure, namely the difficulty of estimating what part of the total damage they have dealt with.

As an illustration of the first aspect, suppose that ten thousand individuals were exposed to a large dose of radiation, of the order of 200 r. Then perhaps one hundred of the children of these exposed individuals would be substantially handicapped, this being in addition to the number handicapped from other causes. In this case the connection with the radiation exposure could be established by a statistical study.

As an illustration of the second aspect, suppose the whole population of the United States received a small dose of extra radiation, say 1 r. Then there is good reason to think that, among a hundred million children born to these exposed parents, there would be several thousand who would be definitely handicapped because of the mutant genes due to the radiation. But these several thousand handicapped children might be, so to speak, lost in the crowd. Society might be more impressed by the one hundred more obvious cases of the preceding paragraph than by the more hidden several thousand cases of this paragraph.

We should not disregard a danger simply because we cannot measure it accurately, nor underestimate it simply because it has aspects which appeal in differing degrees to different persons. Two conclusions seem to be clear and of importance: We should proceed with due caution as regards all agents which cause mutations; and we should vigorously pursue the researches which will in time give us a more precise way of judging all aspects of the risk.

(X) SOME REMARKS ABOUT APPROXIMATE ESTIMATES

Up to this point of the discussion the conclusions of the geneticist are pretty clear; the mutant genes induced by radiation are generally harmful, and the harm cannot be escaped.

But as yet this report has not furnished much of a basis for converting these conclusions into practical advice. Remembering that we must eventually balance risk against risk, it is obviously desirable to try to learn, as definitely as circumstances permit, the answer to the question: *how* great would be the genetic harm done by various doses of radiation?

Section XII of this report will respond to this question. But before giving the various replies, there should be some preliminary explanation concerning the nature of the answers given.

Science, and particularly the branch which deals with the physical world about us, has succeeded in giving highly precise answers to many questions. When one talks about the velocity of light he does not need to say that it is *something* like three hundred thousand kilometers per second: he is justified in saying that it *is* 299,793 km. per second, and that the final integer is almost certainly not off by more than two units.

But when you ask an experienced surgeon what your chances are of surviving a serious operation, and if he answers "something like nine chances out of

ten," then you accept that as a reasonable and helpful estimate. You do not distrust him because he gives you a rought estimate. Indeed you would have good cause to distrust him if he tried to give a highly precise answer.

In other words, there are many situations in which science can give only rough estimates. These estimates can nevertheless be very useful. No one should disdain such an estimate because it is rough, nor should anyone consider such estimates unscientific.

In Section XII there will be stated the results of certain approximate calculations. The theory behind these calculations is on the whole well understood; but it is seldom the case that one knows with much accuracy the numerical values that enter into the calculations. One may, for example, say, "I don't know, in any direct measured sense, how many mutants would result if all the genes in a human fertilized cell received one roentgen of radiation. But using a pretty definitely known value for the mutation rate in certain genes of the mouse; and also knowing fairly well (in this case from experiments with fruit flies) how to pass from the measured rate for a few genes to the rate which probably applies to a germ cell as a whole; and then making the unfortunate but necessary assumption that these mouse and fruit fly figures apply reasonably well to man—using this procedure I come out with estimates for the number of mutants which would be produced in man by a given dose of radiation. Because of the uncertainties, I think it prudent to state not a single final result, but rather a range of result with estimated lower and upper limits. I wish that we had direct experimental evidence which would firm up this estimate. But I don't have to be too apologetic, for a large amount of biological reasoning has been successfully based on this sort of procedure. Man differs widely from lower forms of life in all the obvious, and in many other, respects. But the fundamental processes inside cells tend to be curiously alike, from the simplest creature of a single cell, up to man."

It may turn out that the uncertainties in the quantities which enter the calculation are so great that the resulting uncertainty in the final answer is itself so very broad that the calculation simply does not furnish a useful estimate. But it may also turn out that, despite some considerable uncertainty in the constituent factors, the answer can be stated with a range of uncertainty which is small enough so that the estimate is useful.

It seems necessary to emphasize this matter of approximate estimation, so that no one will improperly conclude that a statement is unreliable because it involves a range of values. On the contrary, such a statement, when made in a situation like the present one, should be viewed as all the more dependable precisely because it does not pretend to an unwarranted accuracy.

(XI) HOW MUCH RADIATION ARE WE NOW RECEIVING?

If we are to talk about how harmful certain radiation doses may be, we should gain some idea of the amount of radiation we are already receiving from various sources.

The Committee will release a report specially devoted to this particular subject, which summarizes in detail all the kinds, sources, and amounts of radiation. In the present report, only that minimum amount of information will be given which is necessary for our current discussion.

Neglecting several minor contributions (all of which will be treated in the longer report), man is at present receiving radiations from the following:

(1) *Background Radiation*

This is the radiation which results from natural causes (cosmic rays, naturally occurring radium, etc.) not under our control. Each person receives on the average a total accumulated dose of about 4.3 roentgens over a 30 year period. At high altitudes this dose is greater, because of the increase of cosmic rays. Thus this background is as high as 5.5 r in some places in the United States.

(2) *Medical X Rays*

According to present estimates, each person in the United States receives, on the average, a total accumulated dose to the gonads which is about 3 roentgens of X-radiation during a 30 year period. Of course, some persons get none at all; others may get a good deal more.

(3) *Fall-out from Weapons Testing*

The Atomic Energy Commission[8] is doing a technically competent and a socially conscientious job of measuring fall-out: but it does not follow from this that one can answer, with high precision, all questions about the biological risks involved. What they usually measure (which, technically speaking, is a beta-ray activity in air) has to be translated over into what is genetically important (namely, the gamma ray dose to the gonads). The estimation of the latter of these quantities from the former is a pretty complicated business.

Besides those just mentioned, there are certain further uncertainties in the fall-out values. The measurements are necessarily taken far apart, and there is known to be considerable local variation due to meteorological conditions and topography. The radioactive dust, when it settles out of the air, is subject to weathering, as when it is washed off of buildings by the rain and carried to locations where it may affect fewer persons. Also individuals inside houses, or other shelters, will be considerably less exposed than those in the open air

Thus one cannot expect the figures on fall-out to be very precise ones. We have been informed that the AEC scientists are confident that the actual true dose figures are less than five times their stated estimates, and are also greater than one-fifth of these stated estimates.

It should be noted that the figures on fall-out as stated by the Atomic Energy Commission make only a conservative correction for weathering and shelter; and thus their figures, at least in regard to this point, tend to overstate the danger rather than the opposite.

With these understandings, it may be stated that U. S. residents have, on the average, been receiving from fall-out over the past five years a dose which, if weapons testing were continued at the same rate, is estimated to produce a total 30-year dose of about *one tenth of a roentgen;* and since the accuracy involved is probably not better than a factor of five, one could better say that the 30-year dose from weapons testing if maintained at the past level would probably be larger than 0.02 roentgens and smaller than 0.50 roentgens.

The rate of fall-out over the past five years has not been uniform. If weapons testing were, in the future, continued at the largest rate which has so far occurred (in 1953 and 1955) then the 30-year fall-out dose would be about twice that stated above. The dose from fall-out is roughly proportional to the number of equal sized weapons exploded in air, so that a doubling of the test rate might be expected to double the fall-out.

The figures just stated are based on all information now available from both the Atomic Energy Commission and the Armed Forces, and have been estimated as part of a study carried out for this Committee by Dr. John S. Laughlin, Chief of the Division of Physics and Biophysics, Sloan-Kettering Institute, and Dr. Ira Pullman, loaned to this study by the Nuclear Development Corporation of America. In their estimation correction has been made for weathering and shelter effects in accordance with the latest experimental data.

(4) *Atomic Power Plants*

As yet the general population has not received radiation from atomic power plants or from the disposal of radioactive wastes. These are future sources of radiation that might become dangerous.

(5) *Occupational Hazards*

The preceding four points apply to everyone. Unless proper precautions are taken, persons who are close to equipment emitting X rays who are engaged in experimental work in atomic energy, who operate atomic plants, who test weapons, who mine or otherwise handle radioactive material, etc., are subject to the risk of greater radiation exposure during their work.

(XII) HOW HARMFUL ARE RADIATION-INDUCED MUTATIONS?

As has already been indicated, there are various ways of estimating genetic harm, various attitudes which can be taken as to what is most serious and significant. But this situation should not be allowed to confuse or conceal the massive fact that, by whatever chain of argument or reasoning, all geneticists come out with the same basic conclusions.

[8] Under the Department of Defense other measurements, relating to fall-out, are also being made.

(A) Thus the first and unanimous reply to the question posed by the title to this section is simply this: *Any radiation is genetically undesirable*, since any radiation induces harmful mutations. Further, all presently available scientific information leads to the conclusion that *the genetic harm is proportional to the total dose* (that is, the toal accumulated dose to the reproductive cells from the conception of the parents to the conception of the child). This tells us that a radiation dose of 2X must be presumed to be twice as harmful as a radiation dose of X; but it still doesn't tell us the amount of harm we would be doubling.

(B) Second, we remember that mankind has for ages been experiencing, as the so-called spontaneous mutations, a certain rate of (generally harmful) mutations due to natural and uncontrolled causes (cosmic rays, heat, chemicals, etc.). It is not entirely unnatural to think of this burden of mutations as a sort of "normal" burden on society.[4] Therefore it seems to be illuminating to ask: how much additional "man-made" radiation will it take before this "natural" amount of genetic mutation (to which we are at least in some senses adjusted) will be doubled?

The calculations which lead to an estimate of this "doubling dose" necessarily involve the rates of both spontaneous and radiation-induced mutations in man. Neither of these rates has been directly measured; and the best one can do is to use the excellent information on such lower forms as fruit flies, the emerging information for mice, the few sparse data we have for man—and then use the kind of biological judgment which has, after all, been so generally successful in interrelating the properties of forms of life which superficially appear so unlike but which turn out to be so remarkably similar in their basic aspects.

In view of the inevitable uncertainties, it is rather surprising that the final estimates, as made by numerous specialists of this Committee and in other countries, do not differ more than they do. The lowest figure which has been responsibly brought forward for the doubling dose is 5 r, and the largest estimates range up to 150 r or even higher. Recent work with mice (which are, after all, mammals) gives some basis for thinking that the doubling dose is not as high as 150 r. The experience in Japan gives some basis for thinking that the doubling dose is larger than 5 r. Indeed it is clear that the doubling dose must be at least as large as the background radiation (which is between 4 and 5 r, over 30 years, in the United States). This, in fact, would be the value of the doubling dose if spontaneous mutations were due to background radiation alone, heat and chemical agents making no contribution.

Thus various arguments reduce the 5–150 r range, and several experienced geneticists have recently made estimates in the narrower range of 30 r to 80 r.

In summary then of this particular point: Each individual, on the average inevitably experiences during his reproductive lifetime a certain number of harmful spontaneous mutations from natural causes. He would experience an additional *equal number* of harmful mutations if he received a certain dose of radiation during that same period. This is known as the "doubling dose." The actual value of the doubling dose is almost surely more than 5 r and less than 150 r. It may very well be from 30 r to 80 r.

The first portion of this (Section XII) said that twice as much radiation gives twice as much harm. This second portion goes a bit further. It says that something like 30 r to 80 r (or at a further extreme, 5 r to 150 r) of extra radiation dose would do mankind twice the harm it is now experiencing from spontaneous mutations.

(C) The two preceding portions of this Section are clearly not really satisfying. They do indicate in quantitative terms how increases in radiation increase the harm. But anyone still wants to know in more specific terms, if possible, *how serious* is this harm that we may be doubling. If city traffic increases until the risk of crossing the street is doubled, then we will presumably still cross the street; for the risk per crossing is, after all, a very small one. If highway traffic increases until the risk in taking a thousand-mile drive is doubled, then many persons might well hesitate, for the risk is now unpleasantly high.

And this is the point at which it becomes most clearly evident that different geneticists find meaningful rather different approaches to the problem of genetic damage.

As has been stated previously, from one point of view the best index of genetic damage is the totality of tangible genetic defects of living individuals—

[4] There is some basis for hoping that we may eventually be able to control at least a part of both spontaneous and radiation-induced mutations.

say such things as mental defects, epilepsy, congenital malformations, neuromuscular defects, hematological and endocrine defects, defects in vision or hearing, cutaneous and skeletal defects, or defects in the gastro-intestinal or genitourinary tracts. Roughly 4–5% of all live births in the United States have defects of this sort; and of all of these, perhaps about half—or 2% of the total live births—have simple genetic origin and appear prior to sexual maturity.

If mankind were subjected to a "doubling dose" of radiation, then the present level of 2% of such genetic defects would rise, and would eventually be doubled. More explicitly, consider the next one hundred million births in the United States. This is about the number of children that will, in the future, be born to the presently alive population of the United States. Of these 100,000,000 children, something like 2,000,000 will experience genetic defects of the sort listed, these resulting from the deleterious "spontaneous" mutant genes which have been induced by natural causes excluding man-made radiation. If we were to be subjected, generation after generation, to an additional doubling dose of man-made radiation, then this present tragic figure of 2,000,000 would gradually increase by 2,000,000 more cases, up to an eventual new total of 4,000,000. It would, to be sure, take a very long time to reach this equilibrium double value. Perhaps 10% of the increase, or 200,000 new instances of tangible inherited defect, would occur in the first generation.

Since at various places this report considers a radiation dose of 10 r, it may be useful to state the tangible inherited defects from a dose of that size. A dose of 10 r would, on the above basis, give rise to some 50,000 new instances of tangible inherited defects in the first generation, and about 500,000 per generation ultimately, assuming of course an indefinite continuation of the 10 r increased rate and also assuming a stationary population.

These figures by no means measure *all* of the genetic damage that would result from a doubling dose; but they do make tangible and impressive the fact that a doubling dose of radiation would cause real personal and social distress.

(D) There is another way of looking at this problem of genetic damage, and that consists of trying to make some useful sort of really long-term, fully complete estimate. This consists of estimating the total number of mutant genes which would be induced in the whole present population of the United States and passed on to the next appearing 100,000,000 children, were this whole population to receive a certain total radiation dose to the gonads. In this instance we will use a dose of 10 r, since a dose of that magnitude appears later in this report in the recommendations. Having estimated this total number of transmitted mutants induced by a dose of 10 r, one then can only say, when he wishes to translate this over into harm or damage, that each one of these mutants must eventually be extinguished out of the population through tragedy. This statement does, of course, not hold in the detailed sense that one thinks of tracing each individual mutant gene until the line which bears and transmits it is overcome by the accumulating handicaps it imposes. The statement holds only in a statistical sense. Some lines of mutant gene will die out merely through normal chance procedures of inheritance. Others will multiply through these same chance procedures. But these normal chance effects cancel out; and the *statistical extinction* of the mutant genes is accomplished only through tragedy.

Concerning these estimates of total number of mutants, three things should be said. First, they are clearly not really satisfactory to any genetecist. Too much has to be assumed, too little is dependably known.

Second, this kind of estimate is not a meaningful one to certain geneticists. Their principal reservation is doubtless a feeling that, hard as it is to estimate numbers of mutants, it is much harder still, at the present state of knowledge, to translate this over into a recognizable statement of harm to individual persons. Also they recognize that there is a risk involved in extrapolating from mouse and Drosophila data to the human case.

Various remarks can, however, fairly be made in favor of this estimating attempt. Two largely independent methods lead to about the same results, and this increases one's confidence. Although the extreme ranges of the estimates differ widely, the mean estimate for any one geneticist is not very different from the mean for any other. Even the "guessing" which is involved hardly deserves that name, for it is based on long years of experience.

So that the final thing that should be said is that in spite of all the difficulties and complications and ranges in numerical estimates, the result is nevertheless very sobering.

Six of the geneticists of this committee considered the following problem: suppose the whole population of the United States received one dose of 10 roentgens of radiation to the gonads. What is the estimate of the total number of mutants which would be induced by this radiation dose and passed on to the next total generation of about one hundred million children? Each geneticist calculated what he considered to be the most probable estimate, and then bracketed this by his minimum and maximum estimates. Each thus said, in effect: "I feel reasonably confident that the true value is greater than my minimum estimate and less than my maximum. My best judgment, as stated in a single figure, is what I have labelled the most probable estimate."

The most probable estimates as thus calculated by the six geneticists do not differ widely. They bunch rather closely around the figure 5,000,000. Four of the six estimates are very close to that figure, and the other two differ only by a factor of 2.

These six geneticists concluded, moreover, that the uncertainty in their estimation of the most probable value was about a factor of 10. That is to say, their minimum estimates were about 1/10, and their maximum estimates about 10 times the most probable estimate.

This calculation assumes a stable value for the total population. This calculation is admittedly somewhat complicated and disappointingly vague. It is, to some geneticists, not a very meaningful way of looking at the problem. To others it adds up to something at least reasonably clear, and in any event very serious.

(XIII) FALL-OUT

There has been concern about the possible genetic harm due to the fall-out of radioactive material which results from the testing of atomic weapons. Certain aspects of this problem will be discussed in the reports of the other committees of this study (fall-out on grazing and cropland; fall-out in the sea and possible concentration in marine organisms; the distribution of fall-out material by the winds and in the upper atmosphere; possible pathological damage due to long-lived isotopes built into our bones; etc.). The present comments relate only to the question of genetic damage.

From the point of view of this Committee there are two summary remarks that should be made. First, since *any* additional radiation is genetically undesirable the fall-out dose is genetically undesirable.

Second, the fall-out dose to date (and its continuing value if it is assumed that the weapons testing program will not be substantially increased) is a small one as compared with the background radiation, or as compared with the average exposure in the United States to medical X rays.

(XIV) RECOMMENDATIONS

In light of the considerations which have been reviewed by this Committee, and which have been, at least in major outline, summarized in this report, this Committee has several recommendations.

These recommendations should all be interpreted in the light of the basic fact that *any* additional radiation is genetically undesirable. Therefore our society should hold additional radiation exposure as low as it possibly can. If certain figures (such as 10 roentgens) occur in a recommendation, it should most emphatically not be assumed that any exposure less than that figure is, so to speak, "all right": nor should it be for a moment assumed that disaster will suddenly descend if one of these figures is exceeded.

In any case in which a figure is stated, it is with the idea: stay just as far under this as you can; do not consider that this is an amount of radiation which is genetically harmless, for there is no such figure other than zero.

Opposing the fact that any further radiation is genetically bad is the practical fact that further radiation, from certain sources at least, is probably inevitable. The factors which argue for an increase in radiation are not genetic, and should obviously be appraised by a group much more representative than this Committee. Thus our recommendations will have to be evaluated by others, who must decide what decisions society should or must make. As geneticists we say: *keep the dose as low as you can.*

Thus we recommend:

(A) That, in view of the fact that total accumulated dose is the genetically important figure, steps be taken to institute a national system of radiation exposure record-keeping, under which there would be maintained for every individual a complete history of his total record of exposure to X rays, and to all other gamma radiation. This will impose minor burdens on all individuals of our society, but it will, as a compensation, be a real protection to them. We are conscious of the fact that this recommendation will not be simple to put into effect.

(B) That the medical authorities of this country initiate a vigorous movement to reduce the radiation exposure from X rays to the lowest limit consistent with medical necessity; and in particular that they take steps to assure that proper safeguards always be taken to minimize the radiation dose to the reproductive cells.

(C) That for the present it be accepted as a uniform national standard that X-ray installations (medical and nonmedical), power installations, disposal of radioactive wastes, experimental installations, testing of weapons, and all other humanly controllable sources of radiations be so restricted that members of our general population shall not receive from such sources an average of more than 10 roentgens, in addition to background, of ionizing radiation as a total accumulated dose to the reproductive cells from conception to age 30.

(D) The previous recommendation should be reconsidered periodically with the view to keeping the reproductive cell dose at the lowest practicable level. If it is feasible to reduce medical exposures, industrial exposures, or both, then the total should be reduced accordingly.

(E) That individual persons not receive more than a total accumulated dose to the reproductive cells of 50 roentgens up to age 30 years (by which age, on the average, over half of the children will have been born), and not more than 50 roentgens additional up to age 40 (by which time about nine-tenths of their children will have been born).

(F) That every effort be made to assign to tasks involving higher radiation exposures individuals who, for age or other reasons, are unlikely thereafter to have additional offspring. Again it is recognized that such a procedure will introduce complications and difficulties, but this committee is convinced that society should begin to modify its procedures to meet inevitable new conditions.

(XV) CONCLUDING COMMENTS

The basic fact is—and no competent persons doubt this—that rediations produce mutations and that mutations are in general harmful. It is difficult, at the present state of knowledge of genetics, to estimate just how much of what kind of harm will appear in each future generation after mutant genes are induced by radiations. Different geneticists prefer differing ways of describing this situation: But they all come out with the unanimous conclusion that the potential danger is great.

This report recommends that the general public of the United States be protected, by whatever controls may prove necessary, from receiving a total reproductive lifetime dose (conception to age 30) of more than 10 roentgens of man-made radiation to the reproductive cells. Of this *reasonable* (not *harmless*, mind you, but *reasonable*) quota of 10 roentgens over and beyond the inevitable background of radiation from natural causes, we are now using on the average some 3 or 4 roentgens for medical X rays. This is roughly the same as the unavoidable dose received from background radiation. It is really very surprising and disturbing to realize that this figure is so large, and clearly it is prudent to examine this situation carefully. It is folly to incur any X ray exposure to the gonads which can be avoided without impairing medical service or progress.

The 10 roentgen recommendation applies in an average sense to the population as a whole. We also include a recommendation concerning the upper limit of exposure that any one individual should receive. These limits would of course apply to persons whose occupations involve radiation exposure, but they are intended as broad and uniform regulations which apply to any and every individual.

The fall-out from weapons testing has, so far, led to considerably less irradiation of the population than have the medical uses—and has therefore been less detrimental. So long as the present level is not increased this will continue to be true; but there remains a proper concern to see to it that the fall-out does not increase to more serious levels.

One important lesson which results from this study is the following: The present state of advance in atomic and nuclear physics on the one hand, and in genetics on the other hand, are seriously out of balance. We badly need to know much more about genetics—about all kinds and all levels of genetics, from the most fundamental research on various lowly forms of life to human radiation genetics. This requires serious contributions of time, of brains, and of money. Although brains and time are more important than money, the latter is also essential; and our society should take prompt steps to see to it that the support of research in genetics is substantially expanded and that it is stabilized.

We ought to keep all of our expenditures of radiation as low as possible. Of the upper limit of 10 roentgens suggested in Recommendation C, we are at present spending about one third for medical X rays. We are at present spending less—probably under one half a roentgen—for weapons testing. We may find it desirable or even almost obligatory that we spend a certain amount on atomic power plants. But we must watch and guard all our expenditures. From the point of view of genetics they are all bad.

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SUMMARY REPORT OF THE COMMITTEE ON PATHOLOGIC EFFECTS

Appreciation of the pathologic effects of radiation on man has required of this Committee and its subcommittees, consideration of voluminous experimental work on animals, as well as such direct data on human beings as are available. When the results of controlled experimental studies are considered in the light of the human data, it is found that the sequence of pathological changes is indeed quite similar in man and in animals, although man has certain definable peculiarities of response.

The human data include:

Results of excessive exposure to X-rays and radium in the early days;

Results of more moderate exposure to different forms of radiation, as experienced by cyclotron workers;

Results of introduction of naturally occurring radioelements into the body, notably radium preparations and thorotrast;

Effects of exposure at Hiroshima and Nagasaki;

Observations on populations irradiated by fallout;

Additional observations from clinical radiotherapy, use of artificial isotopes in therapy, a very limited number of accidents in atomic energy work, and certain statistical surveys of large groups.

Experimental work covers the whole field and includes studies of acute and chronic effects on many species of animals.

Certain human effects have to be assumed from consideration of experimental knowledge: for example, early effects of high doses to the central nervous system, and results of absorption of most of the artificially produced isotopes, and it is fair to say that the lethal dosage of penetrating radiation for man is less well known than for many other species.

Radiation has been added to the means of production of casualties in warfare. Not only can radiation cause death or immediate, or delayed injury by itself, but exposure to it intensifies the seriousness of burns or other injuries. The acute lethal dose for half of a given population is in the range of 400 to 600 r.

Despite the existing gaps in our knowledge, it is abundantly clear that radiation is by far the best understood environmental hazard. The increasing contamination of the atmosphere with potential carcinogens, the widespread use of many new and powerful drugs in medicine and chemical agents in industry, emphasize the need for vigilance over the entire environment. Only with regard to radiaion has there been determination to minimize the risk at any cost.

It appears, however, that a fairly clear general picture of human radiation effects can be presented. Members of this group and of its subpanels, while recommending various points of departure for greater consideration and further research, were in no case of the opinion that any sort of "crash program" would be desirable or profitable.

The various means whereby persons may be overexposed to radiation will have a great deal of influence on the over-all effects. For example, the exposures at Hiroshima and Nagasaki and a few exposures in accidents in atomic energy plants, involved radiation to the whole body in which the clinical effects reflected mainly injury to the blood-forming tissues and intestinal tract. These tissues are very sensitive to radiation but have a great power of recovery.

Where, on the other hand, exposure has been suffered at a relatively low level from time to time over a period of years, a variety of injurious effects may be encountered, such as leukemia and skin cancer. Among those who have adhered to present permissible dose levels, none of these effects have been detected.

Shortening of life span may result from exposure to radiation not only as a consequence of damage to a specific tissue, as seen in the development of skin cancer and leukemia, but also as a result of such general factors as lowered immunity, damage to connective tissue, or premature aging. Older members of the populations seem to be more sensitive to this nonspecific damage. The shortening of life correlates roughly with dose of radiation, but has not yet been demonstrated at low doses. The following table indicates life shortening in radiologists, who may well have received doses in the course of their occupation ranging from very slight to about 1000 r.

Average age at death

	Years
Physicians having no known contact with radiation	65.7
Specialists having some exposure to radiation (dermatologists, urologists, etc.)	63.3
Radiologists	60.5
U. S. population over 25 years of age	65.6

Shielding of even a portion of the body from radiation lessens the effect out of proportion to the relative amount of tissue protected. Therapeutic radiation to a single portion usually is much greater than the lethal level of total body radiation.

Radiation may have its prominent effects in particular parts of the body when it is applied locally, and this may take place in two ways. First, an external source may be so handled as to direct its radiaion to a particular part; in this way many of the early radiologists suffered acute or chronic injury to the hands, which has also occurred in more recent atomic energy accidents.

In the second instance, a radioactive substance may be taken into the body and deposited where it is a source of constant local irradiation until it is eliminated. Bone disease in radium workers and lung disease in miners of radioactive ores (both leading to cancer as a late development) are well-known examples of this mode of exposure. It is worth noting that the atomic energy industry, through diligence, has apparently avoided exposures leading to this type of injury.

It is thus characteristic of the radiations that their effects may manifest themselves not only immediately, but perhaps only after a long period of intermittent radiation, or may even be long delayed after a single exposure. One of the particular tasks of the panel has been to see all of these effects in a common perspective. They will be discussed here in terms of the effects of radiation on the important organs and tissues of the body, since it is a well known fact that some are more readily injured by radiation than others, and that injury to some has more serious consequences than to others.

Among the more serious effects of radiation are those on the blood, since the vital blood forming organs are particularly sensitive to radiation injury. The white blood cells are decreased in number soon after radiation, and in fatal cases they almost disappear before death. Other acute changes in the blood give rise to disorders in the clotting mechanism and a bleeding tendency, and the formation of antibodies against infections is impaired. These changes lead to acute illness in the second week (perhaps a little later in man), heralded by decrease in the white cells.

In the next few weeks anemias may occur due to deficiencies in red blood cell formation and survival. Those victims living through the first month usually recover, but in certain individuals, or where radiation is continued, there is a further serious breakdown of blood cell formation.

Some late effects of radiation appear as leukemias, which are found to arise a few years after radiation. This disease, relatively rare in man, may show manifold increase in persons subjected to a nearly fatal single dose (Hiroshima data) or in those whose professional work has exposed them to higher than acceptable permissible dose rates.

Effects on the intestinal tract are also critical in the early period. Vomiting and diarrhea occur within a few hours. This is a common complication of X-ray treatment to the abdomen, but is not fatal. It seems to be mediated through the vegetative nervous system and is probably not related to later damage.

Within a few days (usually four or five) after radiation, more serious effects occur. Failure of the cells lining the intestine to replace themselves results in denudation of the surface, with intractable loss of fluid and salts; complicated by ulcerations, spread of infection, and bleeding.

Late effects are seen after heavy radiation therapy, and resemble those seen in some other heavily irradiated tissues: overgrowth of connective tissue (fibrosis) and decrease in the number of functioning epithelial cells. Cancer has occurred in animals given overwhelmingly large doses of isotopes in insoluble form by mouth.

Effects of radiation on skin have been widely observed. On the first day an erythema, resembling that of sunburn, appears but is transitory. A few days later a somewhat more persistent erythema occurs which may be associated with pigmentation. Ulceration may occur in this period after high doses. Much later, atrophic changes are seen, with marked deficiency of the blood supply and intractable ulceration; such a chronically damaged skin is a fertile bed for cancer development. The Marshall Island group, while receiving total body radiation insufficient to produce serious changes, had rather marked secondary skin lesions from direct contact with fallout material. Slight local vascular changes have been observed after two years, but serious after effects are not anticipated. Falling of hair was temporary in these persons; heavy dosages are required to make it permanent. In animals, destruction of the pigment cells causes regrown hair to be white, but such loss of pigment seems not to take place in men under comparable conditions.

Bone: Early radiation effects are not of note, except that retardation of growth of epiphyses of immature bones occurs and may produce serious results in children given local radiation therapy. Late effects are seen in radium poisoning, where we see repeated destruction and repair, culminating in widespread destructive changes in which bone sarcoma is likely to appear.

Lung: Early after large doses we see congestion and increased secretion. Here, again, the late-appearing changes are of greatest importance: fibrosis, and development of cancer, which has been very common in mining areas where large concentrations of radon gas were inhaled.

Thyroid: An early and persistent effect is depression in secretory activity, which is used as the basis of the radioiodine therapy of hyperthyroidism. No serious late local effects of thyroid radiation in adults have been recorded, although some leukemias have followed heavy radioiodine treatment. A small proportion of children treated with X-ray to the upper part of the body, however, develop thyroid cancer later on, suggesting a specially high sensitivity of the child's thyroid.

Eye: The only noteworthy lesion is cataract of the lens, which is a late response. It is much more readily produced by neutrons than by X-rays, therefore, has been most prominently observed in cyclotron workers.

Gonads: A single sublethal radiation dose to a male may result in sterility after two to three weeks, followed by a slow recovery. Chronic treatment results in a gradual reduction in number, motility and viability of sperm. This is the most sensitive indicator of chronic damage so far observed, being measurable in dogs at ten times the permissible dose rate. Larger doses (about equal to the total-body lethal dose) permanently sterilize males and females. Experience with the Marshall Islanders, the exposed Japanese, and certain accident cases indicate that total body doses up to about 40–50% of the lethal have no permanent effect on human fertility.

Central Nervous System: Observations in man are quite limited. Very high doses given to animals result in loss of coordination and excitement soon after

irradiation. At later stages, various effects are seen which indicate sensitivity of particular cells and areas.

Effects on Embryos: Treatment of embryos at various stages of development may lead to highly specific malformations depending on the exact developmental stage at the time of irradiation. At critical stages, relatively low dosages (those permitting survival of the mother) may cause serious malformations. These changes must be distinguished from genetic mutations, as one is often tempted to call abnormal offspring mutations. The type of malformation discussed here would not perpetuate itself genetically, and would result from radiation during gestation.

It must also be remembered that there are various other agents causing malformations during development, of which German measles is a well-known example.

A few factors influencing sensitivity might be mentioned. Very young or very old animals have increased sensitivity to lethal effects. Growing tissues are generally more readily damaged. States like hibernation delay the appearance of radiation damage but do not prevent it. Moderate stresses seem not to effect sensitivity but severe ones such as burns or exhausintg exercise, have a deleterious influence, augmenting sensitivity.

Local radiation in sufficient amount to almost any part of the body may produce cancer, the chance of tumor development being somewhat related to dose. Since the cancer cell is an altered type of a normal tissue cell, it has often been suggested that cancer is a somatic mutation, like a genetic mutation but arising in a tissue cell which perpetuates the character by its growth.

All types of induced and spontaneous tumors appear not to arise at once, but to pass through a serious of preliminary stages; and radiation induced tumors take a particularly long time to develop. Radiation induced cancer occurs in the absence of a generally abnormal state of the tissue of origin. Mouse experiments show that shielding of a part of the body will prevent radiation leukemia and that shielding of one ovary will prevent tumor from developing in the other; and several of the tumors appearing late after irradiation seem to be produced in response to indirect mechanisms. If somatic mutation is a necessary part of the induction of cancer, it would seem to play a minor role.

We have so far considered effects of overdosage of radiation in various forms. The question must necessarily be considered, as to whether much smaller amounts of radiation harmless to individuals, might be deleterious to large populations. Because of the striking difference of germinal and somatic cells the former carrying on from generation to generation injuries received, the Genetics Committee has recommended for large populations permissible dose levels of radiation lower than those which are safe for any one generation. As the permissible dose level which they have hypothesized as desirable for large populations were to be applied there would be no demonstrable somatic effect, although a theoretical minor shortening of life span could not be ruled out.

As regards internal contamination, independent data on Rongelap inhabitants and Japanese fishermen indicate that a considerable proportion of the lethal dose of external radiation was received by individuals who barely exceeded, and only for a short period, the permissible internal burden.

The only situation worth considering in relation to large-scale pathologic effects would then be widespread contamination with Strontium–90, which is a long-lived (half life 10,000 days) readily absorbed, bone-seeking isotope which tends to fall out generally over the earth rather than in accordance with the usual close or intermediate fallout pattern. It has already been found that some young individuals have retained 0.001 microcuries or one-thousandth of the permissible dose. This amount if maintained through life would yield 0.2 rep (equivalent r) to the skelton.

In developing an unequivocally safe amount, we can recall that a certain degree of radiation exposure has always been with us, even excluding X-rays, in the form of gamma radiation from minerals, cosmic rays, and radioelements normally in the body. These levels vary greatly from one location or altitude to another and are not considered to produce harmful effects.

There seems no reason to hesitate to allow a universal human strontium (very similar chemically to calcium) burden of 1/10 of the permissible, yielding 20 rep in a lifetime, since this dose falls close to the range of values for natural radiation background. Visible changes in the skelton have been reported only after hundreds of rep were accumulated and tumors only after 1500 or more.

In relation to world-wide contamination, food chains are important. Fallout contaminates plants through ground and leaf deposition; animals eat these plants. Because in fact milk and cheese are human sources of radiostrontium, being high in calcium. Throughout this chain, strontium is discriminated against relative to calcium, which reduces the hazard somewhat. It must be remembered that in regions where soil and water are low in calcium, calcium and strontium will be more readily taken up.

As to therapy of radiation injury: while treatment is difficult, some success has been achieved with antibiotics and properly timed blood transfusions. Shielding of a portion of the body appears to give a degree of protection disproportionately large for the mass shielded. Experiments set up to explain this fact may help in developing a rational treatment. Also, various forms of treatment given immediately before radiation have been devised, but do not appear in any sense practical. Studies of this sort may, however, provide a basis for future discoveries.

Because of the nature of this report, specific recommendations regarding needed research are omitted here, but will be published later when the subcommittee reports and other appendices are published in full.

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OUTLINE OF APPENDICES

(To be published later)

I. Subcommittee on Acute and Long Term Hematological Effects of Atomic Radiation—E. P. Cronkite, Chairman:
- Introductory Comments by Chairman.
- Acute Hematological Response to Single Doses of Penetrating Radiation.
- Long Term Effects on the Blood of a Single Exposure.
- Effects of Repeated Low Level Exposure.
- Usefulness of Hematologic Studies in Control of Radiation Injury.
- Conclusions.
- Bibliography, 1940–55.

II. Report of Subcommittee on Internal Emitters—A. M. Brues, Chairman:
- Statement of the Problem.
- Fallout Conditions.
- Acute Toxicity.
- Chronic Toxicity: Site of Injury.
- Effects on the Lung.
- Ruthenium; Cesium; Activation Products.
- The Alkaline Earths:
  - Metabolism.
  - Toxicity.
  - Sr 90—Radium Comparison.
  - RBE, Alpha and Beta Rays.
  - Absorption of Strontium.
- Radioiodine.
- Radiation from Particles and Hot-Spots.
- Permissible Dosage to Large Populations.
- Therapy by Removal of Radioelements.
- Bibliography.

III. Report of the Subcommittee on Acute and Chronic Effects of Radioactive Particles on the Respiratory Tract—Ralph W. Wager, Chairman:
- Introduction.
- Sources and Nature of Airborne Radioactive Particles.
- Nuclear Detonations.
- Description of the Respiratory Tract, Anatomy and Physiology.
- Fate of Inhaled Particles.
- Radiation Effects on the Respiratory Tract.
- Conclusions.
- Recommendations.
- Bibliography.

SUMMARY REPORT OF THE COMMITTEE ON METEOROLOGICAL ASPECTS OF THE EFFECTS OF ATOMIC RADIATION

CHAPTER I. DEBRIS FROM NUCLEAR TESTS

A. Introduction

Nuclear weapons produce atomic clouds which rise to heights dependent principally upon the energy released and also on the type of burst (air, surface, underground, etc.). Weapons in the kiloton range leave most of their radioactive debris in the troposphere, while megaton weapons are powerful enough to inject significant quantities of radio-active material into the stratosphere. Once the debris is injected into the atmosphere, it is rapidly spread over the earth by atmospheric processes, and eventually deposited on the surface of the earth, in a complex manner. Among the many problems are included: the way in which debris is mixed and transported by the atmosphere, both vertically and horizontally, the mechanism of removal from the troposphere and deposition on the ground, and the rate of penetration from the stratosphere through the tropopause and into the troposphere for eventual removal.

1. *Categories of fallout.*—The problem of the removal of radioactive debris from the atmosphere and its deposition in the biosphere may be divided into three phases: (1) Early or "close-in" fallout, that which occurs within the first ten to twenty hours following a nuclear explosion; (2) Intermediate fallout, that which occurs during the first weeks following the burst; and, (3) Delayed fallout, the slow removal of small particles which may continue for months and even years, particularly after a high-yield thermonculear explosion.

The principal mechanisms by which the removal occurs are gravitational settling, scavenging of radioactive particles by falling precipitation, and deposition by diffusion resulting from the everpresent turbulent eddies of the atmosphere. Although all principal mechanisms of removal play a role in each phase of the fallout, the primary emphasis shifts from gravitational influences in the early fallout to precipitation scavenging in the intermediate phase to an as yet poorly understood combination of diffusion and scavenging in the delayed fallout.

2. *Measurements.*—The most direct measurement of radioactive deposition is that made from the soil since it represents the main natural surface onto which the particles fall. Difficulties arise from the fact that rain may remove some of the activity by runoff or soaking deeper into the ground. As a measure of the true radioactivity on the ground in determining plant or animal intake of strontium 90, for example, soil sampling is obviously the most acceptable solution. But, for an accounting of the amount which has been deposited, the soil analysis may be unsatisfactory if the sampling is performed, at say, yearly or multiyearly frequency. Soil sampling on a frequent basis may be impractical.

Measurement of radioactivity by use of hand monitoring equipment is standard practice in areas where the radioactive deposition is significantly above normal background. This kind of observation is almost entirely useless outside of the areas of close-in fallout.

For daily, weekly or monthly fallout collections, the New York Operations Office of the Atomic Energy Commission recommends the use of a one-square-foot sheet of gummed film mounted horizontally on a stand three feet above the ground. An extensive, world-wide network of daily gummed film collection at about 250 locations has been operated by the Atomic Energy Commission for several years.

Finally, since there is evidence that much of the radioactivity deposited outside of the close-in area is brought down in precipitation, the collection of whole water samples is a method of obtaining the radioactivity of particles.

Air concentration.—Measurement of air concentration near the earth's surface has been achieved by a variety of sampling procedures. Filtration equipment of many types has been successfully employed, but the efficiency of the filter material for various particle sizes, particularly in the sub-micron range, must be determined before quantitative interpretation of the data can be made.

The fact that the upper atmosphere contains significant atomic debris has been known for several years. Sampling of the upper air by aircraft has been achieved by using the motion of the aircraft to pass air through a filter paper. The British report the presence of fission products at the peak altitude of their aircraft, 48,000 feet. The Japanese have measured the radioactivity by carrying aloft Geiger counters on balloons. By subtracting the cosmic ray counts from the total, the remainder is ascribed to fission products. American scientists do not view this procedure with favor for the low levels of radioactivity found over most of the world.

Instrumentation for the measurement of radioactivity by its effects on the electrical properties of the atmosphere also are of use only in those regions where the fission product concentrations are comparatively high.

B. Close-in fallout

1. *Description.*—Close-in fallout is the radioactive material from an atomic explosion which is deposited on the ground within a few hundred miles of ground zero, and which is down in some ten to twenty hours.

There is a fundamental difference between the fallout from an atomic device detonated at the ground and the fallout from one detonated so high that the fireball does not touch the ground. In the case of the surface burst, large quantities of surface material are broken up, melted, and even vaporized, and some of this material comes in intimate contact with the radioactive fission products. Then, after the atomic cloud has stopped rising and the violent updrafts associated with the explosion have subsided, the larger and heavier particles start falling back to the ground. The result is an area around ground zero and extending downwind which is covered in a more or less systematic way with radioactive particles.

In the case of an air burst in which the white-hot fireball never reaches the surface, the radioactive fission products never come into close contact with the surface material; they remain as an exceedingly fine aerosol. At first sight this might be thought to be an oversimplification, since there have been many cases in which the fireball never touched the ground, but the surface material was observed to have been sucked up into the rising atomic cloud. Actually, however, in such cases a survey of the area has shown that there has been a negligible amount of radioactive fallout on the ground. Though tons of sand and dust may have been raised by the explosion, they apparently did not become contaminated by fission products.

Experience has shown that an atomic device exploded on the surface distributes about 70–80 percent of its fission products on the ground within a few hundred miles of the burst point. A somewhat larger percentage will take part in the close-in fallout from an underground burst, and a smaller percentage will be scavenged from a near-surface burst or tower shot.

In order to make a quantitative study of the manner in which close-in fallout occurs, one must have a knowledge of the following parameters: wind structure, yield and height of burst, and kind of surface.

As each particle falls, it is carried horizontally by the wind at each level. The time during which it is falling through a given layer is inversely proportional to its rate of fall. Thus its horizontal travel during its entire fall from an initial height can be expressed as a summation of its horizontal travel in each layer. The rates of fall of atomic particles vary with particle size, shape, density, as well as the altitude.

Although no experimental information is available on the effects of precipitation during this initial stage of the atomic cloud, it is evident that significant deposition can occur from this cause. However, the effect would be most marked

from smaller yield bombs, since the bulk of the debris from larger bombs rises well beyond the rain-bearing strata.

2. *Height and size of the atomic cloud at the time of stabilization.*—It is evident that the physical size of the atomic cloud will have an effect on the distribution of the close-in fallout. The height to which the debris is carried will determine how far downwind a given particle size will drift, and the horizontal extent will serve to spread the fallout over a larger area.

In the first few seconds following an atomic detonation, the fireball grows rapidly, until the pressure inside the fireball is roughly that of the ambient air. At this point its temperature is still many thousands of degrees higher than that of the atmosphere around it, so it is much less dense, and the buoyancy of the atmosphere forces it to rise. However, it does not necessarily rise like a hot "bubble" or balloon, but in most cases, it develops a strong toroidal internal circulation and rises in the form of a smoke ring.

As the smoke ring rises, its internal circulation draws air in at the bottom and incorporates this new air into the cloud. The result is a very large growth in the size of the cloud as it rises, due mostly to the entrainment of the air from each level through which it passes.

It is clear that the cloud will gradually cool during its rise, due to radiation, the entrainment of the outside air and adiabatic expansion. When the mean temperature inside the cloud is the same as that of the ambient air at the same level, there will be no further buoyancy and the cloud as a whole will cease rising. However, at this point the kinetic energy of the toroidal circulation may still be considerable. For devices with yields of a few kilotons, the smoke ring circulation breaks up at about the same time that it reaches its point of stabilization, but for devices in the megaton range this toroidal circulation continues to pump air in at the bottom for ten to twenty minutes.

The net result of this pumping action after stabilization is a significant increase in the horizontal size of the atomic cloud, since the air which is drawn in at the bottom is forced out radially. Observations of this effect in the case of megaton devices are hindered by the fact that the structure of the cloud becomes confused.

The atmospheric stability will vary with season and latitude, and this accounts, in part, for the difference between the altitude of a cloud detonated in a tropical atmosphere and one of the same yield in a middle-latitude winter atmosphere. The most noticeable difference between these two regimes is the height of the tropopause.

3. *Distribution of radioactivity within the cloud.*—Since it is difficult to obtain enough samples of the radioactive debris while it is still within the cloud to determine its initial distribution, the most reliable estimates of this distribution have been based on the observed fallout and a reconstruction of what this initial distribution must have been.

It is clear from the observations of the rising cloud that almost all of the lighter debris is carried aloft in the smoke ring cloud. Apparently a certain fraction of particles are large enough to be thrown out of this ring, and these are left behind in the stem. However, in the stem there are violent updrafts for the first few minutes, so all but the very large particles will continue to be carried aloft.

For a surface or near-surface burst, the type of terrain must have a significant influence on the particle size and activity distribution within the cloud.

4. *Prediction of close-in fallout.*—At the outset it would be well to state what use can be made of a prediction of the fallout area from an atomic burst. At the risk of oversimplifying the case, here are some of the pertinent factors:

> Wind observations, now almost invariably made with sounding balloons, give winds which are not entirely representative of the winds which will affect the falling atomic debris. This is because winds change with time and place and because wind observations, as all meterologists recognize, are subject to a certain amount of error. Forecast winds, by the same token, are usually even further in error. A number of studies have been made of this subject. For example, a recent study by the Air Weather Service indicates that mean vector errors in 24-hr forecasts range from about 60 percent of the observed wind at middle altitudes to over 70 percent of the observed wind at 100 mb (about 53,000 ft.). These mean vector errors correspond to wind errors of 18 to 29 knots. It is perhaps significant that these forecast errors at the higher levels (40,000–55,000 ft.) are about the same as the root-mean-square deviation of the wind from the mean wind, and at lower levels (about 20,000 ft.) the 24-hr forecast error is about

half that of the normal climatological deviation. If one had to rely on forecasts 24-hrs old, he would be just about as well off if he used climatological data or persistence in computing the fallout.

The mushroom cloud from a multi-megaton device may rise entirely above the normal coverage of our radiosonde and RAWIN network, since it is generally considered impractical to plot and analyze current weather data at levels above 100 mb., or about 53,000 ft. Thus, unless special efforts are made there will simply be no wind data at all for the winds which will affect the debris during the first part of their fall. The effects of vertical motions in the atmosphere, possibly including currents arising from bomb-produced fires, may also be enough to alter the fallout pattern.

It should be fairly evident from the discussion in the preceding section that there are still a number of questions concerning close-in fallout about which we are still somewhat uncertain. Any fallout computation, even given perfect information on the wind field, will have a degree of uncertainty as a result of the assumptions on which it is based.

With these factors in mind, it appears unlikely that a weather forecaster, even given the computing aids which he would need to compute a fallout pattern, could on short notice and in a time of emergency give a detailed and reliable forecast of the close-in fallout. He could with a fair degree of assurance delineate the general sectors in which the fallout would be mostly likely to occur, but he could not tell where a given dose rate contour would lie. If one is dealing with a military situation in which an enemy is dropping atomic bombs, then the forecaster's problem is further complicated by the fact that he would presumably not have accurate knowledge of the height of burst and fission yield of the weapon.

It must be emphasized, however, that the above statements do not necessarily apply to the prediction of the fallout from a test device, where many of the uncertainties mentioned can be removed. It is possible, by the use of a special upper air-sounding network, to obtain wind information over a limited area which is considerably more reliable and current than that obtained from the routine upper air net, and which extend to a greater altitude. Moreover, there is usually no doubt about the yield and burst height of the device during a test. Thus, it is much more likely that an accurate forecast of the fallout pattern can be made under the favorable conditions which exist during a test. Even here, there remains a degree of uncertainty, as witnessed by the fallout which occurred on some inhabited atolls during the 1954 tests in the Pacific—though this might have been forecast if there had been the refined fallout computing aids which exist today.

Finally, if one does not have to make use of forecast winds at all, but can introduce all the detail of a careful synoptic analysis "after-the-fact", including the time variation of the wind at each level, and compute the fallout on a high-speed computer, it is possible to reproduce the fallout patterns which have occurred from the U. S. surface bursts with considerable accuracy. The radiological monitoring data show a certain amount of spread in the observations because of the detailed effects of terrain and atmospheric turbulence. When the reconstructed pattern or computed fallout patterns are compared with observed values, the minor differences are usually accounted for by small-scale features in the wind structure. Where the winds apparently behave as expected, predictions verify within a factor of two over most of the area. Where they do not, the peak dose rate is often correctly predicted at various distances from ground zero although displaced relative to the observed peaks.

C. Intermediate fallout

Although gravitational settling continues to play an important role for many days, and the downward diffusion of debris from the atomic cloud as it is moved about by the upper winds also becomes important, the primary removal of debris after the first day or two following a burst occurs in areas of precipitation. As the cloud of debris continues to be diluted by the atmosphere, concentrations decrease and it becomes necessary to collect the fallout and wait until the natural radioactivity has decayed before measurements can be made.

From Nevada test series, it has been found that less than 5% of the total beta radioactivity produced is collected by the gummed film network in the United States. Stewart, Crooks, and Fisher have estimated from observations in the British Isles that about half the radioactive dust in the troposphere from Nevada tests is deposited in approximately 22 days and that 80% of the deposition by rain occurs during the first transit of the cloud over England.

The importance of precipitation in bringing debris to the ground after the first day or so following an atomic explosion is strikingly shown in the average daily activity found on gummed films exposed in the United States during the Teapot Nevada test series in the Spring of 1955. In light rain, on the average, over twice as much activity is collected by the gummed film as compared to dry days and this increase becomes more apparent as the rain gets heavier. Various studies have shown that anywhere from four to more than ten times as much debris is deposited during periods of rain as compared with dry days.

On a few occasions, rain has coincided with the passage of a fresh cloud of debris from a Nevada test, resulting in local increases of background radiation to about 1 mr./hr. beyond a few hundred miles from the test site.

In the absence of precipitation, the effects of turbulence as well as gravitational settling are important.

Removal of debris by impaction on natural surfaces, buildings, etc., resulting from the movement of air around these surfaces must be appreciable. Various studies have shown radioactive particles are found on leaves, branches, etc. An experiment conducted at the Naval Research Laboratory with an 80-mesh stainless steel wire screen and with ordinary cheesecloth faced into the wind showed that in the absence of rain as much as 10 to 100 times the activity collected on the horizontal gummed film can be collected on the screen or cloth. In a two-month period during the Teapot series, a total of 50% more activity was collected on the cheesecloth than on a horizontal gummed film of similar size. Studies of the vertical distribution of chloride particles also indicate a depletion near the ground over land areas, presumably a result of impaction on natural surfaces.

D. Delayed fallout

In contrast to the results from the Nevada tests, measurements of radioactive debris concentrations in the troposphere showed a continued increase over England during the 10-month period following the thermonuclear tests in the Pacific in 1954. Similar increases in ground-level concentrations have also been observed by the Naval Research Laboratory in the United States and elsewhere.

This delayed fallout is a consequence of the extreme heights reached by debris from thermonuclear explosions, more than 80,000 feet, which results in the storage of large amounts of small particle-size debris in the stratosphere. The existence of such a distribution has been confirmed by aircraft measurements over the British Isles in August and September 1954 and again in early 1955 which show a very large increase in air concentration above about 35,000 feet. This debris eventually moves through the tropopause into the troposphere, from where it is removed by precipitation scavenging and by deposition.

1. *Transport in the stratosphere.*—The stratospheric levels in question are mainly in a region where relatively sparse synoptic data on the structure or air currents are available. However, they are mainly in a region of hydrostatically stable air and soundings indicate, in general a relative high degree of steadiness of stratospheric currents.

The winds in the stratosphere seem to have a predominant zonal component. The material injected at a certain locality will spread to other longitudes faster than to other latitudes. Material injected at a certain time in a vertical column may move more rapidly, or even in a different direction, at one level with respect to another. This shearing motion of the large-scale air currents represents a powerful factor for the spreading of an originally localized cloud to all longitudes within a few weeks.

All stratospheric circulation cells undergo more or less marked changes during the course of the seasons. Superimposed on the seasonal trend are day-to-day wind fluctuations caused by migrating or oscillating pressure systems. The present-day knowledge of independent stratospheric pressure systems is very limited. But it can be assumed that the stratosphere reacts, at least partly, to the migrating cyclones and anticyclones of the troposphere. Over periods of several weeks the net effect of the stratospheric wind variability will be similar to a process of large-scale eddy diffusion acting mainly in the horizontal directions.

2. *Diffusion in the stratosphere.*—One may approach the question of vertical diffusion in the stratosphere in three ways: first, using first principles; second, using natural gaseous tracers and third using man-made probes.

(*a*) *First principles.*—If asked for criteria to predict vertical mixing at the ground from meteorologically-observed parameters, one would point, in all likelihood, to three items: vertical temperature gradients, wind speed and wind shear. The greater the temperature stability the less the vertical mixing. It is primarily

on this ground that the stratosphere has been viewed as a region of quiescence in comparison with a turbulent troposphere below it.

With regard to wind speed, it seems fairly clear that an absence of horizontal kinetic energy will be associated with little or no vertical motions but, it is not evident that high wind speed necessarily will produce vertical turbulence. In any event, the lower stratosphere has a variety of speeds.

In the Richardson number, which under special conditions predicts the onset of turbulence, it is the shear rather than the wind speed which is significant. There is as large an assortment of wind shears in the stratosphere as in the troposphere, barring the layer adjacent to the jet streams in the troposphere.

One must conclude that on one count—probably the most important—stratospheric vertical mixing should be much smaller than tropospheric and that on the other two scores, it need not be.

(*b*) *Gaseous tracers.*—Ozone is the first such atmosphere property which comes to mind. It has been established that the ozone concentrations below the ozone maximum (about 25 km) are often in excess of the photochemical equilibrium amounts. It appears that the day-to-day variations and much of the seasonal variation of total ozone reflects changes in the non-equilibrium ozone in the "protected" region below the maximum. It is generally accepted that exchange processes transport ozone downwards from the region of ozone maximum. Three types of exchange process have been considered. The first involves large-scale meridional circulations in the stratosphere. There are some reasons for accepting such a meridional circulation involving both hemispheres but the evidence is not very impressive. A second exchange process is turbulent mixing. This is difficult to evaluate because of the lack of information on the magnitude of the mixing coefficient. It does seem, however, that the mixing coefficient required to provide the needed flux of ozone is not unreasonable. The third exchange process may be called "Gross austausch" since it involves the vertical motions associated with travelling cyclones and anticyclones. There is good evidence for this effect in the correlations between total ozone and the pressure field. It also provides a qualitative explanation for the annual variation of total ozone.

With the possible exception of the large-scale meridional circulation, the exchange processes described above will operate to bring ozone into the troposphere where it is destroyed at lower levels by particulate matter. The study and measurement of the ozone exchange should be applicable to the exchange of nuclear weapon debris.

Water vapor probably has no marked sink (due to cloud formations or precipitation) near the tropopause. Thus, changes in the gradient of water vapor mixing ratio should be a clue to the comparative upper tropospheric-lower stratospheric mixing intensities. The use of moisture as a tracer suggests but does not clearly indicate little vertical mixing in the lower stratosphere.

(*c*) *Man-made probes.*—Both parachutes and balloons have been used regularly to measure small-scale vertical motions in the stratosphere and the results generally reveal the stratosphere to have greater vertical motions than the troposphere. Also, aircraft report turbulence in the stratosphere. This evidence for comparatively short period vertical motions is clouded by the question of the rôle of the platform. The growth of the rising balloon, for example, alters the flow around it which may be the cause for the apparent vertical motions deduced from its ascent rate. Further, as with any measure of vertical motions, the probe does not distinguish between non-dispersive vertical motions like gravity waves, and true diffusing elements.

3. *Mixing through the tropopause.*—In a practical definition the tropopause is the level of minimum temperature of a high-altitude sounding, or the layer of maximum change of vertical lapse rate of temperature when no minimum temperature is encountered. Mean height-latitude cross sections of the atmosphere show that the tropopause is quasi-horizontal only in equatorial and polar regions, at approximately 18 and 9 km, respectively. The break occurs normally between 30 and 60 deg. latitude where the mean tropopause has either a significant slope or lacks uniqueness of definition so that multiple tropopauses are assumed by some authors even for mean conditions. Individual soundings may show considerably day-to-day fluctuations of the tropopause level, in connection with the passage of cyclones and anticyclones. Therefore, the tropopause is far from being a well defined geometrical surface and can hardly be considered an internal boundary which separates two distinct kinds of air masses. Air may move vertically through the mean tropopause level, or horizontally through the tropopause breaks. However, net radiation and convection processes are assumed to exist which result in a marked tendency towards re-establishment

of the tropopause at preferred levels just above the atmospheric layer in which the content of liquid and vaporous water is significant and condensation-precipitation cycles are dominant.

Four main types of exchange of air, or air properties through the tropopause may be distinguished: (i) small-scale vertical exchange, or vertical eddy diffusion—(ii) medium-scale penetration of tropospheric air into the stratosphere above extremely intense convective cells (heavy squall lines, frequently connected with tornadoes)—(iii) large-scale entrainment of stratospheric air into tropospheric systems such as cyclones, jet streams, hurricanes—and (iv) mean transport by vertical branches of large-scale to world-wide circulation cells.

4. *Tropospheric removal.*—The very small particles which are originally in the stratosphere and reach the troposphere weeks, months and even years after the detonation of a thermonuclear weapon, must eventually be deposited in the biosphere. However, the mechanisms by which these small particles are finally removed from the troposphere are not clear and the data concerning this problem is inconclusive.

Investigations of the rate of removal of natural radioactivity from the lower troposphere, both in the United States and in Germany, indicate that about half the activity is removed in a period of about one or two weeks. However, the particles involved are extremely small (probably less than 0.01) and are concentrated near the ground, so that the results may not be applicable to the fallout problem. On the other hand, Langmuir has shown that the collection efficiency of precipitation for very small droplets (less than 1) is small, but again the results may not be applicable to the fallout problem, where electrostatic and surface tension phenomena are different. Agglomeration between natural cloud elements and radioactive particles is operative for small particles.

Conflicting evidence on the rapidity of tropospheric removal is also found in studies of the actual fallout. Stewart, Crooks and Fisher, in Britain, estimate from indirect reasoning that deposition in rain exceeds dry deposition by a factor of twenty for thermonuclear explosions, a study of gummed film results in the United States does not bear this out—average monthly deposition at 40 monitoring stations during September and October, 1954, shows no correlation with either total rainfall during the month or the number of days with rain at the station. Again, using the British data, it is seen that the specific activity of the lower atmosphere showed a more than fourfold increase during the interval from 10 weeks after the Pacific tests to 50 weeks after if the data is corrected for decay. Similar increases were found by the Naval Research Laboratory. It is hard to reconcile this increase in tropospheric concentration with the rapid cleansing of the troposphere.

E. Analysis of stratospheric storage from radiostrontium fallout data

1. ***Statement of the problem.***—The fission product of greatest interest in terms of long-term hazard from nuclear detonations appears to be Sr^{90}, and estimates of the rate of deposition of this isotope in the biosphere are needed. Unfortunately, our knowledge of the physical mechanisms involved is too meagre to deal with this problem on a theoretical basis. Although it has been established that a considerable amount of debris is injected into the stratosphere and that this debris slowly mixes downward into the troposphere and is eventually deposited on the ground, the average storage times in the stratosphere, and even in the troposphere, are uncertain. Among the many unknowns in attempting a theoretical analysis are the initial distribution in the stratosphere and the physical mechanism of stratospheric removal. Even if the latter were known, we are at present unable to make quantitative estimates of the rates or intensities of these physical processes. However, due to the biological uncertainties in estimating the hazard from Sr^{90}, a precise answer is not needed, and even a gross estimate would be useful.

2. ***Analysis by W. F. Libby.***—Dr. W. F. Libby of the Atomic Energy Commission has published an estimate of the stratospheric storage time based on the estimated stratospheric content and on the observed deposition, with little or no reference necessary to the physical mechanisms involved. Essentially, the annual deposition is divided by the amount in the stratosphere, yielding the fractional removal during the year. If the fractional removal rate is assumed constant (i. e., the stratospheric content is assumed to decrease exponentially) the mean residence time of the debris is given by the ratio of the stratospheric content to the deposition.

The basic data used by Dr. Libby are the stratospheric content immediately after the completion of the Castle (Spring 1954) tests in the Pacific and the deposition of Sr^{90} during the following year or so as measured in three ways, a

world-wide gummed film fallout network, the Sr^{90} content of Chicago rainfall and air filter measurements at Washington, D. C. From these results, Libby concludes that the mean storage time for debris in the stratosphere is approximately 10 ± 5 years.

3. *Conclusion.*—Stratospheric storage not only serves to delay the fallout of debris, but also to disperse it over the globe, minimizing the chance of locally high concentrations of debris. At present, the amount of Sr^{90} in the stratosphere from nuclear weapon tests is far too small to approach maximum permissible concentration even if it were to be all deposited now. However, if the testing programs of the several countries producing thermonuclear weapone were to intensify, stratospheric storage time may become a critical item in terms of hazard to mankind. For this reason, a continuing program to investigate this phenomenon is needed, including actual measurements of the radioactivity in the stratosphere and improved and more representative methods of observing fallout.

CHAPTER II. ATMOSPHERIC RADIOACTIVITY FROM CIVILIAN APPLICATION OF NUCLEAR ENERGY

A. Sources of contamination

The hazards of atmospheric contamination from the military uses of atomic energy have tended to overshadow other possible sources of contamination, principally because, to date, relatively insignificant contamination has occurred from non-military sources. Certainly, the near future will see a tremendous increase in the utilization of nuclear energy for peaceful purposes, including the production of electric power, medical, industrial and agricultural applications, and nuclear propulsion of air, sea and land vehicles.

As far as can be seen today, the largest potential use of nuclear energy will be in the production of electric power and the discussion is based on this aspect of the problem, however, other applications could conceivably double the values used in the estimates given here. A consensus of estimates of global power requirements and of the proportion of this energy which will be supplied by nuclear sources indicates that by 1975 there will be a nuclear heat energy production of 10^8 to 10^9 kilowatts and by the year 2000 this will increase to 10^9 to 10^{10} kilowatts.

These rates of production will produce enormous amounts of fission products. However, most of these will be in solid or liquid form at present day processing temperatures and it can be expected that such material will not be intentionally released into the atmosphere. Of the remaining volatile fission products, storage and "cooling" of the fuel before processing can reduce the activity materially. The two volatile isotopes of most interest are 10-year krypton 85 and 8-day iodine 131. Only the 10-year krypton is sufficiently long lived to be relatively insensitive to the cooling time of the fuel before processing. There are two aspects to the problem of radioactive hazard from these sources, large-scale contamination on a global or hemispheric basis and local or regional contamination in the areas of processing plants.

B. Large-scale contamination

1. *Krypton 85.*—The long half-life of Kr^{85} results in the accumulation of this isotope in the atmosphere. If by the year 2000 nuclear thermal power has risen to 10^{10} kilowatts, the world inventory of radiokrypton would be of the order of 10^{10} curies. Mixed uniformly through the mass of the troposphere (4×10^{21} grams of air), the resulting sea-level concentrations would be less than 10^{-8} curies/meter^3. Since most of the activity is likely to be released in the middle latitudes of the northern hemisphere, large scale concentrations of 3 to 5 times the global average could be experienced in these latitudes.

No value for the maximum permissible concentration of Kr^{85} is presently available. If, from the chemical and radiological similarity, we assume that it is analogous to radioxenon, then the estimated worldwide concentration in the year 2000 is about two orders of magnitude less than the maximum permissible concentration. However, such comparisons are extremely questionable and it is important that maximum permissible concentration levels be established for Kr^{85}.

2. *Iodine 131.*—The problem of I^{131} in the atmosphere is largely dependent on the fuel recharging interval and the cooling time. For each combination of fuel cycle and cooling time it is possible to calculate the total amount of I^{131} in the atmosphere. This is an equilibrium value assuming no removal at the source or after release. Total amounts of I^{131} in the atmosphere based on the estimated nuclear energy production in the year 2000 are given in the following table.

Total I^{131} (curies) in the atmosphere per 10^{10} kilowatts of nuclear energy

	Decay time before release		
	none	10 days	100 days
Fuel recharging frequency:			
Once a year	6×10^{9}	3×10^{9}	10^{6}
10 times a year	6×10^{10}	3×10^{10}	10^{7}
Continuous	2×10^{11}	10^{11}	4×10^{7}

The present maximum permissible concentration of I^{131} is 3×10^{-9} curies/meter^{3}. If the I^{131} is mixed with the whole mass of the troposphere, then 10^{10} curies would produce the maximum permissible concentration. However, the assumption of world-wide tropospheric mixing is unwarranted for an isotope with a half-life of 8 days. Assuming the term large-scale contamination in the case of I^{131} can at most involve a 20° or 30° band of latitude in the northern hemisphere, and that vertical mixing may be incomplete, then even for large-scale considerations an atmospheric burden of 10^{8} or 10^{9} curies of I^{131} may approach the maximum permissible concentration, and appropriate cooling or decontamination measures must be used.

C. Local contamination

It is evident that consideration of the average contamination over major portions of the globe cannot approach the hazard to be found in local areas downwind from sources of contamination. Locally, higher concentrations that would exist 10 to 100 miles from fuel processing plants (assuming something of the order of 1% of the world's fuel to be processed at any single site) could add an additional factor of 10 to 100 in the case of Kr^{85} and several thousand in the case of I^{131}. Also, transitory excess concentrations due to unfavorable meteorological conditions could raise local concentration by an additional one to two orders of magnitude.

The above effects are cumulative so that concentrations of I^{131} about 10^{4} times the global average could occur regularly near fuel processing plants in the northern temperate latitudes, rising occasionally to 10^{5}–10^{6} times the global average during unfavorable meteorological conditions. Deposition by precipitation could increase the possibilities of harmful effects. Further detailed analysis would be required in order to indicate under what conditions the concentrations of krypton, iodine, or other isotopes would exceed permissible limits. In any case, it seems that a combination of reasonably conservative fuel cooling periods, some progress in off-gas cleaning, and a judicious choice of fuel processing locations, is indicated to minimize the adverse effects of unfavorable meteorological conditions. At the larger plants, meteorological scheduling of gas releases may be required. These principles are applied today, and will become increasingly important.

D. Accidental releases

There is the possibility, even if remote, that a large high-power reactor or fuel processing facility could be damaged or destroyed by accident and release part or all of the contained fission products to the atmosphere. The results of such an event could well be catastrophic, and extend over great distances. Estimates of areas of damage range upwards of thousands of square miles for very large reactors. By the year 2000 the release of only about 1% of the worldwide Sr^{90} inventory that could then exist, even if mixed uniformly throughout the global troposphere, could produce concentrations on the order of 5×10^{-10} curies m^{-3} or about twice the currently recommended maximum permissible concentration. This same 1%, if deposited on the surface, could seriously contaminate the entire area of the earth. It is more likely, in the event of such a catastrophe, that the activity would remain concentrated in a much smaller area near the source. Still, the operation of any significant fraction of the earth's nuclear reactors without proper safeguards would be of concern to all.

E. Conclusions

The solution to radioactive air pollution problem is the same as in other air pollution problems, prevention of the escape of pollutants to the atmosphere. Thus, primary consideration must be given to engineering features limiting the

escape of hazardous gases either during normal operations or accidents. As additional safety factors meteorological research to locate plants in areas where unexpected releases will do the least damage is desirable. Finally, it should be pointed out that the release of a hazardous substance by any country may affect other countries—particularly in the same latitude belt. International control to establish and maintain high standards of safe plant operation is essential.

CHAPTER III. USE OF RADIOACTIVITY IN ATMOSPHERIC STUDIES

A. *Natural radioactivity*

There exist two important sources of naturally occurring radioactivity in the atmosphere: (1) cosmic ray interactions in the stratosphere and (2) the rock and soil of the earth's outer crust. The study of the cosmic ray induced products entails considerable difficulties because of the low level of activity. On the other hand, the radioactive substances which originate in the earth can be detected and measured with relative ease.

Radon and thoron are released as gases in the radioactive decay of radium and thorium which are found in all rock and soil. The concentration of these gases and their distribution in the atmosphere is determined by their half-lives and meteorological conditions. Although it is considered generally that the relative amounts of the various natural activities are dependent on meteorology, very few correlations with specific meteorological parameters have been made, in spite of the fact that measurements have been carried out over a period of many years. At the present time, insufficient data are available to make reliable estimates of the global distribution of radioactivity in the air over land, although it is known that at some distance from large land masses the radioactivity concentration is exceedingly low. Measurements indicate that the amount of radon decreases rapidly with altitude to about one half the surface value at one kilometer.

Radon and thoron and their daughter products would seem to provide an easily detectable tracer for the study of the vertical "Austausch." Ground level measurements indicate that exchange phenomena within even a few feet of the surface have marked effects on the concentration of radioactivity. Such measurements might well be carried on in conjunction with micro-meteorological observations. From consideration of the lifetimes of the radioactive isotopes which are involved, it is obvious that even for relatively low wind velocities, horizontal transport of these radioactivities over distances of several hundred miles is entirely possible. The study of simultaneous variations in concentration over these distances should be valuable if the locations were carefully selected to avoid the effects of terrain. Land to sea measurements should be especially interesting.

Instances of increases in radon concentration coincident with air pollution have been reported. Since atmospheric radioactivity and pollution are strongly affected by the stability of the lower atmosphere this effect is not surprising. For the same reason it is quite possible that a relationship could be established with the tropospheric scattering of electromagnetic radiation.

Experiments have shown that the radon and thoron decay products are attached to submicron particulates. The details of the attachment process are not well understood; for example the relationship between various ionic species or the number and kind of nuclei. These radioactivities exist in the form of a readily detectable submicron aerosol which generally follows the surface wind pattern. These small particles, and incidentally other pollution, appear to be removed from the lower atmosphere in a matter of days, principally through precipitation. Further study of this removal process, carried out at different locations and for a variety of climatological conditions would perhaps shed some light on the scavenging efficiency of precipitation.

The natural radioactivity of precipitation is considerable and is easily measurable. The mechanism for the entrainment of the radioactive particles in rain droplets is not certain. From theoretical considerations, the probability for attachment of these very small particles in rain is quite low. It has been suggested that the radioactive ions could themselves act as condensation nuclei. On the other hand, there is the possibility that clouds of charged radioactive particles could act as a sort of "trigger" for electrical phenomena leading to cloud electrification and precipitation. Experimentally, the difficulties of working with large volumes of rainwater are partially offset by the large activities encountered. The actual air volumes swept out by precipitation is very great

and it would seem that there are possibilities for tracing air masses by using natural radioactivity.

Traditionally atmospheric radioactivity has been associated with atmospheric electricity and might well supplement studies in this field. The radon and thoron decay products are charged and can be collected by electrical means. They are estimated to cause about one half of the ionization in the lower atmosphere. Certain of the theories of atmospheric and cloud electrification are quite sensitive to changes in the ion concentration. Since large changes in the radioactivity concentration are the rule, further studies carried out in conjunction with atmospheric electrical measurements should be valuable.

The most extensively studied of the cosmic ray induced isotopes found in the atmosphere have been C^{14}, H^3 and Be^7. Probably both short term increases in fossil CO_2 from industrial sources and the long term global distribution could be detected using sensitive techniques. H^3 is present in the air principally in the form of tritiated water and will probably find its most useful applications to hydrology, although more extensive sampling of precipitation is no doubt desirable. Because of its relatively short half-life, Be^7 may be of very great importance in the study of the rate of mixing between the stratosphere and troposphere. Unfortunately, there is a great lack of experimental information suitable for correlation with meteorological phenomena.

B. Debris from weapons tests

The debris injected into the atmosphere from the testing of nuclear weapons can provide a useful tool for investigating atmosphere phenomena. However, two basic limitations on the usefulness of the approach must be recognized:

1. The source strength and distribution in space is largely unknown. Such important information as the distribution of particles with altitude, the exact configuration of the stabilized cloud, the relation of particle size to activity, the fractionation of elements within the cloud, etc., is not available.
2. Sampling techniques are imperfect. Air concentration measurements are difficult because of the low concentrations and small particle sizes involved. Ground collections result from either deposition of the particles themselves or by precipitation scavenging.

Using the gummed paper collection system described in Chapter I, it has been possible to obtain certain valuable meteorological information on such items as: a measure of the cross-equatorial transport and some feature of the general circulation from U. S. Pacific tests, scavenging by the upper portions of rain clouds of the particulate fission products, an estimate of rapidity of the removal of particulates from the troposphere, an estimate of the rate of transport from the stratosphere to troposphere.

Using aircraft sampling procedure, it has been possible to obtain estimates of the rate of lateral spread of an atmospheric contaminant and vertifications of meteorological trajectories.

By following the Tritium released by the CASTLE series of weapons tests it has been possible to estimate the removal time for atmospheric water molecules.

It is likely that the potential of even the existing unclassified information on radioactivity released by weapons tests has not been exhausted. This potential would be enhanced by disclosure of additional information on weapons, debris measurements, and source strengths. For example, the weapons tests offer an opportunity to determine storage and transit time parameters for surface water sheds of almost any size. By comparing the amount and level of radioactivity in rainfall and runoff as a function of time following a weapons test, it would be possible to measure those parameters which are vital to studies of ground water, river runoff, and flood forecasting.

C. Artifically introduced radioactive tracers

Artifically introduced radioactive tracers can serve meteorology in at least three fields: first, through the delineation of the airflow and rates of diffusion; second, in hydrometeorology, including studies of condensation, precipitation, evaporation and hydrology; and third, in atmospheric electricity.

As a tracer of air motions, radioactive substances are in competition with fluorescent dye particles, sulfur dioxide and other non-radioactive substances. Their advantages lie in the possibility of being able to treat large-scale atmospheric phenomena which otherwise require too large amounts of source material, in being able to utilize tracers which partake in the particular process under

investigation and, in certain cases, in our ability to detect the presence of the tracer instantaneously in the field. In any specific experiment it will be necessary to weigh economic, safety and scientific factors in the use of radioactive tracers over non-radioactive tracers.

Regions in which it would be highly desirable to further knowledge concerning air trajectories are in the neighborhood of jet streams, in cols, in hurricanes to measure both the three dimensional airflow and define the air comprising the eye, and in the Antarctic. In the field of dffusion, the use of radioactive tracer material can further knowledge of diffusion near the ground for air pollution studies, etc., and of diffusion in the stratosphere and tropospheric and stratospheric mixing.

The radioactive tracer material which appears to be most promising for the above meteorological studies is tritium. Tritiated water would be washed out, thus making for additional complications. Tritium in the form of ordinary hydrogen is acceptable although costs of analysis of the sample might be high. For the large-scale experiment to establish the tropospheric-stratospheric exchange tritiated methane has been suggested. Tritium has the advantageous properties of emitting a weak beta particle, of being available without difficulty, and of having a reasonably long half-life.

Water molecules are readily marked by tritium so that in any experiment in which the travel of water vapour is desired it becomes feasible to introduce tritiated water as a tracer. If sufficient amounts of tritium were available, a large-scale experiment to study the hydrologic cycle could be devised. Even on smaller scales, tritiated water could be used to study such features as the evaporation from a ponded lake, water sources for dew, contributions of local transportation or evaporation from local bodies of water to precipitation elements, etc.

Activation analysis techniques extend the possibilities for studying very small particles (such as sodium chloride) that play an important role in condensation and ice formation. Radiosilver can be introduced in a prẽparation of silver iodide to be able to determine the presence of silver iodide in the precipitation which was alleged to be stimulated by it. By releasing another tracer which would be scavenged with equal efficiency by precipitation it might be able to determine whether the silver iodide has played a role in the formation of the precipitation.

Finally, the ionizing properties of radioactive substances can be used to make local changes in the electrical fields of the atmosphere, to determine if such changes affect weather processes.

CHAPTER IV. THE EFFECT OF ATOMIC EXPLOSIONS ON WEATHER

A. *Introduction*

From the beginning of time, man has looked beyond the field of meteorology in the hope of finding some explanation for the vagaries of weather. Many inventions of man—gunpowder, radio, airplanes, and television—have been blamed for changes in weather and climate. It is only natural that atomic and thermonuclear explosions, being among the most dramatic achievements of mankind, would come in for their share of the blame.

There seems to have been an increase in unusual and undesirable weather in the past decade. When submitted to rigorous statistical tests, these apparent abnormalities do not exceed the limits that can be expected by chance and are consistent with accepted meteorological principles involving large-scale (hemispheric) weather patterns which could not be directly affected by the explosions. The failure to detect statistically significant changes in the weather during the first ten years of the atomic age is no proof that physically significant changes have not been produced by the explosions, but it does show that a careful physical analysis of the effects of atomic and thermonuclear explosions on the atmosphere must be made.

The energy of even a thermonuclear explosion is small when compared to most large-scale weather processes. Moreover, it is known that much of this energy is expended in ways that cannot directly affect the atmosphere. Even the fraction of the energy which is directly added to the atmosphere is added in a rather inefficient manner from the standpoint of affecting the weather. Meteorologists and others acquainted with the problem are readily willing to dismiss the possibility that the energy released by the explosions can have any important direct effect on the weather processes. However, there remains the possibility that

the explosion will serve as a trigger mechanism to divert some much larger natural store of energy from the path it would otherwise have followed.

Three general means by which this might be accomplished have been considered:

1. The debris thrown into the air by the explosion may have some catalytic effect on the behavior of clouds and thereby change the regime of cloudiness or precipitation over wide areas.
2. The radioactive nature of the debris will change the electrical conductivity of the air, and this may have some effect on more directly observable meteorological phenomena.
3. The debris thrown into the stratosphere by the explosion may interfere with the passage of solar radiation and thereby serve to decrease the temperature of the earth.

Our present knowledge of atmospheric physics makes difficult a final authoritative evaluation of any of these possibilities.

The results of studies and experiments conducted by various organizations show the following:

1. The debris which has been thrown up into the atmosphere by past detonations was found to be ineffective as a cloud-seeding agent. Since the techniques for testing nucleating efficiency are not entirely satisfactory, the condensation and freezing nuclei produced by nuclear explosions and their effect on the formation of clouds and the precipitation process must be continually investigated.

2. The amount of ionization produced by the radioactive material is insignificant in affecting general atmospheric conditions. Various theories on the possible connection between the electrical properties of the atmosphere and the precipitation process are still in the developmental stage.

3. Dust thrown into the air by past volcano eruptions decreased the direct solar radiation received at the ground by as much as 10–20%. The contamination of the atmosphere by past nuclear tests has not produced any measurable decrease in the amount of direct sunlight received at the earth's surface. There is a possibility that a series of explosions designed for the maximum efficiency in throwing debris into the upper atmosphere might significantly affect the radiation received at the ground.

4. Much of the increase in severe storms reported in recent years can be traced directly to the improved methods of reporting severe storms that normally occur.

No statistically significant changes in the weather during the first ten years of the atomic age have been found, yet careful physical analysis of the effects of nuclear explosions on the atmosphere must be made if we are to obtain a definite evalution of this problem. Although it is not possible to prove that nuclear explosions have or have not influenced the weather, it is believed that such an effect is unlikely.

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SUMMARY REPORT OF THE COMMITTEE ON EFFECTS OF ATOMIC RADIATION ON OCEANOGRAPHY AND FISHERIES

1. To Whom Is This Report Addressed?

In writing this report we have had four groups in mind—research administrators, statesmen, scientists and the public. For those who have responsibility for the support of research, we have attempted to outline the scientific questions that need to be answered as a basis for intelligent policy, the means by which they can be attacked by classical research methods at the outset, and the broader problems of the oceans that can be hopefully attacked by the use of radioactive tracers. For the statesmen who have responsibility for national and international policy, we have attempted to formulate recommendations, based on our present small body of knowledge and our awareness of our larger area of ignorance, concerning the national and international actions and agreements that are necessary for the happy exploitation of the oceans in the new atomic age. For the scientists, we have attempted to summarize what is known about the actual and potential effects of radioactive materials in the oceanic realm and the interest of marine

scientists in these substances. For the public, to which we all belong when we are outside our own specialties, we have summarized the levels of calculated risk that must be balanced against the wonderful promise of atomic energy for the welfare of mankind.

2. How Does the Atomic Energy Program Affect the Oceans?

We have considered three aspects of the atomic energy program that directly involve the oceans and, therefore, the marine sciences: weapons tests over or in the sea, disposal of radioactive wastes from nuclear power plants, and the use of radioactive substances in increasing our understanding of the oceans and of the creatures that live in the sea. These different aspects cannot easily be separated. Weapons tests and the disposal of radioactive wastes present great opportunities for studying the oceans. On the other hand, increased knowledge of the oceans is essential to avoid or minimize the destruction of marine resources in the development of atomic energy.

The continuing development of atomic energy will produce progressively greater amounts of radioisotopes, and with them greater amounts of radioactive waste material. Since the oceans cover 71% of the earth, and ultimately receive the drainage from the land, they are the principal reservoir where radioisotopes will finally accumulate. Relatively small quantities are now being added to the surface waters of the ocean as fallout from weapons testing programs, and in a few places as waste materials.

When nuclear reactors for the production of power are put into large-scale operation, as they certainly will be in the foreseeable future, the oceans will be seriously considered for the disposal of large quantities of wastes. Even if direct and intentional disposal at sea is not practiced, reactors may be built along seacoasts or on rivers near large population centers and accidental pollution may occur.

The problem of disposal of radioactive wastes is similar in character to, though potentially far greater in scope than other problems of pollution. An object lesson can be drawn from our experience with the disposal of human and industrial wastes in inland water bodies and coastal waters and with the smog problem that afflicts many of our large cities. During the early stages of the growth of industries and populations in cities, wastes were added to nearby lakes or bays, and to the air, in what seemed at the time to be innocuous quantities. As a matter of fact, the quantities were small enough to be purified by natural processes. In the course of time, however, the quantities increased insidiously so that today many natural waters cannot purify themselves and without expensive treatment are dangerous to humans.

In almost every case the problem was ignored until it had become formidable in magnitude. Short-range solutions were employed, based on inadequate knowledge, special interest, and what we now know was an unfounded confidence in the capacity of the atmosphere and the waters to absorb noxious substances. As a result, unnecessary damage was done to human beings and their environment. Much of this could have been avoided if an adequate program of scientific investigation had been started sufficiently far in advance and if scientifically based policies had been followed.

It is imperative that the nature of the wastes associated with the development of atomic energy be evaluated in advance. We know that purification of waters receiving radioisotope waste will proceed only by dilution, by precipitation and settling on the bottoms, and by the decay of radioactivity. Nothing could be done to reverse an undesirable accumulation that might result from ill-considered disposal of this type of waste.

There is no question of trying to keep all of this material out of the sea. It is certain that some of it can be safely added. Tolerability of materials must be determined, and the locations where they should be put must be wisely selected in terms of the quantity and character of the radioactivity. It is not possible today to see clearly the problems of the future; we can only define the studies that must be made to provide a scientific basis for wise evaluation, and urge that these studies be begun without delay. The costs of such studies may seem large, but they are actually negligible in terms of the potential benefits. They are also very small when compared to the total present expenditures for the development of atomic energy. We cannot wait to begin these studies until radioisotope pollution becomes serious, for it is irreversible.

3. Is There Naturally Occurring Radioactivity in the Sea?

Yes, but one of the remarkable characteristics of the ocean is the extremely low level of the natural radioactivity. Marine animals and plants living more

than a few hundred feet beneath the surface are bombarded by much less natural radiation (radioactivity plus cosmic rays) than is received by terrestrial plants and animals.

For example, although radio potassium accounts for about 90 percent of the activity in the sea, it is present in most igneous rocks at about 100 times the concentration found in the ocean. Uranium, radium, and thorium are 3,000 to a million times more concentrated in rocks than in the sea. This raises an interesting scientific question concerning the character of genetic change and evolution in many marine creatures. It emphasizes the need for basic biological studies on marine organisms. Because of their experimental difficulty, such studies have been comparatively neglected during the past few decades.

4. Have Weapons Tests Added Measurable Amounts of Radioactivity to the Sea?

Yes, though in terms of the total radioactivity of the sea the amount is negligible. Radioactivity in the waters of the test area is of course very greatly increased at the time of tests, and even after diffusion over thousands of miles concentrations remain that are readily detectable. Two days after the 1954 tests in the Pacific the radioactivity of the surface waters near Bikini was observed to be a million times greater than the naturally occurring radioactivity. This material was transported and diluted by ocean currents, and four months later concentrations three times the natural radiation were found 1,500 miles from the test area; thirteen months later the contaminated water mass had spread over a million square miles. Artificial activity had been reduced to about one-fifth the natural activity, but could be detected 3,500 miles from the source.

5. In What Other Ways Will Radioactive Materials be Added to the Oceans?

In England radioactive wastes are being piped into the Irish Sea from an atomic installation. In the United States, wastes from laboratories and hospitals are being carried to sea in containers and dumped. At Oak Ridge, some of the fission products are discharged into the Tennessee River system. At Hanford, water from the Columbia River is used for cooling and returned to the river with some induced short-lived radioactivity. Waste products from the uranium fuel processing plants are now being confined, some in containers, others in pits in the ground. When the power reactors and fuel processing plants reach their expected development many rivers will have to be used. It will not everywhere be practical to confine the wastes locally. Transporting them to sea in barges or by other means may then be necessary in many cases. Although we may be sure the atomic installations will be carefully engineered and maintained, accidental discharge of waste may occasionally occur. On those occasions intense radioactivity may reach the sea.

6. Has the Atomic Energy Program as Yet Resulted in Serious Damage to Marine Life?

Probably no. We know that radioactive radiation is damaging to living things and that marine organisms tend to concentrate many fission product elements. But there is no evidence that any lasting damage has been done to the animal or plant populations of the sea or large inland water bodies by the release of radioactive substances.

Certainly in the weapons test area terrestrial forms were killed or injured by the tests. The evidence concerning marine life is not conclusive, but biologists feel certain that deleterious effects occurred in the near vicinity. There is, however, no evidence that populations have been affected after the dilution and transport mentioned above. This is a subject on which intensive studies are essential before a definite answer can be given. We know that "high" levels are lethal, and that "low" levels may have no direct effect, but we cannot give quantitative values for "high" and "low" except in a few cases. Low levels, which produce no measurable effect in the organism itself, may produce genetic effects and thus influence the marine populations in the future, but there is no conclusive evidence that this will be undesirable.

7. Do Living Things Take Up Radioactive Materials into Their Bodies?

Yes. Radioactive materials added to the sea can remain in solution, precipitate and settle on the bottom, or be taken up by the plants and animals that live in the water. The plants of the sea are mainly microscopic in size, but they can concentrate many thousand-fold those elements that are necessary to them. Radioactive substances are also absorbed on the body surfaces of living things. Small plants and animals serve as food for the larger forms and the radioactive materials are passed on from one to another. The amount of each element accumu-

lated in each form depends upon the rate at which it is taken up, either directly or as food, and the rate it is excreted. Some of the radioactive materials remain in the body for relatively long periods of time and may accumulate to a considerable degree. Others may be lost rapidly and very little will accumulate.

This statement is a great over-simplification. Different plants and animals require and accumulate different elements. Shell fish, for example, concentrate calcium and strontium in their calcareous shells; fish concentrate zinc. It will be necessary to know among other things both the composition of the waste, and the populations in the area, before any particular disposal operation can be evaluated.

8. Are All the Radioactive Elements Equally Harmful?

No. Those elements that living organisms naturally accumulate and that have long radioactive half-lives are more harmful than others. Radioactive strontium, and to a lesser extent, cesium and its daughter barium, cerium, praesodymium and promethium represent particular hazards to human beings from ocean disposal.

9. How Much Radioactive Waste Will be Produced by Nuclear Power Reactors in the Future?

The answer to this depends upon how optimistic one is concerning the development of nuclear power. One estimate assumes that within about 50 years nuclear fission will be producing about half as much power annually as the peoples of the world are using today from all sources.

Accumulations year after year will eventually result in a constant quantity of radioactivity, such that the rate of radioactive decay will balance the rate of production of fission products to give what has been called the steady state. This should be approached within a few decades after full production is reached. The waste radioisotopes at this point would equal between one and two times the total natural radioactivity in the world oceans. This is roughly a thousand times the amount produced so far in weapons tests.

10. What Means Are Being Considered for Disposing of Radioactive Wastes?

The methods being considered fall into two categories, isolation and dispersal. It is probable that a judicious combination of the two methods for different types of wastes or for different countries will be essential. Chemical treatment of the wastes to isolate usable fractions, or those, like strontium and caesium, that decay most slowly, offers promise in simplifying the problem. For isolation, permanent storage in tanks or introduction into geological structures such as salt domes are being studied by other committees. The only place on earth where dispersal can be considered practical is in the ocean. Because it is large and fluid, the ocean could provide immense dilution. Because of its depth, and the stratification of water-masses with differing densities, various degrees of isolation may be possible. It is a prime purpose of this report to emphasize the need for investigation as to whether this possible isolation is adequate.

11. Will it be Safe to Introduce Very Large Quantities of Radioactive Wastes from Atomic Power Indiscriminately into the Sea?

The answer is certainly no, but the strongest negative must be given for coastal waters and for the upper water layers everywhere that are the home of commercially important fishes. These surface waters interconnect and are in continuous motion. Anything added in one spot will, in the course of a few decades at most, be carried to all parts of the world. There is no place in the sea where very large amounts of radioactive materials can be introduced into the surface waters without the probability of their eventually appearing in another region where human activities might be endangered.

It should not be forgotten that the coastal waters enter the harbors and estuaries and would carry any waste materials there with them; and that many of the major fishery resources of the world are concentrated over banks and near coasts, and would become contaminated.

We must also remember that all plants and animals in the sea, from the smallest bacteria to the largest whale, play a part in concentrating, transporting, and dispersing radioactive and other dissolved and suspended materials.

12. Does This Mean that Large Quantities of Radioactive Wastes Should Never be Dumped in the Sea?

No, not necessarily, but it does mean that the length of life of the radioactive material, its role in biological processes, and the mixing rate of the ocean should be carefully studied before large quantities of wastes are introduced into the

sea. Unfortunately, although we know the decay time of most radioactive substances, we know very little about the exchange processes in organisms and in the water. We do know that even the bottom waters of the deep ocean basins slowly exchange with those of the surface, but the rate of this exchange is uncertain.

13. From What Is Known, Where Would be the Safest Place to Dump Radioactive Wastes in the Sea?

At the present time it is only possible to give rough engineering estimates based on order-of-magnitude calculations.

Remembering the importance both of isolation (to allow time for radioactive decay) and dispersal (to reduce the amount of radioactivity per unit volume) the problem is to find places in the ocean where the rate of transfer of radioactive materials to the surface waters would be slow, or where great dilution would occur before radioactive materials came in contact with marine food products or human beings, and preferably where both conditions would prevail.

There are some places where a contaminant could be isolated for long periods. For example, it is estimated that in the deepest parts of the Black Sea the "flushing time" is about 2500 years. This is the time required for most of the deep water to move near to the surface and be replaced with new water mixing downward. In this respect the Black Sea is unique. Elsewhere the "age" of the deep water indicates that exchange with the near surface waters goes on less slowly. Thus in the deeps of the Atlantic and Caribbean the time required for replacement of the water with new water from near the surface is probably only a few hundred years. Some oceanographers believe that the Atlantic deep water sank from the surface in high northern latitudes about 150 years ago.

We are fairly certain that substantial amounts of long-lived radioactive materials, dumped on the bottom in the deep sea, would remain isolated for more than 100 years and that during this period they would become diluted by mixing through an enormous volume of deep water. We do not understand the nature of the physical and biological exchange processes between the deep and surface waters well enough to be able to say whether in the steady state, after decades of nuclear power production, deep sea disposal would give adequate protection of the commercial fisheries from long-lived fission products such as strontium. Large quantities of short-lived fission products could certainly be disposed of safely in this way.

14. Can Radioactive Materials be Used to Learn About the Oceans and to Increase the Harvest from the Sea?

Yes. For example, an understanding of the flow of material through food chains is essential to the effective use and conservation of the food resources of the sea. The natural elements used by the marine plants and their transfer to the commercially valuable fish and shellfish can be studied on a large scale, using radioactive isotopes. As these readily detectable substances are traced through the various steps of the food chain—plants, animal plankton, small fish, large fish—the efficiencies and inter-relationship of the various levels should become much better known. This knowledge is of fundamental importance for the evaluation of the potential of the living resources of the sea as a source of food and other marine products, and as a basis for their full utilization and conservation.

Radioactive materials, both natural and man-made, can also be used in the study of oceanic mixing processes and circulation. These processes serve to supply marine plants with the fertilizers they need from deeper waters, as well as to dilute and disperse radioactive wastes dumped in the sea. At present we cannot measure, but can only estimate the mixing rates. The ability to trace radioactive materials, even though present in great dilution, will permit us to obtain quantitative information. Improved knowledge of the mixing processes and of currents will help man to locate and evaluate unexploited resources of fish and other food organisms.

For example, thirteen months after radioactive materials were introduced into the seat fallout from weapons tests in the Marshall Islands, a research vessel traced their distribution in the Western Pacific. The extent to which radioactivity was taken up by plankton and fish was measured, as well as the extent to which activity was mixed downward and transported westward in the western limb of the great North Pacific eddy. These measurements showed the average speed at which materials were carried away from the test area, giving convincing proof of the transport and mixing of material over a vast region.

Large amounts of radioactive tracers ranging in magnitude from curies to megacuries can be used at sea in studying oceanographic problems, including the problems of fisheries, and thus laying the ground work for increasing our harvest from the ocean. Smaller amounts are needed in the laboratory. We are here concerned not with the general problems of physiology and biochemistry but with specific ecological studies, including investigations of the efficiency of transfer of energy along the food chain, rates of filtration, concentration of elements and compounds in various tissues, the rates of accumulation and excretion of elements and compounds, the passage of substances across biological membranes, the concentration and role of biotic and antibiotic substances in the sea, the dynamics of marine populations, including the mass of living material in a given volume of water, the flux of organic substances from one organism to the other and between the organism and the sea water, and the inter-relations of animal and plant communities. In both field and laboratory experiments fission products are useful but some problems require the use of artificially radioactive substances produced by other means. An outstanding example is the use of carbon 14 to study the efficiency of various steps in the food chain. Large quantities of this material are needed for field studies in restricted water bodies. Though the cost would be high, the value of the results would more than justify the expenditure.

CONCLUSIONS AND RECOMMENDATIONS

1. Tests of atomic weapons can be carried out over or in the sea in selected localities without serious loss to fisheries if the planning and execution of the tests is based on adequate knowledge of the biological regime. The same thing is true of experimental introduction of fission products into the sea for scientific and engineering purposes.

2. Within the foreseeable future the problem of disposal of atomic wastes from nuclear fission power plants will greatly overshadow the present problems posed by the dispersal of radioactive materials from weapon tests. It may be convenient and perhaps necessary to dispose of some of these industrial wastes in the oceans. Sufficient knowledge is not now available to predict the effects of such disposal on man's use of other resources of the sea.

3. We are confident that the necessary knowledge can be obtained through an adequate and long-range program of research on the physics, chemistry, and geology of the sea and on the biology of marine organisms. Such a program would involve both field and laboratory experiments with radioactive material as well as the use of other techniques for oceanographic research. Although some research is already underway, the level of effort is too low. Far more important, much of the present research is too short-range in character, directed towards *ad hoc* solutions of immediate engineering problems, and as a result produces limited knowledge rather than the broad understanding upon which lasting solutions can be based.

4. We recommend that in future weapons tests there should be a serious effort to obtain the maximum of purely scientific information about the ocean, the atmosphere, and marine organisms. This requires, in our opinion, the following steps: (1) In the planning stage committees of disinterested scientists should be consulted and their recommendations followed, (2) funds should be made available for scientific studies unrelated to the character of the weapons themselves, and (3) the recommended scientific program should be supported and carried out independently of the military program rather than on a "not to interfere" basis.

5. Ignorance and emotionalism characterize much of the discussion of the effects of large amounts of radioactivity on the oceans and the fisheries. Our present knowledge should be sufficient to dispel much of the over-confidence on the one hand and the fear on the other that have characterized discussion both within the Government and among the general public. In our opinion, benefits would result from a considerable relaxation of secrecy in a serious attempt to spread knowledge and understanding throughout the population.

6. Sea disposal of radioactive waste materials, if carried out in a limited, experimental, controlled fashion, can provide some of the information required to evaluate the possibilities of, and limitations on, this method of disposal. Very careful regulation and evaluation of such operations will, however, be required. We, therefore, recommend that a national agency, with adequate authority, financial support, and technical staff, regulate and maintain records of such disposal, and that continuing scientific and engineering studies be made of the resulting effects in the sea.

7. We recommend that a National Academy of Sciences-National Research Council committee on atomic radiation in relation to oceanography and fisheries be established on a continuing basis to collect and evaluate information and to plan and coordinate scientific research.

8. Studies of the ocean and the atmosphere are more costly in time than in money and time is already late to begin certain important studies. The problems involved cannot be attacked quickly or even in many cases, directly. The pollution problems of the past and present, though serious, are not irremediable. The atomic waste problem, if allowed to get out of hand, might result in a profound, irrecoverable loss. We, therefore, plead with all urgency for immediate intensification and redirection of scientific effort on a world-wide basis towards building the structure of understanding that will be necessary in the future. This structure cannot be completed in a few years; decades of effort will be necessary and mankind will be fortunate if the required knowledge is available at the time when the practical engineering problems have to be faced.

9. The world-girdling oceans cannot be separated into isolated parts. What happens at any one point in the sea ultimately affects the waters everywhere. Moreover, the oceans are international. No man and no nation can claim the exclusive ownership of the resources of the sea. The problem of the disposal of radioactive wastes, with its potential hazard to human use of marine resources, is thus an international one. In certain countries with small land areas and large populations, marine disposal of fission products may be essential to the economic development of atomic energy. We, therefore, recommend: (1) that cognizant international agencies formulate as soon as possible conventions for the safe disposal of atomic wastes at sea, based on existing scientific knowledge; and (2) that the nations be urged to collaborate in studies of the oceans and their contained organisms, with the objective of developing comparatively safe means of oceanic disposal of the very large quantities of radioactive wastes that may be expected in the future.

10. Because of the increasing radioactive contamination of the sea and the atmosphere, many of the necessary experiments will not be possible after another ten or twenty years. The recommended international scientific effort should be developed on an urgent basis.

11. The broader problems concerned with full utilization of the food and other resources of the sea for the benefit of mankind also require intensive international collaboration in the scientific use of radioactive material.

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REPORT OF THE COMMITTEE ON THE EFFECTS OF ATOMIC RADIATION ON AGRICULTURE AND FOOD SUPPLIES

I. GENERAL

The Committee interpreted its task as requiring its members to survey the scientific aspects of that great sequence of events which precedes the delivery of food items to the ultimate consumer, and to do so from two separate viewpoints. These were (1) the beneficial effects that may result from the deliberate involvement of radiation of any sort with constructive intention, or what has been spoken of so frequently as the "peaceful uses of atomic energy," and (2) the harmful or disadvantageous effects of radiation of any sort due to nuclear warfare, to accidents involving atomic power plants, or even to a slowly rising background of radiation that conceivably may follow as a result of atomic technological developments in industry.

Public and private funds are currently being expended in the United States for research in agriculture and food processing at a rate in the vicinity of 300 million dollars annually. And undeterminable but not insignificant fraction of this considerable body of research involves radiation or radioisotopes. Members

of the Committee did not believe it to be incumbent upon them to defend or justify, to criticize or to challenge applications of atomic radiation to agriculture that have been developed or are under discussion. They did not wish to evaluate the programs of particular agencies or groups, but instead with judicial mind to examine the accomplishments and the potentialities, the implications and the limitations of radiation as related to the production and processing of agricultural products.

One broad conclusion is that there is not imminent any drastic change in *agricultural production* as a result of the application of radiation. However, radiation techniques provide new tools for research and may aid agricultural production by improving and enhancing the efficiency of production methods.

The Committee is strongly of the view that the applications of radiation will be of far greater immediate consequence to agricultural research than directly to agriculture, and that most of the benefits that may arise to agriculture, as manifest in the availability of an adequate and varied supply of wholesome food for man, wherever he may be, will come as a summation of many improvements, small and large, in materials, in plants and animals and in the technology of husbandry and processing developed through programs in agriculture and food processing research.

Changes therefore may be expected to come in a series of little steps, none of which in themselves may be of great impact, but which, through the years, are likely to be impressive in their total.

Another broad conclusion is that the slowly rising background of radiation caused by weapons testing in peacetime at the present rate is not likely to impair or interfere with food production. Levels of radiation considered tolerable by man are below those believed to have effects in plants or animals that would place food production in jeopardy. However, the high levels of radiation which might develop in small or large areas as a result of atomic or thermonuclear weapons in wartime, or from mishaps with nuclear power plants in peacetime could have catastrophic effects on agricultural production that might be of long duration, because of injury to personnel and animals, disruption of services, and contamination of soil, vegetation and water supplies.

II. TRACER STUDIES IN AGRICULTURAL RESEARCH

In the consideration of the beneficial effects of radiation the Committee endeavored, not wholly successfully, to separate in its thinking those benefits that may arise from additions to the pool of basic knowledge about plants and animals and their welfare, from those more direct effects that may specifically result from the exposure of plants, animals or agricultural products to radiation. Tracer studies in the biological sciences have already been enormously fruitful in aiding the elucidation of essential metabolic processes in plants and animals, and may be expected to be increasingly so as the number and diversity of such experiments increases. When there is knowledge and understanding of a process then comes the opportunity to control it for a desired end; in this way the art of agriculture is transformed to the science of agriculture.

They endeavored to make the separation mentioned above because of the conviction that there is nothing unique about radioisotopic studies as applied to agricultural research. Tracer techniques, however, frequently permit answers to be obtained to questions which seemed previously unanswerable by conventional experimentation. The involvement of isotopes puts a new dimension into metabolic studies, and areas, formerly dark, may now stand out in relief.

It is worthy of comment that many of the applied problems involved in the arts or technology of agriculture are as susceptible to study by procedures involving radioisotopes as are those more basic questions of plant and animal physiology or nutrition. Excellent examples of this type of employment of isotopes are to be found in work on the placement and recovery of phosphorous fertilizers in soils, the efficiency of various methods of application of insecticides, fungicides and herbicides, the determination of post-harvest residues of such chemicals, the extent of utilization of feed components by animals, etc. It is to be anticipated that there will be greatly increased use of tracer radioisotopes in the solution of such applied problems, and that the immediate dividends from such research may be considerable. Further, it is likely that new methods of employing isotopes advantageously will be developed; the ingenuity of investigators in this field should not be underestimated.

Because of the unanimity of their views as to the enormous potentialities of isotope tracers as a research tool in agricultural science and biology generally, the Committee gave some consideration as to whether there are limitations in

facilities for training or funds for specialized equipment for such studies. The consensus seemed to be that motivation for the use of such techniques must come from individual investigators themselves, that the necessary know-how is to be found in almost all research institutions, and that progress in agricultural research is not at the moment limited by inadequacies in dissemination of knowledge and techniques. There was, however, a feeling that much of the graduate training in this field is rather informal, that more universities might consider establishing courses in which the methodology, techniques and principles of this new and powerful science are expounded, and that there is an additional need for an advanced training program for specialists in radiochemistry and radiobiology who may be developers of new techniques or interpreters of new applications of potential value in agricultural research.

III. EFFECTS OF RADIATION ON CROP PRODUCTION

It is abundantly established that mutations can be induced in many plant species by exposure to x-radiation, gamma radiation and other forms of radiation. The changes which result are possibly due to chromosome deletions or abberations. There is some difference of opinion as to whether radiation-induced mutants intentionally obtained are qualitatively identical with those which occur spontaneously from naturally occurring mutagenic agents, but there is no doubt that their frequency is increased. Even so the mutation rate in most species is still very small, and furthermore most mutations are disadvantageous. The investigator seeking to exploit this phenomenon must expect to have to handle very large populations, and so far has been able to look only for desirable changes that are reflected in morphology or appearance and therefore can readily be seen, or for changes which can be recognized by some blanket method such as inoculating all irradiated plants with disease organisms in the hope of finding one or more exhibiting resistance to infection.

It is likely that characters at present unrecognized also undergo change and that there are unexplored potentialities for effecting improvement in quality that may alter the demand for the plant, or in physiological properties that may alter the relationships of the plant with its environment.

It would be a mistake to imply that this new development has greatly simplified the tasks of those involved in crop improvement. On the contrary, it has made them more complex, but, by extending the boundaries, offers many new possibilities. It is not to be expected that acceptable new agronomic varieties can be obtained by simple irradiation of present varieties, though this is possible if large enough populations are examined. In general, however, back-crossing and recombination are needed to add the new characteristic to a crop plant acceptable in other respects.

As yet relatively few new varieties of economic plants, developed from radiation-induced mutants, have actually been introduced and widely planted. These, however, do attest to the potentialities of the procedure. Much of the research effort in this field has properly been devoted to the investigation of techniques, to such vital questions as the determination of the particular stage of development at which radiation exposure may be most effective, and the comparative mutability of crop species. It appears that different species cannot be expected to respond in an identical manner. More perhaps is known about this aspect of corn genetics than of any other major crop plant.

Mutations in micro-organisms may similarly be induced by exposure to various types of radiation, though at considerably higher radiation levels than with crop plants. The changes induced have been shown to include the degree of virulence and host range of certain pathogenic fungi. The suggestion has repeatedly been made that the plant pathologist should examine this phenomenon so as to anticipate disease-resistance requirements in a breeding program. As yet, however, there have been no significant results along these lines. Considerable success has been achieved in the development of greatly enhanced antibiotic production by some molds through radiation-induced mutation and selection. Similar genetic changes in the case of other micro-organisms have produced information about the likelihood of genetic control of metabolic processes.

There is considerable evidence that bud mutations or somatic mutations can be induced by radiation, and that this phenomenon can be exploited in the development of new strains of crop plants that are normally propagated by cuttings and grafting. This may be of special value in the improvement of some such crops, but as yet there have been no striking accomplishments in this direction. Progress in such studies is however inevitably slow because of the

nature of the materials, the length of time necessary to recognize a desirable change, and to produce the stocks necessary for field evaluation.

Since the mutation rate of plants may be enhanced by radiation, presumably there is some possibility of the appearance of undesirable mutants in areas where the background radiation becomes higher than normal for any reason. This may be of some significance in connection with waste disposal practices or atomic accidents. There is, however, no evidence of such changes in areas containing radioactive springs or ores. This may be due to lack of intensive examination of the vegetation of such areas, and such surveys are to be encouraged. However, the likelihood of appearance of undesirable lines under radiation levels that would be tolerated on other grounds seems small.

There is no evidence that plant growth is stimulated or crop yields increased by exposure to low levels of radiation, despite earlier well-publicized claims to this effect. Radioactive fertilizers, used in a conventional manner, produce yield increments no greater than expected from ordinary fertilizers.

Plants accumulate nutrient elements present in the root zone in solution or absorbed onto soil colloids, but non-nutrient elements are not excluded and may similarly be taken up. The availability of radioisotopes has greatly improved the understanding of plant nutrition and soil-plant relationships, and may be expected to aid substantially in the improvement of cultural practices, as indicated earlier. Through the use of isotopes it has been demonstrated unequivocably that certain elements can enter the plant through the leaves. This is of some consequence in relation to fall-out. Radioisotopes of long life or high activity if deposited in fall-out from an atomic or thermonuclear incident are likely to be accumulated in crop plants by root uptake from the soil and entry through the foliage. Some of the products deposited may be initially quite insoluble, but may become soluble through weathering. Others, initially soluble, may be irreversibly fixed by many soils in a form not readily available to crops. It appears at present that Sr^{90} and I^{131} are the chief radioactive elements which are of concern in such circumstances. The subsequent use of such crops presents a great diversity of problems depending on the level of radioactivity, its nature and the specific use of the crop. The Committee was interested to learn that the Department of Agriculture is preparing for farmers some informational material relating to these problems.

The Committee desires to examine further the available information on the inter-actions of fall-out components with soil their entry and accumulation in crop plants in order to determine whether there is available the necessary basic information from which appropriate agronomic recommendations could be formulated for agricultural operations in areas that may have undergone any likely level of contamination.

IV. EFFECTS OF RADIATION ON ANIMAL PRODUCTION

Whereas it appears that crop improvement programs may be considerably aided by the availability of radiation-induced mutants that may have certain desirable characteristics capable of incorporation into an agronomically acceptable variety, currently available evidence does not suggest that a similar approach with animals would be so rewarding. This statement is made not from a belief that farm animals are inherently less responsive to radiation than plants but because physical differences of size, cost, generation time, etc., militate against extensive studies with animals, and act as obstacles that cannot readily be overcome. Probably only with poultry and to a lesser degree with swine would it be possible to handle large enough populations, and even here, if one extrapolates from the smaller laboratory animals, the chances of improvement seem slim. At present one such study, with chickens, is known to be underway.

Limited whole-body exposure studies with farm animals have primarily been carried out to investigate physiological and pathological changes, often with the intention of transferring the information by analogy to problems of responses in man. The sequence of changes induced in most farm animals by heavy radiation exposures has been well defined. There are one or two examples however of the use of radiation exposure as a research tool for inhibiting certain functions in animals. For example various functions in the oviduct of poultry can be blocked by proper radiation techniques thereby permitting a study of the contribution made by the parts of this organ.

Much of the work with radioisotopes in the animal field centers around problems of animal nutrition and metabolism, and substantial progress has been made both in the elucidation of fundamental problems of animal physiology as

well as in those of a more applied character, such as the utilization of feed constituents, and the incorporation in animal tissues of inorganic constituents of forages. The experimenters in this field at present encounter one serious difficulty, which in the case of the larger farm animals greatly limits the scale of activity. This is the problem of the salvage or disposal of animals after use in experiments involving radioisotopes or radiation exposure. Even in the case of short half-life isotopes and at tracer levels only, the animals cannot be marketed through the usual outlets. This problem is of course much more serious with dairy or beef cattle than with hogs or poultry because the cost to the program is so much greater. Moreover, this limitation tends to restrict undesirably the scale and scope of such experiments, with the result that the conclusions may be less surely established than if the numbers of animals used were larger.

It appeared to the Committee, therefore, that essential research on farm animals using radioisotopes or radiation is being discouraged by the high costs involved because animals must be destroyed at the termination of experiments. It recommends that a special committee be appointed to study this problem and to develop procedures and standards that, if followed and enforced, would adequately protect the consumer, but permit the marketing of animals that in experimentation have been brought into contact with radioactive substances or exposed to radiation.

The welfare of the livestock population is enhanced if troublesome insect pests can be controlled or eradicated. As mentioned earlier, insecticide studies have been greatly aided by the availability of radioisotopes as tracers, but in addition there may be certain opportunities for control of insect pests by taking advantage of radiation-vulnerable stages in their life cycles. Eradication of the screw worn fly from the southeastern United States is to be attempted, based on the virtual elimination of this fly from the island of Curacao by the release of males rendered sterile by radiation exposure. This technique may not be generally applicable to all insects pests.

V. RADIOISOTOPES IN AGRICULTURAL PRODUCTS AND FOODS

The Committee discussed in detail some of the difficult problems that may arise because of the presence of a radioisotope burden in agricultural products and foods higher than that "naturally occurring." The applicable legislation in this area is clouded with uncertainties, because the very possibility was not envisaged by those who enacted the laws and defined the responsibilities of the agencies that protect the public food supply. There are no permissible limits for radioisotopes in foods; any burden above the "natural" is regarded as undesirable. The current interpretation of the law places isotopes in the same category as poisonous additives. It is difficult, however, to be wholly consistent in this, inasmuch as the normal radioisotope burden varies considerably in different agricultural products, and in the same product from different locations. Moreover, the testing of atomic and nuclear weapons is placing in soil, water, and air, the world over, radioisotopes not formerly present, though at extremely low levels. Th "natural content" of foods now consumed by animals and man is not the same as in the pre-atomic age. Though extremely small, the increment is measurable, and inescapable.

It is to be anticipated that there will be in the years ahead a slowly rising background of radiation manifest in agricultural and food products by the presence of the isotopes of elements not previously found therein or of "unnatural" levels of radioactivity. Atomic warfare might greatly increase the rate of this development. As pointed out earlier in this report, radiostrontium is particularly the element which would cause concern in the latter event. Forage directly contaminated with fall-out, if consumed by farm animals soon after deposition might cause radiation injury from the presence of insoluble radioactive products. Strontium is metabolically similar to calcium and moves into bone and other calcium-accumulating tissues or fluids. Much is known of the relative behaviors of calcium and strontium but there appears to be no way of wholly preventing strontium retention. There is some evidence that poultry may "decontaminate" or "detoxify" themselves by reason of a continued dilution through transfer to eggshell. In meat animals certain tissues might be consumable if boned out, but such an expedient would be beyond the ordinary scope of meat inspection. Dairy products would contain radiostrontium for some considerable time after ingestion of strontium-containing forage. Moreover, all avail-

able feeds, in heavily contaminated aras, might contain significant levels of radiostrontium, perhaps for years.

At present it is not possible to say at what level a food, otherwise wholesome, becomes unwholesome or deleterious by reason of the presence of an unnatural burden of radioactivity. There is a great deficiency of requisite data on the long-term biological effects that may follow the ingestion of such foods by animals and man. Situations in which such information might be of great public importance are not inconceivable and possibly inevitable.

The Committee therefore urgently recommends that appropriate experimentation be immediately activated to provide specific information about possible total or cumulative biological effects that might follow the ingestion of such foods. It further urges that the planning of such experiments be broadly based, and that the development of the experimental designs and details of their subsequent execution be most carefully considered in order that the emerging data will be acceptable as a basis for the crucial decisions that ultimately will have to be taken, and directly of value to the regulatory agencies charged with the protection of the public interest.

VI. ENVIRONMENTAL CHANGES AND ECOLOGICAL STUDIES

In the decades ahead there is a strong possibility that the general background of radioactivity in agricultural areas will rise. Contributing to this would be fall-out, if weapons-testing continues, and wastes from nuclear power plants or isotope processing plants. As indicated in the report of another Committee every effort will have to be made to contain radioactive wastes. Atomic warfare, or accidents involving nuclear power sources could of course greatly augment the background and pose difficult problems of land-use for agricultural purposes. Limited ecological studies are in progress in the vicinity of certain A. E. C. installations, but it may be wise to consider this general problem somewhat more widely and to attempt to establish, through careful sampling, the present background in representative agricultural areas, and in their chief crop and livestock products.

Research activities might appropriately be carried out on areas near weapons test sites where substantially greater changes in background would be anticipated. The distribution in the environment, in the soil at various depths, in the vegetation, in the wildlife, in the streams, etc. would all be pertinent. The rate of accumulation in soil as affected by land use ought to be studied. Forested land, rangeland, rotation grassland, and plowland, irrigated and non-irrigated, may each present a different situation. It is possible that certain of the State Agricultural Experiment Stations might be in a position to undertake limited surveys of this type on areas likely to be under their control for some considerable time in the future.

The Committee recognized clearly that sustained monitoring and ecological research activities of this type are expensive and are not apt to be professionally rewarding to the individuals participating therein, because trends and conclusions would emerge only slowly. However, to be able to recognize changes in the levels of radioactivity in the environment and in products removed therefrom, and to follow movements in the system, may well be in the public interest from a long-range viewpoint.

VII. EFFECTS OF RADIATION ON PLANT OR ANIMAL PRODUCTS (FOOD PROCESSING)

A recent development in food technology, potentially of considerable and possibly dramatic significance, is the recognition of the fact that radiation can be used as a means of preserving certain foodstuffs or of lengthening shelf life, either unrefrigerated or refrigerated. The radiation source may be gamma rays or high energy electron beams. No radioactivity is induced in the irradiated material. Feeding experiments to date indicate that foods so irradiated will prove to be suitable and safe for consumption by man. Parasites in meat and meat products can be killed by exposure to penetrating radiation; and undesirable post-harvest changes in plant products, such as the sprouting of potatoes, can be delayed.

The prime objective in radiation processing is to destroy microorganisms, or so greatly to reduce the microbial population (radiation pasteurization) that spoilage is long delayed. To accomplish this, very heavy radiation exposures are necessary because microorganisms are much less sensitive to radiation than are animals and higher plants. The food processor is particularly attracted by

the fact that the radiation exposure can and should be carried out after packaging.

The acceptability of some radiation sterilized foods is open to doubt because of the development of off-flavors, and changes in odor or in the texture of the tissues. Much of the developmental work in this field however has been of a rather empirical nature, and it is possible that through research means may be found to repress some of these undesirable changes.

Although the feasibility of radiation sterilization has been amply demonstrated, the economics of the various processes have not yet been established. This development has largely been financed by the military with the Army Quartermaster Corps as the primary agency involved, but there has been a broad basis of cooperation in industry and elsewhere, with some technical guidance and evaluation by Advisory Committees of the National Academy of Sciences. Having in mind the magnitude and coherence of the current broad programs in this area the Committee was of the opinion that the potentialities of this use of radiation are being thoroughly explored, and that the interests of the food consumer will be adequately protected. At a later date the Committee expects to review particularly the evidence of wholesomeness and acceptability of irradiated foods.

VIII. COMMITTEE MEMBERSHIP

The names and institutional affiliations of members subscribing to this report are listed below. In their deliberations they were aided by Douglas M. Whittaker of the Rockefeller Institute and Charles I. Campbell of the staff of the National Academy of Sciences-National Research Council. As Consultants the Committee is indebted to A. J. Lehmann, Food and Drug Administration, Robert Somers, Meat Inspection Service, U. S. D. A., J. Wolff, Atomic Energy Commission.

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Summary Report of the Committee on Disposal and Dispersal of Radioactive Wastes

INTRODUCTION

Experience in handling the waste disposal problems to date is mostly limited to conditions as they exist in the areas of the national atomic energy establishments. The determination of hazards from the disposal of wastes in these areas, most of which are in remote and somewhat isolated regions, involving relatively short periods of time, has to date revealed no deleterious effect on the public or its environment.

This does not provide, however, a completely adequate basis for projecting the magnitude of the hazard into the vastly expanded realm of industrial atomic power production. Not only does the problem itself take on new significance with the projected amount of wastes, but environmental factors which may lie dormant under conditions existing in the remote areas take on full blown importance when viewed under the more stringent requirement for highly populated areas.

Many such problems immediately come to the surface as a result of consideration of the long-term legal and insurance aspects. These problems reflect first of all a need for deeper understanding of the basic issues and for more refined measurements, and not merely for greater but still unknown factors of safety. Long-term responsibilities, moral, legal, and financial, stemming from the ownership of atomic wastes simply come into sharp focus when it is emphasized that the radioactive life of the wastes would probably exceed by several centuries the official life of the organization itself. Legal and insurance requirements, therefore, will undoubtedly have a great deal to do with the shaping of rigid administrative policies with respect to these long range aspects of the atomic waste disposal problem. It may be difficult to maintain an adequate balance between objectives which primarily must emphasize the legal requirements and those

which in the broad biological sense must establish the foundations for a truly preventive approach to this problem.

PRESENT STATUS OF PROBLEM

The following listing summarizes the conclusions regarding the status of waste dispersal and disposal operations:

1. The safe handling and ultimate disposal of radioactive wastes is an important technical, economic and administrative aspect of the nuclear energy industry. Waste operations must be thoroughly integrated with all other phases of nuclear energy operation.
2. From a technological standpoint the highly radioactive wastes resulting from the processing of reactor fuels constitute the bulk of the problem. To date essentially none of those wastes has been disposed of, i. e., returned to the environment. Tank storage is presently utilized as an interim answer to this problem.
3. Wastes resulting from normal reactor operations are an important consideration, but technically represent a problem for which solutions are generally available.
4. Research and development have indicated possible feasible systems for ultimate controlled disposal of highly radioactive wastes, but considerably more work is required to bring these systems to the point of economic operating reality.
5. Major technical and economic considerations underlying the waste problem are:
   a. Characteristics of nuclear fuels and chemical (or other) processing associated with them.
   b. Separation of specific isotopes from the wastes and use of these materials to economic advantages.
   c. The proper selection of the site for nuclear facilities—especially reactor and fuel processing plants.
   d. The detailed quantitative evaluation of the environment in order to assess its capacity to receive radioactive materials without creating deleterious effects on the environment.
   e. Systems for the physical handling and transportation of highly radioactive materials.
6. Major policy and administrative considerations relevant to the regulation of the waste problem are:
   a. The establishment, perhaps through private enterprise, of suitable waste disposal services.
   b. The regulation and control of waste disposal practices through existing and traditional state, interstate and local channels where feasible.
   c. Continuation and strengthening of established practices in relations with the public and its agencies.

RELATION TO NUCLEAR INDUSTRY GROWTH

Based on the best estimates available (which vary over rather wide ranges) and, to a substantial extent on technical judgment, the indications are that the principal source of fission products from nuclear reactors in the next decade will arise from the generation of electricity at nuclear powered central stations. On the basis of present developments, the second most important source probably will be reactors for naval service. Compared with these, other sources are comparatively small and amount to substantially less than the uncertainty in the estimates of the principal uses.

By 1965 the average rate of reactor heat release is estimated to be about 11,000,000 kilowatts. Naval service will account probably for 20 percent of this output in 1965. This rate of heat release will result in the production of somewhat over 10 kilograms of fission products per day in 1965.

In addition, the presence of radioactive wastes in quantity will have a profound effect on certain non-nuclear industries which may be damaged by air or water contaminated with radioactive wastes. Numerous wet-processing industries are likely to be detrimentally affected by radioactive wastes even in trace concentrations. Among this vulnerable group are those requiring water of the highest purity, such as for the manufacture of photographic film. Other industries which should be alerted to the problem are pharmaceutical manufacturers and food processing companies. It is not possible, at this time, to enumerate with assurance the industrial processes which can be completely

eliminated as subjects of this potential hazard, without the assembly of extensive research and statistical data applicable to specific operations.

RELATION TO FUEL PROCESSING AND TYPES OF REACTORS

Neither the type of fuel nor the length of irradiation time greatly influence the accumulated total radioactivity of fission products. Atfer approximately three years decay the residual radioactivity is essentially the same for various irradiation times, assuming constant heat generation during the irradiation period.

Essentially all of the radioactive material from fuel separations processes must be kept from the environs to maintain human exposures within maximum permissible limits. An important problem which possibly limits storage volume is the rate of heat removal from the containers. After solvent extraction wastes are concentrated by supplied heat to about 2000 gallons per ton of irradiated uranium, the heat of radioactive decay will continue the concentration to 100–500 gallons per ton. Practical heat removal mechanisms may require that more concentrated waste produced by other separations processes be diluted to the same volume range. More concentrated fluid wastes also need stronger, less economical containers. The volumes of stored waste accumulated by 1980 are estimated at 20 x 10^7 gallons, by 1990 at 60 x 10^7 gallons and by 2000 at 240 x 10^7 gallons.

The future possibility of high burn-up of reactor fuels might ultimately result in a situation where processing may be unwarranted. This would not change accumulation of fission products, but would have a profound effect on waste storage and disposal considerations. Similarly, the development of non-aqueous chemical processing methods would be important in modifying the waste management problem.

ISOTOPES PROBLEMS

The technical and administrative problems associated with the transport, use and disposal of radioactive materials in medicine, biology, and industry will undoubtedly grow in complexity and quantity as the demand for the use of these radioactive materials increases. The expanding demand is already apparent in the rapidly increasing number of individual isotope users as evidenced by the expansion of the isotope distribution program. The program for the distribution of reactor-produced radioisotopes is nearing one decade, having been initiated on August 2, 1946. During this period more than 100,000 shipments of radioisotopes have been made from AEC facilities to some 3,200 institutions throughout the United States. These materials are being applied in science, agriculture, medicine and industry. The Oak Ridge National Laboratory, the principal radioisotope production facility in the United States, has shipped approximately 130,000 curies to date.

All indices of radioisotope utilization reveal continued rapid growth. A look at the last three years of the program shows a growth in the number of using institutions from 1,400 to 3,200. This is an increase of approximately 125%. There has been a 100% increase in annual numbers of shipments made since January 1, 1953. The principal growth during the period has been in the industrial use of radioisotopes.

However, of even greater significance in connection with environmental and hazard control problems is the ever increasing desire for larger and larger individual sources of radioactivity. Requirements for intense radiation sources are obviously at their earliest stages. Such uses as food and pharmaceutical sterilization, promotion of chemical reaction, and other yet unknown applications will undoubtedly result in a much more extensive use of mobile and more widespread sources of intense radiation.

Increased use, especially of highly active materials and the increase in the production of by-product materials at widely scattered geographical locations will result in ever increasing new technical and especially administrative problems in both the transport of the material and the disposal of the wastes, in order to protect the environment against normal and potential emergency hazards.

Compliance with existing transportation regulations present few significant problems in the shipment of by-product material even through certain specific limitations exist. However, consideration should be given to a complete critical review of existing ICC, Civil Air, Coast Guard and Postal regulations to bring them in line with current requirements and radiation safety knowledge.

The radiological health and safety record in the nation-wide use of radioisotopes is excellent. Incidents which have come to the Atomic Energy Commission attention involving significant overexposure of personnel are exceedingly small; fewer than 10. In large measure this may be attributed to active educational efforts in radiological protection through a field advisory service to isotope users and through effective and practical licensing practices.

At present activity levels of use of radioisotopes and with the wide dispersal of users substantial environment health problems do not exist due to waste disposal or other practices resulting in the introduction of radioisotopes into the environment.

ITEMS REQUIRING FURTHER STUDY

The following listing summarizes conclusions in this area:

1. Geophysical and geochemical aspects of ultimate disposal of highly radioactive wastes.
2. Site selection for various nuclear facilities, particularly chemical processing plants and their location with respect to suitable waste disposal areas.
3. Transportation of highly radioactive materials.
4. Relationship of introduction and development of nuclear facilities to basic public health, social and economic situations extant or resulting from such development.

PROBLEMS OF ACCIDENTAL HAZARDS

The following conclusions in respect to the consequences of accidents involving radioactive materials appear warranted:

1. The problems of waste disposal could be international in character and must be solved technically so that the total environment is maintained at a low level of radioactivity in order that accidents that are bound to occur will not be disastrous.
2. The type of accident that could result in a catastrophic spread of radioactive materials is the complete vaporization of the core of a reactor and its release to the surroundings. The probability of a catastrophic accident with a properly designed nuclear reactor is extremely small.
3. Reactor waste processing plants or storage facilities offer a greater hazard on a long-term basis than any single reactor.
4. Accidents in handling, transport, and chemical separation of radioactive materials, while locally severe, should not affect a wide public area and, in all cases, the contaminated areas can be cleaned up.
5. The probability of accidents in handling radioactive isotopes and low-level radioactive materials is similar to that in handling other types of lethal substances.
6. Use of nuclear reactors to drive ships appears feasible from a consideration of the consequences of possible accidents provided uranium-233 and plutonium are kept to a minimum. The technology of the use of nuclear reactors to drive locomotives and commercial airplanes has not developed to the point where the committee can form a judgment as to the consequences of possible accidents.
7. Development of improved methods to limit the volumes of wastes produced in nuclear power reactors is justified from the viewpoint of the hazards due to possible accidents.
8. Continuous and vigorous appraisal of reactor and fuel processing plants design and operation and waste storage will be required in all nations using atomic energy in order to keep the radioactivity level of the world environment at tolerable levels.
9. Improved safety devices for control of transients in nuclear reactors should continue to be vigorously developed.
10. Further tests are required of reactors to evaluate their ability safety to withstand power excursions which may occur as a result of unusual operating circumstances.
11. Until such time as advances in the technology of reactors lessen potentially hazards substantially, sealed buildings properly designed, constructed, and tested should be required for all nuclear reactors to be built in or near populated areas.
12. All operations involving radioactive materials in sufficient amounts to create possible health hazards should be supervised by trained and responsible people.

FALL-OUT CONSIDERATIONS

It is apparent that as of the present time the dispersal of radioactive material resulting from weapons testing has not been an environmental contaminant of substantial public health significance. However, because of various unknown factors regarding distribution and ultimate fate of this material, plus the potentials of possible wider spread and more frequent weapons testing it is also apparent that the subject in all of its aspects merits meticulous and continuing attention. The problem of fall-out is one of international significance and should be studied and evaluated on that basis, perhaps looking forward to international cooperation in control.

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THE NATIONAL ACADEMY OF SCIENCES—NATIONAL RESEARCH COUNCIL

The National Academy of Sciences—National Research Council is a private non-profit organization of scientists, dedicated to the furtherance of science and to its use for the general welfare.

The Academy itself was established in 1863 under a Congressional charter signed by President Lincoln. Empowered to provide for all activities appropriate to academies of science, it was also required by its charter to act as an adviser to the Federal Government in scientific matters. This provision accounts for the close ties that have always existed between the Academy and the Government, although the Academy is not a governmental agency.

The National Research Council was established by the Academy in 1916, at the request of President Wilson, to enable scientists generally to associate their efforts with those of the limited membership of the Academy in service to the nation, to society, and to science at home and abroad. Members of the National Research Council receive their appointments from the President of the Academy. They include representatives nominated by the major scientific and technical societies, representatives of the Federal Government, and a number of members-at-large. More than 3000 of the foremost scientists of the country cooperate in the work of the Academy-Research Council through service on its many boards and committees in the various fields of the natural sciences, including physics, astronomy, mathematics, chemistry, geology, engineering, biology, agriculture, the medical sciences, psychology, and anthropology.

Receiving funds from both public and private sources by contribution, grant, or contract, the Academy and its Research Council thus work to stimulate research and its applications, to survey the broad possibilities of science, to promote effective utilization of the scientific and technical resources of the country, to serve the Government, and to further the general interests of science.

EXCERPTS FROM PATHOLOGIC EFFECTS OF ATOMIC RADIATION, STUDY BY THE NATIONAL ACADEMY OF SCIENCES, NATIONAL RESEARCH COUNCIL

APPENDIX I

REPORT OF THE SUBCOMMITTEE ON ACUTE AND LONG TERM HEMATOLOGICAL EFFECTS OF ATOMIC RADIATION

Eugene P. Cronkite, Brookhaven National Laboratory, Chairman
Carl V. Moore, Washington University School of Medicine
William N. Valentine, University of California Medical Center
Victor P. Bond, Brookhaven National Laboratory
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ACUTE AND LONG TERM HEMATOLOGICAL EFFECTS

On January 7 and 8, 1956, the Subcommittee met at the National Academy of Sciences in Washington, D. C. with all members present. A preliminary report was written by Drs. Bond and Cronkite and distributed to Subcommittee members. The comments of the members have been incorporated into this report.

INTRODUCTORY COMMENTS BY THE CHAIRMAN

Mankind has always lived in an environment suffused with radioactivity from natural and unavoidable sources such as radioactive minerals and cosmic rays. Natural radioactivity varies greatly in degree throughout the world. Intensities tend to be much lower at sea level and on most small islands, with the exception of Baltic islands, throughout the world. At high altitudes cosmic ray activity increases significantly. Similarly natural radioactivity from minerals increases significantly in some mines, and in the water supplies of some areas. For example, water in the Joliet area in Illinois contains relatively large quantities of radium and its daughter products. Since the discovery of x-rays by Konrad Roentgen and natural radioisotopes by Becquerel and the Curies, there has been a steady increase in the amounts of radiation to which segments of mankind are being exposed. With the development of nuclear weapons and the spread of atomic energy by industrial activities, the levels of world wide radiation will unquestionably continue to increase.

At the present time there is confidence that the increment to the naturally existing radioactivity is but a small fraction of that believed to exist prior to the testing of atomic weapons and the presently developed atomic energy industry. However, when one specifies the diverse sources of radiation to which large numbers of mankind are being exposed, it is quite evident that serious concern must be felt by physicians and scientists for the possible influence of such radiation upon individuals, selected groups, and whole populations. In the course of the deliberation of this panel attention was called to the existence of the following types of exposure to radiation to which human beings were exposed, voluntarily and involuntarily.

1. Natural sources
2. World wide low-level fallout
3. Roentgenographic surveys of large segments of the population for tuberculosis and cancer
4. Dental x-rays
5. Industrial fluoroscopy and radiography
6. Fluoroscopy of infants
7. Fluoroscopy for shoe fitting
8. Diagnostic x-rays
9. Medical and scientific use of tracers in human beings
10. Therapeutic use of radioisotopes and x-ray
11. Tracer radioisotopes in agriculture and industry
12. Research and power reactors
13. Ionizing radiations for food sterilization
14. Experimental accelerators

During the early years when the diagnostic and therapuetic uses of x-ray were being developed, heavy and repeated exposure to physicians, physicists, nurses, and technicians resulted in serious injuries. Historically, the occurrence of leukemia and aplastic anemia as late sequelae of exposure to x-ray and radioactive substances was well documented in the medical literature prior to 1937. Presumably Madame Curie died from an aplastic anemia. In recent years, while great care has been taken to avoid heavy radiation exposure, there is little knowledge on the hazards of repeated smaller does, especially in regard to late effects on the blood forming organs.

The record of the atomic energy development, which involved the handling of large amounts of dangerously radioactive material is an example of the effectiveness of the controlled environment.

Dose dependence and correlation of other effects with hematologic effects

In order to set the acute and chronic hermopoietic effects of radiation into the proper perspective with regard to overall radiation effects, the whole body radiation syndromes as a function of dose of radiation are summarized:

After very large doses of radiation delivered in a short time, a typical clinical syndrome is produced in animals. On the basis of observed symptomatology, it has been useful to name this symptom complex the central nerous syndrome (CNS). In animals, doses in excess of many thousands of r, are necessary to produce this complex. There are species variations. The "threshold" for this syndrome in man is not known and this syndrome has not been observed in man. Symptoms referable to the nervous system and GI tract appear promptly. Death may occur "under the beam" or within a few hours. In laboratory animals this syndrome invariably results in death either promptly, or later as a result of the next clinical syndrome which results in death at a later time. If the nervous symptom complex subsides, or if the dose has been smaller (in the region of 900–5,000 r for laboratory animals; dose for man not known), a symptom complex termed the gastrointestinal syndrome (GIS) appears. Nausea and vomiting appear shortly after exposure. Diarrhea and tenesmus become severe. The GI symptoms may be intractable or may subside for a variable period. Fluid and electrolyte loss from the GI tract progressively produce dehydration and eventual vascular collapse, and death that may occur during the 1st and 2nd weeks. This picture was observed in the Japanese casualties and has been well studied in laboratory animals. Death from this syndrome has been prevented experimentally in some dogs by adequate fluid and electrolyte replacement; in addition spontaneous recovery may occur after the lower doses in this range. However, the survivors then experienced another symptom complex that may result in death. It is this third symptom complex characterized by signs and symptoms referable to bone marrow depression which characterizes the lethal dose range.[1] By custom, the mortality from this syndrome has been tabulated as of 30 days after explosure. There are reasons, to be discussed later, why a 30 day tabulation may be too short for human beings.

A fourth phase of deaths was observed during the 2nd and 3rd months after exposure in the Japanese casualties in which in some instances the causes of death were not clear; however, pancytopenic sequelae were still present. Hemopoietic recovery was in progress but defects in proliferation and maturation were observed in the pathologic sections of marrow. Following the third month, deaths are infrequent and it becomes increasingly difficult to ascribe deaths to the effects of radiation since phenomena observed are those which may result in death in any non-irradiated population. In some instances cause of death was uncertain although pancsytopenia was prominent.

There is evidence that large single doses, or repeated small doses of radiation can produce diverse neoplasia, genetic defects, and shortened life span in select controlled *animal* populations. However, attempts to ascribe a specific role to irradiation in neoplasia in *human* populations becomes an exceptionally complex biometric study because of the increasing contamination of the atmosphere by industry with potential carcinogens, and the introduction and widespread use of an array of clinically useful drugs, whose long term effects in man are imperfectly understood, but which in some cases have produced severe blood dyscrasias. Accordingly, in all of the discussions on the long term effects of single doses of radiation on *man*, and the effect of repeated or low level exposure, one must be especially cognizant of the fact that the "effects" are *deduced* by statistical *correlations*, and cannot be proved by controlled experimentation, nor can other causative factors be eliminated. In this era of awakened public interest to the hazards of radiation, it is especially important that preoccupation with the hazards of ionizing radiation does not becloud the searching mind of the scientist or the responsible citizen to the presence of other hazards of equal importance. This is not an attempt to minimize the hazards of ionizing radiation with respect to the development of blood dyscrasias and other late effects. It is most

[1] Sublethal refers to the lower doses of radiation that will produce no deaths within a given period of time, usually taken as 30 days in animals. The lethal range extends from the threshold dose at which only rare deaths occur in this time interval, to the level at which virtually all exposed will die (the LD 1 to LD 99 range). Doses above the LD 99 level are termed supralethal. In all ranges, however, ultimate longevity is reduced to some degree.

important to bear in mind that the incidence of bone marrow failure[3] and leukemia has increased significantly in the United States in groups in whom there is no known overexposure to ionizing radiations. Today no informed physician believes that exposure to ionizing radiation has either a beneficial or stimulating effect on the blood.

In the course of the deliberation of this Subcommittee, attention was focused upon the known effects of nuclear explosions, the immediate and long term effects of single exposures from all causes, and the long term effects of intermittent and continuous explosure to radiation of diverse types. In the latter category, the Subcommittee felt that a reasoned judgment could not be made because of the paucity of realistic quantitative data on the degree of exposure.

ACUTE HEMATOLOGICAL RESPONSE TO SINGLE DOSES OF PENETRATING RADIATION

Although the available sources of hematological data on human beings exposed to total body external radiation have serious limitations, they were considered to be reasonably consistent among themselves to allow characterization of the time course of change in peripheral elements following exposure. The sources of data included the reports of the Japanese exposed to immediate radiation from atomic weapons, the account of the human beings accidentally exposed to fallout radiations at the Pacific Proving Grounds in March, 1954, the reports of human beings exposed to reactor accidents in the laboratory, and data on patients with incurable neoplastic disease exposed to therapeutic total body irradiation. The pattern of response of the peripheral blood elements changes with increasing radiation dose. In the following description, changes are divided into those that occur in the sublethal range, and those that occur in the lethal range (doses that result in some mortality within 60 days of exposure). This division is arbitrary, since the patterns of change merge imperceptibly, and each category covers a range of doses and thus degree of effect. When the dose is increased from sublethal levels to lethal levels, the lag period between exposure and depression is progressively shortened.

Response after sublethal doses

The *neutrophil* count shows an initial use in the first 12 to 24 hours followed by a sharp drop, to or below, the pre-exposure level. The count then fluctuates around or slightly below the pre-exposure level until the 3d or 4th week, following which definite depression is observed. The time of maximum depression occurs during the 5th or 6th week or even later, and is followed by a gradual return to pre-exposure levels. Complete recovery may require several months or more.

The drop in *lymphocytes* is early and profound. Little or no evidence of recovery in the high sublethal range may be apparent several months after exposure, and return to former levels may not occur for months or years. The total white count parallels closely the change in neutrophil count.

The *platelet* count shows little or no change over the first three weeks following exposure. At approximately the end of the 3rd week the platelet count falls. The time of maximum depression is remarkably constant at sublethal dose levels, and occurs on the 28th to the 32nd day of post-exposure.

No trend in eosinophile, basophile, or monocyte counts can be definitely ascertained. This may result in part from the larger errors inherent in counts of these cells. In the absence of hemorrhage, the hematocrit may show slight depression. This effect is probably due to a combination of inhibition of erythropoiesis and shortened life span of the red cell.

Response after doses in the lethal range

The *neutrophil* count may rise during the first two days following exposure. The count then falls steadily to reach values below 1,000/mm^3 by the 5th to the 10th post-exposure day, depending on dose. In survivors, recovery begins during the 5th week, but many not be complete for several months.

The *lymphocyte* count drops to vanishing levels within 12 to 24 hours of exposure; recovery is not apparent for several weeks; and it may not be complete for several months, or for a year. The total white count parallels the neutrophil count.

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[3] Synonymous with aplastic anemia, refactory anemia, hypoplastic anemia. Aplastic anemia has been observed to terminate in leukemia. The occurrence of aplastic anemia after use of diverse drugs is common clinical knowledge.

The *platelet* count in the lethal range, in marked contrast to that at lower doses, may drop precipitously, starting approximately on the 4th day, and platelets may virtually disappear from the peripheral blood by the 10th day.

Changes in the eosinophiles, basophiles, and monocytes counts cannot be characterized definitely at this time. The hematocrit [3] is not appreciably affected until hemorrhage occurs, severe gross external or occult internal bleeding may occur as early as the 9th day, depending primarily on the time at which the platelet count reaches dangerously low levels. This may occur from the second to the fifth week, with peak incidence in the 4th week in the low and mid-lethal dose ranges. The degree of response as a function of dose varies for the several blood elements. The platelet and lymphocyte counts are affected by very small doses of radiation, and are reduced to minimal levels before the lethal dose range is reached. The neutrophil count, however, does not reach minimal values until the lethal range is reached.

Comparison of man and other mammals

The time course of changes in the leucocyte and platelet counts in human beings is definitely different from that observed in lower animals. In man, severe depression of these elements occurs later, and recovery is more delayed. Similarly, the time of deaths in man resulting principally from hematological depression differs from that of laboratory animals. In most laboratory species, essentially all animals alive on the 30th post-exposure day will remain alive for several months, although the life span is shortened. In man, however, the peak incidence of death from marrow depression occurs during the 4th and 5th post-exposure week (Hiroshima and Nagasaki data). Thus an LD 50, 30 day consideration is inadequate to characterize the acute lethal dose response of man, and an LD 50, 60 days would be preferable.[4] The extensive serial blood counts obtained in human beings exposed to fallout gamma radiations were relied on heavily in characterizing the hematological responses of human beings exposed to external radiation. Admittedly the dose rate with fallout was much lower than with prompt radiation and may have reduced the effectiveness somewhat. These individuals received, in addition to gamma radiations, beta radiations of the skin, and probably a minimal degree of internal contamination. It was the consensus of the Subcommittee that neither the beta lesions nor the low level of internal contamination significantly contributed to the pattern of change observed. This view was supported by the general agreement of these data with other less extensive data on human beings who did not receive additional skin lesions or internal contamination; and the lack of correlation between the severity of hematological change and the extent of beta lesions in those exposed to fallout radiation. The reservation was held, however, that data are inadequate to establish this view with certainty, and that synergistic effects cannot be ruled out.

Mortality and morbidity from whole-body radiation

The pan-hemopoietic depression contributes in large measure to morbidity and mortality following total body irradiation. In the sub-lethal and low lethal ranges, the response observed is consistent with other clinical pancytopenic states. Neutrophil depression increases the susceptibility to infection and platelet depression contributes to the bleeding tendency. Correction or treatment of these defects during the first few weeks may permit survival in some individuals who might otherwise have succumbed. The concept of total body x-radiation as primarily a pancytopenic state, while useful, is probably an oversimplification, particularly in low and high lethal ranges.

Susceptibility to infection is well established and the pathogenesis may well involve interference with specific immune mechanisms, phagocytosis, and migration of leukocytes in addition to simple neutrophil depression.

Susceptibility to bleeding is well correlated with platelet depression. However, additional factors may be involved such as lipid antithromboplastins (Tocantins) or disturbances in the β lipoprotein transport mechanism (Nickson and Bane).

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[3] Admittedly the hematocrit can be misleading since it represents both changes in plasma volume and red cell mass. However, in general decreases in hematocrit represent a diminution in red cell mass for one reason or another (loss, hemolysis, or no new production).

[4] The reservation must be made here that the exposed Japanese population were heterogeneous with respect to age, sex, physical condition and degree of added trauma from burns or blast. The extent to which these factors affected survival time has not been determined. In studies on laboratory animals the converse is true—homogeneous populations are studied.

In some instances, the latter changes are similar to changes induced by heparin administration to rabbits. The relation of these alterations to the bleeding tendency has not been established. It was the consensus that frank heparinemia is not a contributing cause of bleeding.

After *higher doses*, death ensues even if hemorrhage and infection are corrected. Germ free rats die at dose levels moderately higher than the lethal dose for rats in the natural state. These animals die later with severe hemorrhage and anemia. At dose levels in excess of the LD 100, 60 day level, individuals die within the first week presumably from fluid imbalance and vascular collapse correlated with marked damage to the intestinal epithelium. It is clear that at all dose levels, poorly known and little understood biochemical changes[5] occur which may contribute to mortality in the exposed individual. Our knowledge is inadequate to determine at the present time to what extent such biochemical changes may prove lethal in themselves even when infection and hemorrhage can be treated adequately.

Lethal dose for man

No data are available to allow adequate characterization of the LD 50 value for man. The degree of hematological depression observed in patients receiving total body x-radiation indicates that the current estimate of 450 r is a reasonable estimate for x-radiation as employed in the clinic. A recent re-evaluation of the data from Hiroshima and Nagasaki indicated a value higher than this for immediate gamma radiation from the bomb. Geometrical and depth-dose considerations can be interpreted to indicate that the LD 50 for man exposed to immediate gamma radiation and fallout gamma radiation from the atomic bomb may be lower than this figure. A large degree of uncertainty exists in both approaches, and more biological and physical data are required to settle the issue. The situation is complex, and it became evident that it is not possible to extrapolate with confidence from one condition of radiation exposure to another, or from animal data to man.

Threshold dose for detectible effects

There appears to be no threshold[6] dose for changes in the peripheral platelet count and possibily for other elements of the blood. Changes at very low dose levels, however, can be detected only in a relatively large population. Nothing is known about subtle changes in the blood forming organs at dose levels so small that changes in the blood picture cannot be detected.

Diagnosis of radiation exposure and its severity

The diagnosis of exposure to radiation and its severity is made on the basis of the history and physical and laboratory examinations, as with any disease. Available estimates of air roentgen dose received obtained by physical means should be considered in evaluating the degree of exposure, but should never in themselves be taken as an index for disposition or treatment since *tissue dose* and *distribution* of absorbed energy ultimately determines effect not dose in air. Any degree of radiation exposure should be avoided if possible. If exposure is necessary under emergency conditions, severe hematological depression may be expected at doses of 100 r or more measured in air, from immediate radiation from the bomb or from the gamma radiation from fallout material. With human exposure a wide spectrum of ages and of state of health is likely to be involved. Thus it is not possible to predict acccurately the severity of response that might be expected for a population exposed at various dose levels. There is evidence from the human beings exposed to fallout radiation that children may be more severely affected than are young adults. Whether this is due to inherently greater sensitivity or to an increased depth dose due to smaller size is not known. From animal data, it has been postulated that elderly individuals may be more seriously affected than young adults.

Therapy of radiation injury

Recommendations for therapy are given for 1) conditions where exposed individuals can be carefully and individually handled because they are limited in number and adequate facilities exist for taking care of them, and 2) ex-

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[5] Apparently decreased respiratory quotient (RQ) in animals, increased excretion of amino acids in irradiated human beings, etc.

[6] The threshold concept may be incorrect since inability to detect effects at lower doses may only be a manifestation of inadequate criteria for effect. For practical purposes a threshold might be classified as that dose where statistically sifinificant differences are detected. For many effects this may necessitate extremely large samples.

posures at the catastrophic level where adequate medical observation and care are impossible. In the first category, of cardinal importance in therapy is careful observation, good nursing care, and treatment on an individual basis of any condition that may arise. Antibiotics in general should not be given prophylactically, and should be administered only if infectious processes develop that would be treated with antibiotics in the absence of radiation. Prophylactic use of antibiotics may be considered if the neutrophil count drops below 1,000/mm^3. Prophylactic use of antibiotics may be considered particularly where severe wounds or other complications may be present. If antibiotics are used, they should be given in large doses and the broad-spectrum drugs should be employed. Fluids should be given as indicated clinically. Blood should not be given prophylactically, but only as indicated from clinical and laboratory findings. Fresh whole blood by direct silicone multiple syringes without anticoagulant or collected in plastic bags or platelet transfusions may be of some value in controlling purpura and other hemorrhagic manifestations. The use of drugs without clear indication is discouraged because of their unknown and possibly harmful effects on the irradiated individual whose metabolism is deranged. Parenteral administration of drugs should be held to a minimum because of the added trauma in an individual susceptible to purpura and infection. At present there are no specific prophylactic or therapeutic agents[7] that should be stock-piled for us ein the hematological depression and the resulting disease state following exposure to total body irradiation.

Under catastrophic conditions it of course will not be possible to adhere to the above regimen. The principles of therapy remain the same, except that here there is a much better potential case for widespread and empirical antibiotic dispensation, particularly to individuals in which burns, mechanical wounds or other added trauma exist.

LONG TERM EFFECTS ON THE BLOOD OF A SINGLE EXPOSURE TO IONIZING RADIATION

The best single source of information on this subject is the Japanese survivors exposed at Hiroshima and Nagasaki in August, 1945. Of necessity, data on the immediate radiation effects are fragmentary and data on the exposed individuals between the 16th week and the 2nd year are not available. In 1954 a statistical analysis of the hematological data obtained by studies on Hiroshima survivors and a control population, carried out from 1950–1953 (ABCC Program ME SS) showed that there was no evidence for an increase in leukopenia, leukocytosis or anemia in the exposed as compared to the control population. During this period several cases of aplastic anemia were encountered among the Nagasaki survivors. However, it cannot be definitely stated that these cases were due to atomic radiation. Up to late 1953, no cases of aplastic anemia had been found in the Hiroshima survivors. It should be noted that "aplastic" anemia is not an uncommon blood dyscrasia in the Japanese.

Incidence of leukemia

In contrast, an increased incidence of leukemia among survivors has been clearly established. It has long been known that repeated or single doses of radiation could increase the incidence of leukemia under controlled experimental conditions in laboratory animals. Accordingly, an intensive investigation of the incidence of leukemia in the irradiated survivors in Hiroshima has been carried out by the ABCC. This study is continuing and a statistical analysis, based on the verified cases of leukemia occurring in the Hiroshima survivors, establishes beyond reasonable doubt that the incidence of leukemia was significantly increased in exposed individuals.

The following graphs and tables are based on the 1947–1953 incidence of leukemia in Japan and have been supplied by the Committee on Atomic Casualties of the National Research Council.

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[7] Antibiotics of great value in the therapy of infection in the exposed individual are not considered here as specific drugs. Such prophylactic agents known to this panel such as sulfhydryl compounds, hypoxia inducing drugs, spleen or bone marrow preparations, etc., claimed or shown to favorably modify acute radiation injury in animals, have no place as yet in the treatment of human radiation injury.

Leukemia in exposed persons—Number and rate by presence of radiation symptoms and distance from hypocenter

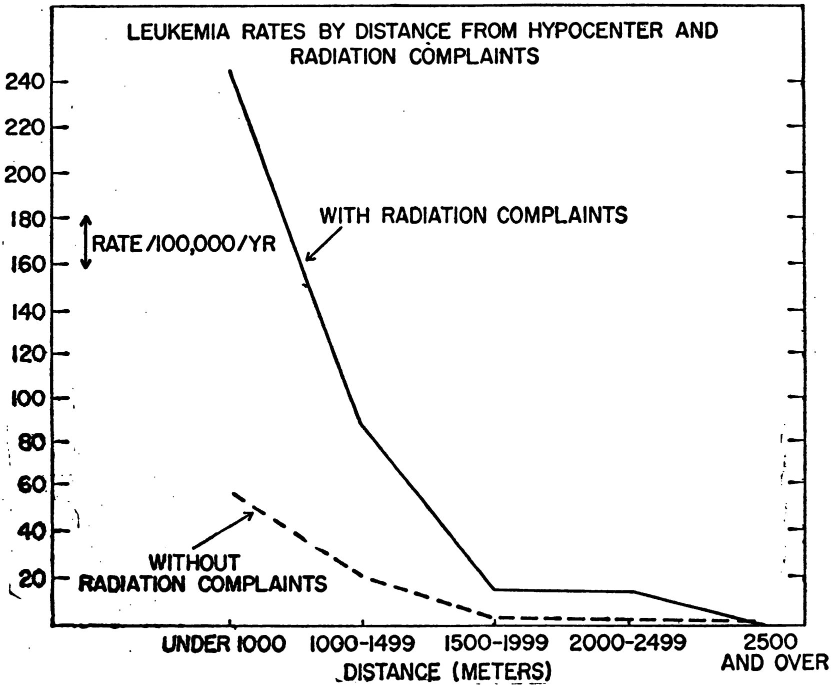
Distance from hypocenter—meters	Hiroshima population [1]			Number of cases of leukemia [2]			Incidence		
	SRC [3]	NRC [4]	Total	SRC	NRC	Total	SRC	NRC	Total
Under 1,000	750	450	1,200	14	2	16	246.2	58.6	175.8
1,000 to 1,499	2,250	8,250	10,500	15	13	28	87.9	20.8	35.2
1,500 to 1,999	1,750	16,950	18,700	2	4	6	15.1	3.1	4.2
2,000 to 2,499	950	16,250	17,200	1	1	2	13.9	0.8	1.5
2,500 over	850	49,650	50,500	0	8	8	--------	2.1	2.1
Total	6,550	91,550	98,100	32	28	60	64.4	4.0	8.1

[1] Source: Population estimated and rounded off to the nearest 50 persons. These population figures were based on the Commission's 1949 Radiation census and the Japanese national census (1950). Numbers of survivors with severe radiation complaints were estimated from observations made by the Commission's genetics department on 19,675 Hiroshima survivors of childbearing age.

[2] Source: Listing of Leukemia Cases in Hiroshima and Nagasaki, Sept. 1955. Cases are restricted to those in persons resident in Hiroshima at the time of diagnosis, and described in the listing under the heading, Diagnosis Acceptable.

[3] SRC: Significant radiation complaints—Epilation or purpura on history not confirmed by competent physical examination or medical records.

[4] NRC: No radiation complaints.



Leukemia in persons exposed within 1,500 meters of the hypocenter—Number and rate, by sex and age ATB [1]

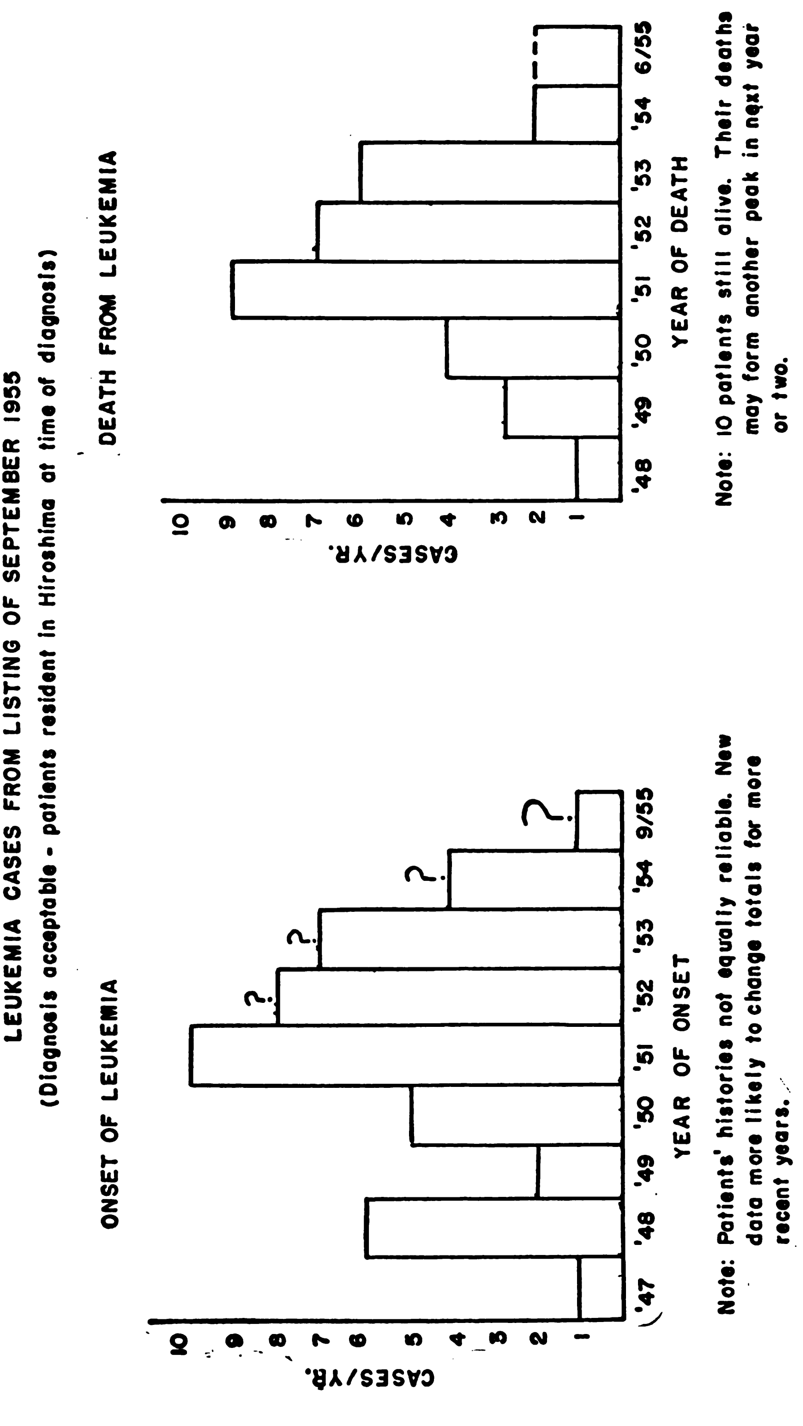
Age ATB	Hiroshima population, 1950		Number of cases of leukemia [2]		Incidence, annual rate per 100,000	
	Male	Female	Male	Female	Male	Female
0 to 9	839	878	6	6	94.3	90.1
10 to 19	995	1,490	7	2	92.8	17.7
20 to 29	458	1,352	3	6	86.4	58.5
30 to 39	713	1,118	3	2	55.5	23.6
40 to 49	902	1,016	3	2	43.9	26.0
50 to 59	606	572	1	2	21.8	46.1
60 to 69	236	278	----------	1	----------	47.4
Total	4,749	6,704	23	21	63.8	41.3

[1] Source: "Estimated Number of Survivors in Hiroshima City in 1950," Preliminary Report, Death Certificate Survey.

[2] Source: Listing of Leukemia Cases in Hiroshima and Nagasaki, September 1955. Cases are restricted to those in persons resident in Hiroshima at the time of diagnosis, and described in the listing under the heading, Diagnosis Acceptable.

Leukemia rates in USA as listed on the 1951 record of Vital Statistics

	Male	Female	Overall rate
White	7.6	5.3	6.1
Nonwhite	4.0	2.7	----------


LEUKEMIA CASES FROM LISTING OF SEPTEMBER 1955
(Diagnosis acceptable - patients resident in Hiroshima at time of diagnosis)
ONSET OF LEUKEMIA
CASES/YR.
1 2 3 4 5 6 7 8 9 10
'47 '48 '49 '50 '51 '52 '53 '54 9/55
?
YEAR OF ONSET
Note: Patients' histories not equally reliable. New data more likely to change totals for more recent years.
DEATH FROM LEUKEMIA
CASES/YR.
1 2 3 4 5 6 7 8 9 10
'48 '49 '50 '51 '52 '53 '54 6/55
YEAR OF DEATH
Note: 10 patients still alive. Their deaths may form another peak in next year or two.

The preceding tables and graphs clearly indicate that the incidence of leukemia increases with the dose of radiation. The incidence of radiation complaints at 2000 to 2500 meters is open to question since the dose received at these distances was between 8 and 35 r according to the best source of information.[3] It is noteworthy that radiation complaints are based on late interrogation and not on medical records by competent observers at the time of the bombing and as such are prone to subjective error in memory by the individuals interrogated. At closer distances this possible error is probably negligible because of the unquestioned and recorded histories of radiation injury. The peak incidence of onset appears to have been passed in 1951; however, scattered cases are still being observed, but relationship to confirmed previous exposure to radiation injury is obscure.

Since precise data on the radiation doses received at Hiroshima and Nagasaki have not been made available to the ABCC it is not possible to set a "Dose threshold" for induction of leukemia in man by whole body radiation. Such information is vital to the future welfare of human beings who may be forced to live in contaminated areas. In addition, with the growing belief that there may not be threshold levels for radiation effects it becomes absolutely essential to obtain the most reliable estimate of the exposure wherever osbervable effects have been detected.

In Japanese atom bomb survivors the leukemias have been predominantly myeloid. However, Dr. Furth of the committee emphasizes that radiation induced *lymphatic* leukemias in Europeans have been observed. In this connection it is of interest that lymphatic leukemia is a rarity in Japan suggesting that radiation is prone to induce the type of leukemia that might occur spontaneously.

Hematologic changes preceding development of obvious leukemia

In the course of studies on Japanese atomic bomb survivors, observations on hematologic changes preceding development of overt chronic myelogenous leukemia were made in a number of cases. In routine surveys, blood studies revealed evidence of a generalized proliferative effort by the bone marrow, many months before obvious evidence of leukemia. These manifestations were the presence of a small per cent of myelocytes and metamyelocytes, and a very striking increase in absolute numbers of basophils, in the peripheral blood. These changes were accompanied by increaed numbers of platelets and occasional normoblasts. Beginning in November, 1952, biochemical studies on the separated leukocytes demonstrated that in these early pre-clinical cases of leukemia, the polymorphonuclear leukocytes contained very little alkaline phosphatase. These alkaline phosphatase values were similar to those reported by Valentine et al. for neutrophiles in well established cases of chronic myelogenous leukemia.

Subsequent studies in chronic myelogenous leukemia, employing histo-chemical as well as biochemical methods, have shown that 2 per cent or less of segmented polys contain even small amounts of alkaline phosphatase. In contrast, in other conditions with increased polymorphonuclear leukocytes such as infection and myeloid metaplasia, the alkaline phosphatase values are high and practically all segmented neutrophiles contain large amounts of alkaline phosphatase.

It has been postulated that the leukemic cell is deficient and in the precursor stage of development of leukemia, two populations of cells are present with the leukemic type increasing in number until a typical leukemic blood picture is evident. It has been the experience of some of the Subcommittee members that cases of "bone marrow failure" may terminate in leukemia.

EFFECTS OF REPEATED LOW LEVEL EXPOSURE

Increasing numbers of human beings are exposed to repeated does of radiation frequently at very low dose levels. Thus in industry and in AEC installations, in radiologists and radiological technicians, public health surveys and particularly those using fluoroscopy, and in repeated roentgenograms in medical and dental diagnosis, large populations are exposed to radiation at levels well in excess of background.

[3] The Effects of Atomic Weapons, U. S. Government Printing Office, Washington, D. C., 1950.

There are several studies on radiologists and radiological technicians, indicating that statistically the blood counts of such individuals may be altered. Similarly from the vast number of counts on individuals exposed to low level radiations at AEC installations, there is evidence that the so-called maximum permissible dose may result in statistical alteration in the blood count. The possible significance of these small changes is not clear. The slight decrease in neutrophiles or lymphocytes count has little or no significance in itself. It would appear that its significance in relation to the later development of leukemia or other disease that shortens life span should be investigated. Recommendations to this effect are given below. Data are available on the hematological effects of exposure to radiation up to 10 times background. The drinking water of prisoners at Joliet prison in Illinois contain 20 times the radium content of the drinking water of neighboring communities. Extensive study has failed to detect differences ascribable to the increased radium content of the drinking water. In the course of radiotherapy for relatively benign conditions, it seems clear that serious late effects can result from a single exposure or a series of exposures to x- or isotopic radiations. Thus thyroid cancer has resulted in children given x-radiation for thymic enlargement. Similarly, leukemia has been reported in individuals receiving repeated x-radiation therapy for spondylitis, and in patients receiving repeated I–131 for thyroid cancer.

USEFULNESS OF HEMATOLOGIC STUDIES IN CONTROL OF RADIATION INJURY

A large effort at great expense was made by the AEC during the development of atomic energy to determine if routine hematologic studies would detect low level exposure to radiation. It was the consensus that frequent routine studies on personnel exposed to low levels of radiation have a limited value that does not justify the expense. Physical control of the environment by radiation monitoring is an effective means of maintaining a safe environment, and nothing is gained by widespread hematologic studies on personnel. However, it would not be wise to dispense completely with hematologic studies since it is important to have pre-exposure levels in the individuals who may be exposed to radiation such as with those accidentally exposed to fallout radiation. Had base line studies been available relative depression and recovery time as a function of dose could have been more precisely determined. Accordingly it is believed that periodic, perhaps annual hematologic studies should continue on limited groups of individuals who run a greater risk of accidental over-exposure. Certainly, all individuals who have been exposed so accidentally at dose levels of 25 r or more of essentially whole body radiation (single exposure) should have periodic systematic studies to determine the degree of hematologic depression and the recovery rate. These individuals should remain away from an environment where further overexposure is likely until the dose received has been amortized at the rate of 0.3 r/week. The latter is suggested because clinical radiation therapy experience indicates that individuals who have been exposed previously as a result of local therapy or whole body exposure show greater hematologic depression following further whole body radiation. A more fruitful field of hematologic study in relation to chronic radiation exposure would appear to be the periodic study of phosphatase content of the neutrophils and number of basophils, on limited populations, who are known to be exposed chronically, such as radiologists, urologists, orthopedists, x-ray technicians, and dentists.

RECOMMENDATIONS FOR RESEARCH

A. Recommendations With Respect to the Acute Hematological Response of Human Beings to Radiation

(1) The Japanese data from Hiroshima and Nagasaki bombings should be further analyzed in respect to:

(a) Duration of depression of leukocytes as a function of distance and shielding (dose).

(b) Leukocyte counts at various intervals in relation to ultimate survival.

(c) Survival time as a function of distance and shielding (dose), and of age.

(d) The degree of initial blood count depression in relation to the later development of leukemia and other late disease.

(2) Additional and intensive studies should be initiated on human beings receiving radiation to the whole body or large portions of the body in the therapy of malignant disease. Particular attention should be given to the time course of

peripheral blood counts for several weeks following exposure to different doses, and the nature of the clotting defect.[9]

(3) Initiate studies on the cause of death in animals in which death from hemorrhage and infection have been prevented. This refers to deaths within the first few weeks, as opposed to the much later deaths from nephritis, neoplasia, etc.

(4) Although nothing of practical value is now available for the specific therapy of acute radiation injury, it is urged that further research be pursued on the fundamental defects produced by ionizing radiation on mammalian systems. *Medical experience has shown that rational therapy is only developed when the basic physiologic defects are understood.* With this in mind further research is needed. However, it would be unfair to the public to imply that effective therapy can be expected in the near future or indeed that overwhelming doses are ever likely to yield to therapy.

B. Recommendations With Respect to Long Term Effects of a Single Exposure to Ionizing Radiation

(1) Periodic hematologic surveys should be performed on the Marshallese and Americans exposed to fallout radiation in March, 1954. Careful study for cytological changes mentioned above, especially basophilocytosis and immature leukocytes, and routine bistochemical studies for alkaline phosphatase (using peripheral blood smears and either Gomori's Cobalt technique or the azo dye method) should be carried out. In suspicious cases, biochemical determinations for alkaline phosphatase on separated leukocytes should be done. In view of the long "latent period", studies for many years after exposure if not for life, are essential.

(2) The cytologic and histochemical-biochemical studies might well be employed in surveys of radiologists and other chronically exposed groups.

(3) The present concepts of leukemoid reactions and myeloid metaplasia, and the relationship of these disorders to leukemia are obscure. Further studies on the enzyme and metabolic activities of leukocytes in these disorders may lead to a better understanding of radiation effects on myeloid cells and the role of irradiation in leukemogenesis.

(4) It is generally recognized that routine hematologic studies of potentially exposed individuals are wasteful and unproductive. However, studies on select groups by routine and newer techniques are highly desirable, e. g., radiologists, physicist.

(5) It was the consensus that it would be desirable to know the incidence of leukemia in WW I soldiers who were exposed significantly to mustard gas.

(6) Pediatricians have fluoroscoped newborn babies, and a considerable dose of whole body radiation may have been received. A long term follow-up on these exposed children is needed.

C. Recommendations With Respect to Effects of Repeated Low Level Exposure

(1) Since there are geographical locations in which the known radiation intensities vary considerably, it is felt that the incidence of leukemia should be established in:

(a) Island populations (low background except Baltic Islands)

(b) Andes (high background)

(c) Prison and civil population in Joliet (radium content in water higher than normal)

(2) It was the consensus that a ceaseless search should be continuously made for other harmful agents in the atmosphere and our modern diet. It is genuinely felt that preoccupation with radiation may obscure other equally hazardous factors in man's environment.

CONCLUSIONS

At the commencement of the deliberations there was some question in the minds of the Subcommittee as to the objectives and the reasons for establishing it. However, in the course of the discussions it became apparent that in addition to the confusion in the minds of the public there also exists some large gaps in knowledge essential for the understanding and quantification of radiation hazards in the world of today let alone the world of the future. The immediate effects of direct exposure to high intensity radiation are well documented and the relation

[9] One member of the panel reactivated the heparinemia concept of radiation hemorrhage by describing the cessation of bleeding in an irradiated individual with thrombopenia following injection of protamine, an antiheparin agent.

of dose to effect is known with some degree of confidence, even though certain hiatuses exist that are listed in the general discussion and recommendations. In the realm of chronic exposure it was recognized that the unavoidable background level of radiation was known to vary with seasons of the year, geographical location, and altitude above sea level. However, world-wide levels do not seem to be known with sufficient accuracy to determine when a rise in atmospheric level of radiation is definitely occurring. Since there is little quantitative information on the relation of dose to effect under conditions where harmful effects were observed it becomes vital to ascertain natural levels of radioactivity and to try to establish the level of atmospheric radioactivity at which detectable chronic effects might conceivably occur. However, with all of the recommendations contained in this report, it is believed that a "crash-type" research program to obtain needed information is not indicated.

Bibliography—Radiation Effects on Blood and Blood-Forming Tissues of Man—1900—March 1956

Prepared by Marjorie Comstock, Research Library, Brookhaven National Laboratory

INTRODUCTION

The literature was searched for references relating to the effects of radiation on blood and blood-forming tissues of man. In this bibliography, radiation includes radiation from neutrons, alpha, beta, gamma rays, A-bomb, x-rays and internally deposited radioisotopes. References relating to ultraviolet, infra-red, microwaves, thermal radiation and visible light have been omitted.

The following sources were checked:—

Chemical Abstracts (CA) v. 1 (1907)–v. 48 (1954)
Chemische Zentralblatt (CZ) 1897–1906
Current List of Medical Literature (CLML) v. 19 (1950)–v. 29 (March 1956)
Nuclear Science Abstracts (NSA) v. 1 (1948)–v. 10 (March 15, 1956)
Quarterly Cumulative Index Medicus (QCIM) v. 1 (1927)–v. 53 (1953)
Quarterly Cumulative Index to Current Medical Literature (QCI) v. 1 (1916)–v. 12 (1926)
Library Files

In addition to the above sources, Zentralblatt für die gesamte Radiologie was also used to check references. References are available in the Research Library unless otherwise indicated. The secondary source, abbreviated as above in parenthesis, is given when possible for references not available at the Laboratory. Journals have been abbreviated according to the Chemical Abstracts list as far as possible. In some cases where the authors initials were known, they have been added in parenthesis although they did not appear either in the original article or in the abstract.

An effort has been made to check references for pertinency. This could not be done for some of the earlier references due to the lack of abstract journals covering the subject. It was necessary to rely solely on the title for many of these entries.

Every effort has been made to make this bibliography complete. However, there is no way of knowing whether or not this has been accomplished.

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APPENDIX II

REPORT OF THE SUBCOMMITTEE ON TOXICITY OF INTERNAL EMITTERS

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TOXICITY OF INTERNAL EMITTERS

The Subcommittee on Toxicity of Internal Emitters met on January 10 and 11 at Argonne National Laboratory. They discussed the subject from many aspects including a certain amount of classified literature which is not referred to in this report but which it is believed would not alter the conclusions. The agenda were not followed literally in order to promote free discussion, so this report contains a somewhat rearranged version of the discussion.

Statement of the Problem.—The question of safe working exposure levels for industrial and laboratory practice has been intensively studied and is presented in the National Bureau of Standards Handbook 52. (Several members of the Subcommittee agreed that while this represents the best body of data and opinion. it is not the last word, in the matters of use of the biological half-life for all isotopes and the high relative biological effectiveness of alpha rays, in particular. It is thought that the Handbook will probably undergo some changes and that these changes will in general be in the direction of withdrawing a little from the conservatism with which the figures were originally reached.)

Important matters of public concern are: (1) radio-elements (that is, fission or activation products) close to a ground burst of an atomic bomb; (2) more remote fallout; (3) gradual world-wide fallout, especially of strontium-90; (4) consequences of a nuclear reactor accident near a populated center. Other prob-

lems of course exist, but most of the answers will be encompassed in a consideration of these.

The categories of data to be considered include: (1) acute and sub-acute radioelement toxicity and its relation to external radiation damage in an area of heavy contamination; (2) nature and dosage requirements of chronic radiation damage from various radio-elements; (3) physical, ecological, and physiological conditions determining absorption of radio-elements; (4) metabolic handling of radio-elements after their absorption; (5) therapy.

Problems requiring special consideration were: (6) realistic appraisal of absorption from the gut and retention in and absorption from lung; (7) influence of particulate or "hot spot" irradiation, especially in lung and skeleton; and (8) permissible dosage to a large "innocent" population in relation to that to industrially exposed persons for which the present levels are drawn.

The nuclides considered in particular fall in the following groups:

(1) Radium, strontium, barium, calcium; readily absorbed with long-term retention in the skeleton.

(2) Iodine; readily absorbed, of short physical half-life but highly concentrated in the thyroid.

(3) The rare earths, yttrium, and plutonium and other actinides; nearly insoluble and poorly absorbed.

(4) Ruthenium; absorbability ambiguous due to multiplicity of chemical forms.

(5) Cesium; in all probability readily absorbable.

(6) Activation products, various and unpredictable, generally considered unimportant.

Of the above, strontium and iodine received the most consideration. The background of natural radioactivity was also given consideration because of its relation to large population exposure.

Fallout Conditions.—The larger particulates fall out first and smaller ones at a greater distance, for obvious physical reasons. Fission products are plated on the larger, near-in particulates, probably reducing their absorbability. The strontium isotopes, since they exist for a short period after fission as krypton isotopes, escape to some extent from entrapment in larger particles; accordingly, they appear in a smaller ratio than their fission yield in the near fallout and are somewhat enriched at a distance: also, they appear as smaller particles and are more readily absorbed. Rabbits analyzed in Nevada showed a higher skeletal concentration at 133 miles from the test than at greater or less distances.

A detailed study of the effects of the fallout from the thermonuclear explosion of March 1, 1954 on the Marshall Island inhabitants, which is to be published soon (see Cronkite et al. in the references) discusses the body burden from material absorbed into the human body under the conditions of exposure encountered there. Estimates based on excretion rates indicate an initial body burden of Sr^{89} of about the permissible amount, while the I^{131} retained is estimated to have delivered a thyroid dose of the order of 100 rep. Other radioelements (including Ca^{45} and fissile material) are relatively negligible. The opinion of the Subcommittee, that products of neutron activation would be unimportant beside fission product activities, is borne out by this. Data in Table I, from a Japanese publication, are essentially confirmatory, and include autopsy findings which bear out the inferences from measurements on human excreta. It is noted that these low levels of internal contamination existed in persons who received a significant fraction of the human external lethal dose from gamma radiation.

The chemical nature of fallout elements has apparently not been well enough studied to yield many useful inferences about absorbability.

Acute Toxicity.—This would be encountered in pure form only in the event of absorption of specific products. In exposure to large amounts of mixed fission products or by inhalation in a fission cloud, external gamma and beta radiation would be expected to be by far the predominant source of injury.

Acute toxicity from a variety of isotopes generally manifests itself as acute radiation sickness. In combination, bone-seeking and colloidal isotopes act synergistically, due in large part to the fact that spleen and bone marrow are both irradiated in the combined treatment. Feeding of insoluble beta emitters may cause intestinal death, but only after enormous doses, because of the rapidity of their passage through the intestinal tract and because in those places where they remain the longest most of the energy is absorbed by the fecal material which is present there.

Subacute changes depend on the distribution of the element in the body. Hematologic effects are, of course, seen where bone marrow or total body irradiation predominate, and it might be mentioned that strontium-89 or 90, although they produce marked hematologic changes and acute radiation syndrome, produce no or almost no leukemia in mice. The doses required for these subacute changes may be an order of magnitude below those giving acute toxic symptoms. Premature aging, greying of hair, retinal changes, reduced blood volume, and changes in the blood colloids have been observed in dogs given radium and plutonium but not with strontium-90. No explanation of this is available, but these are animals run in parallel experiments. Arterial calcification also occurs in rats after they are given radium in subacute doses, but has not been seen with other nuclides.

Chronic Toxicity: Site of Injury.—It may be stated generally that the more important nuclides will act quite differently depending on the route of administration and their absorbability through the lung or gut, it being generally assumed that these will run parallel.

Materials such as yttrium, the rare earths, and plutonium are very little absorbed by either route, and the predominant hazard will therefore be to the lung. Those with experience in this agree on a general picture of the fate of inhaled, optimal-size particles (those most likely to gain access to the alveoli) in the range of 0.1 to 2.0 microns in diameter. About 25 per cent of this material is exhaled at once; 50 per cent is trapped in the bronchi and is carried up and swallowed within a short time (the half-time being about 9 hours); 25 per cent is deposited in the alveoli and, of this, three-fifths reaches the gut, by way of respiratory passages, leaving a 10 per cent deposition in the lungs. That fraction disappears in cases of inhaled radium sulfate in human cases which have been followed, with a half-time between 100 and 200 days, a very small amount being absorbed.

Absorption of these insoluble materials from the intestine is taken as 0.003 per cent. That was a consensus and obviously may not refer to all possible circumstances. Speaking generally, 10^{-4} is about the highest figure for intestinal absorption which would be found under most conditions. There appears, however, to be an exception in very young animals. Mice before 16 days of age absorb 2 to 3 per cent of plutonium administered either as the citrate by stomach tube or in milk from a plutonium-poisoned animal. From milk it continues to be absorbed in the ratio of 1 : 300 by young adult mice, although when it is present as the citrate it is not. Parenthetically, one suspects that this reflects an ability of the very young animal to absorb particulate material from the intestinal tract, for example, the milk factor. No data are available on other species so far as is known. Citrate does not promote absorption of plutonium in post-suckling animals but versene does.

Soluble materials are presumed to be absorbed both from alveoli and intestinal tract. Thus the route of entry becomes a matter of indifference. Absorption of specific elements will be discussed later.

As to whether any insoluble materials may be handled by plants in such a way as to promote their absorption, there is no positive information. They are generally not taken up by plants as well as by animals. It is reported, however, that for some reason hickory concentrates yttrium from the soil.

Effects on the Lung.—One microgram of plutonium (or 0.06 microcuries) introduced into the mouse lung as an "optimal" aerosol has proved toxic after a few months resulting in bronchial metaplasia, cellular infiltrations, and fibrosis, and carcinomas appeared after a year. The retained amount at that time has gone down to about one-twentieth of the dose administered, which is about what one would infer from the calculations stated above. Ruthenium administered in a similar way also produces pathologic changes, but data are not sufficient to indicate effective dose comparisons. It may be added that fibrotic changes occur in the human lung after 2,000 r of x-ray and rather regularly after 3,000 r. The Subcommittee on the respiratory tract headed by Dr. Wager has reported in greater detail on this topic.

Ruthenium.—The chloride of ruthenium is absorbed from the rat intestine to the extent of 3 per cent. This value is probably higher than for most of the chemical forms, except of course the tetroxide vapor which, if it were encountered (which is unlikely) would be well absorbed from the lung; this has been shown experimentally.

After absorption of ruthenium the chronic effects would most likely be in the skeleton, based on tracer work and some medium level toxicity work. While

ruthenium was found in animals after the Australian test, it is generally considered to be much less important than strontium.

Cesium.—The gamma ray of the cesium isotope 137 has been seen in normal individuals in the last year but in an amount of radioactivity far below that of body potassium. It was encountered as a deformation in the gamma spectrum of total body potassium and radium. No additional information is available, except that it appears to be somewhat variable and it my be correlated with the intake of milk, which apparently is, relatively speaking, a fairly rich source of radioactive cesium.

Activation Products.—Calcium was mentioned as a component of the Bikini ash, but toxicologically speaking would be a very minor contaminant of strontium. Activation of usual environmental elements would also yield corresponding minor amounts of insoluble material (rare earths and silicon) plus P^{32} and Na^{24}. It was reported that $Zinc^{65}$ was formed in high concentration in muscles of post-test Pacific fish, and it is recognized that other unexpected nuclides might turn up under specialized conditions.

The Alkaline Earths.—Strontium has quitely properly received major attention. It is a bone-seeker like the others of the series, and there is ample experimental evidence of its carcinogenicity in both masses 89 and 90. Since the best available human data regarding absorption and toxicity are from radium cases, this occupies first attention.

Metabolism.—Human radium retention in the age range 20 to 35, based on a 24-year follow-up, is described approximately by a power function of time with an exponent near —0.5; that is, retention varies as the inverse square root of time, being about 50 percent of the injected dose at the end of one day and following this function thereafter.

It may be remarked that indeed the retention of most absorbed materials of whatever nature can best be described by a power function unless one wants to construct a series of exponentials, although it may actually be a series of many exponentials. In all instances that have been carefully studied, even that of the retention of tritium oxide in the form of water, several exponentials exist. The rare earths and actinides show a much less steep slope than the alkaline earths.

There is also a species difference; the dog has a slope of —0.2 to —0.3, depending on age, which makes a great deal of difference in the accumulated dose in this animal.

Integrating this function in man, the cumulative radiation dose, assumed after a very brief period to be limited to the skeleton, goes up as the square root of time, and the rate of loss by excretion relative to the amount retained (specific loss), varies inversely with time. This gives one a way of determining when an exposure took place. It has been pointed out that this function gives a quantitative picture very different from that assuming an exponential half-life (which based on loss by chronic patients, where the loss is very low after 20 years, becomes very long).

It is clear, if this expression is correct, that throughout a human lifetime a man would accumulate little more than 100 days' intake. Observations on adolescent boys indicate that the accumulation to age 17 represents about 40 days' intake.

Evidence now bein accumulated in at least two clinics indicate that strontium-85, which is given because it is less toxic than the beta emitting isotopes, is handled by men in a nearly identical manner to radium. The initial loss varies considerably with age, being least in the younger individuals.

Toxicity.—The best available data on radium toxicity brought to our attention deal with a series of patients that received known amounts of pure radium chloride 24 years ago and were followed. The assumed minimum burden producing serious disease after this period has been taken as 1 microgram, but this has been questioned because of the fact that other preparations were in many cases contaminated with meso-thorium and, perhaps more seriously, with radiothorium. One bone tumor has appeared in one of these patients having 3 micrograms pure radium. The patient with the lowest burden in the series, that is, 0.4 microgram, shows diffuse minimal changes by x-ray, whereas other patients at higher doses from 0.6 to 1.0 microgram do not show detectable changes at this time. It can be assumed that the belief that the lowest effective burden is 1.0 microgram of pure radium is probably not in error by more than a factor of 2 from the standpoint of effects at 20 to 25 years.

Assuming the power function of —0.5 cited above, it can be shown that the total radiation dose accumulated at any time is equal to twice the time multiplied

by the burden. Taking the mass of the skeleton as 7 kilograms, some accumulated dosages are given in Table II.

Strontium 90—Radium Comparison.—Data on late toxicities of strontium and radium in small animals have indicated that a factor of 10 on an energy basis is roughly correct. This would suggest that alpha and beta radiations are relatively equivalent in terms of energy. This may be approximately correct, although the relative biological effectiveness of alpha radiation to x-ray of 10 is presently assumed in calculating permissible doses where experimental data are lacking.

Relative Biological Effectiveness (RBE), Alpha and Beta Rays.—Alpha rays from slow neutron absorption in animals containing boron, have indicated that a factor of 1.4 relative to X rays is roughly correct. Experiments with inhaled radon and injected radon show factors of 1.4 to 1.5 for acute effects, and 3.0 for chronic effects. The RBE for mice seems to pass through a maximum around the ionization density of fast neutrons. Observations on yeast also show a maximum in the intermediate range. The consensus is that a radium to strontium-90 factor of 10 may be a little low but not as low as it was formerly assumed. It was agreed by the Subcommittee to calculate radium and strontium both in roentgen equivalents for the present, although this may give strontium a small additional factor of safety over radium.

Absorption of Strontium.—There are considerable variations in the degree of absorption of strontium depending on the contents of the gastrointestinal tract and the demand for calcium. Absorption of radiostrontium by plants has been investigated, and it appears that the uptake is relatively independent of the concentration of carrier; in other words, it is taken up as a contaminant of the water absorbed. This breaks down only at high levels, where radiation effects on the physiology of the plant enter in. Under fallout conditions, it may also be deposited on leaves. Experiments have indicated that, under a variety of conditions, between one and ten per cent of the strontium-90 in the soil is available to a crop of plants. About five per cent of the strontium taken up by the plants is then retained by animals foraging on them after two to four weeks.

Data on the actual stronium-90 content of biological materials and human beings as a result of past and present fallout have indicated that human bones have now reached an amount equal to one-thousandth of a microcurie (that is, of presently accepted permissible human skeletal content) in young children, declining to approximately zero in persons above 40 years of age. Such calcium-rich sources of the isotope as cheese and milk yield values which are higher in respect to the Sr^{90} : calcium ratio.

Another way of looking at the same question is to consider uptake. When this is done, it appears that the strontium retention in man from the diet is 0.3 to 0.6 times what it would be if strontium were an ideal tracer for calcium.

Radioiodine.—Under existing fallout conditions, cattle in Tennessee have shown up to 10^{-3} $\mu c/gm$ of thyroid, but human thyroids have not shown more than 1/100 of this concentration. External counts on monitors in the Nevada test areas have shown not more than 10^{-4} $\mu c/gm$ of thyroid, with all probabilities in favor of a smaller concentration. In view of the short half-life of I-131 it can be concluded that the hazard of bomb tests from this standpoint is negligible.

It has been suggested that the human thyroid is less radio-sensitive than other tissues, such as bone, since after many years of treatment of Graves' disease with radioactive iodine, no cases of resulting carcinoma have been reported. The customary dosages of I^{131} in such cases yield at least 4000 rep to the gland. On the other hand, carcinoma of the thyroid found in children and young adults has almost invariably been preceded by x-ray treatment to the upper part of the body, in amounts such as to yield as little as 200 r to the infant thyroid. It has been estimated that less than 3% of such treated cases yield carcinoma; nevertheless, the data suggest that 200 r is a potentially carcinogenic dose to the infant thyroid. While the possibility exists that the carcinogenic action may be an indirect, hormonal one, it must still be recognized that this, like leukemia, is an instance of significant carcinogenesis by less than 1000 rep. It seems likely that the infant thyroid is unduly suspectible, but that the adult thyroid is not.

Radiation from Particles and Hot-Spots.—One matter which has caused some concern is the effect of intense radiation of a few cells from particulate sources of radiation.

The only available experimental evidence that bears on this question is an experiment in which the skin was irradiated with beta rays, diffusely over the surface, and by point sources yielding the same amount of radioactivity. It was shown that the point sources were considerably less efficient.

In the case of the skeleton in chronic radium or radiostrontium poisoning, a large part of the dose is from the hot-spots due to concentration of the radioactive material in a few haversian systems that were forming at the time the radioelement was administered. All of the integrated skeletal dosages shown in table 2 (except from X rays, where the bone dose is about four times the air dose given) must therefore be looked at with the consideration that the maximal dose in these hot areas is about ten times as great as the average.

Permissible dosage to Large Populations.—This is a matter on which no complete agreement was reached by the Subcommittee. First responses to this question ranged all the way from the permissible industrial level down to no radiation at all. The uncertainty existing here stems from our ignorance as to whether there is a true threshold for such late effects as malignant tumors, and as to the degree of variation in response to equally exposed individuals.

It was agreed that the only rational approach must take into account the natural radiation background to which the population is exposed. Figures on this, from various sources, are given in table II. It is noteworthy that considerable differences exist from place to place, due mainly to differences in gamma radiation from the environment, and in part to variations in radium content of individuals. Since these existing variations have not given rise to any changes in incidences of tumors or other pathologic states sufficient to attract attention, it was felt that an amount of internal radiation sufficient to double the large population background could certainly be considered safe.

Part B of table II shows integrated skeletal radiation dosages which have given rise to various degrees of pathologic change. Two patients from the Elgin Hospital series are included, since it is known that pure radium was administered and the retention curve has been determined. It is noted that tumors require dosages in the thousands of rep; that observable pathologic changes have required a few hundred; that the natural background (including skeletal radium) varies from 7 to 30 in a lifetime (higher in isolated areas, perhaps); that large populations in high radium areas approach 5 rep in a lifetime from natural skeletal radium alone; while 1/1000 of the permissible strontium burden yields 0.2 rep in a lifetime. Exceeding this latter burden by fifty times would yield 10 rep in 70 years. This would not more than double the usual low skeletal background radiation and leave it well within the range of values; the highest backgrounds encountered in any large areas would be raised about one-third. The general belief of the Subcommittee is that this would produce no perceptible effect.

It is noted that the International Commission on Radiological Protection, using a somewhat more arbitrary procedure, has adopted a figure twice as great for the large population (one tenth of the industrial permissible level) so that there is a large measure of agreement.

Therapy by Removal of Radioelements.—This subject was not discussed by the Subcommittee, but was treated thoroughly at a meeting in October, 1955, the transactions of which will be published by Argonne National Laboratory. A summary of the present status follows:

Clinical and experimental evidence to date shows that there are two effective methods of removing radioelements from the body or prophylactically minimizing their deposition. These are the use of zirconium citrate and of chelating agents, particularly ethylenediamine tetracetic acid (EDTA). Both of these have their optimum effectiveness if given immediately after exposure. The use of the two methods in combination, at least experimentally, appears to be more effective than either one alone.

The chelating agents are mainly effective in removing radioelements from the soft tissues and causing their excretion. Under optimal conditions (that is, large doses administered early) they reduce bone deposition by a factor of two. They are effective on the transuranic elements and rare earth fission products, but for known chemical reasons they are not effective on the alkaline earths such as strontium and radium.

Zirconium citrate appears to be effective as therapy for almost all types of fission products, as is to be expected from the postulated modes of its action. It is particularly useful in minimizing bone deposition of radioelements and if given early, at least in the dog, it has been shown to remove almost all the plutonium from all the tissues.

No method has yet been developed for the removal of significant amounts of strontium. From the chemical standpoint the only promising approach to date appears to be one using agents which pick up the radio element by cation exchange, such as zirconium citrate. Other experimental approaches, particularly involving

dietary and hormone therapy with known influence on skeletal metabolism, are under investigation without clear clinical implications at present.

Treatment of individual situations will necessarily be influenced by consideration of route of entry (e. g. lung or in a wound) and the isotopes involved, bearing in mind that most of the experimental work has dealt with intravenous administration and that clinical experience to date has been severely limited.

TABLE I.—*Data on Fallout (calculated from Tsuzuki, loc. cit.)*

A. ANALYSIS OF "ASH" FROM TEST OF MARCH 1, 1954, MADE ON MARCH 26

Isotope	Half-life	Percent of total activity on March 26		Relative atomic yield	Fission yield
		Measured	As of Mar. 1		
Fission products:					*Percent*
Sr^{89}	53 d.	1.0	1.4	74	4.6
Sr^{90}–Y^{90}	27 y.	0.04	0.04	200	5
Zr^{95}–Nb^{95}	65 d.	8.0	10.5	(500)	6.4
Y^{91}	61 d.	8.0	10.8	660	5.9
$Ru^{103,106}$; $Te^{129,132}$; $I^{131,132}$	----------	15.0	?	?	13.5
Ba^{140}–La^{140}	12 d.	11.0	50	300	6.1
Ce^{141}	33 d.	7.0	12.3	410	6.0
Ce^{144}–Pr^{144}	282 d.	4.0	4.3	610	5.3
Pr^{143}	14 d.	16.0	59	830	6.0
Nd^{147}	11 d.	9.0	46	510	2.6
				(Relative to fission yield)	
Activation Products:					
S^{35}	87 d.	0.05	0.06	5.2	0.05
Ca^{45}	152 d.	0.2	0.22	34	0.3
Other:					
U^{237}	7 d.	20	260	1,820	18
Pu^{239}	24,000 y.	0.0004	0.0004	3,500	35

NOTE: After extrapolating activity back to March 1, relative yield is obtained multiplying by the half-life in days. Where 2 isotopes were measured, half of this value is taken for the parent; an intermediate value was taken for Zr^{95} since the daughter has a 35-day half-life.

B. ANALYSIS OF AUTOPSY MATERIAL 207 DAYS AFTER THE FALLOUT (FIGURES IN $\mu c \times 10^{-3}$ PER KILO WET WEIGHT)

	Liver	Kidney	Lung	Muscle	Bone
Ru and Te	----------	0.9	----------	0.2	2
Corrected for decay (as Ru^{106})	----------	1.3	----------	0.3	3
Zr and Nb	1	1	0.4	0.3	2
Corrected for decay	9	9	3.6	2.7	18
Ce and Pr	2	1	0.5	0.5	20
Corrected for decay	3.4	1.7	0.8	0.8	34
Sr	0.6	0.4	0.1	----------	1
Sr^{89} (if 97%)	9.0	6.0	1.5	----------	15
Sr^{90} (if 3%)	0.27	0.18	0.05	----------	0.45

NOTE.—This indicates that internal radiation was well within permissible limits throughout, amounting to a few mrep/day. In the event the figures for skeletal Ce and Sr were transposed in the report, the Sr^{90} burden appears to be at the permissible level.

TABLE II.—*Background and effective radiation dosages*

A. NATURAL BACKGROUND RADIATION, MREP/YEAR

	Libby	Burch and Spiers	Sievert	Other
Cosmic:				
Sea level	35	16		Lea gives 730, which is probably in error.
5,000 feet	50			
10,000 feet	100			
15,000 feet	170			
20,000 feet	375			
Earth gamma		58	94-296	Sievert gives one value of 520.
Earth, granite	110			
Earth, sedimentary	43			
Over ocean	20			
Body K^{40}	19	18.2		
Body C^{14}	1.5	1.0		
Body radium (dose to skeleton)	[1] 6.7-67			Rochester, N. Y., value 16.

[1] Extremes in Illinois.

NOTE.—This emphasizes the variability in background, even at sea level. In various localities the value might vary from 100 to 420 mr/year; the latter might be taken as the maximum which any large population receives.

B. VARIOUS LEVELS OF SKELETAL IRRADIATION (IN REP)

Patient* with sarcoma (pure radium) at 24 years	6,000
1.0 μc radium retained 24 years after dosage	2,000
Patient* with minimal skeletal changes, 24 years	800
Permissible burden (0.1 μc) sustained 24 years	100
Minimum dose of x-ray reported to induce tumor	1,500
Normal range of background radiation, external and internal, 70 years	7-30
High large-population Illinois radium level, 70 years	4.7
1/1000 permissible Sr^{90} level, 70 years	0.2

NOTE.—patients cited are known to have received pure radium injections. In other reported instances where the thorium chain may have been included in the dose, the radium dose would be calculated as low as 300 rep.

Recommendations.—The Subcommittee made the following suggestions in relation to its study:

Attention should be given to the physiological state of the animal in relation to absorption and toxicity of radioelements, particularly in the case of absorption through the usual routes (lug and gastrointestinal tract). Present information is partial and is practically limited to the alkaline earths and iodine.

Further information on the retention of alkaline earths as a function of age and species and other variables, is needed.

The relation of experimental data on life shortening to the probable picture in man needs clarification, and further verification of the apparent lengthening of life at low doses. These problems are of importance in the isotope toxicity field as well as in relation to external radiations.

The past work on distribution of various radioelements which have not received intensive study should be extended, since unusual radiochemical toxius may be expected to appear occasionally.

Account should be taken of the applicability of the power function to the retention of radioelements, since it would appear that in some cases the present intake levels are much too stringent owing to our past reliance on the half-life concept. Further critical evaluations of the RBE, particularly for alpha radiation, is very desirable for similar reasons. The RBE should be evaluated separately for the several modes of damage.

It is believed that there may be a considerable number of persons who have received radium in the past, who are alive and not seeking medical help. Any means which could be found to obtain an unbiased group of individuals would be extremely desirable, since only in this way can the degree of variability in human response be estimated. For similar reasons, any promising environmental study involving areas of different natural background should be encouraged.

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APPENDIX IV

REPORT OF THE SUBCOMMITTEE ON PERMANENT AND DELAYED BIOLOGICAL EFFECTS OF IONIZING RADIATIONS FROM EXTERNAL SOURCES

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PERMANENT AND DELAYED EFFECTS OF IONIZING RADIATIONS FROM EXTERNAL SOURCES

I. INTRODUCTION

While it was recognized soon after their discovery that x-rays and the radiations from radioactive materials could cause acute injury to living tissue it did not become apparent until later that they could also give rise to more subtle permanent and delayed effects which are of prime significance in considering the problem of permissible human exposure.

Laboratory studies of long term effects have been relatively few, partly because their importance was not early appreciated and partly because they are very expensive and time consuming owing to the necessity for maintaining considerable numbers of animals for all, or most, of the their normal life spans. In consequence the data on late effects in animals are meager in certain areas. Such data as there are in man are sufficiently in agreement with those on other mammals to lead to the expectation that extrapolation from lower mammals to man will be possible with fair accuracy.

It is the present purpose to review the long term biologic effects (other than genetic) of radiations from external sources, with emphasis on man, although results of animal experimentation will be drawn upon to illustrate fundamental principles and mechanisms in radiation biology.

II. PERMANENT AND DELAYED EFFECTS OF IONIZING RADIATION IN GENERAL

If animals, which have been irradiated, escape death initially they appear to recover but tend to die prematurely. It is becoming established that in mammals, shortening of life span is a general effect of whole body exposure to ionizing radiation.

There has been a tendency to seek the cause of premature death as a consequence of irradiation in increased incidence of specific disease, especially cancer. While this may be justified in the case of partial body irradiation from external sources or from locally deposited radioactive materials, it probably is not for whole body irradiation. There is evidence that at the median death time populations of irradiated animals have approximately the same incidence of the same diseases as do the controls at their median death time. This is more reasonably interpreted as indicating that irradiation produces primarily the syndrome of premature aging, with concomitant disease, rather than that it, in itself, induces all the diseases of advanced age.

The major permanent and delayed effects of total-body irradiation may be listed categorically in terms of pathologic entities as follows:

A. Increased incidence and/or severity, at given ages, of disease entities to which particular animal species or populations are susceptible. This may occur, and probably usually does, largely as a result of accelerating the time of onset of these diseases and may therefore be considered a part of the acceleration of aging produced by irradiation. It appears that the clinical history of the animals is compressed or telescoped in time in these respects. Included with these disease entities are cancers of various types to which the animals are susceptible, and possibly also cataracts which develop late in a species in a manner similar to the development of cataracts with age in that species.

B. Temporally accelerated involutional, hypoplastic or atrophic, and fibrotic changes in tissues and organs, which changes do not constitute individual disease entities but which are identical in appearance with those occurring during the "normal" course of aging. There is a high degree of correlation between the degree of temporal acceleration of this pathologic picture of accelerated aging and the size of total-body dose or daily dose rate. It should be emphasized, however, that an accurate evaluation of the degree of acceleration of involutional changes produced by certain doses can only be observed in experimental animals at times, after single doses, long after maximum regeneration and repair of the single insult has occurred, or with chronic irradiation, after irradiation has been stopped and recent acute effects on cells have repaired as much as they will.

Although differences exist among species, some of the radiation-accelerated senescent changes in tissues and organs which have been observed are:

(1) Hypoplasia or atrophy of many tissues and organs, especially the thymus, lymph nodes, splenic lymphatic tissue, bone marrow, germinal epithelium, skin, and bone growth centers. Hypoplastic changes in hemopoietic organs are reflected in lowered blood cell counts.

(2) Fibrotic changes which are fairly generalized, but which are commonly noticed in blood vessels generally, in skin where there appears to be increased collagenous tissue and decreased or degenerate elastic tissue, in and/or around lymphatic organs and endocrine organs, and in the myocardium, where focalized fibrosis occurs with age. If the small arteries of the kidney are affected sufficiently, there may develop nephrosclerosis and hypertensive disease.

(3) Pigmentary changes, including greying of hair and increases of pigments in cells of the epidermis, connective tissues, endocrine glands, myocardium and other tissues.

General physiologic changes which are usually associated with aging and which may be accelerated by irradiation include, among others: decreased muscular strength and endurance, lowered recuperative powers and tissue repair capacity and speed, lowered fertility and potency, generalized reduction of elasticity, and increased blood pressure.

Factors included in these first two categories are among those which constitute the general change which may be called "radiation-accelerated aging." The fundamental mechanisms of this change are as incompletely understood as are those of "normal" aging, except that radiations and their known effects are recognized as accelerating agents. The subsequent development of the histologic changes in both radiation-accelerated and "normal" aging appear qualitatively identical. However, it is at present sometimes difficult to distinguish between accelerated aging effects and induced pathologic effects except by arbitrary definition.

C. Induced disease entities or definitive effects, not necessarily specific for irradiation, which rarely occur spontaneously in animals of any age in the species in question, or certain effects and diseases whose know pathogeneses may differ from those occurring in the aging process but which may produce similar final results. In the latter case such effects occur earlier than the specifically comparable effects associated with accelerated aging.

Included in this category are, among other effects:

1. Nephrosclerosis and related hypertension produced essentially by the relatively direct damaging effects of radiation on cells in the kidneys, e. g. components of vessel walls, so that this disease occurs well in advance of any considerable degree of acceleration of other senescent changes. According to some data, renal hypertension once established and progressive, may increase the vascular sclerotic processes in many regions throughout the body and these changes are sometimes associated also with progressive hypoplasia or atrophy of organs in which the vessels become fibrotic.

2. Cataract formation which occurs well in advance of the generalized aging processes if the radiation dose to the lens has been sufficiently high to damage or destroy cells of the anterior epithelium and/or depolymerize the cement substance between fibers. There are two general histologic types of radiation cataract, the purely vacuolar type, which is reversible, and the granular type which is irreversible. The senile cataract, although similar in the ultimate effect to radiation cataracts, is generally more progressive, occurs usually when other aging changes are considerable, and may have a different pathogenesis.

3. Decreased fertility resulting from direct damaging actions of radiation on gametogenic cells and gametes. The mechanisms by which direct actions of radiations decrease fertility are quite different and more numerous, as far as we know, compared with those involved in decrease of fertility due to aging or accelerated aging.

In senescence of the testis, as in testicular damage resulting from infectious and cachectic diseases and physical and chemical damaging agents, the spermatozoa and more mature spermatogenic elements are affected first and most markedly in the seminiferous epithelium, and decreased fertility, if potency is retained, is due to lack of production of sufficient numbers of spermatozoa. In the case of the senescent ovary, there is depletion and/or failure of development of follicles, which is associated with a complicated endocrine disturbance.

Irradiation effects on the gametogenic cells and gametes themselves may decrease fertility in more than one way, prior to and in addition to, the effects involved in acceleration of aging, and these mechanisms, other than those genetic and relating to the fertility of future populations, are as follows:

(a) by reducing the number of gametes produced through effects on primitive radiosensitive precursors of the gametes. This reduction may be partial or complete, or temporary with partial or complete recovery, or permanently complete, depending upon dose. The number of gametes produced may be reduced also indirectly by other diseases which irradiation causes or accelerates.

(b) by damaging in various ways the gametes produced so that they are incapable of fertilization.

(c) by damaging chromosomes of gametes in such ways that they are capable of fertilization but produce zygotes which are incapable of complete and normal development to full-term viable foetuses and die *in utero*. The results of such effects constitute the condition known as "semisterility," which is genetically transmissible to viable offspring.

4. Increase in incidence of other non-neoplastic diseases not common to the species even during the senescent period. There are not sufficient conclusive data in this area of the problem to warrant discussion, but it would appear worthwhile to indicate that research is needed in this aspect of the study of long-term effects of irradiation.

5. Induction of neoplasms rarely, if ever, occurring spontaneously in a species, but caused by the destructive actions of radiation on specific tissues with the initial establishment of precancerous states. It is possible that the later influences which cause the development of neoplasia from these precancerous states may be brought about by the same mechanisms which cause the development or accelerated development of the neoplasms which are of common occurrence in the species with advancing age.

III. PERMANENT AND DELAYED EFFECTS OF IONIZING RADIATION IN PARTICULAR

A. The Shortening of Life Span by Ionizing Radiation

Since there is some evidence that radiation effects depend upon the age of the animal at the time of exposure, some consideration will be given first to the lethal dose as a function of age.

1. *Lethal Dose as a Function of Age.*—The average of median acute lethal dose, LD_{50}–30 days, for young adult mammals is similar for those species which have been studied and lies within or near the range 600 ± 300 roentgens. While it is customary to refer to LD_{50} of a given strain as if it were a specific property, independent of age, this is not justifiable.

In the mouse the susceptibility is maximal at 30 days then decreases rapidly to that seen in young adults. In the rat, which has been studied more extensively, the LD_{50} at three months of age is about double that at three weeks. Beyond three months it diminishes with age and there is some indication that, for the adult animal, the LD_{50} decreases about as the life expectancy. More study of this relation is required to make it wholly quantitative, but it is evident, now, that the susceptibiliy of a whole population is not describable by a single LD_{50}. The published values are usually obtained from the young adult and are therefore maximal or nearly maximal for the strain. In attempts to estimate LD_{50} in man this age dependence should be taken into consideration.

2. *Life Shortening by Single Doses.*—Existing data on rodents subjected to single whole-body doses of radiation are compatible with the view that life is shortened in proportion to the dose for doses less than about two-thirds of LD_{50}. In this range life shortening is about 25% of the adult span per LD_{50}. With greater doses this effect increases more rapidly, attaining about 50% for survivors of LD_{50}. No similar data are available for longer-lived mammals, or for man, but it may be possible eventually to obtain some estimate of the effects of single doses in man from the Japanese survivors.

There are a few data from the rat indicating that shortening of life for a given single dose is about the same, independently of the age at which it is administered to the adult animal, providing the animal is destined to live long enough to make the shortening wholly manifest.

Owing to the difficulty of detecting small changes with limited numbers of animals it cannot be assumed with confidence that life-shortening is proportional to dose down to small doses at the rate given above. Actually there is reason to suppose that the effect of small doses is less than that indicated by the present data. This point will be discussed in considering the effects of chronic irradiation.

3. *Life Shortening by Multiple Doses or Chronic Irradiation.*—Small laboratory animals subjected to irradiation either at high daily rates for short periods or low daily rates for long periods suffer about 7% life shortening per LD_{50} and the effect is proportional to dose, or nearly so, for doses up to about three times LD_{50}. The data for low daily rates and small total doses in the 100 r range are not definitive, there being almost as many showing prolongation, as shortening, of life. While this again is probably due to failure to use sufficient numbers

of animals to measure small effects, it leaves to be resolved the possibility that low daily doses actually prolong life, unlikely as this may seem.

No satisfactory explanation has been offered for the greater shortening of life from single doses than from divided doses of the same total magnitude. Nevertheless the difference appears to be well established for large single doses. However, because divided doses, at rates at least as high as 120 r per day, cause the same effect per accumulated roentgen as divided doses at quite low daily rates, it is difficult to understand why a single dose of 120 r would not act the same. Possibly it does and the apparent disagreement is owing to inaccuracy of single dose data for doses in the 100 r range and below.

4. *The Concept of Irreversible Injury.*—To account for shortening of life as an after effect of irradiation it may be supposed that radiation injury is in part reversible and in part irreversible and that the irreversible component is equivalent to premature aging in the sense that it ultimately deprives the animal of part of its expected life span. This view of irreversible injury does not prescribe whether it is equivalent to abrupt aging at the time of injury or consists in the initiation of aging processes which gradually develop. It is interesting that limited observations in the rat indicate that irreversible injury is measurable, after an interval of presumed complete repair, as a reduction in acute lethal dose. This suggests that aging or its counterpart is laid down at least partially at the time of injury and is potentially observable as some form of persisting lesion, but as yet this phenomenon has not been related to histopathologic changes to be discussed below.

5. *The Effect of Chronic Irradiation in Man.*—The deaths of 82,441 physicians reported in the Journal of the American Medical Association from January 1, 1930, until December 31, 1954, were reviewed by Shields Warren.[1] In physicians grouped according to possible radiation exposure the average age of death was as follows:

	Years
No known contact with radiation	65.7
Some exposure (dermatologists, gastroenterologists, tuberculosis specialists, urologists)	63.3
Radiologists	60.5
U. S. population over 20 years of age	67.1

In comparison with non-exposed physicians the shortening of life of radiologists is 5.2 years or 11% of the adult life span (after 20 years). If extrapolation from the animal data, reviewed above, is permissible, this would be expected to result from chronic whole body exposure to about 1.5 LD_{50} dose or possibly 1000 roentgens. Although this exposure was partial body and possibly less effective, it seems unlikely that the equivalent whole body exposure differed from the above value by a factor greater than 2 or 3. Consequently it appears that, within these limits at least, extrapolation from short-lived animals to man may be made with some confidence on the basis of percent life-shortening per unit dose.

B. Acceleration of Aging by Irradiation

There are no definitive data on the effects of total body irradiation on the aging processes *per se* of man. Perhaps some information on this effect in man will be forthcoming in time as a result of observations which may be made on survivors of the atomic bombing in Hiroshima and Nagasaki. The delays effects which have been observed in these Japanese are reported subsequently under other headings.

In view of the relation between the degree of acceleration of aging observed histologically in experimental animals and the size of the total body dose which the animals received, and in view of the probability that populations of irradiated animals lying prematurely die essentially as the result of premature aging and associated diseases, it seems reasonable to expect that a direct relation may exist in irradiated populations between the amount of life shortening caused by irradiation and the degree to which irradiation has accelerated aging, and that any total-body dose of external radiation which has caused shortening of life in a population of animals has caused a proportional acceleration of aging.

Some of the localized tissue effects produced by partial body irradiation in humans are histologically similar to changes occuring in the syndrome of premature aging. However, many of these changes, some of which are mentioned

[1] Before the Radiation Research Society, May 19, 1956, Chicago, Illinois.

below, are effects induced directly rather than phenomena of accelerated aging.

In the human skin single loses of 500 r to 700 r of x-rays may produce permanent epilation. Somewhat smaller doses causing temporary epilation may cause decreased pigmentation or greying of hair which returns in the irradiated areas. These doses in the erythema dose range or somewhat higher may also cause increased pigmentation of the skin in the irradiated regions, some degree of epidermal atrophy, and some decrease in sebaceous and sweat glands. Hyperkeratotic areas of skin, vascular sclerosis, and dermatitis may also be late sequelae of irradiation of skin. Surface doses of about 1600 r more or less may produce considerable permanent dilatation of capillaries (telangiectasia) in the area irradiated.

Changes in skin are also late effects of chronic irradiation and are seen commonly in the skin of the hands of persons working with radiations. Roentgen dermatitis and the roentgen ulcers which often develop from this condition are considered precancerous conditions because of the frequent development of malignancy in such regions.

Renal hypertension may be produced in man within periods from several months to several years by single localized doses of x-rays of about 3000 to 5000 r or by chronic irradiation, e. g. by a total dose of 2300 r in 35 daily doses to the abdomen, but there are no definitive dose-effect on the late incidence of kidney disease and arteriosclerosis related to generalized radiation acceleration of aging in man. In rats, nephrosclerosis, with renal hypertension, and generalized arteriosclerosis are characteristic delayed effects associated with accelerated aging changes late in their lives following single total-body doses of 500 and 600 r or greater. Much larger doses localized to the kidney are required to cause these renal changes and associated effects to appear early in the life of irradiated animals.

Irradiation of parts of the brain of man with total doses of about 5000 r or more given as a single dose or within 2 or 3 weeks in fairly large fractions may cause progressive sclerosis of blood vessels with subsequent secondary degeneration of brain tissue and sometimes rupture of blood vessels and hemorrhage from one to several years after exposure.

Hypoplastic and atrophic changes, often associated with arterial sclerosis, have been observed in human hemopoietic organs long after localized irradiation. Permanent effects have also been observed in bone heavily irradiated, and in the length of bones irradiated heavily over their epiphyseal ends during the growth period.

The fragmentary data on delayed effects of localized irradiation in human tissues which are similar qualitatively to changes which are associated with aging, are difficult to interpret in terms of accelerated aging, especially since many of the human cases from which the data are obtained suffered from malignancy or other serious disease processes.

C. Late Hematologic Effects of Irradiation

Data collected during the period after World War II may be summarized very briefly as follows:

With respect to occupational or daily exposures over periods of months or years, doses in the range of 0.5 r/day have been reported effective in producing slight depression of the number of circulating lymphocytes and total leukocytes in the peripheral blood of man. A 77 week period of exposure to doses averaging 0.2 r/week effected a decrease in the number of leukocytes in another group of workers. One authority has found some statistical evidence indicating that doses as low as 0.5 r/year may depress the lymphocyte count very slightly. These and similar findings (which are considered in more detail and tabulated in the review cited at the end of this paper) are not unequivocal, of course, and do not indicate with certainty that injury has occurred to the personnel in question, but they indicate in a general way, the presently accepted estimates of the "lowest effective dose."

Slightly larger exposures, for example a total X-ray dose of 40 r given in increments of 15–20 r, or a total dose of 200–300 r received as a series of daily 5–10 r exposures, caused depression of the peripheral blood lymphocyte level in man, the larger cumulative dose being associated with decreased numbers of all types of white blood cells.

Survivors (924) of the Hiroshima atomic bombing, receiving doses estimated at 400 r total-body, and all showing epilation, revealed relative lymphopenia 2 years after exposure. There was greater than normal variability in the blood picture of this group. Four of 5,075 survivors exposed at less than 1,500 meters in the

Nagasaki bombing revealed, after latent periods of 4 to 7 years, refractory (fatal) anemia, with associated leukopenia and thrombocytopenia.

A statistical analysis in 1954 of the hematologic data obtained by the Atomic Bomb Casualty Commission on Hiroshima survivors 5 to 8 years after the bombing indicated that there was no increase in leukopenia, leukocytosis, or anemia in the exposed as compared to the control population. Up to 1953 no cases of aplastic anemia had been found in the survivors in Hiroshima.

Observable changes in the structure of leukocytes (in contrast to the changes in numbers described above) appear to offer considerable promise as sensitive biological indicators of radiation exposure.

Two of the most sensitive morphologic indicators of radiation effect on blood are increased numbers of refractile neutral red bodies in lymphocytes (observed in humans receiving .05 r per day) and an increased incidence of lymphocytes with bilobed nuclei in peripheral blood, which has been observed in a considerable number of cyclotron workers after they had worked about 3½ months during which they received exposures which did not exceed the M. P. E. The incidence of the abnormal lymphocytes returned to normal after extra shielding was installed.

Although there is much conflicting information and opinion relative to late hematologic effects of exposure to ionizing radiation, a review of the available data leads to the formation of certain fairly clear impressions if not definite conclusions.

Early radiologists and radiation workers developed blood pictures characterized by moderate lymphocytosis and leukopenia. These changes were often sufficiently definite to be recognized on an individual basis, as well as by statistical analysis of grouped data. They have been interpreted in various ways, of which the most likely seems to be as follows: initial injury causes a depression of lymphopoiesis, which is followed by a recovery phase characterized by a compensatory increase in activity. During chronic exposure lymphopoiesis presumably "escapes" from the depressing effect and enters the compensatory hypertrophy state. Most of the early workers had exposures which would undoubtedly greatly exceed present maximum permissible doses.

Studies made during the more recent period reveal that the most characteristic changes following chronic exposures in or below the maximum permissible dose range are slight lymphopenia and morphologic alteration of the leukocytes, particularly lymphocytes. The absence of lymphocytosis might be explained by the fact that even chronic exposures are much more intermittent now than earlier. Furthermore, since both injury and the compensatory hypertrophy are, within limits, proportional to the magnitude of exposure, it is possible that present chronic exposures are too slight, as well as too intermittent, to produce adequate stimulus for "escape" into the compensatory hypertrophic phase.

In any event, morphologic changes are probably the most sensitive indication of radiation injury. This is not surprising, for one might expect to find larger numbers of young cells being released into the blood stream following transient bursts of increased leukocytopoiesis. Furthermore, when parent cells have been injured by radiation, abnormalities of mitosis (with the production of abnormal daughter cells) would be expected to occur more frequently than normally. Hence abnormal as well as early cells should appear in the peripheral blood in increased numbers.

The relationship of this type of change to the incidence of leukemia and other latent effects of exposure is very poorly understood, and it is highly desirable that more information be obtained. The compilation of data representing sensitive hematologic indices of radiation exposure in large groups of radiation workers should be vigorously pursued so that eventually there may be enough long-term studies of the health of the workers to permit adequate evaluation of the significance of the more sensitive radiation-induced biologic changes. Should such changes prove to be truly premonitory of an increased incidence of latent effects, it would be important to adapt biologic monitoring procedures accordingly.

One of the greatest hindrances to the present interpretation of highly sensitive hematologic and presumably other biologic changes is the absence of adequate physical monitoring data for exposures below the maximum permissible dose range, so that evaluation of exposures of individual radiation workers is grossly inadequate for the interpretation of hematologic data of the type under discussion. It is important that in selected situations, individual physical monitoring be instituted which is in the same range of sensitivity, with regard to quantitative interpretation, as the biologic monitoring.

D. Carcinogenesis by Radiation from External Sources

This section deals primarily with malignant disease in human populations exposed to ionizing radiation.

It has been clearly established that malignant disease may arise in tissues heavily irradiated by ionizing radiation. Animal experimentation and experience with irradiated human subjects show that almost any tissue can become neoplastic under the proper conditions of exposure. This report does not treat of the type of radiation-induced malignancy which is usually the result of intense, repeated exposure of a small portion of the body, but will consider only malignant disease in populations in which the entire body or at least a large portion of the body of human subjects has been exposed to acute or chronic doses of ionizing radiation from external sources.

Leukemia is commonly associated with exposure of the body to radiation. The close association between exposure and the disease has been described under three different conditions of exposure. The first is that noted in the radiologists who have been chronically exposed. It should be pointed out that the actual number of cases of leukemia is not great even though the incidence is much higher than that noted in the general male population and in other physicians. March, in a review, was able to find only 37 published cases. Furth and Lorenz state that one reason for such a low incidence of leukemia in these persons who were probably heavily exposed in the early days of x-ray technology is that their exposure was partial body rather than total-body. The average age of the 14 radiologists dying of leukemia from 1928–1949 as 58.8 years.

The increased incidence of leukemia in the Japanese exposed to the nuclear explosion in Hiroshima and Nagasaki is the only example of this disease occurring after a single acute exposure of the entire body to ionizing radiation. In this case there is a good correlation between the diseases and the dose. Even in the highest exposure group, however, the incidence is still small (1.25%). Most of the cases are of the myeloid type. However, lymphoid leukemia is known to be comparatively rare in Japan.

There are two examples which illustrate the increased incidence of leukemia under the third condition, i. e., that observed in persons given therapeutic treatment with x-rays to a large portion of their body. One is found in persons with ankylosing spondylitis who have received intensive treatment with x-rays to the entire length of their spine. A total dose of 2,000 r was not unusual in a treatment series and such series were often repeated. There is a good correlation between the incidence of leukemia and the number of treatment courses. The second example is found in children given one or more treatments to their chests in infancy for enlargement of their thymus glands. In one study of children receiving 100 to 1500 r in 1 to 3 treatments to the chest, 7 of 1,722 treated children developed leukemia. This incidence was ten times that expected for children in the state in which the study was done. There were no cases of leukemia in 1,795 untreated siblings at the time of the study. There were 3 cases of leukemia among 604 children receiving less than 200 r, and 4 cases among 804 children receiving more than 200 r. The children in this study showed leukemia incidence of about 0.4%, but they may represent a special situation since the treatment was given when they were very young and since there is no apparent correlation with the x-ray dose.

Aside from leukemia, the only studies on malignant disease in persons exposed to radiation from external sources are found in statistical surveys by Dublin and Spiegelman and by Warren of the cause of death in radiologists and other physicians. Dublin and Spiegelman find that the incidence of malignant disease is lower in physicians than in the general male population of comparable age. It is slightly higher in radiologists but, apparently, not significantly different from that in other specialists or non-specialists who presumably have not received the same exposure to radiation. Interesting is the observation that the highest cancer incidence, twice that in the total group, is found in psychiatrists and neurologists. The data of Shields Warren on a much larger series of physicians show a somewhat higher percentage of cancer deaths in radiologists than in other physicians.

In consideration of animal experiments, uncertainty exists as to whether or not there is a true dose threshold for the production of malignant tumors by irradiation. The answering of this question would require an extremely expensive, massive experimental program employing very large numbers of animals and much time. Experimentation of this kind with low doses is further complicated by the occurrence of spontaneous malignancies to which various

species of animals are susceptible, and also by the variability in response of equally exposed animals. If the fundamental cause, or one of the indispensable factors, in radiation carcinogenesis is the induction of somatic mutations, it would appear possible that radiation carcinogenesis, or perhaps the induction of precancerous states by irradiation, has no dose threshold. However, this reasoning may be applied to other disease states and accelerated aging as well.

In any event the incidence of cancer in exposed populations is not sufficiently great to be regarded as an important contribution to the degree of premature death occurring in a group such as that of the American physicians discussed above.

E. Radiation Cataracts

In this section emphasis is placed upon the effects of the radiations from nuclear disintegrations and high energy particle accelerators on the development of ocular, especially lenticular, lesions in humans. This in no sense deprecates the excellent work done with other animals in which the pathologic development, biochemical changes, and dose-time relationships of lenticular abnormalities have been elucidated. The present reservoir of several thousand persons who have been exposed to the radiations from atomic weapons, and the few hundred humans exposed to the beams of particle generators during the last ten to fifteen years makes it apparent that a reasonably accurate evaluation of the magnitude of the problem of radiation-induction of cataracts in humans can be made from the information currently available.

There are no quantitative or definitive dose-effect data from humans or animals in regard to increased incidence of cataracts late in life as a result of radiation acceleration of aging processes. It is hoped that long-term observation of persons exposed to radiation and of animals in life-span experiments will provide such information.

It is not surprising that a great variety of possible causes of cataracts have been discovered. Histologically the lens is such a simple structure that its possible ultimate response to injury is limited almost exclusively to cataract formation. Considering only idiopathic forms of the disease and excluding those cataracts resulting from injury, including radiation injury, metabolic disease, congenital defects, etc., one is left with a phenomenon definitely related to increasing age.

Despite much effort it is still not yet clear just how the senescent process causes this local change. However, there are differences histologically in the development of this change as compared with the development of radiation-induced cataract. In the aging process the lens grows continuously throughout life but the growth rate becomes slower with advancing age, never reaching zero until the tissue dies in cataract formation. Radiation-induced cataract is the result of direct destructive actions of radiation on the anterior epithelium and possibly on the cement substance between fibers.

It is interesting to note that before it was recognized that radiation cataracts were appearing in cyclotron workers, Evans reported in 1948 that cataract production in mice by fast neutrons relative to x-rays increased significantly with chronic exposure. Young animals exposed during the pre-natal or early post-natal period show markedly greater lenticular radiosensitivity than do older animals.

By December 1948 it was known that at least five nuclear physicists of mean age 31 had incipient cataracts. In January 1949, eleven physicists were examined and ten were found to have cataracts, in three cases severe with definitely impaired vision, in four cases moderate, and in three cases minimal. They were estimated to have received, over periods of 10 to 250 weeks, a median dose of fast neutrons of 50 *n*, while the range of doses was 10 *n* to 135 *n*. At the time the cataractogenic exposures were being received, most of the men were given periodic blood counts, which revealed no change in blood picture warning of overexposure to radiation.

In adult humans exposure to x-rays in excess of about 2000 r has been thought necessary to produce cataracts.

Fillmore, in a survey of the Hiroshima Japanese survivors, based in part upon studies by Kimura in 1949, about 5 years after the detonations, reported 98 cases of cataracts, eighty-five of which were among the 922 survivors 1000 meters or less from the hypocenter. In 1955 Sinskey reported the results of an intensive investigation of 8700 exposed and nonexposed individuals made between May 1951 and December 1953, six to eight years after exposure. There were 154 survivors with posterior subcapsular polychromatic plaques large

enough to be visible with the ophthalmoscope. These radiation-induced pathologic changes in the lens did not in general impair vision significantly when examined, and in most cases were correctable with proper lenses to provide normal vision. Of this group only 25 individuals had vision less than 20/25.

According to Sinskey's study, the human lens is quite sensitive to nuclear radiation in doses which produce epilation and other acute effects but are insignificant with respect to impairment of vision.

Of the approximately 8000 exposed survivors of Hiroshima and Nagasaki who have been examined during the last decade there have been found 10 cases of severe cataract, approximately 25 cases of slightly impaired vision due to posterior polychromatic plaques and perhaps two hundred cases with minimal pathologic lenticular lesions detectable by competent slit lamp examination.

It may be concluded that the atomic bomb explosions over Japan have resulted in negligible loss of vision to date.

F. Effects of Ionizing Radiation on Gametogenesis and Fertility

1. *The Male.*—Spermatogonia are the most radiosensitive cells of the seminiferous epithelium and one of the most sensitive of the body with respect to inhibition of division and with respect to the destructive actions of radiation. Apparently both inhibition of mitosis and destruction of spermatogonia, and differentiation of these cells following irradiation contribute to their disappearance from the seminiferous tubules, the relative contribution of each mechanism varying quantitatively according to size and mode of administration of dose.

The delay in the beginning of regeneration of these cells after irradiation is dependent to a considerable extent upon the dose. Following reduction or depletion of spermatogonia the later germ cell generations undergo maturation-depletion and disappear in the order in which they are formed until a point of maximum hypoplasia is reached. The destruction of some of the cells of more mature generations by relatively high doses may hasten this process. Spermatocytes, spermatids, and spermatozoa are of increasing radioresistance in the order given. The time for the development of maximum hypoplasia of the seminiferous epithelium is about 3 or 4 weeks, sometimes longer, depending upon dose and species, and this time is close to that required for the development of a spermatozoon from a spermatogonium.

Histological sterility, by definition a lack of spermatogenic elements and sperm, may be temporary or permanent and the two often appear very similar upon casual histologic examination. The time factor is of great importance in the prognosis as regards sterility.

For relatively low doses and for certain laboratory animals whose germinal recovery capacity is relatively large, regeneration, if it is to occur, begins before the height of depopulation of germinal epithelium is reached or soon thereafter. With certain higher doses given to such animals, a delay of beginning of regeneration for about 10 months is considered by some to be indicative of permanent sterilization. Actually, in much of the work employing large single doses of radiation the animals were not studied for the maximum time possible or desirable.

It is probable that the critical interval of time for beginning of regeneration varies among species and that for some of the larger animals, including man, whose powers of germinal regeneration are comparatively low, active regeneration may be delayed following severe radiation effects much longer than 10 months. Whether permanency of sterilization or length of the temporary sterile period is due to effects on cells involved directly in spermatogenesis or rather indirectly to effects on supporting tissues is not clear.

The testicular effects of irradiation are qualitatively similar in all mammals studied, including man, but vary quantatively according to differences in testicular radiosensitivity and recovery capacities among species. Whether a dose of radiation sterilizes permanently or temporarily depends at least as much on the natural capacity for regeneration of primitive spermatogenic cells as on the radiosensitivity of the spermatogenic cells existing at the time of irradiation.

Sertoli cells and interstitial (endocrine) cells are relatively radioresistant. Male mammals may be sterilized permanently without prominent histologic changes in the interstitial cells and without decrease in sexual potency or libido.

Following single doses of irradiation and preceding the sterile or subfertile period produced there is a period of fertility, the length of which is much less dependent on low doses than on doses high enough to affect the fertilizing capacity of mature sperm. Lower doses are required to destroy the fertilizing

capacity of sperm than are necessary to affect the viability or motility of sperm present at the time of irradiation and therefore during the initial fertile period.

This first period of continued fertility is due largely to sperm mature at the time of irradiation and possibly to some sperm in the spermatid stage or even a few in the spermatocyte stage, depending upon the dose and the length of the fertile period. The subsequent period of infertility or sterility is due to decreased numbers of sperm produced, and the fertile period following the period of sterility, if recovery occurs, is due to sperm that were developed from cells which were in the spermatognoial stage or were primordial undifferentiated cells at the time of irradiation.

In the initial fertile period litter size is subject to reduction with sufficient dose and the amount of reduction is dependent upon the dose. Litter size in the fertile period after the sterile period is usually normal or perhaps slightly less than normal. Reduction in litter size is explained on the basis of induction in sperm, and perhaps to some extent in precursors of sperm, of chromosomal aberrations which do not interfere with fertilization but which cause death of the zygote or embryo *in utero.* In terms of human considerations the equivalent result would be manifest in the form of increased incidence of spontaneous abortion following death of embryos or foetuses *in utero.*

This reduction in litter size caused by irradiation is called "semisterility", and the condition is transmissible genetically to viable offspring. Chronic irradiation at low daily dose rates appears to be much less effective in the production of semisterility, according to existing data. This may be explained on the basis that low doses are delivered to sperm populations which are continually renewed in the genitalia, and that relatively fewer sperm are subject to doses large enough to induce the chromosomal defects involved in semisterility. The lesser reduction in litter size during the second fertile period after irradiation with large single doses suggests that similar chromosomal defects in primitive spermatogenic cells or primordial undifferentiated cells are either not as significant in terms of the production of semisterility or are largely eliminated in some manner. More long-term investigations of this problem in chronic radiation experiments seems desirable to verify the degree to which regenerated sperm populations are defective in terms of semisterility.

The so-called sterile period may be a period of complete sterility or a period of subfertility or of fertility with reduced spermatogenesis, as manifest by partial atrophy of seminiferous epithelium and partially reduced sperm counts. Since critical or minimal numbers of normal potentially effective sperm per ejaculate are necessary for consistent successful reproduction, practical sterility or infertility may be associated with considerable but subnormal degrees of spermatogenesis. When spermatogenesis is partially arrested the number of sperm produced decreases and the percentages of sperm motile, alive, and normal tend to decrease also. With chronic irradiation spermatogenesis may stabilize at reduced levels for long periods of time if complete arrest does not occur, and further depressions may be slow in occurrence. Reduced sperm count and decreased quality of sperm persist accordingly.

The effects of irradiation on seminiferous epithelium are direct in that irradiation of the body with testes shielded does not produce them.

The effects of x-rays, gamma rays, and neutrons on spermatogenesis and reproduction are qualitatively similar, but neutrons are more potent in their effects on spermatogenesis and five or six times as potent in reducing litter size in matings done during the initial fertile period.

In regard to the efficiency of fractionated versus undivided doses of the same total size in producing testicular effects, there are experimental reports indicating no difference, others indicating less effect with fractionation, and others showing greater effects with fractionation.

Protraction of the dose fraction has little influence apparently on testicular effects unless the protraction is extreme, in which case the effect of a given total dose may be decreased, probably by virtue of permitting biologic recovery processes to operate at a more favourable rate with respect to the rate of production of injury by radiation.

The effects of fractionation of dose on the testes depends upon the size of the dose fraction, the interval of time between fractions, and the total dose. In general, fractionation has less influence on the effect of small total doses than on the effect of large total doses. Fractionation of large doses appears to increase damage in the mechanisms responsible for regeneration of germinal epithelium.

The dose-effect relationships in different species often appear contradictory, but are probably in reality complementary. For each species there is probably a

different dose-time relationship, in irradiation with divided doses, which is optimum for the efficient production of radiation injury. The empirical work which has been done on the testis has already made this apparent. Theoretically, in a tissue in which stem cells are radiosensitive and have the capacity both for active division and for differentiation, the most efficient mode of administration of radiation (per roentgen) to produce sterility in animals of a given species would be that designed, with respect to dose-time relationships, to take advantage of the biologic actions and reactions of the cells themselves. One of the most efficient dose-time relationships in spaced irradiation of the germinal epithelium would be one in which the dose fraction was small enough to permit attempts at division in spermatogonia but large enough to injure many of these cells to the extent that they die when mitosis is attempted, and one in which the time interval between exposures is such that the following exposure is administered when the effect of the previous dose is diminishing. A change of this inter-dose time interval in either direction would decrease the efficiency of the irradiation with respect to utilization of mitotic-linked death of spermatogonia.

In the case of cells having the capacity both for division and differentiation, irradiation tends to diminish the number of resting cells and dividing cells and by inhibition of division to increase the number of differentiating cells. It may be possible to increase to a maximum this effect in spermatogonia by suitable arrangement of the dose-time relationship in chronic irradiation. If the dose-time relationship optimum for maximum differentiation effects was quite different from that optimum for maximum mitotic-linked death, an optium compromised might be found, or these biologic effects could be handled separately with greater efficiency than is now the case.

There has been little investigation of the effects of irradiation on gametogenesis and reproduction in mammals, except for the work on rodents and some recent work on dogs. The single doses to the testes required to cause complete or nearly complete atrophy of the seminiferous epithelium are similar in size in these small animals and in the dog and man as well, all of the doses being within the LD_{50} range. However, the regenerative capacity of the seminiferous epithelium of the small laboratory animals is so great that very large single or divided doses, well above total-body LD_{100} doses, are required to sterilize permanently most or all of the animals of a group.

It would appear from data at hand that the dog, of all of the animals investigated in these respects, is the animal most similar to the human in terms of radiosensitivity and regenerative capacity of seminiferous epithelium. In general both dog and man reveal similar sensitivity which is greater than that in other experimental animals. In both cases, however, there is only little and fragmentary information on the effects of irradiation on spermatogenesis and reproduction, the minimal single or chronic permanent sterilization dose has not been studied definitively, and there is only little known of the regenerative capacity following irradiation.

The following table summarizes careful observations on male beagle dogs subjected to chronic exposure to x-rays from a 1,000 kvp x-ray machine and, in some cases, to neutrons from a cyclotron, 5 or 6 days per week.

Dose/week	Approximate total dose	Duration of exposure	Observations
0.3 r	62 r	4 yr	No significant change in sperm count.
0.6 r	124 r	4 yr	Do.
0.6 r	62 r	2 yr	Little change in germinal epithelium.
0.6 *n*	31 *n*	1 yr	Do.
3.0 r	156 r	1 yr	80 percent sterile; 20 percent reduced sperm counts.
3.0 r	312 r	2 yr	Substantial atrophy of germinal epithelium.
6.0 r	312 r	1 yr	Aspermic.
6.0 r	624 r	2 yr	Marked atrophy of germinal epithelium.
10.2 *n*	398 to 561 *n*	39 to 55 wks	Extreme atrophy of germinal epithelium.
15.4 r	477 r	31 wks	Aspermic after 375 r; sterile 1.25 years postirradiation so far.
15.4 r	634 r	41 wks	Aspermic after 375 r; sterile 1 year postirradiation so far.

Although cases of testicular atrophy in humans following irradiation have been observed since 1904, and were commonly observed soon after the Hiroshima and Nagaski bombings, little is known at present of the ultimate fate of the lesions produced in survivors and the effects of these lesions on fertility.

Regeneration of testes rendered atrophic by various doses and modes of irradiation has not been studied definitively in man. There are, however, isolated cases which have been studied to some extent, and there are reports of a zoospermia or oligonecrospermia or sterility in radiologists. Most of these cases were not studied carefully and extensively and in very few instances are there any reports or accurate estimates of doses involved. However, on the basis of the rather meager data available, certain estimates may be hazarded.

A single x-ray dose of 500 to 600 r is thought to produce permanent sterility for the human male and a dose of 250 r is thought to produce sterility for about one to two years.

If it is permissible at all to compare the meager human data with the results of many animal experiments in which regeneration was studied, the much more delayed and lower rate of testicular regeneration in man is apparent. Marked depletion of germinal epithelium is produced in the small experimental animals by doses in the LD_{50} range, but regeneration of the seminiferous epithelium is complete or nearly so in a matter of 3 to 5 months.

Man as well as the dog may have fewer of the radioresistant primordial cells, precursors of spermatogonia, or these cells may have less potential than is the case in the smaller or lower mammals. Other possible reasons for the delayed and slow recovery may be intimately associated with differences in metabolic rate and normal differences in rates of spermatogenesis. Factors which determine the relatively late maturation of the normal human testis may also modify the rate of regeneration of the human testis.

2. *The Female.*—Irradiation of the mammalian ovary can cause profound atrophy of the organ with temporary or permanent sterility depending upon the dose. Changes in the ovaries may be followed by dependent atrophic changes in accessory genitalia in most mammals.

The ova and follicular cells are the most radiosensitive cells in the mammalian ovary and cells of the corpora lutea and interstitial cells are relatively radioresistant. The radiosensitivity of the ova and folicular cells varies with their functional states at the time of irradiation. There are also marked differences in radiosensitivity between species. In most laboratory mammals the developing and mature follicles and ova appear to be more radiosensitive than the primordial follicles and ovocytes and some primary follicles persist after fairly large doses of radiation and may begin to develop long after irradiation.

Irradiation may sterilize the ovary by preventing the development of primary follicles of the ovary and by destroying the ova and follicular cells. Histologically, permanent ovarian sterility is indicated by the lack of ovarian follicles.

A dose of radiation which destroys all developing follicles causes failure of development of corpora lutea, which may lead to decrease of interstitial gland cells in animals which have these glands, since new cells will fail to be developed from corpora lutea.

Care should be used in the extrapolation of data from the mouse to human problems in regard to the ovary. The mouse ovary is peculiar in many respects. In it the primary follicles and ovocytes are exceptionally radiosensitive as compared with developing and mature follicles. The mouse ovary also has the tendency to develop invaginated tubular downgrowths of germinal epithelium and ovarian tumors, and these changes are easily accelerated and increased by relatively low doses of radiation. The peculiar differences in the mouse ovary, or the underlying causative mechanisms, are probably responsible for the exceptional radiosensitivity and the irreversibility of the effects of relatively low doses of radiation on the mouse ovary, as compared with ovaries of other laboratory mammals and the human female. In the female mouse a single x-ray dose of 150 r results in permanent sterility.

The size of the litter produced in the initial fertile period after irradiation of female animals is reduced and the size of the litter from irradiated females declines more rapidly with rising dose than the size of the litters from irradiated male mice.

Total body irradiation appears to produce greater effects on the ovary and on fertility in female animals than irradiation of ovaries alone with equivalent doses.

Since sterilization of the human ovary is a radiotherapeutic practice under certain circumstances, considerable data have accumulated on the radiosensitivity of this organ.

Single doses to the ovaries of 125 to 150 r may produce amenorrhea in 50% of women. A single dose of 170 r can produce temporary sterility for a period of 12 to 36 months. A dose of 500 r produces permanent sterility in most women,

but young women may require a larger dose. Doses between 500 r and 624 r have produced permanent sterility in 94% of a group of women (34 of 36 patients), and a localized dose of 625 r has produced permanent castration in a whole group of 72 patients.

3. *Sterility Doses for Men and Women.*—It seems quite possible that the single doses necessary to cause permanent sterility in 100% of men and women may not be far apart. However, there is insufficient information on men to permit an intelligent guess as to the exact amount of the difference.

It would appear that both male and female humans are probably among the most radiosensitive of those mammals studied, with respect to gonadal effects of irradition. It is also probable that the differences between single temporarily sterilizing doses and single permanently sterilizing doses of radiation are relatively small in the case of humans as compared with most of the laboratory mammals studied. On the basis of the data available the single gonadal dose of x- and gamma-radiation which would permanently sterilize most human males and females may be of the order of 500 to 625 r.

In animals with relatively poor gonadal regenerative capacity, such as the human, chronic irradition may be relatively of more serious consequence, and this tends to be supported by data from experiments on dogs. It is such exposure which may constitute the greatest practical human hazard as far as sterility is concerned.

A few experimental data indicate greater radiosensitivity in prepubertal animals, especially foetuses.

G. Effects of Irradiation on Growth and Development

This section is concerned with post-natal development and growth and does not include a detailed discussion of the effects of irradiation on prenatal development and organogenesis *per se.*

Regenerative and repair processes of the body appear to be fairly sensitive to the effects of ionizing radiation and inhibition of these processes may be very persistent, especially if vascular integrity and patency are impaired. Much more quantitative investigation of these aspects of the problem is needed, under circumstances of both total-body and localized irradiation.

Quantitative studies with rats seem to indicate that growth, as measured by body weight, is decreased by repeated exposure to as little as 24 r per week of whole body irradiation. It has been shown that a significant decrease in body weight can be produced by a schedule of repeated whole body exposures which does not cause any decrease in levels of hemoglobin or absolute neutrophils.

Localized irradiation of the epiphysis has been shown to cause measurable inhibition of bone growth and shortening of bones in humans and animals. In general, the greatest effect is seen in the youngest animals. Localized irradiation of the jaws has been followed by decrease in tooth growth.

Studies on children exposed to the atomic bomb in Japan indicate that growth and maturation are slightly retarded. The production of malformation by exposure of embryos or foetuses to irradiation has been investigated extensively in experimental animals. The production of relatively severe malformations in viable human offspring by irradition *in utero*, known from some clinical experience, has been confirmed by studies in Japanese atomic bomb survivors. The study of post-natal effects upon growth produced by irradiation of the foetus, however, has been neglected generally.

An extensive series of measurements on 4800 children at 6, 7, and 8 years after exposure to the atomic bomb at Hiroshima revealed in general that growth was retarded and maturation delayed. In another study involving several hundred children surviving the atomic bombings at Hiroshima and Nagasaki in 1945 and studied in the 2nd, 4th, and 5th years after irradiation, it was reported that the physical growth and development of the children were adversely affected, and the resulting retardation of their height, weight, and skeletal development was still evident at the end of 1950. The investigators expressed the belief that factors other than radiation may have contributed to the effects described. This study has been considered by some to be at variance with other studies on the same material.

Studies of children who had been irradiated *in utero* during the atomic bombings in Japan are noteworthy. In one series of 74 irradiated and 91 control children, roentgenographic survey failed to reveal differences in incidence of skeletal abnormalities between the exposed and control groups. A study of 4400 individuals who had been exposed to the bomb *in utero* or as children up to age 10 revealed 33 cases of microcephaly, with associated mental retardation

in 15 cases, and 19 cases of leukemia. There were also cases of mild visual disability among those now 16 to 19 years old who were exposed within 1800 meters of hypocenter. Observations on 205 children 4½ years old, who had been exposed at Hiroshima within approximately 1200 meters of hypocenter during the first half of uterine life, indicate that central nervous system defects were produced.

The mechanisms of growth inhibition by radiation are not understood. Biochemical and cytologic studies of animals in which growth has been inhibited appears to be indicated. The late effects, including life-span studies of exposure to ionizing radiation in pre-natal and early life merit further study.

IV. COMMENTS AND RECOMMENDATIONS

It appears likely that the after-effects of whole body external irradiation are quite general, consisting of irreversible injury to all the organ systems to at least some degree. Specific organ pathology or the incidence of specific disease is not prominent, however, except following large single doses or high intensity chronic irradiation and in even these cases, for the most part, the disease entities are not unusual, but occur earlier in the life of the animal. Although not clearly established at low chronic dosage levels premature aging with shortening of life span appears to be common to all whole body exposure. This effect is sufficiently large that it may provide a better criterion for limitation of exposure than increased incidence of specific disease.

The effects of partial body irradiation on life-span have not been studied except with internally deposited radioactive materials for which the local dosage is not usually well known. Consequently, comparisons with whole body effects are difficult. With partial body irradiation, when highly localized at least, local pathology is probably the best criterion for exposure limitation.

Because most pathologic studies have been made on animals dying or sacrificed during chronic irradiation, rather than after exposure and repair, the permanent after-effects have not been well separated from the total injury and related quantitatively to dose.

Animals prematurely aged by irradiation have not been studied to determine those changes which presumably have occurred in their physiological efficiency.

Except for alterations of pre-natal development very little is known of the after-effects of either whole or partial body irradiation in the young in comparison to mature animals.

Agents which, when administered to animals at the time of irradiation, permit them to survive doses which are ordinarily lethal, do not, according to scanty existing information, reduce the late effects. Consideration should be given to the possibility of increasing the reversibility of radiation injury and of diminishing thereby the late effects.

In anticipation that definitive information on man can be approached only by extrapolation from animals along with comparison to meager human data, studies on animals should be widely extended, not only with respect to experimental numbers to increase the accuracy of observation, but also to a greater variety of species to ascertain the generality of quantitative dose-effect relations.

V. REFERENCES

This report is based largely on the following detailed reviews, which were prepared in anticipation of the report. These reviews contain bibliographic references to most of the specific experimental works and reviews consulted in the preparation of the report.

Each of the following papers is an University of Rochester Atomic Energy Project Technical Report.

1. Blair, H. A. Data Pertaining to Shortening of Life-Span by Ionizing Radiation. University of Rochester Report UR-442 (1956).
2. Casarett, G. W. The Effects of Ionizing Radiations from External Sources on Gametogenesis and Fertility in Mammals. University of Rochester Report UR-441 (1956).
3. Hempelmann, L. H. Malignant Disease in Human Populations Exposed to Ionizing Radiation. University of Rochester Report UR-446 (1956).
4. Hursh, J. B., and Noonan, T. R. Some Late Effects of External Irradiation on Growing and on Adult Mammals. University of Rochester Report UR-445 (1956).

5. Ingram, M. Latent Hematological Effects of Exposure to Ionizing Radiations. University of Rochester Report UR-444 (1956).
6. Tuttle, L. W. Radiation Cataracts. University of Rochester Report UR-443 (1956).

APPENDIX 5

(A LETTER FROM JAPAN)

ST. PAUL'S UNIVERSITY,
(RIKKYO DAIGAKU),
Ikebukuro, Tokyo, Japan, June 4, 1957.

Dr. C. HOLIFIELD,
Chairman of Radiation Subcommittee of Joint Committee on Atomic Energy, United States of America.

DEAR DR. HOLIFIELD: Enclosed is a copy of our comment on Dr. Libby's paper which was publicated at the meeting of the American Physical Society on April 26, 1957. I should appreciate it very much if you would kindly arrange our paper to offer at the committee.

Very sincerely yours,

MITUO TAKETANI, *Professor,*
IWAO OGAWA, *Assistant Professor,*
TADAYOSHI DOKE, *Assistant Professor,*
Department of Physics, St. Paul's University, Tokyo, Japan.

ST. PAUL'S UNIVERSITY (RIKKYO DAIGAKU)

IKEBUKURO, TOKYO, JAPAN

Distributed to: Dr. Masao Tuzuki, Dr. Fumio Yamazaki, Professor Koichi Murachi, Professor Yasuo Miyake, Professor Eizo Tajima, Professor Yoshio Hiyama, Professor J. Rotblat, Professor L. Pauling

1. It seems quite sure that the data submitted by many nations to the Scientific Committee of the United Nations in April 1957 show no significant differences among these data concerning the levels and distributions of strontium 90 accumulated on the ground at present. However, with respect to the estimated value in the future we find appreciable differences, e. g., contrary to Dr. Libby, who insists that the levels of strontium 90 accumulated on the ground in the United States may not exceed the present amount if the nuclear explosion tests are stopped, the British scientists made the estimate of the amount as about 2.5 times of the present one in about 10 years. Although these differences come from the diverse estimates of the amount of strontium 90 remained in the stratosphere, based on our own data we have made the nearly same estimate as that of the British scientists.

2. According to Dr. Libby most people obtain their calcium through milk products. Then he assumes a discrimination factor of 20 against strontium 90 from the soil to the human bones. However, in the world there are many people who obtain primely their calcium through vegetables, especially in the Orient. In such a case, it seems quite natural that we should estimate the discrimination factor appreciably lower than the case of depending on milk products. For instance, in Japan 50 percent of calcium source for the people is rice. Accordingly we should naturally estimate the discrimination factor as about 4.

3. Taking into account the above discussions, we may conclude that, if nuclear weapon tests are continued at the present rate for the coming three or four years, the radiation dose from strontium 90 deposited in the bones of those who have the Japanese-like food habit will reach the average value of the dose from natural radiations, e. g., cosmic ray, terrestrial radiation, and natural Ra in human bones.

4. Furthermore, Dr. Libby, in his report, seems to have made a definite mistake about the hazards by the very weak radiation. He investigated the correlation between the altitude difference of the cosmic ray and occurrence of bone cancer and leukemia, but such a method is nonsense unless the many kinds of natural radiations other than the cosmic ray are taken into account, because their doses are three times of the doses irradiated by cosmic ray, and these variation from place to place is rather appreciable.

On such a problem, the authorities in the MRC report of UK state that: "On the whole the experiments seem in favour of a proportionality between the frequency of tumours produced in a given length of time and the amount of radiative material in the body even at low dose levels."

If we accept such a proportionality, we may be able to conclude that the number of people in whose bodies cancers are produced will increase considerably. However, we consider that the detection of such hazards due to the nuclear weapon tests is very difficult, because a *percentage* of the occurrence of the hazards is quite small, although large in the absolute number. Taking into consideration the probability mentioned above, we should keep the dose from strontium 90 in human bone lower than that from natural radiation as possible.

5. Concerning caesium 137 very few data are available at present, and so we can not as yet make any accurate estimate of the amount in the human body in the future. However, this element seems to become an important material in the future if nuclear test explosions are continued at the present rate.

6. Recently we have continuously detected a considerable amount of long-life α-emitter in the air. Although we have not yet completed the radio-chemical analysis, we are afraid that there may be included an appreciable fraction of elements with very low MPC., such as Plutonium 239.

JAPANESE GENETICISTS ON RADIATION

The following "Statement concerning the genetic effects of radiation upon man" was prepared in April by the Genetics Society of Japan and the Japan Society of Human Genetics and sent out by them to a number of colleagues in other countries.

"With the increasing utilization of atomic energy, man inevitably has greater chance of being exposed to radiation than he has previously had. Generally speaking, any kind of radiations causes some damage to organisms. Particularly, their genetic effect is serious for the following reasons:

"1. It has been demonstrated by many experiments that radiations induce genetic changes or 'mutations' in organisms. Man cannot be exempt from this rule. Some such mutations occur naturally, but radiations raise their frequency.

"2. The great majority of these mutations are deleterious to mankind. Their effect may appear in the next generation, but more commonly only in subsequent generations. Therefore, the apparent escape of the next generation from such an effect does not ensure the genetic safety of all descendants.

"3. The incidence of mutation increases in proportion to the total dose of radiation given to the gonad. Whether irradiation is continuous or intermittent, the same amount of mutation is induced in either case, provided that the total dose is the same, since the mutation which was once induced persists even after the end of irradiation and is handed down to progeny. Thus the genetic effect of radiations through the gonad is fundamentally distinct from their direct damage to the body, which may disappear after the end of irradiation.

"4. Human population acquires natural mutations which are of very low incidence. These mutations are removed by natural selection, and the newly-appearing mutations and those removed by selection are mutually balanced, the incidence of mutant genes is thus kept in equilibrium. Additional mutations artificially induced by irradiation cause the break-down of this equilibrium, and an increase of the mutant genes possessed by the population. Such a change will lead to a gradual increase of individuals handicapped in physical strength or in mental capacity, increases the sacrifices of individuals and the burdens of the society, and leads to eventual disaster for mankind.

"From what has been pointed out above, we are led to conclude that any amount of radiation, however small it may be, is deleterious to the heredity of man. Although a certain dose has been set as 'permissible' for people engaged in the operation of X-rays and radioactive apparatus or substances, this is only aimed at the safety and health of those people themselves. However, as far as the gentic effect on their descendants is concerned, there is no theoretical limit below which danger may be entirely excluded.

"Although there can be hardly any question about the necessity for the peaceful utilization of atomic and other radiation energies, it is still all the more important to guard against any misuse or misoperation of such energies. This is not only for the safety of the present generation, but also for the health and prosperity of our descendants. Also, we must be on guard against the gentic effects of atomic or hydrogen bomb tests, which increase the level of radioactive contamination in the air and water.

"Under such circumstances, we geneticists eagerly hope that the general public will realize the urgency of the question at issue, and that effective means for its solution will be taken promptly."

APPENDIX 6

A SELECTION OF CORRESPONDENCE AND STATEMENTS TO AND BY THE ATOMIC ENERGY COMMISSION CONCERNING THE SCIENTIFIC AND TECHNICAL ASPECTS OF FALLOUT.

STATEMENT BY LEWIS L. STRAUSS, CHAIRMAN, UNITED STATES ATOMIC ENERGY COMMISSION, FEBRUARY 1955

At a news conference on December 17, 1954, I stated that the staff of the Atomic Energy Commission was studying the subject of fallout and expressed the hope that information about it would be made public at a later date. "Fallout" is the word now applied to a phenomenon that follows the explosion of a nuclear weapon. Such an explosion, if the fireball touches the surface of the earth, draws up large amounts of materials into the bomb cloud. These materials subsequently fall back to earth as radioactive particles over a large area, mostly down-wind and relatively close to the point of explosion—although the lighter particles are carried great distances. The main radioactivity of fallout decreases very rapidly with time—for the most part, within the first hours after the explosion. An in-the-air explosion where the fireball does not touch the earth's surface does not produce any serious radiological fallout hazard.

Since nuclear weapons are in possession of the USSR, the Commission believes the American people wish to be informed regarding the dangers of nuclear explosions and the measures which individuals can take to protect themselves if an atomic attack should ever occur. Therefore, the Commission has condensed in the attached Report the information which can be made public at this time on the effects of the explosions of high-yield nuclear weapons.

The following excerpts and summarized sections contain the highlights of the Report itself.

FALLOUT PATTERN OF 1954 TEST IN THE PACIFIC

The very large thermonuclear device tested at Bikini Atoll on March 1, 1954, was detonated on a coral island and the ensuing fallout contaminated an elongated, cigar-shaped area extending approximately *220 statute miles down-wind and varying in width up to 40 miles*. In addition, there was a contaminated area up-wind and cross-wind extending possibly 20 miles from the point of detonation. Data was collected from 25 points on 5 atolls located from 10 to 330 miles down-wind (generally east) from Bikini Atoll. Due to an unexpected shift in the direction of the prevailing winds in the higher altitudes, the fallout missed the observation rafts that had been placed farther north previous to the test firing. The estimated contour of the pattern of fallout is, therefore, based only in part on data obtained from actual measurements and partly on calculations.

Data from this and other tests permits *estimates* of casualties which would have been sufferer within this contaminated area if it had been populated. These *estimates* assume: (1) that the people in the area would ignore even the most elementary precautions; (2) that they would not take shelter but would remain out of doors completely exposed for about 36 hours; and (3) that in consequence they would receive the maximum exposure. Therefore, it will be recognized that the estimates which follow are what might be termed *extreme* estimates since *they assume the worst possible conditions.*

On the basis of our data from this test and other information, it is estimated that. following the March 1, 1954, test explosion, there was sufficient radioactivity in a down-wind belt about 140 miles in length and of varying width up to 20 miles to have seriously threatened the lives of nearly all persons in the area *who took no protective measures.*

Some distance farther from the point of detonation, at about 160 miles down-wind and along the exis of the ellipse, the amount of radioactivity would have seriously threatened the lives of about one-half of the persons in the area who took no protective measures.

Near the outer edge of the ellipse, or approximately 190 miles down-wind, it is estimated that the level of radioactivity would have been sufficient to have seriously threatened the lives of 5 to 10 percent of any persons who might have remained exposed out of doors for all of the first 36 hours.

Thus, about 7,000 square miles of territory down-wind from the point of burst was so contaminated that survival might have depended upon prompt evacuation of the area or upon taking shelter and other protective measures.

At a distance of 220 miles or more down-wind, it is unlikely that any deaths would have occurred from radioactivity even if persons there had remained exposed up to 48 hours and had taken no safety measures.

The estimates cited above do not apply uniformly throughout the contaminated area inasmuch as the intensity of radioactivity within a region of heavy fallout will vary from point to point, due to such factors as air currents, rain, snow, and other atmospheric conditions. Because of this and because most persons, if given sufficient warning, probably would evacuate the area or take shelter and other precautionary measures, the actual percentage of fatalities could reasonably be presumed to be considerably smaller than these extreme estimates.

PROTECTION AGAINST FALLOUT

In the area of heavy fallout the greatest radiological hazard is that of exposure to *external* radiation, which can be greatly reduced by simple precautionary measures. Exposure can be reduced by taking shelter and by simple decontamination measures. Test data indicates that the radiation level, i. e., the rate of exposure, indoors on the first floor of an ordinary frame house in a fallout area would be about one-half the level out of doors. Even greater protection would be afforded by a brick or stone house. Taking shelter in the basement of an average residence would reduce the radiation level to about one-tenth that experienced out of doors. Shelter in an old-fashioned cyclone cellar, with a covering of earth three feet thick, would reduce the radiation level to about 1/5000, or down to a level completely safe, in even the most heavily contaminated area. Designs of shelters of simple yet effective construction have been prepared by the Civil Defense Administration and are available to the public.

Radioactive material deposited during the fallout may or may not be visible but would be revealed by radiation detection instruments such as Geiger counters. Any falling dust or ash that can be seen down-wind within a few hours after a nuclear explosion should be regarded as radioactive until measured by a radiation detection instrument.

Care should be taken to avoid the use of solid foods or liquids that may contain fallout particles.

If fallout particles come into contact with the skin, hair, or clothing, prompt decontamination precautions such as have been outlined by the Federal Civil Defense Administration will greatly reduce the danger. These include such simple measures as *thorough bathing of exposed parts of the body and a change of clothing.*

INTERNAL RADIATION EFFECTS

Two other factors must be considered in evaluating possible hazards from radioactive fallout. The first is the effect of internal radiation from fallout particles swallowed in food or liquids. The second is the effect of radiation upon the germ cells which transmit inherited characteristics from one generation to another. It should be noted that in neither case is there reason to believe that weapons testing programs of the United States have resulted in any serious public hazard.

The radioactive forms of strontium and iodine are the constituents of fallout which are of principal concern as internal sources of radiation through ingestion. The concentrations of these substances from nuclear detonations to date have been monitored at many localities, and the amounts detected have been insignificant, compared to concentrations which would be hazardous.

GENETIC EFFECTS OF RADIATION

There is a wide range of admissible opinion as to the genetic effects which radiation might have upon future generations, and conclusive data is not available at present on which to base an incontrovertible forecast. However, it is important to recognize that the average amount of radiation exposure received

by residents of the United States from all nuclear detonations to date has been about the same as the exposure received from one chest X-ray. The Commission's medical and biological advisers do not believe that this small amount of additional exposure is any basis for serious concern at this time.

BLAST AND HEAT EFFECTS

Two important characteristics of any nuclear explosion, other than those from fallout, are the effects of blast and heat, which are of the same nature for a thermonuclear bomb as for the earlier and smaller atomic bombs. The intensity and area of the blast and heat effects increase in relation to the greater energy yield of the explosion. Much information on these two effects has already been published by the Atomic Energy Commission, but it might be recalled that an atomic bomb of the earliest type, equivalent to 20,000 tons of TNT, would produce blast and heat sufficient to destroy, or damage severely, buildings within a radius of more than one mile from the explosion point. The United States has developed fission bombs many times as powerful as the first atomic bombs, and hydrogen weapons in the ranges of millions of tons (megatons) of TNT equivalent.

PROTECTION AGAINST BLAST AND HEAT

The hazard from both burn and blast effects well *outside* the central target area would be reduced greatly by shelter. Clothing or almost any kind of shelter would reduce the danger of direct burns, although there might be danger of clothing and structures becoming ignited. Also, shelter would materially reduce the hazard of blast injury by affording protection against flying or falling debris. As is generally known, the shelter afforded by ordinary city buildings would not suffice within the central area surrounding the point of explosion of a large nuclear weapon. For this reason, the Federal Civil Defense Administration recommends evacuation of the central areas of target zones on early warning of approaching attack.

FALLOUT FROM NEVADA TESTS

Only relatively small nuclear test explosions are conducted at the Nevada Test Site, in contrast to the tests of high-yield thermonuclear devices at the Pacific Proving Grounds. In Nevada, as well as in the Pacific, all tests are planned for times when forecast weather conditions minimize the possibility of fallout hazard. High air bursts at the Nevada Test Site have produced no significant fallout; heavy fallout from near-surface explosion has extended only a few miles from the point of burst. The hazard has been successfully confined to the controlled area of the Test Site. The highest actual dose of radiation at an off-site community has been estimated to be *less than one-third of the greatest amount of radiation which atomic energy workers are permitted to receive each year under the Atomic Energy Commission's conservative safety standards.*

CONCLUSION

In the event of war involving the use of atomic weapons, the fallout from large nuclear bombs exploded on or near the surface of the earth would create serious hazard to civilian populations in large areas outside the target zones. The Atomic Energy Commission hopes that these dangers will never be experienced by mankind. However, until the possibility of an atomic attack against us is eliminated by a *workable international plan for general disarmament*, the study and evaluation of the effects of weapons which might be used against us and the improvement of our means of self-defense are a paramount duty of our Government.

A REPORT BY THE UNITED STATES ATOMIC ENERGY COMMISSION ON THE EFFECTS OF HIGH-YIELD NUCLEAR EXPLOSIONS

1. Considerable information on the effects of the explosions of atomic weapons has been made public by the Government since the first nuclear detonations in 1945. The handbook, "The Effects of Atomic Weapons", published in 1950, is being revised and brought up to date to include the effects of thermonuclear weapons, as a result of the most recent tests at the Pacific Proving Grounds. References to the effects of thermonuclear explosions have been made in several

official statments, beginning with Chairman Strauss' description of the phenomenon of "fallout" at a White House news conference on March 31, 1954. The following statement is designed to condense and correlate information, some of which already has been made public and other portions of which have been of a classified nature until now.

2. The effects of nuclear tests are evaluated for civil defense planning as well as for military and technological purposes. So long as nuclear weapons are in possession of any unfriendly power, the Commission believes the American public will wish to be as fully informed as possible as to the nature and extent of the dangers of nuclear attack and of the protective measures that can be taken by individuals and communities to avoid or minimize those dangers if we should be attacked.

3. Test conditions, which must necessarily form the principal basis of evaluating the effects of nuclear explosions, may differ markedly from those which might be expected if nuclear weapons were used against our population in wartime. It would be difficult to predict the size or kind of bomb an enemy might use against us in event of war, the exact means of its delivery, the height at which it would be exploded, or the number of bombs which might reach a given target. Nevertheless, the facts to follow are the fundamental ones at this time.

FOUR EFFECTS OF DETONATIONS

4. A nuclear detonation produces four major characteristics—blast, heat, immediate nuclear radiation, and residual radioactivity. Of these, the first three are essentially instantaneous, while the fourth has a more protracted effect. The phenomena of blast, heat, and nuclear radiation from the detonation of a thermonuclear bomb are of the same nature as those of earlier and smaller atomic bombs. The nature of the phenomena is, in general terms, standardized whether the bomb be a 20,000-ton (TNT equivalent) atomic weapon or a thermonuclear one of many times that power. The intensity and area of the blast, heat, and nuclear radiation increase in relation to the greater energy yield of the explosion. Information on these effects has been extensively publicized; therefore, the remainder of this report deals principally with effects other than heat and blast.

5. Residual radioactivity, although in no sense exclusive to high yield thermonuclear detonaions, does become a mater of major concern when a large thermonuclear device of the type used in the 1954 tests in the Pacific is exploded. The fallout of radioactivity from such an explosion, may, under certain conditions, settle over wide areas. Therefore, the extent and severity of this radioactive fallout has been a subject of continuing study since the first full-scale thermonuclear tests at the Pacific Proving Grounds on November 1, 1952. The results of these studies and of our evaluation of data obtained from the latest tests in the Pacific in March, 1954, are described in subsequent parts of this report.

6. It should be noted that if we had not conducted the full-scale thermonuclear tests mentioned above, we would have been in ignorance of the extent of the effects of radioactive fallout and, therefore, we would have been much more vulnerable to the dangers from fallout in the event an enemy should resort to radiological warfare against us.

BLAST AND HEAT EFFECTS

7. The effects of blast and heat from a nuclear explosion are relatively localized. One A-bomb of the earliest type equivalent to 20,000 tons of TNT (20 kilotons) would produce blast sufficient to destroy or damage severely residences within a radius of more than one mile from the point of burst. Within a radius of about a mile and a half, residences would be so damaged as to be unusable without repairs. A principal hazard to human beings would come from flying and falling debris and from fires due to such causes as broken gas and electric lines or overturned stoves. The area in which injuries to human beings would be caused by blast, therefore, would be about the same as the area of damage to structures.

8. The United States, as announced previously, has developed fission bombs many times as powerful as the first A-bombs, and hydrogen weapons in the ranges of millions of tons (megatons) of TNT equivalent. For these larger weapons, the blast effects can be calculated approximately by means of a scaling law, namely, the distance at which a given blast intensity is produced varies as the cube roots of the yields of the explosions.

9. Similarly, the heat and burn effects of nuclear explosions can be estimated from accumulated data. These effects, of course, are influenced by prevailing atmospheric conditions. The time element also is a prime factor. Very large weapons deliver heat over an appreciably greater period of time than smaller weapons. A given quantity of heat from a high-yield weapon, delivered over a longer period of time, will produce somewhat *less* severe burns than the same quantity of heat from a nominal detonation.

PROTECTION AGAINST BLAST AND HEAT

10. The hazard from both burn and blast effects in the *outer* affected areas would be reduced greatly by shelter. Clothing or almost any kind of shelter would reduce the danger of direct burns, although there might be some danger of clothing and structures becoming ignited. Also, shelter would materially reduce the hazard of blast injury by affording protection against flying or falling debris. The Federal Civil Defense Administration has made extensive studies of shelters and has issued plans for several simple and inexpensive types which can be utilized by householders. As is generally known, the shelter afforded by ordinary city buildings would not suffice within the central area surrounding the point of burst of a large nuclear weapon. For this reason, the Federal Civil Defense Administration recommends evacuation of the central areas of target zones on early warning of approaching attack.

RADIATION EFFECTS

11. The immediae nuclear radiation, i. e., the neutrons and gamma rays released instantaneously with the explosion of a large weapon on or near the ground, does not present a serious hazard beyond the area where heat and blast are of great concern.

FALLOUT RADIATION

12. However, particles with residual radioactivity produced by a detonation (as opposed to the immediate nuclear radiation) may fall out over an area much larger than that affected by blast and heat, and over a longer period of time. All nuclear detonations produce radioactive materials, but the nature and extent of the radioactive fallout depends on the conditions under which the bomb is fired. The main radioactivity of a bomb's fallout increases very rapidly with time—for the most part, within the first hours after the detonation.

FALLOUT FROM IN-THE-AIR DETONATIONS

13. In an in-the-air explosion where the fireball does not touch the earth's surface, the radioactivity produced in the bomb condenses only on solid particles from the bomb casing itself and the dust which happens to be in the air. In the absence of material drawn up from the surface, these substances will condense with the vapors from the bomb and air dust to form only the smallest particles. These minute substances may settle to the surface over a very wide area—probably spreading around the world—over a period of days, or even months. But they descend extremely slowly with the result that, by the time they have reached the earth's surface, the major part of their radioactivity has been dissipated harmlessly in the atmosphere, and the residual contamination is widely dispersed.

FALLOUT FROM SURFACE DETONATIONS

14. If, however, the weapon is detonated on the surface or close enough so that the fireball touches the surface, then large amounts of material will be drawn up into the bomb cloud. Many of the particles thus formed are heavy enough to descend rapidly while still intensely radioactive. The result is a comparatively localized area of extreme radioactive contamination and a much larger area of some hazard. Instead of wafting down slowly over a vast area, the larger and heavier particles fall rapidly before there has been an opportunity for them to decay harmlessly in the atmosphere and before the winds have had an opportunity to scatter them.

15. The area of hazard from radioactive fallout from a surface or near-surface explosion of a thermonuclear weapon is much larger than the areas seriously affected by heat and blast. The large radioactive cloud of a thermonuclear explosion rises with great rapidity to the highest levels of the atmosphere

and spreads over hundreds of square miles in the first hours. During this time the winds toss the extremely radioactive particles about and the pattern of the radioactive fallout is determined by the size of the particles and by the direction and velocities of the winds, including those up to 80,000 feet and above. The nature of the surface of the earth on which the bomb is fired also must be taken into consideration. Because of these variables, it is impossible to apply a single fallout pattern to all thermonuclear detonations, even test explosions conducted under selected conditions. However, with adequate knowledge of atmospheric conditions, including wind directions and velocities up to high levels and meteorological reports, the fallout region for any detonation usually can be predicted with considerable accuracy. In general terms, the region of severe fallout contamination from the detonation of a thermonuclear weapon fired on or near the surface can be described as an elongated, cigar-shaped area extending down-wind from the point of burst.

FALLOUT PATTERN OF 1954 TEST IN THE PACIFIC

16. The very large thermonuclear device fired at the Bikini Atoll on March 1, 1954, was exploded on a coral island. Coral consists of calcium carbonate, thus the detonation's radioactivity was spread by particles consisting largely of unslaked lime which, during the hours of descent, was slaked by moisture in the atmosphere. These particles ranged between 1/1000th and 1/50th of an inch in diameter and were, on the average, somewhat adhesive. The prevailing winds were westerly so the bomb cloud moved generally to the east and deposited the radioactive particles in varying amounts over an elliptical or cigar-shaped area. About 160 (statute) miles down-wind from the point of burst the early fallout was observed in the form of fine particles which looked like snow. Fallout began there about eight hours after the detonation and continued for several hours.

17. The roentgen is the commonly accepted unit of measurement of radiation dosage. A dose of about 25 roentgens of radioactivity received by a person over a brief space of time will produce temporary changes in the blood. A dose of some 100 roentgens received in a short interval may produce nausea and other symptoms of radiation sickness. About 450 roentgens delivered over a day or so might be fatal to approximately half of the persons so exposed. However, because of the body's repair processes, a total radiation dose which would be serious if incurred in a few minutes would produce much less effect if spread over a period of years. These statements may be helpful in understanding the data which follow.

18. The test explosion, at ground surface, contaminated a cigar-shaped area extending approximately *220 statute miles down-wind and varying in width up to 40 miles.* In addition, there was a contaminated area up-wind and cross-wind extending possibly 20 miles from the point of detonation. Data was collected from 25 points on 5 atolls located from 10 to 330 miles down-wind (generally east) from Bikini Atoll. Due to an unexpected shift in the direction of the prevailing winds in the higher altitudes, the fallout missed the observation rafts that had been placed farther north previous to the test firing. The estimated contour of the pattern of fallout is, therefore, based only in part on data obtained from actual measurements and partly on extrapolation, i. e., calculations based on known data, including factual information obtained during previous tests of smaller devices.

19. Data from this test permits *estimates* of casualties which would have been suffered within this contaminated area if it had been populated. These estimates assume: (1) that the people in the area would ignore even the most elementary precautions; (2) that they would not take shelter but would remain out of doors completely exposed for about 36 hours; and (3) that in consequence they would receive the maximum exposure. Therefore, it will be recognized that the estimates which follow are what might be termed *extreme estimates* since *they assume the worst possible* conditions.

20. On the basis of our data from this and other tests, it is estimated that, following the test explosion on March 1, 1954, there was sufficient radioactivity in a downwind belt about 140 miles in length and of varying width up to 20 miles to have seriously threatened the lives of nearly all persons in the area who *did not take protective measures.* During the actual tests, of course, there were no people in this zone. Inside Bikini Atoll at a point 10 miles downwind from the explosion it is estimated that the radiation dosage was about 5,000 roentgens for the first 36-hour period after the fallout. The highest radiation

measurement outside of Bikini Atoll indicated a dosage of 2300 roentgens for the same period. This was in the northwestern part of the Rongelap Atoll, about 100 miles from the point of detonation. Additional measurements in Rongelap Atoll indicated dosages, for the first 36 hour period, of 2000 roentgens at 110 miles, 1000 roentgens at 125 miles, and, farther south, only 150 roentgens at 115 miles from Bikini.

21. Some distance farther from the point of detonation, at about 160 miles down-wind and along the axis of the ellipse, the amount of radioactivity would have seriously threatened the lives of about one-half of the persons in the area who *failed to take protective measures.* It is estimated that the radiation dosage at that point was about 500 roentgens for the first 36-hour period.

22. Near the outer edge of the cigar-shaped area, or approximately 190 miles down-wind, it is estimated that the level of radioactivity would have been sufficient to have seriously threatened the lives of 5 to 10 percent of any persons who might have remained exposed out of doors for the first 36 hours. In this area the radiation dosage is estimated at about 300 roentgens for the first 36 hour period.

23. Thus, about 7,000 square miles of territory down-wind from the point of burst was so contaminated that survival *might* have depended upon prompt evacuation of the area or upon taking shelter and other protective measures.

24. At a distance of 220 miles or more down-wind, it is unlikely that any deaths would have occurred from radioactivity even if persons there had remained exposed up to 48 hours and had taken no safety measures.

25. The estimates cited above do not apply uniformly throughout the contaminated area inasmuch as the intensity of radioactivity within a region of heavy fallout will vary from point to point due to such factors as air currents, rain, snow, and other atmospheric conditions. Because of this and because most persons, if given sufficient warning, probably would evacuate the area or take shelter and other precautionary measures, the actual percentage of deaths could reasonably be presumed to be considerably *smaller* than these extreme estimates.

PROTECTION AGAINST FALLOUT

26. In an area of heavy fallout the greatest radiological hazard is that of exposure to *external* radiation. Simple precautionary measures can greatly reduce the hazard to life. Exposure can be reduced by taking shelter and by utilizing simple decontamination measures until such times as persons can leave the area. Test data indicate that the radiation level, i. e., the rate of exposure, indoors on the first floor of an ordinary frame house in a fallout area would be about one-half the level out of doors. Even greater protection would be afforded by a brick or stone house. Taking shelter in the basement of an average residence would reduce the radiation level to about one-tenth that experienced out of doors. Shelter in an old-fashioned cyclone cellar, with a covering of earth three feet thick, would reduce the radiation level to about 1/5000, or down to a level completely safe, in even the most heavily contaminated area. Designs of shelters of simple yet effective construction have been prepared by the Civil Defense Administration and are available to the public.

27. Radioactive material deposited during fallout may or may not be visible but would be revealed by radiation detection instruments such as Geiger counters. Any falling dust or ash that can be seen down-wind within a few hours after a nuclear explosion should be regarded as radioactive until measured by a radiation detection instrument and found to be harmless.

28. Care should be taken to avoid the use of solid foods or liquids that may contain fallout particles.

29. If fallout particles come into contact with the skin, hair or clothing, prompt decontamination precautions such as have been outlined by the Federal Civil Defense Administration will greatly reduce the danger. These include such simple measures as *thorough bathing of exposed parts of the body and a change of clothing.*

30. If persons in a heavy fallout area heeded warning or notification of an attack and evacuated the area or availed themselves of adequate protective measures, the percentage of fatalities would be greatly reduced even in the zone of heaviest fallout.

FALLOUT FROM NEVADA TESTS

31. Only relatively small nuclear test explosions are conducted at the Nevada Test Site, in contrast to the tests of high-yield thermonuclear devices at the Pacific Proving Grounds. In Nevada, as well as in the Pacific, all tests are

planned for times when forecast weather conditions minimize the possibility of fallout hazard. Methods of forecasting weather patterns in these areas are improving steadily. High air bursts at the Nevada Test Site have produced no significant fallout; heavy fallout from near-surface explosions has extended only a few miles from the point of burst. The hazard has been successfully confined to the controlled area of the Test Site. The highest actual dose of radiation at an off-site community has been estimated to be *less than one-third of the greatest amount of radiation which atomic energy workers are permitted to receive each year under the Atomic Energy Commission's conservative safety standards.*

INTERNAL RADIATION EFFECTS

32. Several basic facts should be kept in mind in evaluating the hazard from fallout radiation. First, radiation is not a new phenomenon created by the explosions of fission and thermonuclear weapons. Since the beginning of life, living things have been exposed constantly to radiation from natural sources. Cosmic rays from space constantly pass through our bodies. We are exposed to "background" radiation from radium and radon in the soil, water and air. Our bodies have always contained naturally radioactive potassium and carbon.

33. As pointed out earlier, detonations of all atomic weapons produce radioactivity, a portion of which is carried to high altitudes and over great distances in the form of fine particles. The percentage of this radioactivity which travels beyond the relatively near area of the explosion depends largely on the conditions under which the bomb is fired, the percentage being higher for in-the-air bursts where the fireball does not touch the earth's surface. The most widespread radioactivity is produced only by the longer-lived fission products, since the radioactivity of the shorter-lived products decays and disappears before the particles come down to earth in a matter of days, weeks, months, and even years. The longer-lived radioactive products may be distributed over the entire earth. However, as the particles are carried farther and farther to remote areas, the possibility of significant amounts of fallout decreases.

RADIOSTRONTIUM FALLOUT

34. One of the most biologically important radioactive substances found in fallout is strontium–90. It has a long lifetime—nearly 30 years on the average. Radiostrontium has a chemical similarity to calcium and, therefore, when taken into the body it has a tendency to collect in the bones. Radiostrontium can enter the body in two ways—by inhaling or by swallowing. Normally, the amount inhaled would be small compared with the amount one might swallow. Fallout material deposited directly on edible parts of plants may be eaten along with the plants, but washing the plants before they are eaten would remove most of this radioactive material. However, rainfall carrying the radiostrontium down to earth may deposit it in the soil where it can be taken up, in part, by plants and incorporated into plant tissues, later to be eaten by humans or by grazing animals which, in turn, provide food for humans.

35. Since the start of nuclear tests, careful measurements have been made of the distribution of radiostrontium over the earth's surface, in the soils, in plants and animal tissues, in the oceans, in rain, in the atmosphere and in all forms in which it might be expected to occur. The results of this study are reassuring. The amount of radiostrontium now present in the soil as a result of all nuclear explosions to date would have to be increased many thousand times before any effect on humans would be noticeable.

RADIOIODINE FALLOUT

36. Among the shorter-lived fission products involved in the study of internal radiation, the most biologically important is radioiodine–131 with an average life of only 11.5 days. Even though this product may be widely spread after a nuclear explosion, the possibility of serious hazard is limited by its relatively short life. Like the nonradioactive form of the element, it concentrates in the thyroid gland and, in excessive quantity, conceivably could damage the thyroid cells.

37. Scientists of the Atomic Energy Commission have estimated that the average exposure of people in the United States from radioiodine in the fallout from the entire series of tests in the spring of 1954 was only a few percent of the annual dose that can be received year after year and still have no noticeable effects.

38. These two isotopes—radiostrontium and radioiodine—constitute the principal internal hazards from the radioactivities produced by the detonations of atomic weapons, both fission and thermonuclear. The Atomic Energy Commission has been engaged for three years in a broad study of the radioactive forms of these isotopes and conducts year-round monitoring of these radioactivities in many locations. Any accumulation of these materials can be detected with great sensitivity so that ample warning of potential hazard could be given long before any actual danger occurred from test detonations. The amounts of radiostrontium and radioiodine which have fallen outside the areas near the test sites as a result of all atomic tests up to now are insignificant compared to concentrations that would be considered hazardous to health.

GENETIC EFFECTS OF RADIATION

39. One other effect of radiation must be considered in evaluating the long-range possibilities of hazard from nuclear detonations. This is the possible genetic effect upon the germ cells which transmit inherited characteristics from one generation to another. At our present stage of genetic knowledge, there is a rather wide range of admissible opinion on this subject.

40. In general, the total amount of radiation received by residents of the United States from all nuclear detonations to date, *including the Russian and British tests* and all of our own tests in the United States and the Pacific, has been about one-tenth of one roentgen. This is only about 1/100th of the average radiation exposure inevitably received from natural causes by a person during his or her reproductive lifetime. It is about the same as the exposure received from one chest x-ray.

41. The medical and biological advisers of the Atomic Energy Commission believe that the small amount of additional exposure of the general population of the United States from our nuclear weapons testing program will not seriously affect the genetic constitution of human beings. Nevertheless, we are continuing our thorough study of the entire question and will continue to report our findings to the American people.

SUMMARY

42. The Atomic Energy Commission hopes that the information on nuclear weapons effects contained in the foregoing report will never be reflected in human experience as the result of war. However, until the possibility of an atomic attack is eliminated by a workable international plan for general disarmament, the study and evaluation of weapons effects and civil defense protection measures must be a necessary duty of our government.

43. Inevitably, a certain element of risk is involved in the testing of nuclear weapons, just as there is some risk in manufacturing conventional explosives or in transporting inflammable substances such as oil or gasoline on our streets and highways. The degree of risk must be balanced against the great importance of the test programs to the security of the nation and of the free world. However, the degree of hazard can be evaluated with considerable accuracy and test conditions can be controlled to hold it to a minimum. None of the extensive data collected from all tests shows that residual radioactivity is being concentrated in dangerous amounts anywhere in the world outside the testing area.

44. In the event of war involving the use of atomic weapons, the fallout from large nuclear bombs exploded on or near the surface would create serious hazards to civilian populations in large areas outside the target zones. However, as mentioned in the foregoing Report, there are many simple and highly effective precautionary measures which must be taken by individuals to reduce casualties to a minimum outside the immediate area of complete or near-complete destruction by blast and heat. Many of these protective measures, such as shelter and decontamination procedures, have been detailed by the Federal Civil Defense Administration.

UNITED STATES ATOMIC ENERGY COMMISSION,
Washington 25, D. C., August 8, 1956.

Hon. CLINTON P. ANDERSON,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR SENATOR ANDERSON: Enclosed for the information of the Joint Committee on Atomic Energy are the Atomic Energy Commission's analyses on the following:

1. *The Biological Effects of Atomic Radiation*, a study by the National Academy of Sciences, 1956.
2. *The Hazards to Man of Nuclear and Allied Radiations*, a report by the United Kingdom Medical Research Council of June 1956.
3. *Race poisoning by Radiation*, an article by H. J. Muller, appearing in the Saturday Review, June 9, 1956.

Primary attention has been devoted to the two basic documents—the reports of the National Academy of Sciences and the United Kingdom Medical Research Council. These are competent, well written reports and we trust that an increased public understanding of the effects of atomic radiation will result from their publication. We note, however, that with the exception of the article "*Radioactive Fallout through September 1955*" by Eisenbud and Harley to be published this month in SCIENCE, there were no major data presented in either the National Academy of Sciences' report or the United Kingdom Medical Research Council report not already known to the Atomic Energy Commission and previously reported in open literature.

Except for some difference in the Strontium–90 data, the data, conclusions and recommendations of both reports were in good agreement considering the complexity of the problems and the independence of the two studies. The reports recommended an additional restriction as to the total radiation exposure to be permitted over a number of years. It is not anticipated that the reports will create any major change in our position regarding our weapons testing position or the Atomc-for-Peace program.

Or did they merely get to the point of recognizing a genetics hazard?

Both the NAS and the UK reports consider the genetics aspect of radiation as being paramount. It is with this factor principally in mind that upper limits of whole bodily exposure over a long period of time were recommended. Based on these recommendations and those forthcoming from the International Commission on Radiation Protection, the AEC may consider placing an upper limit of yearly exposure for atomic energy workers. However, the average exposure to atomic energy workers during past operations has been so far below the maximum permissible level that the placing of a yearly upper limit would not be expected to impose any major restriction.

The NAS report recommended an upper limit of 50 roentgens for *individual* exposure up to age 30, and 10r during the like period for the *general* populace. Except in the case of the March 1, 1954, incident involving intensive fallout in the Marshall Islands area, no individual outside the testing areas has been exposed to even the 10r maximum recommended for the populace as a result of fallout from the U. S. nuclear testing program. The NAS report estimates that if the nuclear weapons tests were continued at the present rate the average exposure for the general population of the United States over a 30-year period would be about one-tenth of a roentgen. In summary, the report was reassuring as regards nuclear weapons testing; it did not attempt to face up to the problems of an atomic war; and, finally, it was preoccupied with the potential hazards inherent in a developing era of large scale atomic power.

As to the Strontium–90 accumulating in the biosphere, the AEC will continue its extensive program of maintaining collection stations throughout the world and of analyses of the samples. This close and continuing checking system will provide ample warning of any significant upward trends in the Strontium–90 content of the biosphere before hazardous levels would be approached. It is indicated in the NAS report that the highest levels observed throughout the world are about 1/100 of the Academy's estimate of permissible concentration for the population as a whole. Furthermore, our knowledge of present pollution from radiostrontium is more exact and more extensive than that with respect to any other atmospheric pollution.

Sincerely yours,

K. E. FIELDS, *General Manager*.

Enclosures:
1. Critique of NAS Report
2. Critique of British Medical Research Council
3. "Race Poisoning by Radiation"—Comments on

(Enclosure 1)

CRITIQUE OF THE REPORT OF THE NATIONAL ACADEMY OF SCIENCES

THE BIOLOGICAL EFFECTS OF ATOMIC RADIATION BASED ON (1) "A REPORT TO THE PUBLIC," AND (2) "SUMMARY REPORT."

To understand and best evaluate the implications of this report it is important to bear in mind the background of the individual scientists who made the study and their relationship to the National Academy of Sciencies-National Research Council and to the Government.

The NAS-NRC is not a Government organization. True, it was established by President Lincoln in order to have a distinguished body of scientists with whom the Government could consult at the time of the Civil War. On the other hand, it is a self-perpetuating body of free American scientists who control the membership of the Academy without any Government appointments. While various Federal agencies may appoint representatives to the various divisions of the National Research Council (the operating body of the NAS), they serve to bring problems to the Council for advice, and not to control the actions or the opinions of Council.

In the case of this study, the President of the NAS, Dr. Detlev W. Bronk, called together some 100 American scientists to carry out the study as individual citizens. While some of the scientists were Government employees and top advisers to Government on scientific matters, they were not acting in these capacities in their participation in the study.

The study was undertaken largely as a result of the concern felt throughout the country following the March 1, 1954 thermonuclear test explosion at Bikini, as a result of which a number of Marshall Islanders and Japanese fishermen were irradiated by fallout debris from the explosion. Subsequently, a number of scientific bodies in the U. S. passed resolutions requesting that a study be made of the possible effects on the human race of continued nuclear weapons testing.

In April, 1955, the Rockefeller Foundation provided the NAS with funds for undertaking a very broad study of the effects of atomic radiation. The subject reports are the final fruits of this study, which will be a continuing one.

Whereas the AEC has always been aware of the possible hazards from fallout from surface bursts of atomic weapons (see "Effects of Atomic Weapons," 1952), it had been even more aware of possible hazards to nearby livestock and the public generally from serious accidents which could conceivably occur to large production reactors such as those at the Hanford Works. The Bikini fallout incident made it abundantly clear that fallout *was* important from the standpoint of continued weapons testing and as a factor in civil defense planning. The problem of radiation effects has been under continuing review by the AEC and by the joint U. S., U. K. and Canada Tripartite meetings. In addition, the AEC has contributed a major portion of the basic scientific data for the deliberations of the National Committee for Radiation Protection and the International Commission for Radiation Protection.

A few words are in order on the general approach of the NAS study committees. They did not include an evaluation of the effects of an atomic war. As Dr. Bronk stated in the press conference of June 12, 1956, he could not define an atomic war so he asked the committees to limit themselves to peacetime atomic energy activities including weapons testing.

In the Foreword to the Summary Report, Dr. Bronk stated: "The use of atomic energy is perhaps one of the few major technological developments of the past 50 years in which careful consideration of the relationship of a new technology to the needs and welfare of human beings has kept pace with its development. Almost from the very beginning of the day of the Manhattan Project careful attention has been given to the biological and medical aspects of the subject. By contrast, the automobile revolutionized our pattern of living and working, but we are only now beginning to appreciate the problems of safety, urban congestion, nervous tension and atmospheric pollution which have accompanied its development. In the same way, the development of the aircraft industry outran our knowledge of how to meet the environmental needs of the human beings it intended to transport through the skies."

The scientists, save for the geneticists, were all person who had actively participated in the past in the efforts to reduce industrial toxicological hazards, air pollution, stream and harbor pollution, and soil and crop pollution, and de-

struction which has occurred with developing industries largely uncontrolled until serious damage had already taken place. They are determined that with a much greater body of knowledge to draw on concerning radiation effects, similar situations will not arise as a result of the rapidly growing atomic energy industry with its even greater potential dangers.

Consequently, once they had assured themselves on two points, namely: weapons testing at the present rate and with present safeguards was not a present menace, and the safety precautions of our present atomic energy operations were indeed effective, they became preoccupied with pointing out the problems inherent in a greatly expanded atomic energy industry. There constantly recurs through the report the idea that all is well today but for the future let us be very careful indeed.

In summary, the report was totally reassuring as regards nuclear weapons testing, it did not attempt to face up to the problems of an atomic war, and finally it was preoccupied with the potential hazards inherent in a developing era of large scale atomic power.

Summary Report of the Committee on Genetic Effects

This Committee consisted of geneticists, one authority on radiation pathology, one authority on radiological physics and radiation hazard control, and a mathematician, Dr. Warren Weaver of the Rockefeller Foundation, who chaired the group.

They considered the genetic effects against the background of present knowledge concerning radiation as a cause of mutations in microorganisms, plants, insects, and mice, bearing in mind the tendency of modern civilization to conserve all human life whether perfect or imperfect. They call attention to the perhaps greater importance of mutations which are relatively inapparent such as defects in resistance to disease processes, decreased fertility and curtailed life span, and impaired physical and mental vigor. The more dramatic mutations, monsters, still births, and early developmental defects leading to abortion and miscarriage are not apt to be passed on to another generation. The apparently relatively negative results of the genetics survey of the survivors' first generation at Hiroshima and Nagasaki serve to emphasize the validity of this point of view. This study demonstrated that with the methods used and the radiation dosages received, the heavily irradiated surviving population was not sufficiently large for it to be possible to demonstrate a statistically significant difference in the number of mutations in the offsprings of irradiated parents as compared with offsprings of non-irradiated control parents. It did not prove in any sense of the word that there was no genetic effect.

Following a general discussion of the mechanisms of genetic change especially as produced by radiation, both natural and artificial, the committee made certain recommendations. In doing so they used natural background radiation exposure (i. e., radiation from cosmic rays, igneous rocks, radium and radiopotassium in our bodies, etc.), and the so-called spontaneous mutation rate as base lines. In addition they were unanimous that no increase in the spontaneous mutations rate was desirable and that all radiation exposure to the germ cells at whatever rate of exposure did indeed increase the mutation rate in proportion to the total exposure received at the time of conception. Consequently they stated that all radiation exposure to the gonads was detrimental and consequently radiation exposures should be kept at the minimum consistent with the overall needs of a society.

They then observed that half of the American children were born of parents approximately 80 years of age or less. They noted that by the age of 30 the average American would receive germ cell exposures as follows:

1. Background or natural radioactivity ---------------------------------- 4.3r
2. Medical X-rays --
3. Fallout from weapons testing if continued at rate for the past 5 years -- 0.1r (0.02 to 0.5r)

They then estimated that the exposure necessary to double the mutations rate in humans lay between 52 and 150r, more likely 30r to 80r, but also that different gene loci were quite different in their sensivity to radiation. Taking these observations into consideration they felt that if the population as a whole were to receive no more than 10r man-made exposure to radiation to the germ cells prior to the age of thirty no serious consequences would result. They, therefore, recommended that no one should receive a total accumulated dose to the repro-

duction cells of more than 50r prior to the age of thirty without clear cut medical reasons, and that in any event the average exposure of populations as a whole should not exceed 10r by the age of thirty. They point out that at present about ⅛ this figure is already being used up by medical x-ray exposures many of which could with proper precautions be greatly reduced.

As to occupational exposures the Committee considered this to be a limited group—no estimates were made as to its actual or potential size.

As finalized in the report the recommendations are:

1. There should be a national system of keeping radiation exposures on all persons as is now practiced at AEC establishments.
2. Medical exposures to the germ cells should be reduced.
3. No more than 10r by age thirty for the population as a whole.
4. The subject should be reviewed periodically with a view to possible further reduction in exposure.
5. No body, however, employed, should receive more than 50r of exposure prior to the age of 30.
6. For special activities inherent in which are a greater liability to over-exposure individuals who for one reason or other are unlikely to procreate should be selected.
7. The state of knowledge in the field of genetics has been outrun by our knowledge in the field of physics.
8. Keep all exposures to the germ cells as low as possible for radiation exposure is generally detrimental to living cells.

In essence, this Committee formalized the current thinking on the subject. It did not come up with any new or startling conclusions or recommendations.

The Committee on Pathologic Effects of Atomic Radiation

This Committee was composed of scientists well versed in radiation pathology and chaired by Dr. Shields Warren, Director of the Cancer Research Institute of the New England Deaconess Hospital, Boston, Massachusetts, and was for five years—1948 to 1952—Director of the Division of Biology and Medicine of the Atomic Energy Commission.

This group and subcommittees on blood, lung, delayed effects, and toxicity of ingested radioactive materials reviewed the present state of knowledge and found that our knowledge of immediate effects was much greater than for delayed effects. They observed a five year lessened life span for American radiologists, estimated to have received from a few roentgens to 1000r of exposure as compared with physicians not using radiation—and agreed that until we had more precise knowledge of the cumulative effects of repeated small exposure of the whole body to radiation the rule of thumb recommended by the Genetics Committee could equally well apply to medical effects. That is, no one should receive more than 50r total accumulated dose to the reproductive cells by age 30—and no more than 50r for each decade thereafter. This, they felt, would assure that any life expectancy curtailment would be exceedingly minor, and the likelihood of induced leukemia minimal. They noted that as far as effects on the blood-forming organs, the intestinal tract, etc., are concerned, none of these effects have been detected among those who have adhered to present permissible dose levels.

As for the hazards from ingestion and radioactive materials, they confirmed the validity of existing National Committee for Radiation Protection and International Commission for Radiation Protection recommendations and as for the most important of the fission products in fallout, namely Strontium-90 they stated "there seems to be no reason to hesitate to allow a universal human strontium burden of 1/10 of the permissible yielding 20 rep in a lifetime. * * * Visible changes in the skeleton have been reported only after hundreds of rep were accumulated and tumors only after 1,500 or more." The permissible level referred to is that recommended by the NCRP for industrial workers. The Committee noted that although "some children have accumulated a measurable amount of radioactive strontium in their bodies, the amount is quite small—a thousandth of what is considered a permissible dose. The Committee concluded, "then, that Strontium-90 is not a current threat, but if there were any substantial increase in the rate of contamination in the atmosphere, it could become one."

Committee on Meteorological Aspects of Atomic Radiation, Chairman Harry Wexler, U. S. Weather Bureau

In this part of the report there is the fullest discussion of fallout from nuclear weapons. They distinguish between kiloton bursts when the cloud does not penetrate to the stratosphere and megaton bursts where the cloud does. They estimate that with surface bursts, i. e., where the fireball touches the ground 70–80% of the residual radioactivity falls out nearby, i. e., with small weapons a few miles, with larger ones up to 300 miles or more. They emphasize the ease of predicting this "nearby" fallout pattern after the fact and the problem of predicting its precise pattern prior to detonation.

They speak of intermediate fallout, i. e., material of small particle size released below the stratosphere and some 80% of which falls out within three weeks in the same hemisphere in which it originated and tending to uneven distribution associated with rainfall and wind patterns along a broad band in the same general latitude as that of its origin. Finally, they refer to delayed fallout of material which has gained entry into the stratosphere. It is slow with an average storage time in the stratosphere of 10 years, plus or minus 5 years. This delayed fallout tends to distribute itself more or less uniformly over the surface of the earth over the years.

They state that "at present, the amount of Sr 90 in the stratosphere from nuclear weapons tests is far too small to approach maximum permissible concentration even if it were all deposited now." They urged a continuing program to check on the amount of radioactivity in the stratosphere as necessary so that if there were to be a greatly increased rate of thermonuclear weapons testing activities we would know at the earliest moment when it was time to slow down in terms of potential hazard from Sr 90 to man.

There is also a discussion of the radioactivity from fallout of the intermediate and delayed variety. They point out that it is usually too feeble to measure with a hand monitor—that air sampling does not give precise results as the amount of the passing air does not bear a direct relationship to what falls on the ground. The best measure of the actual fallout available to date are laboratory analysis of fallout on gummed paper, in collecting pots, and actual analysis of the soil.

There is a discussion of atmospheric radiocontamination as a result of uncontrolled release of materials such as radio-krypton and radioiodine from power reactors and processing plants. They point out that continued control over release of these products as is now done is essential. Control is by permitting a "cooling" time for short-lived radioactive materials to decay away, by off-gas cleaning, and by scheduling release of materials with due regard to meteorological conditions at the time.

There is a section on possible uses of radioactive materials in the study of the science of meteorology. Natural radon gas in the air can be helpful in understanding vertical movements of air from the land. Weapons tests have taught much with respect to lateral spread of air masses at various altitudes—how rain scavenges the atmosphere of particles—the rate of transport from the stratosphere to the troposphere and the removal time for water from the atmosphere. Experiments could be conducted using introduced radioactive materials under controlled conditions to study airflow and diffusion rates, hydrometeorology, i. e., condensation, precipitation and avaporation, and to study electricity of the atmosphere especially the possible relationship of electrical fields to the weather.

As to effects of nuclear weapons testing on the weather the committee stated:

1. Nuclear Weapon debris was not effective as a seeder for rain.
2. The amount of ionization produced is insignificant in meteorological terms.
3. There has been no measurable decrease in the amount of direct sunlight reaching the earth whereas volcanoes have been known to decrease it by as much as 10–20% for appreciable periods of time.
4. The apparent recent increase in severe storms is probably the result of "improved methods of reporting."

Committee on the Effects of Atomic Radiation, Oceanography and Fisheries, Chairman Roger Revelle, Scripps Institute of Oceanography

This group viewed the past record of this country with respect to pollution of streams, waterways and harbors with extreme repugnance. They point out

that 71% of the earth's surface is ocean and that eventually everything gets into the oceans.

They note that the sea as compared to the land is relatively nonradioactive. Natural radioactivity of the seas is 1/100 that of igneous rocks. As a result of weapons tests they report the following: two days after Operation Castle was over in the spring of 1954 there was a millionfold increase in radioactivity of the surface waters near Bikini; that after four months 1500 miles away it was three times the normal amount and that at 13 months are area of surface water contamination had spread over a million square miles, and that at a distance of 3500 miles from Bikini the "artificial" radiactivity was ⅕ the natural.

They concluded that to date there has probably been no damage to life in the sea except that at the test site proper. They call attention to concentration of radioactivity by plant forms in the sea and warn repeatedly against indiscriminate dumping of radioactive wastes into the sea. They discuss the "flushing time" of the Black Sea 2500 years as compared with perhaps 100 or 200 years for the shelf-deeps of the Atlantic and Caribbean. They stress they need to know much more about the ocean depths and their movements. (The International Geophysical Year has a very large-scale study of the depths planned for 1957–58.) This committee would apparently permit "controlled" sea disposal especially of short-lived radioactive materials. They recommend that "Industrial agencies formulate conventions for the safe disposal of atomic wastes at sea, based on existing knowledge." This would seem to be a very logical and necessary move. To date, except for small amounts of short-lived material, the U. S. has not dumped any radioactive wastes in the sea. We are still storing all process wastes in tanks.

They further recommend collaborative studies of the oceans and their organisms and though a beginning has been made urge a greater effort. Finally, they contend that in ten or twenty years certain radiatracer experiments will not be possible because of widespread low level contamination of the seas. This may well be true.

Committee on the Effects of Atomic Radiation on Agriculture and Food Supplies, Chairman Prof. A. G. Norman, University of Michigan, Ann Arbor, Mich.

This group first discussed the application of atomic energy techniques to the agricultural sciences. They feel great advances will be forthcoming, but perhaps not as soon as some claim. They note the value of radioactive tracer studies in improving our knowledge of how most economically to apply fertilizers, and to improve plant nutrition. They note the great potential value of ionizing radiation to induce mutations in speeding up crop improvement programs. They point up the invaluable contribution tracer studies can make to our understanding of animal nutrition. They touched on the problem of radioisotopes as possible contaminants in food products and point out that present law classes radioisotopes of any sort or in any amount as poisons. They urge a more realistic approach to this inasmuch as no food product is or ever has been literally free of radioactivity.

There is a general discussion of possible effects of fallout and the like on the ecology of the country. The committee recommends that it may well be in the public interest to expand the present programs to a continuous study of the changes in levels of background radiation and the movements of radioactivity in the system. (This is in essence an activity that the AEC has already underway and is expanding very much along the lines recommended.)

Finally, there is a statement concerning use of radiation for food processing. They note that relatively low exposure will destroy parasites in meat and inhibit sprouting in potatoes and onions. They also note that for sterilization extremely large doses are required (millions of roentgens). They felt this area of development was moving as rapidly as warranted and that the interest of the consumer will be adequately protected. They expect at a later date to review the evidence for wholesomeness and acceptability of irradiated foods.

Committee on Disposal and Dispersal of Radioactive Wastes, Chairman Abel Wolman, Johns Hopkins University

This group considered the magnitude of the problem not as it is today but as it will become with full scale production of power by nuclear reactors. They note that to date essentially none of these wastes has been returned to the environment. It is being stored in tanks. They point out the importance of developing

more economic methods of handling these wastes to the total development of atomic power. They have no quarrel with present practices but are concerned at the future magnitude of the problem. They estimate that by 1980 there will be 20×10^7 gallons of wastes to deal with. These must, they say, be contained in some form or other. AEC has a large program to cope with this problem on two fronts—one, to produce perhaps by sintering a non-leachable stable mass and, two, to remove by separation the worst offenders, Sr^{90} and $Cesium^{137}$.

They note present practices with regard to radioisotope production, transportation and utilization are sound, but suggest review from time to time as this very rapidly expanding activity cotninues.

The discussion of reactor accidents as a hazard is quite general. They urge continued requirement of containment of the reactor itself for all but small research reactors as practiced today in this country. They urge constant vigilance and conclude that the extreme hazard—total vaporization of a reactor—is unlikely.

In other words, this entire study adds up to reassurance for the present, and repeated urgings to keep vigilant lest this new technology needlessly get out of hand.

Enclosure 2

Critique of British Medical Research Council, the Hazards to Man of Nuclear and Allied Radiation

A Report to the British Medical Research Council

The British Medical Research Council is a governmental body and was directed by the Prime Minister on 29 March 1955 to appoint a committee under the chairmanship of Sir Harold Himsworth to review the existing scientific evidence on the medical aspects of nuclear and allied radiation.

This report consists of eight chapters. The first four chapters deal with basic understandings of radiation and its biological effects, the fifth chapter with existing and foreseeable exposures due both to peacetime uses of atomic energy as well as to nuclear detonations in testing and in warfare, the sixth part with recommendations of permissible exposure and the seventh and eighth parts with summaries and conclusions.

Chapter I is an introduction to the report.

Chapter II discusses in simple terms the nature of radiation and its action on living cells. It deals with well known units, methods of measurement and biological effects.

Chapter III discusses the effects of radiation on the health of the individual. It includes discussions of the early effects upon the Japanese at Hiroshima and Nagasaki and the later development of an increased incidence of leukemia among the survivors. The British state they have demonstrated an increased incidence of leukemia in patients with arthritis of the spine treated with x-rays. They cite also American statistics on the increased evidence of leukemia in radiologists. They conclude that radiations can induce leukemia but do not quantitate the expose necessary for such an effect short of large single doses as at Hiroshima and Nagasaki.

There follows a discussion of radiation as an inducer of cancer and a conjecture that 1000r exposure to radon gas and its daughter produces induced lung cancer in the Schoneberg and Joachimsthal mines. Paradoxically, they go on to say that there is no evidence that external x- or gamma rays can cause lung tumors in man.

There is a discussion of radiation as a cause of bone tumors drawn principally from the reports of cancer of bones in radium dial workers and individuals given radium therapeutically. Most of this is American data. They feel there is not much of a factor of safety in the present maximum permissible concentration for radium. They indicate the risk of development of bone cancer from x-ray or gamma exposure in industry is insignificant. There is brief mention of skin cancer as induced by radiation, and thyroid gland cancer. Again the likelihood of this sort of thing from industrial exposure under modern controlled conditions is insignificant except, of course, in the event of accidental overexposure.

Radiation cataracts are mentioned as a hazard subject to ready control.

This report seems to understate effects of radiation on life span which has been so clearly proved in experiments with animals at, to be sure, radiation doses somewhat above permissible levels. The National Academy of Sciences report emphasizes this effect and cites the reduced life expectancy of American radiologists.

Both reports mention effects of radiation on developing fetuses, and the temporary sterility in males exposed to a few hundred roentgens at a single exposure. The British report is totally reassuring on the effects of occupational exposures on fertility.

Chapter IV is a very lengthy genetics effects discussion with many figures, tables and calculations and a critique of the Atomic Bomb Casualty Commission genetics stud: in Japan. This is a highly technical discussion and comes out with the same conclusions as does the National Academy of Sciences, namely that a dose of radiation which would double the mutation rate of a relatively small group of prospective parents would produce no noticeable effects. "For levels of radiation up to the doubling dose, and even some way beyond, the genetics effects of radiation are only appreciable when reckoned over the population as a whole and need cause no alarm to the individual on his own account."

Chapter V discusses natural radioactivity—radiation from appurtenances of civilization and occupational exposure to radiation. The report concludes that diagnostic medical X-rays produce exposures to the germ cells of the order of 22 percent that of background and constitute the most important source of man made irradiation. It is estimated that the United Kingdom Atomic Energy Authority's employees receive an average of 0.4 r per year.

The estimated external radiation exposure to people in Great Britain from fallout from all past nuclear tests has been quite minimal. "* * * Including all ordinary atomic bombs exploded before December 1955, and calculating all the radioactivity which they have contributed and will contribute over the next 50 years, it is found that the total dose which a man, continuously out of doors, night and day, would receive is 0.005 r. To this dose from ordinary atomic bombs must be added the dose of thermonuclear weapons. For these latter the dose from the radioactivity still to be deposited is more important. It can be estimated that the accumulated dose from thrmonuclear weapons is 0.002 to 0.003 r with another 0.027 r still to come. All these doses together add up to about 0.035 r from weapons already exploded. This is a maximum dose. The loss of radioactivity from weathering has not been taken into account, nor has the protection afforded by buildings in and around which most people in this country spend a large part of their lives. It would be realistic to divide the dose by three for weathering and by seven for protection afforded as a result of time spent in houses. The average inhabitant of this country may therefore receive in the next 50 years between 0.001 and 0.002 r from this fallout, or 0.02 to 0.04 percent of the radiation that he will receive during the same period from natural surroundings."

The report has this to say about the effects of a continuing program of testing: "* * * if the firing of both types of bomb were to continue indefinitely at the same rate as over the past few years, there would be a buildup of activity gradually reaching a plateau in about a hundred years time which, on the same basis of calculation, would give the average individual a dose over a period of 30 years of 0.026 r or about 0.9 percent of what he would receive in the same period from natural sources."

An important radioactive component of fallout material is Strontium-90. This isotope may be deposited in the bone and when present in sufficient quantities can cause bone cancer. The United Kingdom Medical Research Council report estimates that to date about 0.011 curies of Strontium-90 per square mile has fallen and that future deposits from past tests may produce a maximum of 0.045 curies of strontium-90 per square mile by 1965. These data are immediately evaluated in the report, "* * * these figures should be viewed against the background of the fact that the top one foot of soil has always contained on the average about one curie per square mile of the equally, if not more, dangerous naturally occurring radium."

They estimate the hazard from plutonium in fallout as very small. They feel Cesium 137, Iodine 131 and Barium 140 are of very little significance outside a nearby area of very heavy contamination. They estimate the gonadal dose as 1 percent of natural background and diagnostic radiology as 22 percent. The discussion of atomic warfare is too scant to consider here.

Chapter VI, Assessment of the Hazards of Exposure to Radiation, is in essence a summary of the foregoing—pointing out the differences between effects on the individual and genetic effects. They conjecture that no "authoritative recommendation will name a figure for permissible radiation dose to the whole population additional to that received from natural sources, which is more than twice that of the general value for natural background radiation." This is

estimated by the British at 0.1 r per year, hence 3 r in 30 years and 7 r in 70 years. The National Academy of Sciences estimate is an average 4.3 r in 30 years from natural background exposure and they recommend 10 r as the top figure for average exposure of the population as a whole before age 30.

As to the hazard from strontium^{90} the report states "if the concentration in human bones showed signs of rising greatly beyond one-hundredth of that corresponding to the maximum permissible occupational level" they would feel that immediate consideration were required. This figure is 10 times the highest they report in man today. The National Academy of Sciences report states "It appears, then, that strontium^{90} is not a current threat, but if there were any substantial increase in the rate of contamination of the atmosphere, it could become one."

The conclusions are to all intents and purposes identical to those of the National Academy of Sciences report.

1. Adequate justification should be required for the employment of any source of ionizing radiation on however small a scale. This is not explicitly stated in the National Academy of Sciences report but is inherent in it.

2. Dose levels to the individual—0.3 r per week—200 r in a lifetime for occupational exposures and no more than 50 r the first 30 years of life.

3. No more than twice natural background from man made sources for the population as a whole.

4. The present and foreseeable hazards from external radiation due to fallout at present rate of testing is insignificant. As to internal hazards from strontium^{90} at its present level no detectable increase in the incidence of ill-effects is to be expected. "Nevertheless, recognizing all the inadequacy of our present knowledge, we cannot ignore the possibility, that if the rate of firing increases and particularly if greater numbers of thermonuclear weapons are used, we could within the lifetime of some now living, be approaching levels at which ill-effects might be produced in a small number of the population." This is a rather roundabout way of saying, "let's be careful."

5. a. All sources of radiation should be under close inspection. A personal record not only of doses of radiation received during occupation but also of exposures from all other sources such as medical diagnostic radiology should be kept for all persons whose occupation exposes them to additional sources of radiation. The National Academy of Sciences report would seem to include the whole population in its similar recommendations.

b. Present practices in medical diagnostic radiology should be reviewed with the object of clarifying the indications for different special types of examination now being carried out and defining more closely, both in relation to the patient and to the operators, the conditions which should be observed in their performance. This says, in effect, "let's tighten up on unnecessary exposures."

c. The uses of radiotherapy in non-malignant conditions should be critically examined—again, a warning to tighten up on unnecessary exposures.

d. The small amouts of irradiation from miscellaneous sources, such as x-ray machines used for shoe fitting, luminous watches and clocks, and television apparatus should be reduced as far as possible.

6. They end with a plea for better vital statistics. No comparable recommendation appears in the National Academy of Sciences report.

(Enclosure 3)

Comments on "Race Poisoning by Radiation"

By H. J. Muller

Professor Muller's remarks in regard to mutation changes resulting from nuclear warfare are in conformity with generally held views of geneticists. It is noted that Dr. Muller is a member of the National Academy of Sciences Study Committee on Genetics and the report issued by the Committee was unanimous.

With regard to the peacetime use of nuclear energy, Professor Muller presented estimates of life shortening based on two assumptions, i. e., that an atomic energy worker would receive the maximum permissible exposure every week for a 40 year working period and that the life shortening would be proportional to the total radiation dosage received. As indicated in Professor Muller's article and by figures released by the Atomic Energy Commission, the exposures to atomic energy workers have been considerably less than the maximum permissible amounts ("relatively few workers receive more than a fifth of this amount").

The possible effect of life shortening was considered by the Committee on Pathologic Effects of the National Academy of Sciences study on the biological effects of radiation. The Committee made the following statements:

"The shortening of life correlates roughly with dose of radiation, but has not yet been demonstrated at low doses."

"As the permissible dose level which they (Genetics Committee of the N. A. S.) have hypothesized as desirable for large populations were to be applied there would be no demonstrable somatic effect, although a theoretical minor shortening of life span could not be ruled out."

We are in complete agreement with Professor Muller's remarks that atomic energy "operations must be carried on with such rigorous safeguards that those working on the projects will feel no fear for themselves or their descendants."

In this connection, the AEC may consider placing an upper limit of yearly exposure for atomic energy workers. The average exposure to atomic energy workers during past operations, however, has been so far below the maximum permissible level that the placing of a yearly upper limit would not be expected to impose any major restrictions.

UNITED STATES SENATE,
COMMITTEE ON ARMED SERVICES,
January 31, 1957.

Hon. LEWIS L. STRAUSS,
Chairman, United States Atomic Energy Commission,
19th and Constitution Avenue, Washington 25, D. C.

DEAR ADMIRAL STRAUSS: Attached for your information is copy of letter just received which, as you will see, is signed by five professors of Yale University. The points brought out in this letter are to me extremely important, and coming from the source they do, I feel deserve most careful attention by the officials of our Government, particularly those working with and deciding the policy on development of nuclear weapons.

With kind regards, I am

Sincerely yours,

PRESCOTT BUSH,
United States Senate.

Enclosure.

JANUARY 21, 1957.

The Honorable Senator PRESCOTT BUSH,
United States Senate,
Washington, D. C.

DEAR SENATOR BUSH: At your request we are sending you an outline of the conversation you had with the members of the Yale Department of Biophysics in Woodbridge Hall on December 18th, 1956 on the subject of radiation hazards. The discussion covered the following points which we wished to emphasize to you.

1. The effects produced by nuclear weapons are world-wide. The radiation from our tests will not only be felt elsewhere, but similar weapons tested in Russia, England, or Australia will produce effects in our country as well as their own. The radiation effects are world-wide because most of the radioactivity is carried up into the stratosphere where it remains for such a long time (around ten years) that it becomes distributed over the whole world before the radioactive particles fall to earth or decay.

2. The effects produced by radiation are not reversible. If we were to find, ten years from now, that the people of this country had received too much radiation, we would be at an impasse because the residual effects would already have been produced. If we were to find that the population of this country, or of any country, or of the world for that matter, had received too much radiation, it would be too late to do anything about it. It seems clear that the present rate of world-wide weapons testing is bound to increase and therefore any margin of safety based upon our present rate of testing is sure to be wrong.

3. The effects of radiation are not completely understood. At the present time one of the big areas of disagreement among scientists is on the amount of strontium 90 (one of the radioactive elements produced only in the nuclear weapons tests) which should be permitted to accumulate in the world. Everyone's calculations of the amount of strontium 90 now at large are based on the AEC's data as presented by Commissioner Libby. The disagreement is on how much strontium 90 the human body is able to tolerate. In the twenty years since levels were set up for the maximum amount of radiation to which people

should be exposed, tolerance levels have been revised downward periodically and drastically. For example, the following table indicates the steady downward trend:

Estimated permissible level for occupational risk (in milliroentgens per week)

Year:	
1930	1,000
1936	500
1949	300
1956	100

We feel that there is a good chance that the tolerance levels will be still further revised downward in the future, as it has been in the past. If this were to happen, we certainly shall find ourselves in the tragic situation of having already exposed our population to radiation levels higher than those considered safe.

4. Because radiation effects seems to be cumulative, we may end up by exposing the population of this country to so much radiation from medical x-rays and nuclear weapons testing that we have no safety margin for radiation produced by nuclear power plants. At the present moment, the people of this country receive more radiation through medical x-rays (which of course are usually justified medically) than most of the other countries of the world. We, therefore, would be the first to suffer from the effects of nuclear weapons testing.

5. Because of the points mentioned above, we feel that the decision as to the extent of nuclear weapon testing should be made by a larger group of people than is at present making this decision. Obviously involved in the whole matter of weapons testing is the relative importance of the radiation danger about which we have spoken versus the useful information to be gathered for defense purposes. We do not advocate prohibition of nuclear weapons testing, but we do wish to see discussed the important point of the possible danger from such tests because we feel that the danger is real and that the people should not be misled about what is involved.

Yours sincerely,

(Signed:) ERNEST C. POLLARD,
Chairman, Biophysics Department.
RICHARD B. SETLOW,
Associate Professor Biophysics.
FRANKLIN HUTCHINSON,
Assistant Professor Biophysics.
WALTER R. GUILD,
Assistant Professor Biophysics.
HAROLD J. MOROWITZ,
Assistant Professor Biophysics.

APR 6, 1957.

Hon. PRESCOTT BUSH,
United States Senate

DEAR SENATOR BUSH: I am writing in response to your letter of January 31, 1957 forwarding a copy of a letter of January 21, 1957, signed by five members of the Yale University Department of Biophysics: Ernest C. Pollard, Richard B. Setlow, Franklin Hutchinson, Walter R. Guild and Harold J. Morowicz. I appreciate your patience in permitting the staff to take the necesary time to study the issues presented by the authors which as you know involve broad national defense policy and highly technical genetical and biophysical considerations. As was pointed out in the interim acknowledgment of the General Manager of March 13, 1957 and in earlier telephone discussions with your staff, the questions raised required the participation of several AEC divisions in the preparation of the final reply.

The letter of Professor Pollard and his colleagues, as is to be expected from their scientific standing and their familiarity with certain aspects of the problem, displays a high degree of appreciation of some of the factors involved in the limitation of human exposure to radiation from nuclear weapons test programs and from other sources. These and other factors upon which decisions regarding our various programs must be based are not only under constant study by members of our staff and by our consultants, but those factors involving biological effects of radiation and the exposure of persons to radioactive fallout from weapons tests are the subject of a substantial portion of our research programs in the fields of biology and medicine.

Our staff is in general agreement with the basic scientific statements made in the letter described by the authors as an outline of their conversaion with you on December 18, 1956. Because of the complexity of the subject, a brief summary of the considerations involved cannot be very definitive. I regret that we do not have available for comment a more complete statement of the authors' views.

I have asked our Division of Biology and Medicine to make an analysis of the various points briefly expressed in the letter in the light of our current thinking on problems of radiation protection. I believe you will be interested in their report, copies of which I enclose.

My own comments on specific statements contained in the letter are limited to a consideration of paragraph 5, which appears to summarize the conclusions of the authors, and which reads,

"Because of the points mentioned above, we feel that the decision as to the extent of nuclear weapon testing should be made by a larger group of people than is at present making this decision. *Obviously involved in the whole matter of weapons testing is the relative importance of the radiation danger about which we have spoken versus the useful information to be gathered for defense purposes.* We do not advocate prohibition of nuclear weapons testing, but we do wish to see discussed the important point of the possible danger from such tests because we feel that the danger is real and that the people should not be misled about what is involved."

I have underlined one sentence of paragraph 5 to emphasize that a basic consideration in decisons involving weapons testing is the relationship of the test programs to our national welfare. While the Atomic Energy Commission has certain responsibilities for the development and production of nuclear weapons appropriate to the estimated military needs of the nation, there are involved here, as you are of course aware, questions of national policy which extend far beyond the specific responsibilities of the Commission.

In the quoted paragraph above, the authors refer to "useful information to be gathered for defense purposes." Knowledge gained from past weapons tests have made it possible for us to increase reliability and safety, to improve the yield of explosive devices while reducing the amounts of material used, and to provide us weapons capable of being used tactically. One of the principal objectives of recent tests has been to develop weapons which may be used in defense against attacks and to reduce the radioactive fallout from weapons. As has been announced, the most recent weapons test series in the Pacific was planned and carried out with yields of explosive forces much less than those of the 1954 series.

In reaching decisions to recommend test programs for the approval of the President, the Atomic Energy Commission and the Department of Defense attempt to take advantage of all available information. The number of persons who can participate in the total of all of the considerations involved is necessarily limited. On those facets of the problem which do permit wider participation, we have sought and received counsel from many sources. In addition to persons with whom we have formal relationships of one kind or another, there is constant give and take with a great number of scientists and other professional persons here and abroad.

The total of our contacts represents, we believe, a good cross section of informed opinion throughout the world.

We share the author's view that possible dangers from weapons tests should be discussed and that the people should not be misled about what is involved in exposure to radiation from weapons tests or from peaceful uses of nuclear energy. The potential hazards should neither be minimized nor exaggerated. We believe that the extensive data which have been collected, particularly in programs sponsored by the United States and by the United Kingdom, demonstrate that the degree of potential hazard to the public is very low.

Much of the pertinent information is summarized in reports published last year on studies made concurrently but independently by the National Academy of Sciences and by the British Medical Research Council. Copies of these reports are enclosed for your convenient reference.

I hope that you will find the information which we are submitting helpful. If I can be of further aid to you, by arranging for discussions with appropriate Commission personnel or by other means, please call on me.

Sincerely yours,

W. F. LIBBY, *Acting Chairman.*

Enclosures:

Comments by Division of Biology and Medicine.

Reports of National Academy of Sciences and British Medical Research Council.

REPORT ON QUESTIONS RAISED BY YALE DEPARTMENT OF BIOPHYSICS IN LETTER TO SENATOR PRESCOTT BUSH DECEMBER 18, 1956

Prepared by Division of Biology and Medicine
U. S. Atomic Energy Commission March 27, 1957

The following report discusses a letter from members of the Department of Biophysics, Yale University, to Senator Prescott Bush, January 21, 1957. The letter is signed by Ernest C. Pollard, Richard B. Setlow, Franklin Hutchinson, Walter R. Guild, and Harold J. Morowitz. The letter was transmitted by Senator Bush January 31, 1957, with the following note:

"Attached for your information is copy of letter just received which, as you will see, is signed by five professors of Yale University. The points brought out in this letter are to me extremely important, and coming from the source they do, I feel deserve most careful attention by the officials of our Government, particularly those working with and deciding the policy on development of nuclear weapons."

The letter is described by the authors as an outline of a conversation on the subject of radiation hazards between Senator Bush and members of the Yale Department of Biophysics, December 18, 1956. The letter includes five numbered paragraphs, each of which discusses a point introduced by the initial sentence. For convenience in the following discussion, reference to each of these paragraphs is by number, with the point stated first, followed by our comment.

Because our comments may appear to be rather critical, we wish to state in advance that the letter is unusually well written considering the brevity with which the authors attempted to express their ideas. Thoughtful treatment of the subject is, of course, to be expected from a group of such competence and professional standing. The comments are as follows:

Paragraph 1. The paragraph is limited to factual statements on which there is general agreement.

Paragraph 2. "The effects of radiation are not reversible." The authors apparently have in mind some very specific effects of radiation, such as genetic effects, which are generally considered to be not reversible. We do not believe that, in general, the statement can be supported. For example, the transient effects characteristic of radiation sickness are appropriately described as reversible. Whether or not those effects of exposure of an individual to radiation which may, but not necessarily do, result in leukemia or cancer are to be considered as reversible or not reversible may be a question of semantics. For example, if a number of persons receive equal doses of radiation and one contracts leukemia as a result, it would appear that in the case of the one person some of the effects of the radiation were not entirely reversible. It is not apparent, however, that this is necessarily true in the cases of those who failed to contract the disease.

"If we were to find, ten years from now, that the people of this country * * * or of any country, or of the world * * * had received too much radiation, it would be too late to do anything about it." This is true, of course, whether the consequences implied by "too much radiation" are extremely serious or of relatively minor import from the point of view of their total impact on the population as a whole. The nature of the biological effects of radiation is such that it is impossible to categorically divide levels of exposure to radiation into *safe* and *unsafe* values, if one uses the term *safe* in the sense of absolute freedom from risk.[1] This is because the severity of biological effects, or the probability of a serious effect, increases from very low values for low exposures to radiation to high values for high exposures to radiation.

For example, it is generally considered that any exposure to radiation, no matter how small, makes a correspondingly small contribution to the probability of genetic mutation. On page 22 of the National Academy of Science Summary Reports it is stated that "U. S. residents have, on the average, been receiving from fall-out over the past five years a dose which, if weapons testing were continued at the same rate, is estimated to produce a total 30 year dose of about one tenth of a roentgen * * *" By comparison, it is stated, p. 21, that each person in the United States receives from natural background

[1] While the word *safe* is defined as freedom from risk, it is commonly used to describe situations in which the risk is not zero but is too low to be of concern or, possibly, to be recognized.

radiation "a total accumulated dose of about 4.3 roentgens over a 30 year period" and, from medical x-rays, "on the average a total accumulated dose to the gonads which is about 3 roentgens of x-radiation during a 30 year period." The dose rate required to double the natural rate of genetic mutation, if continued over a period of many generations, is estimated, p. 24, to lie in the range of 30 roentgens to 80 roentgens. On the basis of these estimates, if weapons tests were continued at the present rate, the effect of radioactive fallout from the tests on the genetic mutation rate would be to increase it by less than 1% of its natural value. A very small fraction of the population would be affected and a much smaller fraction would be seriously affected. The risk to the average individual, and the impact on the population as a whole, would be extremely small. Yet, since some individuals might be seriously affected, we must assume that even at these low levels of exposure people are receiving "too much radiation" unless we are convinced that the probable benefit to each person outweighs the risk.

Some perspective on relative values may be obtained by observing, on the basis of estimates quoted above, that exposures to radiation from natural sources (radioactive materials naturally in the earth's crust and cosmic rays from outer space) are more than ten times as high as those estimated to result from weapons testing if continued at the present rate. These are levels of radiation in which the human race has developed and prospered. Whatever risk there may be to the individual member of the race as a result of these relatively low additional exposures, it is quite low compared to many common hazards of life.

It appears that the authors are conscious of the smallness of the risk involved at the levels of exposure under discussion, since in paragraph 4 of their letter they restate their fear in the words, "* * * we may end up by exposing the population of this country to so much radiation from medical x-rays and nuclear weapons testing that we have no safety margin for radiation produced by nuclear power plants." That they also recognize the element of balance of risk against probable benefit is reflected in the statement in paragraph 5 of their letter, to wit: "Obviously involved in the whole matter of weapons testing is the relative importance of the radiation danger about which we have spoken versus the useful information to be gathered for defense purposes."

Summarizing our discussion of paragraph 2, it appears that whether or not ten years from now the population will have received too much radiation as a result of weapons tests will depend not only on the number of persons who may have suffered as a consequence of our weapons tests but also upon the number who may have been enabled to avoid suffering as a result of the contributions of the tests to our defense efforts.

Paragraph 3. "The effects of radiation are not completely understood." It would, perhaps, be more appropriate to say that the processes of life are not completely understood. We probably know more about the biological effects of radiation than those of any other element in our environment; certainly more than we know about the effects of many drugs and organic chemicals developed during the past few years, and perhaps more than about such factors as psychological stress, diet, exercise, etc.

"At the present time one of the big areas of disagreement among scientists is on the amount of strontium 90 * * * which should be permitted to accumulate in the world. Everyone's calculations of the amount of strontium 90 now at large are based on the AEC's data as presented by Commissioner Libby. The disagreement is on how much strontium 90 the human body is able to tolerate." The AEC has been supporting a broad program of investigation of strontium 90 in the atmosphere, soils, plants, animals and humans for more than three years. Most of the investigators are employees of other government agencies, universities, hospitals, etc. A similar program has been under way in the United Kingdom for about the same length of time, and other countries are undertaking studies of this kind. The best data available on the human content of strontium 90 is that in the report, "Strontium 90 in Man" by J. L. Kulp, W. R. Eckelmann, and A. R. Schulert of Columbia University, published in *Science*, February 8, 1957. The skeletal concentrations observed by Kulp, et al, are generally less than 0.01 of those considered acceptable in general population groups by such scientific groups as the International Commission on Radiological Protection, the National Academy of Sciences, and the Medical Research Council of the United Kingdom.

We believe that the principal area of disagreement between scientists well informed in this field is not on how much strontium 90 the body can tolerate,

in the usual sense of the word, but whether or not there may be a very small probability that a low concentration of strontium 90 in the skeleton of an individual will result in bone cancer. There is no experience of production of bone cancers in humans by strontium 90. Experience in humans is limited to the effects of radium as, for example, in the case of the luminizing industry. Estimates of the effects of strontium 90 to be expected in humans are based on comparative studies of strontium 90 and radium in animals and comparative studies of radium in animals and humans. Because bone cancers occur normally in animals (and in humans), and because cancers induced by radioactive materiels cannot be distinguished from those which occur from other causes, the relationship between concentrations of strontium 90 in the skeleton and the resultant probability of bone cancer can be determined only by conparing rates of occurrence in animals containing strontium 90 with rates of occurrence in normal animals. The increase in rate of occurrence that can be measured with confidence depends upon the numbers of animales under observation.

From studies made over the past ten years, it is estimated that from 1,000 to 10,000 times the concentrations of strontium 90 currently found in the skeletons of young humans would be required to produce a detectable increase in the normal rate of bone cancer. Some of our best informed radiobiologists are confident that somewhere below these concentrations the probability of a resulting increase in the rate of bone cancer would be zero, while others believe it possible that even at the very lowest concentrations there may exist correspondingly low probabilities of a bone cancer resulting from strontium 90. In the latter case, the situation is similar to the genetic case discussed in connection with paragraph 2. The question then becomes not how much can the human body tolerate but what is the risk at low levels of exposure in relation to the probable benefits associated with the exposure.

While informed radiobiologists disagree on the subject of whether or not relatively low concentrations of strontium 90 in the skeleton might conceivably result in small increases in the rate of occurrence of bone cancer, they generally agree that is such increases in rate do occur, they will not be large enough to be detectable, even at concentrations much higher than those currently observed. The degree to which speculation on the possibility of an increase in the rate of bone cancer too small to observe has become a matter of public concern is in striking contrast to the apathy with which the public accepts the fact that the rate of lung cancer has increased over the past 20 or 30 years by a factor of about ten.

"In the twenty years since levels were set for the maximum amount of radiation to which people should be exposed, tolerance levels have been revised downward periodically and drastically. . . . We feel that there is a good chance that the tolerance level will be still further revised downward in the future, as it has been in the past. If this were to happen, we certainly shall find ourselves in the tragic situation of having already exposed our population to radiation levels higher than those considered safe." In our discussion of paragraph 2, we have indicated that we do not consider that any level of radiation, no matter how low, can be considered absolutely "safe". If the exposure of the whole population to radiation at a certain level were to result in the premature death of one person, the result would be a tragedy. If it resulted in the death of several persons, it would be a greater tragedy. But, if one is realistic, one must admit that risk to human life is involved not only in other phases of our defense effort but, indirectly, in many decisions involving our foreign policy. In the case of radioactive fallout, the significant risk is not from weapons tests, but from the possibility of a nuclear war in which levels of radiation exposure may be higher by factors ranging upward to tens of thousands or hundreds of thousands. Obviously, this would be a much greater tragedy.

Paragraph 4. "Because radiation effects seem to be cumulative, we may end up by exposing the population of this country to so much radiation from medical x-rays and nuclear weapons testing that we have no safety margin for radiation produced by nuclear power plants." Estimates quoted in discussion of paragraph 2 indicated that at present rates of weapons testing, anticipated exposures to radiation from fallout are about one-thirtieth of current average rates from medical x-rays. At these relative magnitudes, a four percent increase in the use of medical x-rays would wipe out any gains resulting from the elimination of fallout from weapons testing. However, the relative merit of exposing persons to radiation from medical x-rays, nuclear power reactors, and fallout from weapons tests must depend upon the contribution that each may make to per-

sonal and national welfare. The primary consideration with respect to radiation from fallout is, how important to our national welfare is our nuclear weapons program?

Paragraph 5. "Because of the points mentioned above, we feel that the decision as to the extent of nuclear weapons testing should be made by a larger group of people than is at present making this decision." Decisions as to the extent to which weapons tests shall be conducted involve highly sensitive information including weapons design and capabilities, military strategy, and national policies. This limits participation in these decisions to persons officially responsible for our national security.

In evaluation of the radiological hazards associated with weapons tests for use in national planning, the Atomic Energy Commission not only seeks existing information from all available sources but supports extensive research on the production, dissemination, biological uptake, and effects of radioactive materials released by nuclear detonations. The number of persons with whom the Commission has contact on various phases of the question is very large.

"Obviously involved in the whole matter of weapons testing is the relative importance of the radiation danger about which we have spoken versus the useful information to be gathered for defense purposes." We believe this to be the primary consideration.

"We do not advocate prohibition of nuclear weapons testing, but we do wish to see discussed the important point of the possible danger from such tests because we feel that the danger is real and that the people should not be misled about what is involved." The discussions contained in reports issued last year by the National Academy of Sciences and the British Medical Research Council not only explain the possible dangers from weapons tests quite thoroughly, but present differences in the views of various radiobiologists. These reports were written by study panels composed of leading scientists in the respective countries, and it is our belief that they present a good cross section of scientific opinion on these subjects. Copies of the following reports are provided herewith for transmission to Senator Bush:

(1) National Academy of Sciences, "*The Biological Effects of Atomic Radiations, A Report to the Public.*" It is recommended that this brief, non-technical survey of the subject be read before referring to the other reports.

(2) British Medical Research Council, "*The Hazards to Man of Nuclear and Allied Radiations.*" This report has been made more readable than many scientific reports without sacrifice of technical accuracy. It is suggested that Chapter VII, pages 70 ff., summarizing the discussion, be read first. The summary statements refer to paragraphs of the text by number, facilitating the selection of portions of principal interest.

(3) National Academy of Sciences, "*The Biological Effects of Atomic Radiation, Summary Reports.*" This is a summary of individual reports by six committees. While it covers much of the same ground as the report of the British Medical Research Council, views expressed in the two reports involve some basic differences. For example, the American report considers it most probable that below certain levels of exposure to radiation, the contribution of the exposure to the probability that an individual will develop cancer or leukemia is absolutely zero; while the British report considers it likely that even at the lowest levels of exposure there are correspondingly small but real contributions to the probability that the individual will develop one of these diseases.

(4) National Academy of Sciences, "*Pathological Effects of Atomic Radiation.*" Appendix II of this report summarizes the considerations related to the irradiation of persons and populations by radioactive materials within the body.

It is desirable that the general public be as well informed as is practical on all phases of radiation hazards. We know of no information on this subject which is withheld from the public, but because of the complexity of the subject, it is difficult to transmit it comprehensively in a form or forms generally usable by individual laymen. Introduction of even the primary facts into the day-today thinking of a substantial proportion of the general public is a problem in education of which the solution may require many years.

SUMMARY

At the present time, it cannot be stated categorically that there is a level of radiation which is absolutely safe. Mainly because of genetic considerations,

there exists the possibility that some biological effect—perhaps difficult to detect—will be produced at very low doses. The degree of risk, however, would be correspondingly low.

The probability is remote that any individual would be seriously affected by low level radiation contributed by fallout from weapons tests. Unless, however, the risk is absolutely zero, a low individual probability when extrapolated to a large population will result in the statistical possibility that some members of the population may be affected. Because the risks here are very much smaller than the normal hazards of life, it is impossible to estimate the maximum number of persons who might be affected.

The dose of radiation outside the body resulting from fallout contributes about 1/30 of the dose to the population from either natural sources or from medical X-rays. Current concentrations of strontium 90 in the skeleton as a result of fallout are substantially below (about 0.01) levels considered acceptable by recognized independent scientific bodies.

The important question then is whether or not the discontinuation of weapons tests by the United States for the sole purpose of reducing the presently small risk from fallout would not be more than offset by the loss of data, vital to the national security, which weapons tests yield. In the absence of an international disarmament agreement, freezing our weapons technology at the present level may constitute a far greater risk. In such an event it becomes necessary to speak of radiation risk in terms of lethal doses to large segments of the population in the event of nuclear attack, in contrast to the present discussion which is in terms of fractions of permissible levels and statistical possibilities.

WASHINGTON, D. C., *April 25, 1957.*

Dr. ALBERT SCHWEITZER,
Lambarene Hospital, Lambarene, Gabon, French Equatorial Africa.

DEAR DR. SCHWEITZER: I am writing you as a scientist, to present data bearing on a scientific fact: The degree of hazard to humanity from radioactive fallout from nuclear weapons tests.

In the press on April 24, I read your statement from Oslo on the hazards of nuclear weapons testing, and in this way learned of your fears that the present testing program may be dangerous. Since I have spent much time during the past several years in the study of this question, I am taking the liberty of writing you. Also, since your statement was issued to news media and received wide public attention, I am making this letter public in the belief that every possible action should be taken to increase public understanding on the important question of weapons testing.

Your belief in the sanctity of life, and the dedication with which you have devoted your own life and talents to unselfish causes, have made a deep impression on the minds of persons throughout the world. Your concern over the possible effects of nuclear tests is characteristic of the humane and sensitive qualities which you always have displayed, and for which you are justly honored. Along with these qualities, I know you have the intellectual strength and integrity to seek the truth wherever it lies. It is in this spirit that I write you, believing that you will welcome whatever facts I may be able to provide regarding radioactive fallout from weapons testing.

I do not know what data you have utilized in studying this question, but I seriously doubt, from the evidence of your statement, that you have had access to the most recent information. Immediately after reading your statement, I sent you a copy of a speech which I gave recently regarding what we know from scientific studies on fallout radiation and its effects. I am enclosing with this letter a copy of a paper which I am presenting on April 26 before the American Physical Society. I hope these documents will be of use to you. They demonstrate that an intensive effort has been made to calculate on theoretical grounds, and to determine from sample collections, the actual levels of radioactivity in the soil, in water, in food products, and in human bodies as a result of weapons tests.

If you have gained the impression that United States official statements do not take into account the possible hazard from internal radiation—and I fear from your statement that you have—I hasten to assure you that this is not the case. Government statements have dealt extensively with this matter. It has likewise been considered at length in a report prepared by scores of eminent scien-

tists for the National Academy of Sciences, and in England by the British Medical Research Council, both reports appearing in June of last year.

Particularly since the summer of 1953, the Atomic Energy Commission has conducted an intensive study of worldwide fallout which has revealed most of the information now available on this subject. These studies have included analysis of soil, plants, foods and other materials from many parts of the world. The United States Government has furnished this information without reserve to the United Nations Scientific Committee on Atomic Radiation, which was established at the recommendation of the United States and which has studied data provided by other countries.

Although there are some differences in the findings of scientists in this country and abroad, there is general agreement upon the approximate magnitude of the fallout and the rate at which it is descending from the stratosphere. Perhaps there is less agreement about the magnitude of the physiological effects which can be expected to result from fallout radiation. Nevertheless, it is very generally agreed, among those who have studied the question, that the radiation exposures from fallout are very much smaller than those which would be required to produce observable effects in the population. The U. S. Government agencies have been continuously concerned with maintaining this condition of very small test radiation hazard and have never neglected study and action to reduce it.

I do not mean to say that there is no risk at all. What I should like to demonstrate to you is that the risk is extremely small compared with other risks which persons everywhere take as a normal part of their lives. At the same time, I ask you to weigh this risk against what I believe would be the far greater risk—to freedom-loving people everywhere in the world—of not maintaining our defenses against the totalitarian forces at large in the world until such time as safeguarded disarmament may be achieved. Of course, a workable, safeguarded system of international disarmament is a paramount objective of the United States Government, and one which we must work for and hope and pray will be achieved.

To go into more detail on the question of risk from worldwide radioactive fallout, there are two possible hazards. The first is the genetic hazard due to radiation of the reproductive organs by penetrating gamma radiation, and the second is the hazard due to the irradiation of the bones by assimilated strontium-90, taken up largely through food. These two possible hazards should not be confused; there is no reason to fear genetic hazard from strontium-90, since it accumulates in the bones and does not appreciably irradiate the reproductive organs.

In order to understand the degree of these hazards, it is necessary to compare the amount of radiation dosage received from fallout with the amount of radiation dosage normally received by all living things because of the natural radioactivity in the environment. In this way, it is possible to put the hazards from weapons testing into the context of normal human experience.

When this kind of comparison is made, it becomes apparent that we all carry in our bodes, and have in our surroundings, amounts of radioactivity very much larger than those derived from radioactive fallout.

Cosmic rays, which come from outer space, have their radiation effect progressively diluted as they pass through the atmosphere. Thus, a person living at an altitude of about one mile above sea level receives a dosage of cosmic rays approaching double that of a person who lives at sea level. There are other variations in the natural "background" dosages. For example, people living in certain localities of uranium or thorium mineralization will receive much more radiation than the average, and their ancestors have received these much higher doses over centuries in many parts of the world. Living in a brick house, rather than in a wooden house, will, with certain kinds of bricks in certain parts of the world, increase radiation exposure many times over that from test fallout.

The additional radiation dosages which persons receive from fallout are small compared to these natural dosages and even the variations in the natural dosages. To be specific, the dosage to new bone as in children which results from strontium-90 at present is about the same as the additional dosage which a resident at sea level would receive from cosmic rays if he moved from a beach to the top of a hill a few hundred feet high.

There is no question that excessive dosages of radioactive strontium can cause bone cancer and leukemia in animals, so we should not casually dismiss

the possibilitiy of harmful results from test fallout. However, keeping in mind that populations are exposed to natural radiations considerably greater than the fallout dosages, we can attempt to determine whether these have caused any detectable effects. We can examine, for example, whether there is any obvious increase in the rate of occurrence of bone cancer and leukemia in populations living at higher altitudes or in regions of uranium mineralization, etc.

Examination of available records does not disclose any such effects. However, vital statistics have not always been carefully kept, and further studies are being carried on under the aegis of the United Nations Committee to determine whether any such effects can be detected. One fact is apparent, however—it certainly is not our normal experience that people can appreciably increase the occurrence of these dread diseases by moving to a higher altitude or by moving from a sedimentary soil, where the uranium content is low, to an igneous or granitic surface, where the uranium content is very much higher, or by moving from a wooden to a brick or concrete house.

Another way of evaluating the possible risk from strontium-90 in fallout is through comparison with the permissible concentration of strontium-90 recommended by authoritative groups. The permissible amount of strontium-90 for atomic energy workers in the United States is about 2,000 times the present strontium-90 content of new bone in the United States resulting from fallout. (Strontium-90 concentrations in the rest of the world are generally lower than those in the United States.) Authoritative groups have recommended that, on grounds of general prudence, the permissible limit for whole populations be one-tenth of that for atomic energy workers. On this basis, the present level for new bone, that is, in children, in the United States is somewhat less than one percent of the maximum permissible concentration for the population.

Perhaps a word of explanation should be given regarding these maximum permissible concentrations. As you know, scientists do not speak of "risks" or "hazards" in the sense that the words ordinarily are used. They try to measure possibilities almost to the limits of the finite; therefore, "risk" includes the possibility of effects far beyond the range of the probable or detectable. The maximum permissible concentrations are not safety limits, rather, they indicate that at considerably larger concentrations, perhaps tenfold greater, there would be definitely detectable effects.

So far, I have been discussing principally the possible risks from radioactive strontium. Radioactive fallout includes other materials which do not accumulate inside the body, but do not emit penetrating radiation which can irriadiate the sex organs and other parts of the whole body from the outside. Such radiations can produce genetic mutations.

Again, in evaluating the possibility of genetic effects from fallout, we should try to compare it with normal experience. The external dosages from fallout, that is, those which might cause genetic effects, have averaged between one and five thousandths of one roentgen per year in the United States during the last three or four years. This figure should be compared with a normal dosage of 150 thousandths of one roentgen per year from cosmic rays and natural radioactive materials in the environment. In other words, the external fallout radiation has been from 0.7 percent to about three percent of the natural radiation exposure.

As another example, in certain countries of the world a brick house might easily have enough natural radioactive material in the walls to give up to 40 thousandths of a roentgen more exposure per year than a wooden house and a concrete block house gives about 100 thousandths of a roentgen more annually. These dosages range between 8 and 100 times the dosage due to test fallout.

Obviously, the genetic effect of fallout radiation must be very small compared with the genetic effect of natural radiation.

As you pointed out in your statement, radioactivity from tests which already have been held is present in the stratosphere, from which it will descend for years to come. The radioactivity of this material constantly is decreasing through normal radioactive decay. The tiny radioactive particles fall so slowly from the stratosphere that the continuing fallout in the United States just about compensates for the radioactive decay of the radiostrontium already deposited. Therefore, the present level of radiostrontium in the soil is about as much as we shall ever have from tests already fired.

Continued testing would not increase radioactivity on a straight additive basis, since an equilibrium would be established between the added radioactivity and radioactive decay. If tests were to continue until 1983 at the rate of the past five

years, levels in the United States would be expected to reach about four times their present values. Levels about six times the present ones would be reached by the year 2011 if testing were to continue for that long a time.

I hope that I have provided enough information to demonstrate that the risk from nuclear testing at the present rate is small. Of course, a great amount of more detailed information is available, and I shall be glad to supply it to you if you wish. No scientist contends that there is no risk. We accept risk as payment for our pleasures, our comforts, and our material progress. Here the choice seems much clearer—the terrible risk of abandoning the defense effort which is so essential under present conditions to the survival of the Free World against the small controlled risk from weapons testing.

Sincerely yours,

/s/ W. F. LIBBY.

PERE LORENTZ ASSOCIATES,
New York City, N. Y., April 1, 1957.

The Honorable CLINTON P. ANDERSON,
United States Senate,
Washington, D. C.

MY DEAR SENATOR ANDERSON: As you know, almost a year ago the National Academy of Sciences issued Summary Reports on the Biological Effects of Atomic Radiation, in collaboration with the National Research Council and financed by a grant of one million dollars from the Rockefeller Foundation. The reports were signed by one hundred and ten doctors and scientists, grouped into six committees. I am at present engaged in writing a book about atomic energy, for which I am under contract, and I am writing to you to inquire as to whether your committee has knowledge of any action or any research that may have been in accordance with the recommendations made by the six committees. That is, has the Congress appropriated funds for the use of any government agency in instituting studies as have been recommended, or has the Congress directed the AEC to institute studies in accordance with the major recommendations of the National Academy Committees.

1. The Committee on Genetics recommended, among other things, "that, in view of the fact that total accumulated dose is the genetically important figure, steps be taken to institute a national system of radiation exposure record-keeping, under which there would be maintained for every individual a complete history of his total record of exposure to X-rays, and to all other gamma radiation. This will impose minor burdens on all individuals of our society, but it will, as a compensation, be a real protection to them."

"That every effort be made to assign to tasks involving higher radiation exposures individuals who, for age or other reasons, are unlikely thereafter to have additional offspring. Again it is recognized that such a procedure will introduce complications and difficulties, but this committee is convinced that society should begin to modify its procedures to meet inevitable new conditions * * *"

"One important lesson which results from this study is the following: The present state of advance in atomic and nuclear physics on the one hand, and in genetics on the other hand, are seriously out of balance. We badly need to know much more about genetics—about all kinds and all levels of genetics, from the most fundamental research on various lowly forms of life to human radiation genetics. This requires serious contributions of time, of brains, and of money. Although brains and time are more important than money, the latter is also essential; and our society should take prompt steps to see to it that the support of research in genetics is substantially expanded and that it is stabilized."

Has any agency of the government undertaken research in genetics on a substantial scale, as was recommended?

2. The Committee of Pathologic Effects noted that "The increasing contamination of the atmosphere with potential carcinogens, the widespread use of many new and powerful drugs in medicine and chemical agents in industry, emphasize the need for vigilance over the entire environment."

Have there been any directed studies by your committee or any other committee of the Congress, to any United States agency to examine the "entire environment" of a community, or a city, or a rural area, or of any group of controlled individuals, whereby an analyses of the total pollution and of the total contamination might be made?

3. The Committee on Meteorological Aspects stated among other things that "the operation of any significant fraction of the earth's nuclear reactors without proper safeguards would be of concern to all" * * * The report stated further that "it should be pointed out that the release of a hazardous substance by any country may affect other countries—particularly in the same latitude belt; international control to establish and maintain high standards of safe plant operation is essential". This committee also remarked that "as additional safety factors, meteorological research to locate plants in areas where unexpected releases will do the least damage is desirable." * * *

Has your committee or any other body in Congress directed any agency of the government to take steps toward establishing any sort of international control of nuclear reactor plants? Has any scientific organization satisfactorily refuted the recommendation of the Meteorological Committee? If no, why has the AEC issued licenses for the construction of large scale reactors near New York City, Chicago, Detroit and Pittsburgh, if it is desirable "to locate plants in areas where unexpected releases will do the least damage"?

4. The Committee on Oceanography and Fisheries. This committee indicated more than any other, urgent and immediate need for world studies. In their Conclusions and Recommendations they stated, "Within the foreseeable future the problem of disposal of atomic wastes from nuclear fission power plants will greatly overshadow the present problems posed by the dispersal of radioactive materials from weapon tests. It may be convenient and perhaps necessary to dispose of some of these industrial wastes in the oceans. Sufficient knowledge is not now available to predict the efforts of such disposal on man's use of other resources of the sea."

"We are confident that the necessary knowledge can be obtained through an adequate and long-range program of research on the physics, chemistry, and geology of the sea and on the biology of marine organisms. Such a program would involve both field and laboratory experiments with radioactive material as well as the use of other techniques for oceanographic research. Although some research is already underway, the level of effort is too low. Far more important, much of the present research is too short-range in character, directed towards *ad hoc* solutions of immediate engineering problems, and as a result produces limited knowledge rather than the broad understanding upon which lasting solutions can be based."

"We recommend that in future weapons tests there should be a serious effort to obtain the maximum of purely scientific information about the ocean, the atmosphere, and marine organisms. This requires, in our opinion, the following steps: (1) In the planning stage committees of disinterested scientists should be consulted and their recommendations followed, (2) funds should be made available for scientific studies unrelated to the character of the weapons themselves, and (3) the recommended scientific program should be supported and carried out independently of the military program rather than on a 'not to interfere' basis."

"Ignorance and emotionalism characterize much of the discussion of the effects of large amounts of radioactivity on the oceans and the fisheries. Our present knowledge should be sufficient to dispel much of the over-confidence on the one hand and the fear on the other that have characterized discussion both within the Government and among the general public. In our opinion, benefits would result from a considerable relaxation of secrecy in a serious attempt to spread knowledge and understanding throughout the population."

"Sea disposal of radioactive waste materials, if carried out in a limited, experimental, controlled fashion, can provide some of the information required to evaluate the possibilities of, and limitations on, this method of disposal. Very careful regulation and evaluation of such operations will, however, be required. We, therefore, recommend that a national agency, with adequate authority, financial support, and technical staff, regulate and maintain records of such disposal, and that continuing scientific and engineering studies be made of the resulting effects in the sea."

"We recommend that a National Academy of Sciences-National Research Council committee on atomic radiation in relation to oceanography and fisheries be established on a continuing basis to collect and evaluate information and to plan and coordinate scientific research."

"Studies of the ocean and the atmosphere are more costly in time than in money and time is already late to begin certain important studies. The problems involved cannot be attacked quickly or even in many cases, directly. The pollu-

tion problems of the past and present, though serious, are not irremediable. The atomic waste problem, if allowed to get out of hand, might result in a profound, irrecoverable loss. We, therefore, plead with all urgency for immediate intensification and redirection of scientific effort on a world-wide basis towards building the structure of understanding that will be necessary in the future. This structure cannot be completed in a few years; decades of effort will be necessary and mankind will be fortunate if the required knowlerge is available at the time when the practical engineering problems have to be faced."

"The world-girdling oceans cannot be separated into isolated parts. What happens at any one point in the sea ultimately affects the waters everywhere. Moreover, the oceans are international. No man and no nation can claim the exclusive ownership of the resources of the sea. The problem of the disposal of radioactive wastes, with its potential hazard to human use of marine resources, is thus an international one. In certain countries with small land areas and large populations, marine disposal of fission products may be essential to the economic development of atomic energy. We, therefore, recommend: (1) that cognizant international agencies formulate as soon as possible conventions for the safe disposal of atomic wastes at sea, based on existing scientific knowledge; and (2) that the nations be urged to collaborate in studies of the oceans and their contained organisms, with the objective of developing comparatively safe means of oceanic disposal of the very large quantities of radioactive wastes that may be expected in the future."

"Because of the increasing radioactive contamination of the sea and the atmosphere, many of the necessary experiments will not be possible after another 10 or 20 years. The recommended international scientific effort should be developed on an urgent basis."

Has your committee or any other committee in the Congress considered the establishment of a national agency such as is recommended in paragraph 6? Has any agency of the United States Government taken steps to meet with other government bodies in an effort to "formulate as soon as possible, conventions for the safe disposal of atomic wastes at sea"? Has your committee directed any agency of the government to collaborate with other nations "in studies of the oceans and their contained organism, with the objective of developing comparatively safe means of oceanic disposal of the very large quantities of radioactive wastes that may be expected in the future"?

5. The Committee on Agriculture and Food Supplies. This committee recommended, among other things, that "The Committee therefore urgently recommends that appropriate experimentation be immediately activated to provide specific information about possible total or cumulative biological effects that might follow the ingestion of such foods. It further urges that the planning of such experiments be broadly based, and that the development of the experimental designs and details of their subsequent execution be most carefully considered in order that the emerging data will be acceptable as a basis for the crucial decisions that ultimately will have to be taken, and directly of value to the regulatory agencies charged with the protection of the public interest * * *."

"Research activities might appropriately be carried out on areas near weapons test sites where substantially greater changes in background would be anticipated. The distribution in the environment, in the soil at various depths, in the vegetation, in the wildlife, in the streams, etc., would all be pertinent. The rate of accumulation in soil as affected by land use ought to be studied. Forested land, range land, rotation grassland, and plowland, irrigated and non-irrigated, may each present a different situation. It is possible that certain of the State Agricultural Experiment Stations might be in a position to undertake limited surveys of this type on areas likely to be under their control for some considerable time in the future."

Has your committee knowledge of any experimentation being undertaken by any agency of the government "to provide specific information about possible total or cumulative biological effects that might follow the ingestion of such foods"? Also has your committee recommended funds for the Department of Agriculture, or any other government agency, to be used in undertaking the research activities in areas near weapons test sites, as recommended?

6. The Committee on Disposal and Dispersal of Radioactive Wastes—This Committee listed the following items that were considered important enough to require further study:

(1) Geophysical and geochemical aspects of ultimate disposal of highly radioactive wastes.

(2) Site selection for various nuclear facilities, particularly chemical processing plants and their location with respect to suitable waste disposal areas.

(3) Transportation of highly radioactive materials.

(4) Relationship of introduction and development of nuclear facilities to basic public health, social and economic situations extant or resulting from such development.

The Committee on Oceanography would seem to have covered this field thoroughly.

In summary, although the 1954 Atomic Energy Commission Act would seem to vest all responsibilities concerning atomic matter in the hands of the AEC, there seems to be now a public health problem which involves the land, the oceans, the air and agricultural products, and I would be deeply grateful if your committee could provide me with any record of research projects that might have been approved by the Congress, and that might be carrying out some of the recommendations of the before-mentioned committees of the National Academy of Sciences.

Very truly yours,

PARE LORENTZ.

UNITED STATES ATOMIC ENERGY COMMISSION,
Washington 25, D. C., April 9, 1957.

Mr. JAMES T. RAMEY,
Executive Director, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR MR. RAMEY: This is to acknowledge Dave Toll's letter of April 6, 1957, enclosing a letter from Pare Lorentz Associates, Inc., addressed to Senator Anderson dated April 1, 1957.

Be assured the letter will receive our prompt attention, and a reply will be forwarded.

Sincerely yours,

BRYAN F. LAPLANTE,
Special Assistant to the General Manager (Congressional).

UNITED STATES ATOMIC ENERGY COMMISSION,
Washington 25, D. C., May 22, 1957.

Mr. JAMES T. RAMEY,
Executive Director, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR MR. RAMEY: This is in further reply to Mr. David Toll's letter of April 6, 1957, requesting the Commission's comments on a letter dated April 1, 1957, Senator Anderson had received from Pare Lorentz Associates, Inc. To the extent we are knowledgeable we are happy to provide answers or comments in connection with the questions raised by Mr. Lorentz:

1. Congress has not appropriated funds specifically directed toward the establishment of a national system of radiation exposure record keeping, nor has it directed the Atomic Energy Commission or any other agency to institute such a system. Some study has been given this subject by the Commission's Division of Biology and Medicine, by the Advisory Committee for Biology and Medicine and by a number of national and international bodies including the National Committee on Radiation Protection, the International Commission on Radiation Protection, the United Nations Scientific Committee on the Effects of Atomic Radiation, and the World Health Organization. A complete system of record keeping would involve either (a) the necessity for each individual in the population to carry a card upon which would be entered the radiation exposure received at each medical examination and treatment and the radiation received from occupational exposures, or (b) the reporting of each such exposure to a central bureau by radiologists and health physicists, together with some means of accurately identifying the individual's exposure. The first of these methods, to be successful, would require the cooperation of all individuals in the population. Either method would be unwieldly. As a result of increasing interest in radiation exposures, radiologists are voluntarily making more accurate determinations of exposures actually given during examinations and treatments and are keeping better records of such exposures. A number of studies to obtain more accurate estimates of average exposures incidental to medical diagnosis and treatment in representative hospitals are under way. Records of occupational exposure are kept on individuals employed in AEC establishments. Persons

using sources of radiation licensed by the AEC are also required to keep records of personal exposure to demonstrate compliance with regulations covering such use. It may be expected that most private industries employing persons subject to radiation exposure will maintain records of exposure for purposes of legal protection.

As noted in the Commission's 21st Semi-annual Report to Congress, research in genetics was initiated by the Manhattan Project and has been supported on a greatly expanded scale by the AEC. Research in genetics is also supported independently by the National Science Foundation and by the National Institute of Health. There has been no large increase in the amount of Government-supported genetic research as a result of NAS–NRC report. While more rapid expansion could take place with a greater expenditure of Government funds, there is also a need for a large number of competent scientists interested in carrying out the studies.

2. The Congress is well aware of the problem of the increasing contamination of the atmosphere and its possible relationship to the increase in lung cancer, and has set up under the Department of Health, Education, and Welfare the Interdepartmental Committee on Community Air Pollution.

This Committee has official representation from the Department of Agriculture, the U. S. Atomic Energy Commission, the Department of Commerce, the Department of Defense, the Department of Health, Education, and Welfare, the Department of Interior, and the National Science Foundation.

The Committee holds regular meetings in order to discuss in detail the problems connected with air pollution. In addition, there is a budget supplied to DHEW through Congressional appropriation which is being used for research and survey work in connection with air pollution.

3. Some degree of international control of nuclear reactor plants is to be expected from the International Atomic Energy Agency. Such controls will apply only to reactors built and operated by members of the agency.

The Atomic Energy Commission has issued "construction permits" for power reactors at Indian Point, New York, approximately 24 miles from New York City, at Dresden, Illinois, approximately 50 miles from Chicago, and at Lagoona Beach, Michigan, approximately 30 miles from Detroit. In addition, the Commission is constructing a Pressurized Water Reactor at Shippingport, Pennsylvania, approximately 25 miles from Pittsburgh. In the case of the Pressurized Water Reactor the AEC undertook the construction of this reactor after a careful survey of the conditions at the site and the type of reactor which was to be built. It was concluded that the public would be adequately protected by the type of reactor selected and also by providing for a tight container around the reactor to contain radioactive materials in the unlikely event of a reactor accident severe enough to disrupt the heavily constructed primary reactor container system. In the case of the three privately-owned reactors, the construction permit was issued for these only after careful consideration of the general type of reactor plan and with provisos that these too would be surrounded by safety containers to protect the public in the case of an accident. As you are no doubt aware there is now in progress a hearing on the Power Reactor Development Corporation Lagoona Beach reactor.

4. A scientific committee initiated and financed by the Atomic Energy Commission, Office of Naval Research, Fish and Wildlife Service, and with the participation of the National Science Foundation has recently been established by the National Academy of Sciences. The scope of the committee is to:

1. Survey and evaluate the state of knowledge and activity in the various branches of oceanography and recommend broad programs and specific tasks that might be undertaken to advance the oceanographic sciences.
2. Facilitate joint planning among those responsible for the support and conduct of research in oceanography.
3. Stimulate coordinated studies on problems which overlap the traditional boundaries of specialized research, and identify opportunities for the application of knowledge and theory from other sciences and disciplines to problems of oceanography.
4. Develop a focal point for the compilation, exchange, and dissemination of information, and promote the efficient utilization of research personnel and facilities.
5. Provide forums for the discussion of problems of concern to all branches of oceanography (such as manpower, ship and laboratory facilities, instrumentation, data processing, etc.) and foster the search for solutions to those problems.

6. Provide for appropriate scientific representation in international meetings and furnish counsel regarding United States national interests in matters pertaining to the ocean.

It is anticipated that this committee will be a working group, as well as one to give advice which will enable the participating agencies to accomplish objectives even beyond those mentioned in Mr. Lorentz' question.

The Oceanographic Panel of the International Geophysical Year (U. S. participation financed through N. S. F.) is also actively studying phases of physical oceanography which will contribute to knowledge necessary for sea disposal of atomic wastes.

A number of research projects are supported by the Atomic Energy Commission which have a bearing on the biological effects of radioactive wastes which might be disposed of at sea. These are:

1. Woods Hole Oceanographic Institution "Biological and Radiochemical Studies of Coastal Plankton Populations"
2. Marine Biological Laboratory, Woods Hole "Studies on the Physiology of Marine Organisms Using Radiosotopes"
3. Applied Fisheries Laboratory, U. of Washington Radiobiological Surveys in the Vicinity of the Eniwetok Test Site. (This is not the exact title of any one project but is the general subject of their research.)
4. Naval Radiological Defense Laboratory, San Francisco "Study of Soil, Water, Flora and Fauna of the Marshall Islands"
5. The Fish and Wildlife Service, Beaufort, N. C. "The Accumulation of Fission Products by Marine Fish and Shellfish
6. University of Hawaii, Hawaii Marine Laboratory "Radioisotope Uptake in Marine Organisms with Special Reference to the Passage of Such Isotopes As Are Liberated from Atomic Weapons Through Food Chains Leading to Organisms Utilized as Food by Man"
7. University of Hawaii, Hawaii Marine Laboratory "Management of the Eniwetok Marine Biological Laboratory"
8. Stanford University, George Vanderbilt Foundation "Marine Biological Survey of Western Pacific"

In reference to question (6), geophysical and geochemical aspects of ultimate disposal of high-level wastes are being actively considered by a number of research groups and projects. These are described on pages 159–160 of the 21st Semi-annual Report to Congress (January 1957). In addition, a new project now in effect with Scripps Oceanographic Institute is concerned with studies of the circulation of elements of biological importance, and the dynamics of their dilution and concentration, throughout the entire geophysical environment.

5. The recommendation of the Committee on Agriculture and Food Supplies quoted here applies primarily to fallout from nuclear weapons, particularly strontium 90. The AEC has for several years been engaged in extensive research programs covering not only the biological effects that might follow the ingestion of such foods but also studies of the distribution of strontium 90 in the environment, in the soil at various depths, in the vegetation, and in animals under a variety of environmental conditions. The U. S. Department of Agriculture in certain state agricultural experiment stations has cooperated in some of these studies. The AEC has conducted studies of uptake of radioactive materials by plants and wild animals near the weapons test sites but has not found a favorable environmental condition for establishing a large scale agricultural research activity near the test site.

6. In this portion of his letter, after enumerating items considered by the Committee on Disposal and Dispersal of Radioactive Wastes as sufficiently important to require further study, we assume Mr. Lorentz intended to say that that Committee would seem to have covered this field thoroughly. These tiems have been under intensive study by the AEC but may be expected to represent basic problems in the development of nuclear energy for some years to come.

Geophysical and geochemical aspects of the disposal of highly radioactive wastes in soil have been under intensive study for more than ten years at our Hanford plant and to a somewhat lesser extent at Idaho Falls and at Oak Ridge. In these studies we have had the cooperation of the U. S. Geological Survey and the Earth Sciences Division of the National Academy of Sciences.

Studies of site selection for chemical processing plants with respect to suitable waste disposal areas are of course not made independently of the studies mentioned above.

Responsibility for the safe transportation of highly radioactive material is vested in the Department of Commerce, the Civil Aeronautics Board, and the

U. S. Coast Guard. The AEC has worked closely with these agencies and is presently engaged in studies designed to minimize hazards incidental to future transportation requirements of the nuclear energy industry.

The relationship of nuclear facilities to basic public health, social and economic situations is a broad question upon which many studies are being brought to bear; for example, the Joint Committee on Atomic Energy established in 1955 a panel of prominent persons from many professions to study the impact of the peaceful uses of atomic energy. The report of this panel was published in two volumes in January of 1956. The Joint Committee on Atomic Energy and other committees of Congress such as the Committee on Interstate and Foreign Commerce, House of Representatives; the Committee on Government Operations, House of Representatives; and the Senate Committee on Foreign Relations, to mention but a few, haxe from time to time held hearings on various aspects of this subject.

This subject has also been of interest to many other groups including the Atomic Energy Commission, the U. S. Public Health Service, the National Academy of Sciences, the World Health Organization, the United Nations, the International Labor Organization, the Rockefeller Foundation, state health organizations and many other groups. These interests are developing our knowledge of the relationship of nuclear facilities to public health more rapidly than any other aspect of public health. Social and economic impacts arise more spontaneously from economic interests.

In reference to the last paragraph of Mr. Lorentz' letter, we believe that he is in error in assuming that Congress approves individual research projects rather than approving budgets for research programs.

We trust the information supplied above will be of assistance to you in replying to Mr. Lorentz' letter. Mr. Lorentz' letter is being returned to you.

Sincerely yours,

R. W. COOK, *Deputy General Manager.*

Enclosure: Letter dated April 1, 1957.

UNITED STATES ATOMIC ENERGY COMMISSION,
Washington 25, D. C., July 9, 1957.

Hon. CARL T. DURHAM,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR MR. DURHAM: The Atomic Energy Commission approved for unclassified publication the rewritten handbook entitled "The Effects of Nuclear Weapons," copies of which are attached.

New information has been developed since "The Effects of Atomic Weapons" was last published. At the request and with the assistance of the AEC, the Armed Forces Special Weapons Project of the Department of Defense prepared the new edition. The services of Dr. Samuel Glasstone were secured as editor and the material was reviewed by the Federal Civil Defense Administration, by cognizant contractors of the AEC, and by the Department of Defense.

"The Effects of Nuclear Weapons" updates information appearing in the 1950 handbook and the February 15, 1955 release on Effects of High-Yield Weapons. It includes an expanded coverage on fallout referred to in our letter to Senator Anderson dated November 16, 1956. It bears a foreword signed by the Secretary of Defense, Charles E. Wilson, for the Department of Defense, Administrator Val Peterson for the FCDA, and the undersigned for the AEC. The handbook has been printed by the Government Printing Office which also prepared the advanced page-proof copies of those chapters dealing with fallout which were furnished to Representative Holifield of your Committee for the recent hearings on fallout.

Approval of the text by the Commission was unanimous except with respect to a number of points on which Commissioner Murray desired wording or action differing from that believed most appropriate or factual by the other members of the Commission. Commissioner Murray asked that you be informed of the points concerned; these are listed in the second attachment and comment thereon will be provided if requested.

These copies of the Effects of Nuclear Weapons handbook are being sent in advance of the release for the book which is set for publication in afternoon newspapers of Friday, July 12, 1957. When these copies were received it was

noted that the printer had omitted six lines from paragraph 10.24 on page 454. A correction sheet for that page has been included in each book.

Sincerely yours,

LEWIS L. STRAUSS, *Chairman.*

Attachments: As stated above.

Listed below are seven suggestions regarding "The Effects of Nuclear Weapons" handbook, which were made by Commissioner Thomas E. Murray but were not accepted by the Commission.

(1) That Chapters 9, 10, and 11 be submitted to the Advisory Committee on Biology & Medicine for review prior to approval for publication.

(2) That there be inserted in the Handbook a detailed Appendix of latest statistics and opinions relative to world-wide fallout.

(3) That in the last sentence of para. 9.49 which reads: "In fact the external radiation produced by the fallout from a weapon with a fission yield in the megaton range would be extremely small in comparison with the natural background radiation," the word "extremely" be deleted and, following the word "small," (of the order of —— percent)" be added.

(4) That in para. 9.94 of the Handbook, the words "can under some conditions" be replaced with the words "could be expected to" in the sentence which reads: "One is that the residual nuclear radiation can under some conditions represent a serious hazard at great distances from a nuclear explosion, well beyond the range of blast, shock, thermal radiation and the initial nuclear radiation."

(5) That in para. 10.1, the word "will" be used instead of "may" in the sentence which reads: "During the first few days or weeks after the detonation, the radiation levels may be high enough to represent a danger to exposed persons."

(6) That Chapter 10 entitled "World-Wide Fallout and Residual Radiation" give added coverage to the long-term strontium-90 hazard from local fallout.

(7) That in para. 11.122 the word "may" be deleted and the word "find" be changed to "finds" in the second sentence which reads: "The strontium may then find its way, mainly through milk products, into the human body."

APPENDIX 7

1. INFORMATION FURNISHED TO THE JOINT COMMITTEE BY STANLEY H. CLARK, BALTIMORE, MD.

2. INFORMATION FURNISHED TO THE JOINT COMMITTEE BY ALDEN A. POTTER, BETHESDA, MD.

8420 LOOKOUT MOUNTAIN AVENUE,
LOS ANGELES 46, CALIF.,
February 22, 1957.

Mr. GRAHAM DUSHANE,
Editor, Science,
1515 Massachusetts Avenue NW.,
Washington 5, D. C.

DEAR SIR: The article on Sr–90 content in human beings by J. L. Kulp, et al. (*Science*, February 8, 1957) may possibly be interpreted by some readers as indicating that there is little danger at this time from Sr–90. Actually that is very far from the truth.

First, consider the International Commission of Radiological Protection's recent (November 1956) reduction of the maximum permissible dose. It is now considered to be one-third (⅓) of the former value or five roentgens per year (this is occupational). Next we must always consider the occupational maximum permissible concentration separately from the maximum permissible concentration for the entire populace. The accepted reduction factor is ten—that is, the populace maximum permissible concentration should not exceed 0.5 roentgens per year (5×0.1). Applying to the Sr–90 maximum permissible concentration, we see that the populace m. p. c. is 33.3 micromicrocuries gram of calcium rather than the 1,000 micromicrocuries/gram of calcium as stated in the above-mentioned article. In the light of this value let us look at the actual Sr–90 content in human beings from different locations on the earth.

We see that the *average value* which is present in human beings today is only 1/330 of this m. p. c. and will become 1/33 (to 1/16) of the populace m. p. c. by

1970 even if no further nuclear devices are exploded! Now let us look more carefully at the concentration of Sr–90 in some of the 600 specimens analyzed. We find that 13 out of these 600 specimens actually exceed 1 micromicrocurie per gram of calcium *now* (through 1955). Or that they will have 10 to 20 micromicrocuries per gram of calcium by 1970 even though no further tests are conducted! These 13 specimens represent about 2 percent of the specimens which, if applied to the world population, is about 50,000,000 people! It will be noted from Fig. 2 of the *Science* article that these are mostly young people, 20 years or younger.

Of course, if tests are conducted at the same rate as in the past ten years, these 50,000,000 people on the surface of the earth would almost certainly be carrying an amount of Sr–90 equal to or even greater than the populace maximum permissible concentration of 33.3 micromicrocuries/gram of calcium!

Finally, we should note on page 68 of the British Research Council's Report "The Hazards To Man Of Nuclear And Allied Radiations" the following paragraph "In the light of knowledge at present available, we should feel that immediate considerations were required if the concentration (of Sr–90) in human bones showed signs of rising greatly beyond *one-hundredth* of that corresponding to the maximum permissible occupational level."

I believe that this information merits the attention of scientists and public alike.

Sincerely,

STANLEY H. CLARK,
Medical Physicist, Cedars of Lebanon Hospital, Los Angeles, Calif.
NUCLEAR DIVISION, GLENN L. MARTIN CO., BALTIMORE 3, MD., *March 15, 1957.*

10 EDGEVIEW RD., BALTIMORE 4, MARYLAND,
June 10, 1957.

Re hearings on the problem of radioactive fallout from nuclear weapons explosions.

Representative CHET HOLIFIELD,
Chairman, Subcommittee of the Joint Committee on Atomic Energy,
Room F–88, Capital Building, Washington, D. C.

DEAR MR. HOLIFIELD: I would like to submit for inclusion in the Congressional Record my feeling, as a scientist-citizen, on the subject of fallout and radiation injury. I would also like to make a suggestion for resolving the current conflict regarding testing of nuclear devices.

* * * * * * *

Although the medical genetic radiation exposures do exceed the fallout genetic radiation exposures in the United States, these medical exposures are knowingly received under medical supervision. This is not the case with world populace when we consider exposure from radioactive fallout. This, I feel, is the main point—we cannot expect all people to accept biological damage no matter how small or well justified we feel as a nation in exposing ourselves. *If there was general agreement* in the scientific community that there was negligible damage then such exposure would probably be acceptable.

Since the government has clearly stated that the testing of nuclear devices must be continued if our national security is not to be jeopardized, then we should look for solution to this problem which will not appreciably hinder future testing programs. The currently considered solution, that of reducing world nuclear testing to a few megatons per year, obviously does not fulfill the national security need. We must provide a means for testing nuclear devices by other nations as well as ours since world wide development of nuclear devices seems inevitable.

Probably the only solution to this problem is to make tests outside of the earth's atmosphere, i. e., in space. Today, 1957, this is technically feasible. We are now in a position to launch such nuclear devices on missiles that go well above the atmospheric envelope. I will not belittle the test difficulties; the difficulties in obtaining detailed test information; however, I feel certain the problems in this area can be resolved. Certainly the magnitude of an explosion and most of its physical characteristics can be ascertained during detonation in space.

The point I would like to stress technically is that it is completely safe from the radiological hazard standpoint to test weapons in space. (Probably the most serious hazard would be the great light intensity produced during such

tests.) What happens to the radioactive fission products when a nuclear device is exploded, for example, at 100 kilometers? Particles of 2 microns diameter (1 micron equal 1/10,000 centimeter) take a little over three years to fall to the earth's surface from this altitude. Particles of greater diameter take much less time to come to earth (at 8 microns diameter a particle takes about 130 days to reach the earth's surface). However, when the particle diameter approaches 0.2 micron or less, a new phenomena begins to occur. The light from the sun actually will push the particles out of the earth's gravitational field and, in fact, out of our solar system. Thus, for nuclear explosions in space with all particle sizes probably occurring of less than 1 micron, we have a means of cleaning up the fission products; in fact, sweeping them out into space. Surely this is the ultimate in a radiologically safe testing program. It is my hope that this means of resolving the nuclear testing debate will be investigated. If greater costs are necessary for such tests I feel certain that they will be jusified in the light of human well being.

STANLEY H. CLARK.

MEDICAL X-RAY EXPOSURES—NATIONAL VARIATIONS, INTEGRAL DOSES, ETC.

Stanley H. Clark,[1] National Biophysics Conference, Columbus, Ohio, March 4–6, 1957

INTRODUCTION

The development of the uses of atomic energy has focused greater and greater attention on the biological effects, particularly, the effects on man, of all ionizing radiation. The relative furor over the biological effects on man of radioactive fall-out has led to the refocusing of attention on existing and accepted uses of ionizing radiation. Thus, the pressures exerted due to new problems associated with uses of nuclear materials has caused a re-evaluation by a U. N. Committee, by Government bodies, scientific organizations, and individuals, of man's total radiation environment. Most, or all of these studies have indicated that of all the "men created" radiation exposures, that the uses in medicine are currently the most significant. Thus, we find that in the United States National Academy of Science Report, "The Biological Effects of Atomic Radiation" and in the British Medical Research Council Report "The Hazards to Man of Nuclear and Allied Radiations," as well as the U. N. Report on the biological effects of radiation that the medical uses of radiation have been subject to rather critical analysis.

Here is a comparison of the genetic exposure values in the medical uses of radiation as stated in the various national reports.

Values are for the first 30 years of life, and assume exposure at the same rate as at present and in the immediate past.

1. The United States as given in the National Academy of Sciences Report—3 roentgen.
2. The British, as given in the Medical Research Council Report—0.6 roentgen.
3. Sweden—Sivert's Study—0.78 roentgen.
4. Australia—Martin's Study—0.304 roentgen.
5. The U. N. Radiation Studies Committee Report—not yet completed wide range of values for different countries.

Let us now see what factors could account for these differences. (There is no particular order in this listing of factors.)

(*a*) Differences in actual number of diagnostic exposures to the populations of the various nations is one of the most important factors. Since each report sums medical exposures and then divides this lumped sum (in roentgens) by the numbers of people it makes considerable difference how many people in a populace have how many exposures. Related to the number of exposures per populace is no doubt standard of living of that particular nation.

(*b*) We might also ask, what medical uses are included in each nation's report. This calls attention to the fact that only the United States value includes therapeutic uses of X-rays (for non-malignant conditions), and uses of radioisotopes in medicine. These uses account for about 15% of the 3 roentgen value in the National Academy figure.

[1] Cedars of Lebanon Hospital, L. A., Consultant, Medical Division, Oak Ridge Institute of Nuclear Studies, Physicist in Radiology, Medical School, University of Southern California, now with the Glenn L. Martin Company, Nuclear Division, Baltimore 3, Maryland.

(*c*) Difference in physical factors and techniques represents a significant variable. There is considerable more control of physical factors in England and in Sweden due to a combination of factors—socialized medicine; physicists have a closer relationship with radiologists; and early recognition of radiologic physics as a profession. In the United States one finds the private physician performing fluroscopy as well as other medical specialists, dentists, chiropractors, general practitioners, etc., using X-rays with few exceptions without any consultation with a radiologic physicist.

(*d*) Another factor that should be remembered is that of the origin of the various data which were used for averaging.

1. The National Academy averaged from a few United States hospitals and medical groups, however, correlating this data with nationwide uses of X-ray film, etc.
2. The British used hospitals in England and Wales which have hospital physics, groups and extrapolated for the rest of England. "Gonadal doses are based primarily on 1500 patients at one large hospital, (where incidentally particular care is taken to reduce the gonad dose to the minimum)."
3. In Sweden, most of the medical uses of radiation were considered in obtaining the average genetic exposure, no doubt the study was strongly influenced by the Institute of Radiopsysique in Stockholm where procedures are under direct supervision of a physics group.
4. In Martin's report on gonadal exposure due to diagnostic uses of X-rays he uses almost all available literature and the ratios of various diagnostic procedures in one hospital are the basis for his dosage estimates.

(*e*) Additional explanatory notes on factors which contribute to the range of exposure values.

1. The British Medical Research Council figure is really not comparable with the United States value since—to quote from that report—"The value of 22% (0.6 roentgen) should be regarded as a probable lower limit rather than as an estimate. A realistic estimate of the radiation contribution from diagnostic radiology might be considerably greater than this figure". They also discuss the possible factors of 2, 3, and even 10 times this lower limit.
2. In the Australian exposure analysis, J. H. Martin states "that the turn over of patients in the X-ray diagnostic department had already doubled" at the time of his presentation (November 1954), thus his values when brought up to date would be similar to, or greater than, the values for Great Britain and Sweden.
3. S. B. Osborn, whose work forms part of the basis for the British Medical Research Council's estimate discusses in his analysis in Lancet that examinations of the hip and lumbar spine, pyelograms and pelvimetry, although they constitute only 7% of the total number of examinations, none-the-less contribute 75% of the total genetic dosage. Obviously, even minor variations in these procedures from one hospital to another, would cause significant differences in the genetic exposure values. It is interesting to note that Osborne finds that 26.3% of the total populace genetic exposure takes place during radiographic exposure of the fetus in pregnant women.

What incident values mean in terms of whole body effect—the induction of cancer and the shortening of life span.

(*a*) There is need for the integral dose concept when evaluating the medical uses of X-rays. Discussion of radiation exposure of the gonads in man and the ovaries in women require measurements in the vicinity of the reproductive organs and some correction in women for the depth of the ovaries, however in this case we are concerned with the dose at essentially a point and need not be concerned with the energy absorbed in the entire body. However when considering the hazard from diagnostic uses of X-rays which may produce damage in terms of more general tissue damage we need a different concept. The concept most appropriate for evaluating such general body injury is that of the integral dose, or absorbed dose. Specifically, the two important effects related to integral dose are increased incidence of leukemia and shortening of life span. Both are known to be associated with exposure to ionizing radiation.

It is generally agreed that shortening of life span is strongly correlated with the amount of radiation energy abosrbed in the body. This effect then can be most appropriately discussed in terms of the integral dose—a concept originating in England, I believe—it is simply the product of the number of grams or cubic centimeters irradiated, and the number of roentgens to each of the cubic centimeter volumes. Such values are calculated from either X-ray distribution patterns known as isodose patterns, or from an equation developed by Johns. (6)

In general, the first technique is the more accurate. However, for the sake of our calculations, the John's equation has been used to obtain integral doses. When isodose curves were used the area between each isodose line was measured, using a planimeter. This area was then multiplied by the field height and the average roentgen dose to that volume to give the integral dose for that segment. These segment values were then summed to give the total integral dose.

Before comparing particular values of diagnostic integral dose, let us consider how the integral dose changes with change in the energy of the incident X-ray. Assuming that field size and exit dose-rate is kept constant how does the integral dose vary?

The integral dose decreases rapidly with increased X-ray kilovoltage assuming a constant exit dose.

Now turning to the diagnostic usages, we note that the patient exit dose must be essentially constant, regardless of KV, in order to produce useful film darkening or fluoscopic light intensity. This fact when considered in conjunction with the area under the depth dose curve (which is proportional to the integral dose) shows very clearly that in the range from 40 KV to 1 mev that the integral dose decreases substantially with increase KV—thus, we might ask why the diagnostic radiologist does not continue to raise the X-ray KV and thereby reduce the integral dose. There are at least two physical reasons why this is not wholly feasible:

1. The absorption coefficients for different elements in the body, i. e. bone and tissue become so similar at higher kilovoltage that very little contrast is obtained at energies above a few hundred kilovolts.

2. The response of film (and fluorescent screens) decreases with increase KV for a given incident intensity. It becomes evident that there are optional KV values for various parts of the body, depending strongly on what information one is most interested in and the thickness of the particular body cross section. It should be remarked that if too much filtration is used at high energies there will be no reduction of integral dose. There will be a decreased incident dose, but the exit dose must be increased because of the decreased film response at higher KV.

(*b*) Since the increased incidence of cancer, primarily leukemia, is related to the actual bone dosage as well as to the integral dose, it is important to note the depth dose distribution as well in evaluating this particular hazard. It has been pointed out by Hardin Jones and others, that the dosage to long bones, i. e., the dosage to the rib cage, in chest X-rays would be particularly important insofar as the production of leukemia is concerned. It would be well to mention at this point the studies that indicate leukemia is produced by even small amounts of radiation. The best evidence is from the following studies:

1. Studies by Alice Stewart, J. Webb (7, 8), etal in England which indicate an increase in incidence of several malignant diseases including leukemia due to diagnostic X-ray exposures of the pregnant mother, particularly abdomenal exposures. Secondly studies of the survivors of the Hiroshima and Nagasaki nuclear explosives (9, 10), and thirdly exposure of radiologists (11) in the course of their professional activities.

2. Studies which indicate that somewhat larger radiation doses will produce leukemia include a series of infants treated with X-rays for thymus condition (12) and secondly X-ray therapy of anklosing spondylitis (13).

The statistical analysis of this information, as well as the linear relationship between incidence of leukemia and integrated dose is given strong support by Hardin Jones of University of California, in material which will be published in the near future. Shortening of life span as caused by radiation has been borne out by many animal experiments by the shortening of life span of radiologists, and by patients who have been treated with X-rays. Here again, the dose is linear with respect to shortening of life span. According to Jones about 10 days should be subtracted for each roentgen received of whole body radiation. The integral dose ranges from about 11,300 to 56,000 gram-roentgens for 1 roentgen incident dose. This assumes a range of X-ray energies corresponding to 2 mm of aluminum half value layer to 2 mm of copper half value layer.

In the course of using X-rays for treatment of malignant diseases, we encounter total integral dosages ranging from 1 million gram-roentgens to many million (30 or 40) gram-roentgens. This wide range is due primarily to the variation of field sizes that are used for lesions of different dimensions, and to some extent of KV. These large integral doses are actually admin-

istered in daily doses—as many as 30 or 40—in the course of a single treatment. To cite a specific example—using a 5 cm circular field at a focal skin distance of 80 cm with a half value layer of 4.00 mm of copper. The daily dose of 100 roentgen produces an integral dose of about 19,800 gram-roentgens, (remember that one roentgen incident dose produces about 11,300 gram-roentgens). Now let us compare this therapeutic value with some common diagnostic procedures. A film of the lumbar spine taken through the AP direction with the following physical factors—AP thickness 20 cm F. S. D.–71 cm, KV–70, MAS 160. Focal film distance 91.44 cm (36 in.) produces an incident dose per exposure of 4.3 roentgen. This produces an integral dose of 28,810 gram-roentgens. If we take a lateral film of the same person, we will obtain an integral dose of about 150,000 gram-roentgen or the equivalent of some 7½ times the daily integral dose cited for the above cancer therapy! Or to make another comparison, about 3 times the present maximum permissible exposure for one year period! This value would be greatly increased for a heavier person. The explanation for the larger integral dose as compared to the therapeutic dose is almost entirely due to the increase in field size. In the therapeutic example, the field size used was about 20 cm^2 in the diagnostic case it was 1487.5 cm^2 (a standard 14x17 inch film). Of course, the actual X-ray field size as determined by the cone used was circular and somewhat larger even than the area used. The integral dose in chest photofluoraphy would be about 10,000 gram-roentgens.

In fluoroscopy, the literature cites incident dosage rates from 5 to 20 roentgens per minute, depending undoubtedly on the amount of filteration and body thickness. This dosage rate would produce integral dose values from about 30,000 gram-roentgen per minute to about 120,000 gram-roentgen per minute. Frequently fluoroscopies last as much as 5 or 10 minutes which means the absorbed energy approaches that which is actually used for some small field cancer therapy. What does this mean in terms of shortening of life span and increased incidence of cancer? In the case of the shortening of life span we found that a 1 roentgen incident dose (which amounts to about 11,000 gram roentgen would decrease life span statistically by about 10 days. Thus, a single fluoroscopic examination would shorten the life span by as much as 500 days (at a 20 r per minute dosage rate). Or the lateral pelvic radiograph would shorten the average life span by about 140 days! But let us see how these figures compare with some other factors that are known to shorten life span (unpublished, Hardin Jones-University of California).

	Years
25 percent overweight group	3.6
Heart murmur	11
Rapid pulse	3.5
Varicose veins	0.2
Trace of albumin in urine	5.0
Epilepsy	20.0
Skull fracture	2.9

It should be noted that when applied to the total populace millions of man-years of life are lost due to medical radiation exposure (about 7,000,000 man-years each 30 years at the current exposure rate).

With respect to increasing the incidence of cancer particularly myeloid leukemia I would simply like to make a general statement regarding the increased incidence with respect to chest photofluography as a significant example. We find that 1r exposure per year gives a probability of 1 in 100,000 to 1: 1,000,000 of developing leukemia per average individual (7) (14) (15). This probability applied to the 15,000,000 people in the United States who have chest X-rays (in mass chest X-ray surveys) annually would amount to an increased number of cases of myelogenous leukemia; due to these chest X-ray exposures alone 15 to 150 cases per year and each year thereafter. There is in addition speculations that some individuals are genetically more asensitive to the induction of radiation leukemia. Thus a particular sub-population might have a considerably higher probability of the occurrence of radiation induced leukemia.

MISCELLANEOUS-CONCLUSION

Finally let us consider the exposure to X-ray technicians in the course of their training, there are some 40,000 in the United States. They divide up into pairs

and go through the entire radiographic series of exposures normally encountered in diagnostic X-ray work. The genetic exposure for 35 procedures amounts to about 5.6 roentgens for the male technicians and 6.3 roentgens for the female technicians. This exposure corresponds to perhaps 20 to 30 roentgens incident dose (not greater) or about 10 to 15 roentgens of whole body radiation (in terms of integral dose). Thus in this group of technicians due to their X-ray exposure during training we might expect (using a probability of 10^{-6} per roentgen per year) some four cases of radiation induced leukemia per year. The additional exposure in the course of their work is not considered. Even so, since X-ray technicians are generally young people (at the time of training) we would expect this increased incidence of radiation induced leukemia to amount to about 160 additional cases (normally one would expect about 240 cases of both lymphatic and myelogenous leukemia). Hardin Jones states that the leukemia doubling rate is about 30 roentgens full body exposure. Which would give a figure of about 120 cases of radiation induced leukemia. It is hoped that this estimate could be confirmed from actual death statistics of X-ray technicians.

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BETHESDA 14, MD., *July 5, 1957.*

HON. W. STERLING COLE,
Joint Committee on Atomic Energy,
Washington, D. C.

MY DEAR CONGRESSMAN COLE: Through your kind services I beg to submit the enclosed "Critique of Scientific Knowledge" for entry, as the Committee may see fit, in the record of the hearings on The Nature of Radioactive Fallout and Its Effects on Man, held in May-June.

The Kremlin seems to have shifted its position, softened its opposition to "western" genetics which they have ardently rejected—until it came handy in their cold war in Japan against *our* bomb tests.

At the suggestion of the Committee Technical Advisor I am asking the following geneticists with whom I am acquainted to comment, to the committee on copies sent them.

Dr. James F. Crow, National Institute of Genetics, Mishima, Japan.
Dr. Carl C. Lindegren, Southern Illinois University, Carbondale, Ill.
Dr. Stanley H. Emerson, Atomic Energy Commission, Washington, D. C.

Very truly yours,

ALDEN POTTER.

A CRITIQUE OF SCIENTIFIC KNOWLEDGE

Keep that which is committed to thy trust, avoiding profane and vain babblings, and oppositions of science falsely so called (I Timothy 6.20).

The evidence adduced in these hearings on the effect of radioactive fallout on human and other life has shown that even a considerable enlargement of test explosions will not increase the incidence of radioactivity materially beyond what all forms of life have experienced from time immemorial.

A prime consideration is the growing belief that most of the radioactivity that is injurious can be eliminated from military weapons by tests now being planned and conducted, so that possible danger to future generations in case of war will be correspondingly reduced. The larger, fusion bombs are less radioactive in proportion to the success that may be attained in eliminating fission, as in "triggering" the explosion. This may explain the Kremlin's current readiness to suspend tests so they can catch up with our "classified" progress toward clean bombs, while shedding crocodile tears over the genetic horror which they have subtly helped "western" geneticists implant at the Japanese National Institute of Genetics at Mishima.

The attempt to make medical science "objective" has turned on statistical theory and practice which is leading to a great deal of confusion as to the applicability of degrees of probability thus determined. This source of misinformation is by no means confined to research in radioactive fallout as presented in these hearings, but extends over the whole field of science. It will therefore be necessary to attempt some clarification of these basic disputes as they affect the argument over whether observed effects may be linearly extrapolated (whether the extent of biological injury by radioactivity is directly proportional to the extent of such activity, however small), as against the existence of a "threshold" or range of radiation that is either beneficial or at worst not injurious to any life. This critique will contend that thresholds are the rule, not the exception, in nature. Moreover, insofar as a threshold is biologically determined, we shall argue that it cannot be a fixed factor in the survival of any species because of adaptive changes in the organisms involved.

Senator Anderson has cited such an adaptation in the case of the resistance of insects (flies) to DDT; and there are many other such cases in recent experience with insecticides. This factor of adaptive change has been so widespread in medical research that in 1954 the University of Pennsylvania's medical school cooperated with Naval Research in organizing a symposium on drug resistance at the Statler Hotel in Washington; and thereby hangs a tale. The honor guest and speaker at dinner was Dr. Cecil P. Martin of McGill University in Montreal, Canada. He was called upon to speak because of an essay, "A Non-Geneticist Looks at Evolution," published in the American Scientist shortly before, in which he assailed "Western" genetics on the ground that it has produced an untenable theory of evolution, that is, the mutation-selection theory.

This view has gathered some support even among geneticists; witness the recent note (Science, May 10, 1957) of Lindegren and Braun criticizing the views of Dr. George Beadle of the California Institute of Technology as given in his presidential address to the American Association for the Advancement of Science, for his too confident proposal to call nucleic acid a "living" molecule because it is a "carrier" of genes in heredity. The essence of this question lies in the way the species pattern is carried. Is it by a template—a minute replica as Dr. Glass contends—or is the system like our cultural information system, linguistic and therefore *meta*physically informational like an engineer's handbook?

It should be unnecessary to record the views of Dr. Martin here for they have been available in a treatise published last year by the eminent medical publishing house of C. C. Thomas of Springfield, Illinois, entitled "Psychology, Evolution, and Sex." But the evidence adduced in these hearings by the panel of geneticists from the National Academy of Science has studiously omitted any reference whatever to these conflicting opinions, quite as they have been omitted from any and all proceedings and reports of the Academy and, as to the Martin treatise, also from the pages of Science where the book has not been reviewed after almost a year since its publication.

It will be necessary, therefore, to try to clarify the relation between the Darwinian views of Dr. Martin and some few geneticists whom our Academy has deliberately ignored, and those set forth by the Soviet scientists at a recent conference in Japan on genetics where they spoke in the terms of Mendelian theory for the first time since the days of Trofim Lysenko. Is it indeed a mere coincidence that the mechanistic materialism of our Academy with its pretense of

"objectivity" now serves in furthering the curtailment of bomb testing just as the Russian cold war is so intensifying its peace offensive as to produce riots before the American embassy in Tokyo? Are we not ourselves so supersaturated with the prevalent anti-Darwinian philosophy that we refuse to examine such a scholarly treatise as that of Cecil Martin which counters the false alternatives of an alleged conflict between military and genetic security? Only the mutation-selection doctrine with its elmination of competitive stratagems in organic evolution can serve the communistic pacifism of Soviet propaganda; and it is with a sudden acceptance of this idea that they carry the war into Tokyo!

A clarification of these issues on an honestly open-minded, objective basis entails a thorough reconsideration of the basic tenets of *scientific empiricism* which define "the scientific method" ("Scientism" and "positivism" are other terms for this "unity of science" philosophy) as purely inductive. Such a reconsideration is supported by the rising tide of literature indicating the imminence of a conceptual revolution in science. This deductive revolt is renewing the conflict between science and the anthropocentric bigotry which gave rise to the Scopes trial in Tennessee instigated by William Jennings Bryan. This court case sought to ban the teaching of a theory of evolution that does not admit the uniqueness of man which is today propounded by the chemical theory of genetics and the origin of life because, very obviously, *human affairs are certainly not chemically explicable.* Man does not live by bread alone; *but neither does any other form of life.*

The bigotry of this anthropomorphic theology has unmistakably injected the pontifical edict that an "emergent" evolutionary theory may be anthropomorphic upward *to* divinity, but not upward *from* monkeys, in its "extrapolations." Our Fundamentalists thus create a self-idolatry—a God in the image of man; an authoritarian threshold that may not be crossed even by the authorities. This prideful conceit that man is peculiarly unique not only has no support in the Christian Gospels but it seems to beget a peculiar predilection for statistical extrapolation, while banning biological analogy, as scientific evidence.

Mice are not men; so science must perforce extrapolate averages (statistical data) as a "first approximation" or "educated guess," the while indulging in crash projects, not to test empiricism against an alternative postulate, but to "prove" that extrapolations (predictions) are correct and so justify, by pragmatic "proof", the public policy already adopted in "playing safe" with probabilities. This is called "verifying the hypothesis" with "observational evidence." Its successes are proclaimed from the housetops; its failures suppressed like a Nobel Prize gone sour.

In assailing this materialistic biology it would be well to cite further evidence, from outside these hearings, that it does exist and is, indeed, censoriously entrenched in scientific literature and training for "academic freedom" in research. The popularity among biological empirics of a small book written by Prof. Erwin Schrödinger, author of the principal equation of the so-called "quantum mechanics" of atomic physics, "What Is Life?", with its attempt to forecast a physical answer, is attested not only by numerous reprints (including a paperbound, popular edition) but also by the widespread opinion, urged most recently by Linus Pauling in a lecture at the National Institutes of Health in Bethesda, that the genesis and genetics of life can and ultimately must be defined in purely chemical terms. Any other outcome would, *ex cathedra,* be a betrayal of progress in "science."

Since these hearings were recessed early in June, an article has appeared in Science (June 7) based on another lecture at CIT (alphabetics for California Institute of Technology) by an Iowa biophysicist (Robert Sinsheimer) on "First Steps Toward a Genetic Chemistry." Here "steps" in progress *toward* the end *sought*—toward what "we *shall* discover" chemically—are recounted as if time were reversed by some extrasensory perception. The facts are "not a sufficient proof * * * but in a way it is *satisfactory* to believe that the physical basis of the gene—the factor that is passed on from cell generation to cell generation—is physically conserved * * *." (Just as it was satisfactory to believe that "parity" was "conserved" in quantum mechanics—until it was shown, not first by observation, but by straight thinking, that it was untenable however satisfying!)

To our biophysicist certain chemical correlations have provided "a *pleasing* confirmation of our hypothesis," even though inconsistent with other facts. To get rid of these embarrassing obstacles to *pleasure* "more refined techniques could prove to be of *great value*" in the "*hope* that the development of a genetic

chemistry will *help* * * *." Why, indeed, are "elegant tracer studies" credited with a "*pleasing* result"? Why do "we know *painfully* little"?

Obviously because "we" are extrapolating our pullets—counting them before they are hatched. Come what may, "we" are not going to hatch any cockerels as long as any refining of techniques (or elaboration of hypotheses) remains to renew the hope that springs eternal in the chemical breast that we shall soon know how to sex them, not by the Japanese manual methods after hatching, but chemically, before the eggs are laid. Some day there will be an RNA for sex, chemically the same for all life from virus to vireo to mastadon.

Here, indeed, is an endless frontier for scientists; a fiscal rathole down which appropriations and empirical data can be poured endlessely before reaching any conclusions; such as foreign-aid policy for Japan to provide a chemically immaculate conception to displace the recently legalized abortions that are keeping Japan (and China, too) from being any further "under-developed"; that is, overpopulated. Why, indeed, worry about poverty if all we need do in the matter of a trait called parental care is to take the long last step in genetic chemistry, to wit, find the proper, parental test-tube in the gene-bank kept under lead to exclude radioactive fallout? We are definitely assured that there *is* a chemistry of the gene (not yet correlated with DNA chemistry, but soon to be) which, *when* "we have become sufficiently advanced in our understanding" of the raveled sleave of mitosis, *will* enable us to "demonstrate the action of a gene *in vitro*"—so we can live *in vitro* for nine months and then have plenty of fresh milk.

To force a reconsideration of the empirical "unity of science" is more of a task than can be encompassed by this brief critique. Such a reconsideration might, however, be invited by a rather categorical review of the high spots in this concaptual revolution as dimly perceived despite the din of data in the process of erecting our scientific Tower of Babel. Conceivably we might thus stir the well-known, open-minded curiosity of scientists in their zealous search for ways to "ring out the old; ring in the new; ring out the false, ring in the true." For not one but many minds are needed in the process of dismissing all the facets of fallacy involved in a postivistic "idealism" which obscures reality by making it synonymous with actuality so that "objective" means a consensus of *subjective* judgments as "verified by experience," pragmatically.

Life on Earth, as a whole, has no opportunity for the unlimited expansion in which it seeks to indulge. For this reason what men, and all other organisms, have most to fear is not the physical environment and its accidents, but far more what other biological occupants ("fellow creatures") do to that environment in the struggle to survive. What any and all biological coalitions do is not purely physical or fortuitous. It is expediently adaptive in its strategic choices so that all free-will, animal life requires eternal vigilance to choose aright and avoid extinction at the hands of other predators who are also exploiting other organisms as "renewable resources," sometimes symbiotically.

Among men, in a civilized context, Disraeli put the case for vigilance against misinformation in a retort to "factual" argument by the Loyal Opposition in parliamentary debate: "There are three kinds of lies; white lies, damned lies, and statistics." Since then there have been frequent protests against empirical "objectivity" among "experts" who employ "factual" correlations to verify hypotheses. These have pursued the pattern, reflecting Christ's warnings against false prophets set forth by St. Paul to Timothy telling him to avoid "profane and vain babblings, and oppositions of science falsely so called."

There have been side-splitting satires, like Chick Sales' "The Specialist," and, less biting but more specific, Anthony Standen's "Science Is a Sacred Cow" featured by the author's review, with lampooning cartoons, in LIFE seven years ago. Lately a physical chemist turned social philosopher, Prof. Michael Polanyi of Manchester, England, has severely indicated *scientific empiricism* in America in a paper read before a symposium on "Fundamental Concepts and Units of Science," published in Science this past winter.

Most recent and satirical of all is a contribution from Cornell University by zoology professor LaMont Cole, "Biological Clock in the Unicorn" (Science, May 3, 1957). This paper so aptly caricatures statistical correlation in verifying an hypothesis that it is astonishing to find, following it by only two weeks in the pages of Science, the same "quantitative information about the effect of radiation on human health" as that presented before this committee by CIT's Prof. Edward Lewis on "Leukemia and Ionizing Radiation" which Dr. Shields Warren rejected as inadequate evidence of causation in leukemia, supported editorially as follows (DuShane, Science May 17, 1957, p. 963, "Loaded Dice"):

"E. B. Lewis shows that there is a direct linear relation between the dose of radiation and the occurrence of leukemia * * * The meaning of such findings is that any amount of radiation takes its toll of the population and any increase takes a greater toll * * * We are approaching the point at which it will be possible to make the phrase 'calculated risk' for radiation mean something a good deal more precise than the 'best guess.' It is apparent that the atomic dice are loaded. The percentages are against us and we ought not play (with bomb tests) unless we must to assure other victories."

The movement toward clean bombs for the West which was revealed in these hearings and later publicized by the President (and caustically condemned by the Kremlin!) has taken the wind out of the Schweitzer sails (and the Pauling Petition) as related to leukemia and strontium 90 so that what remains of the loading of atomic dice by statistical correlation is now confined to genetic effects as alleged by the panel from the National Academy of Science before the Committee on June 4.

If it is not true that (DuShane) "much can be learned about biological reactions (in man) by appropriate statistical and epidemiological studies," such as that of leukemia conducted by a geneticist and statistician (Lewis) who has the "quantitative information" but not the facts about the disease (the recent rise in the statistical incidence of leukemia has been due, not to improved methods of diagnosis, much less to any increase in casual factors, but to the use of penicillin and other "wonder drugs" to save the patients from infections to which bad blood makes them very susceptible), it is equally fallacious to conclude that a correlation between chromosomes and the statistical results of sexual crossing reveals the true character of survival values in organic evolution.

By the same token it is not competent to conclude from a "linear relation," as did Professor Crow before the Committee, that "death, disease, and misery" produce "natural selection" without considering the occasion for such phenomena; for example, war, famine, and pestilence among men, which would be extremely difficult to correlate with any genes without even greater stretching of hypotheses than has as yet been employed in genetics. ("To multiply auxiliary hypotheses is to goropise." See "The Principle of Simplicity" by Lewis Feuer, Philosophy of Science, April, 1957.)

It is not because fruit flies are not men that the monstrous effects of laboratory irradiation do not prove the thesis that the human race will suffer from bomb tests for centuries to come. It is because mutations are not adaptations that this conclusion is unwarranted, either for men or for fruit flies. Gene injuries are as possible as any other anatomical damage from irradiation. But that they persist as recessives throughout a population (without benefit of the dysgenic effects of medical science among men), is not proved by selected statistical averages that destroy the facts.

Mutations, when they do occur, are baneful, indeed; for they destroy the hereditary homeostasis of the species in much the same sense that a burning of books can destroy a cultural heritage. They are *devil*-utionary, not evolutionary. Unlike adaptations to meet competitive pressures they do not remodel the species for survival and become, as Martin contends, "lingering modifications" which do evolve new genes in the course of unobservable time—time which is measured, not in terms of sidereal or sexual events (years or generations) but in terms of marginal elements in interacting stratagems that cannot possibly be reduced to "quantitative information."

Adaptive changes do not occur in detectable form for easy correlation; but they are not therefore mere accidentally adapted mutations. They are teleological expedients, intentionally produced, in the unobservable realm of the microcosm. This is, indeed, an unverifiable hypothesis. But it is not a "goropised" set of hypotheses devised to verify each other in an endless tautology; nor is it untrue because it is unamenable to empirical confirmation by "the scientific method," as was the case with a similar hypothesis in quantum mechanics, to wit, the conservation of parity, which was found to be *predictably* and *testably* false because it pertained to purely physical, inorganic structure. Not observation, but theory, made this discovery!

In biology theory must perforce pursue analogy—the judging of all living processes by our own, in aspects which every man-on-the-street knows and which require no expertise explanations; and these processes are not predictable in their course. They are expedient; just for today. While they cannot violate the natural laws of the physical order, they are not governed by them. They

are "cybernetically" governed, by negative feedbacks that involve a correction in the current theory of information—a corrective which postulates the existence of a metaphysical factor, that is, a dialectical system of commication analogous to human communication in everyday affairs wherein *conditioned* reflexes are a matter of the most familiar processes of modern education. As Whitehead has pointed out, the more civilized we are, the more automatized is our behavior. (Cf. Hayek, "The Use of Knowledge in Society," Am. Econ. Review, September, 1945.)

Concurring with the Martin thesis, Lindegren and Braun (Science, May 10, 1957) cite literature which holds that gene mutation *per se* does not afford a satisfactory explanation of evolution; also that the gene, like sex, is not primitive; nor is its alleged stability a fact.

Pertinent to the fallacy of genetics as a physical science is my own senior thesis dated 1909 (Minnesota) on "The Cytology of Weismannism." This paper not only rejected the mutation-selection theory in almost the same terms as those of Dr. Martin's treatise of 1956, but in a full review of the evidence in the literature of genetic cytology, nucleic acid was twice mentioned as playing some part in the processes of heredity. Though this theme of the early twentieth century is "dated" in some particulars, there is no more reason now than there was then to believe, with Sinsheimer, that "this hard-won recognition of the role of DNA (nucleic acid) has brought us into a new era in genetics and biochemistry. The gene, once a formal abstraction," he concludes, "has begun to condense, to assume form and structure and defined reactivity."

That this is wishful thinking; that there is nothing new in the evidence at hand today which can change the conviction that a chemical explanation of life can never be adduced, can be seen by comparing the two papers. So that interested readers may do so, "The Cytology of Weismannism" is here submitted for its first printing.

To predict by extrapolation from statistical correlations is the unforgivable sin of biological empiricism. There are some contexts in medical practice having legalistic implications, where statistically "educated guesses" as to "calculated risks" form the basis of "expert" opinion used, for example, in judging degrees of disability and causes thereof, as in insurance payments, suits for damages, social "security", veterans compensation, and the like. But in a social context where the results of taking chances with probability are compounded by a multiplier or even exponentially, as in public health policies or in the anarchy of foreign relations where victory or defeat in global warfare is the risk involved, its success, if any, is at best illusory and its failure catastrophic. Biologicaly and culturally Operations Research is worse than futile; it is fraudulent. In medical practice or research epidemiological figures can at best furnish only clues, never evidence that confirms any hypothesis. If the patient is going to die anyway—a fix in which everyone finds himself sooner or later—an experiment may be in order. But when the fate of a people is in the balance and survival is at stake, in disarmament schemes for instance, or when the health of millions of children is concerned with the standardizing of a vaccine, then a trust in statistical probability as against a laissez faire policy, can be disastrous.

Such an error has just been courageously exposed, even in Science (May 31, 1957), namely, the fiction of the safety of the Salk polio vaccine; nor is this the first Nobel Prize in medicine that has not been rescinded in the light of the pitiless truth as to its falsity. In this exposure of the untruths broadcast by the National Foundation for Infantile Paralysis, falsehoods which are plainly proclaimed, there is a serious omission in failing to state the underlying occasion for them, to wit, the idea that the degree of probability can be scientifically measured in biology as in physics and make (to quote the DuShane editorial on dice again) "the phrase 'calculated risk' * * * mean something a good deal more precise than the 'best guess'," and afford a reliable basis for a public policy based on "appropriate statistical and epidemiological studies."

That hundreds of children were not paralyzed and crippled, or even killed, was the work, not of medical science, but of nature; for had the same children been vaccinated with unattenuated virus very few clinical cases would have developed. The probability that infection will cause the disease is so low that its incidence cannot be taken as a measure of the spread of the virus. Poliomyelitis is a relatively rare disease; so also is leukemia. Vaccines for all such afflictions—and they are multifarious and but dimly classified—would burden the community beyond endurance if promoted as a public charity. The treatment of hog chlorea by a similarly attenuated, living vaccine has admittedly kept the disease active instead of exterminating it; so at long last the live

vaccine is being eliminated by law. It has been unjustified also in poliomyelitis, despite the full responsibility of the National Institutes of Health for promoting its use.

The logical fallacy of statistical biology with its reliance on correlations can be shown by an "anthropomorphic" analogy such as was banned from Tennessee public schools by law and, right now, is "scientifically" tabooed by the "positivistic" ban on "anthropomorphic extrapolations."

The cultural DNA of modern civilzation is plainly *money*. That it is coined from gold and other "precious" metals is as irrelevant to its value as an informational guide for human exchanges and consequent behavior, as is the forming of genes (genetic prices) from nucleotides. It is quite as absurd to believe that the substance of DNA must differ in its physical structure in order to create a man instead of a monkey, as it ever was to believe that an ovum is simply a small edition of an embryo and has only to grow to produce an adult man or monkey.

The assertion of Dr. Glass before the Committee that something like a template (a "mould" or "replica") is involved in the embryonic developmental process, is just this sort of untenable belief for which there is no evidence at all. At fault is, basically, the treatment of "information" as a matter of signals as distinguished from signs or symbols with a metaphysically conditioned system of meaning. It is not at all necessary for the same word (or figure) to be used to convey the same meaning (or value); nor different words to appear when different meanings are conveyed. Ambiguity is rampant in language—as are paradoxes in mathematics. An assumption that there is a similar dialectic in genetics constitutes the only possible alternative for the empirical ambiguities which must be eliminated from science by sound reasoning from realistic principles.

The composition of chromosones, genes, alleles, genomes, nucleotides—or what-have-you in genetic equipment—can never determine what they signify in the organism's behavior and development, just as monetary units of various denominations cannot determine the real values underlying the judgments and consequent behavior of a business community as guided by intelligence (price relations); the values are the same regardless of the prices which the number of dollars or pounds determines. *Changing* prices do distort the normal course of judgment and action just as ambiguity distorts the influence of speech or literature in behavior. The fact that it is an artifice, not a reality, that creates the uncertainty of meaning and of biological behavior in general through *conditioned* reflexes (learned symbols) is the essence of the problem of living together by communicated information. There are no puns or paradoxes in nature's realities.

If a Man from Mars were to observe the number of pounds in a British pocket, and the number of dollars in a Canadian pocket, he would have no way of knowing that they differ in their value even though they look alike. So if he were to use Cartesian coordinates in two dimensions to correlate these numbers with the energy level of observed events (behavior) related to those numbers, would he be warranted in "discovering" that this graphic relation is a natural constant? What should he conclude if the British set up another Newtonian Recoinage Commission, or if an International Monetary Fund changed its "mind" on the proper exchange rate, so that the energy level of events departed from the previously observed, linear correlation? Should he call it a "spontaneous" mutation? Would not some Martian jester turn up to publish a report in a Saturnian Science on the rhythmic clock of *Unicornus martius*?

Of Dr. Bentley Glass discovers (as he has) that the correlation between the traits of *Homo insipiens* and the number of chromosomes is not 48 after all, shall he conclude that all the observers who counted 48 were cross-eyed, or that their staining techniques were defective? Or should he conclude, with an ancient inscription in the Libyan Desert, that "Life is change; to cease to change is to cease to live"? If the nucleotides-to-be-counted with electronic relays when genetic chemistry gets a big enough appropriation to buy a big enough computer from IBM, turn out, as all such research to date has done, only some more negative results in the attempt to correlate their chemistry with their behavior in heredity, will it at long last become reasonable to agree with Dr. Lindegren that genes are not stable after all? Will it finally be admitted that his charge that the evidence of such stability has been a matter of choosing only data that support such an assumption, is warranted?

The loophole in the "laws" of Mendelian inheritance as presented by the geneticists before this Committee, lies in their manipulation of "spontaneous"

cases of genetic change to avoid admitting that any of them are adaptive. Adaptation has been ruled out of the vocabulary of organic evolution so that life is today supposed to be one grand symbiotic brotherhood seeking a communistic goal in selfless abandon. Perish the thought of conflict of purposes acting incompatibly in a tooth-and-claw struggle, or of any innovation in survival that has been other than the result of rare accidents in throwing genetic dice that are *not* loaded! Those big claws on Alaskan king crabs turned pink by our loving kindness, are purely ornamental, love-patting appendages, swords beaten into plow shares like atoms-for-peace in Utopia.

The dispute in these hearings over the interpretation of data has hinged on the idea of a threshold as opposed to linear extrapolation, rather than on the evidence that has here been adduced as to the absence of biological constancy where genetic science has postulated it. What remains to be shown, therefore, is that nature consists of thresholds, not of linear correlations, and that research consists of discovering the status of thresholds, never in tracing imaginary linear relations. This would be obvious but for the linear predilections of the Euclidean influence in science for even in the inorganic realm of physics the field of macrocosmic relations is not universal; witness phase transformations such as melting points and boiling points and their energy relations. A jet plane passes from one range of speed into another with an explosive sound as it crosses the "barrier" into a very different set of relations. Physical constants established by instrumental measurements (as by the National Bureau of Standards), such as those of Hooke's Law on the strength of materials, which are inapplicable to very small sizes such as exist in one of the most recent developments in solid state physics (metallic "whiskers"), are another example.

In biology the operation of sensory perception, from which all knowledge proceeds, is replete with thresholds. The *source* of knowledge is the same for any and all of the creatures that inhabit the earth; it lies in a myriad of wave forms, or bands, filling all possible environments throughout the universe. But there are great areas of this spectral information that are extrasensory for any mammal. Some bands are accessible to insects but not to vertibrates; and some mammals such as rats, bats, cats, dogs, can sense ranges greater or less than other species, man included. Some men can sense a range wider than others; but none can be devoid of all sensation and still live.

Other sensations can be translated into tactual perception, as by Braille, to make individuals "literate", that is, to train their reflexes for communication purposes in guiding behavior. In this aspect of a cultural heritage as it operates genetically—and genocidally—in the human struggle to survive, telecommunication preforms transformations in and out of sensory ranges, with continual improvements for technically conditioned people.

Thus science as we know it is continually extending the range of sensation and communication to new bands and ranges; but these new, extrasensory signals have to be modulated into the normal sensory ranges or thresholds before they can affect behavior. No possible information can be had without reference to these physical wave bands as they have been affected by discontinuities (things) in the pyhsical universe. It is because the laws governing these spectral conditions never change anywhere in eternity, so that the sequence of events they reveal is absolute, that time cannot be reversed or events known that have not yet occurred. (Cf. Anthony Standen on "Causes and Effects" in Science, May 3, 1957, p. 900.) Thus no possible living creature can have any information available for conditioning its reflexes (guiding its behavior) that does not rest on these ordered discontinuities (waves and particles), in the physical environment, that are the source of all certainty in sensory experience.

But if sensory equipment has its thresholds, living conditions are also narrowly restricted to those prevailing in the so-called biosphere. Some heat is essential; much more or less, is lethal. It's a case of not crossing a *threshold* and getting *too much* of the good things of life; including life itself. To avoid too much or too little, animals have evolved sensory equipment which men are still evolving by mechanical instrumentation which facilitates motility and thus also serves predatory purposes in acquiring food. Then there are the "trace" elements, essential to plant growth, an excess of which is toxic. In short, there is no such thing as a "linear" relation between life and the elemental forms of matter and energy. A threshold is the very essence of life.

Linear projections or extrapolations are a semantic fiction originating in the Euclidean concepts that arose in the days when the earth was called flat and everything in geometry was worked out in terms of rectilinear and rectangular frames of reference, a state of affairs that still plagues the problems of solid state physics. Molecular engineering, such as is characteristic of this atomic age,

cannot be conducted in these all too familiar terms. The microcosm, in short, is not at all a replica of the macrocosm, as it was held to be by no less a scientist than Thomas Huxley, father of the equally mistaken geneticist, Julian Huxley. This false analogy is being gradually abandoned though the layman is still deluded by the clutter of orbits in pictures of a now discarded concept of the behavior of electrons in an atom. "Shells" (energy "levels") have become the "truth"; and just now there seems a prospect that "spin" will also be displaced, perhaps by a helical structure. The whole theory of atomic and molecular models seems to be in a highly fluid state of uncertainty that is semantic rather than realistic in character. The meaning of symbols, such as the linear representations of Euclidean geometry, is at stake.

The truth about mathematical semantics seems to be that the so-called "natural" numbers, said to be an infinite continuum, are actually *un*natural in being a progression from an arbitrary origin (zero) in two "linear" directions. When they represent objective measurements of physical realities (rather than "value judgments" which can never be referred to any standard, subjective unit to give commutative character to their meaning) they are dimensional and, however remotely derived by instrumentation, they do form an integrated whole or system through reference to the ultimate unit, an arbitrary standard of length, to wit, the yardstick at the National Bureau of Standards. The resulting figures, called measurements, are always, whether "first" or last, approximations and never fully commensurable.

Truly *natural* numbers are dimension*less*; they have no quantitative meaning. Their field seem to be derived from the symmetries of microcosmic, spherical packing which emerge into specific forms in the periodic table of elements and in crystalline structures. Their order, if any, is timeless and independent of any comparison in size and they omit all but the smallest prime numbers since they are exponential and are not amenable to decimal treatment. No zero, no signs (plus or minus), no incommensurables, approximations or probabilities, no statistical averages, are involved in these symmetrical realities that never signify values either measured or subjectively appraised. Incidentally, it is not true that ratios are independent of dimensional numbers.

Assuredly, these venturesome generalizations need to be as critically reviewed as do the accepted conventionalities of mathematics. They are entered here heuristically, to suggest a clear distinction between certainty and uncertainty, a definitive difference between the possible and the impossible, in order to realize that the element of uncertainty is injected as soon as the information existing in the microcosm is "perceived," that is, *after* the optical structure of the eye, for example, has detected the signals and started, not merely to amplify, but to "comprehend" or classify them relative to the purposive procedures implicit in the neural system of any animal, such as a man or an insect. From then on there is a degree of uncertainty paralleling the logical doubt as to the truth of such semantic signs as are used in communication, even by honeybees.

There is nothing more irrational than the bland assumption that what is not yet known to exist must be considered not to exist, scientifically speaking. As Professor Ballard of Tulane University expresses this in the July, 1955, issue of Philosophy of Science, there is every reason to suspect that a dialectic comparable to human (and apian) language is "also carried on within an individual between distinguishable parts of his organism," a phenomenon which obviously presents "metaphysical" aspects and therefore "problems which mechanics cannot solve." Explanations which are a matter of inference by analogy, and not a matter of observation psychologically, thus lose their aura of mysticism and become scientifically anti-empirical. Rational strategy often rests, not on experimental verification of a suspicion (hypothesis) but on an "anthropomorphic" inference or belief that *all* behavior derives from communicated intelligence; subconscious action (instinctive) is not altogether mechanistic or free from awareness, not even in hereditary phenomena.

So meaning in communication is metaphysically incorporated and only approximately true at "best", while at "worst" (these antitheses are reversible in their "value" judgment, depending on whose ox is being gored) it is deliberately deceptive in its strategy, even though only by the "humor" of a pun. The living, purposeful organism has to learn not to be naive but to correct such illusions as that of the asymptotic approach in the perspective of distance or that of change in the pitch of a sound as its source moves past to create a "Doppler effect."

Dr. Schweitzer to the contrary notwithstanding, John Gunther's medical treatise on cancer and death is grounded on falsehood; for it is Life, not Death,

that needs to "Be Not Proud!" Immortality is impossible. Racial survival is possible, but not certain. Indeed, it too is impossible if all life—all mankind—is to be the objective. A Communistic Utopia is entirely outside the pale of any Creation except that of a demagogic imagination. Humility toward our competitors (our "fellow men") can only be hypocritical. Only the Creator (if any; we can never know), or better the actual order of the universe (which is not beneficent but is the very paradigm of neutrality) can be an object of respect and faith on the part of any moral culture seeking a political order not grounded on the personal discretion of leadership (der Feuhrerprinzip) in determining its blindfolded justice.

Occasionally the life of the individual can and must be treated with the utter *sang froid* of statistical probability and war, for it is always subordinate to the higher order that can be immortal. We can be legally exempt from taxes, but not from death. The termination of the life cycle can only be postponed, even by "Atoms in Our Future," the pleasant author of this pleasant prophesy, Senator Anderson, to the contrary notwithstanding. Men may be blessed with "travel out among the stars" and "pushbutton weather" (which is not altogether lacking even in New Mexico without benefit of atomic energy). But any promise of "even eternal life" is out of the reach of either man or God or, even, of the Positivism of such "science" as that which Sir George Thompson has set forth *ex cathedra* in his "The Foreseeable Future."

In her youth Lily Pons popularized "I Dream Too Much!" Perhaps her vibrant voice could yet teach it to Sir George and the prophetic Senator and persuade them and their sycophants in science to think in terms of thresholds rather than extrapolations.

Appendix 8

Radioactive Fallout

A Short Selected List of References

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Appendix 9

Radioactive Fallout

Selected List of References

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Examines the role of the soil in limiting the spread of contamination.

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Traces the likely fate of radioactive contamination from an atomic bomb explosion or a nuclear reactor accident according to the nature of the ground contaminated, and discusses possible treatments of contaminated land.

Anderson, E. C., and others. Radioactivity of people and foods. Sciences, v. 125, June 28, 1957: 1273–1278.

Andrews, Howard L. Radioactive fallout from bomb clouds. Science, v. 122, September 9, 1955: 453–456.

General discussion of radioactive fallout in the vicinity of nuclear detonations, including mechanics of formation, amount of activity, etc. Discusses maximum permissible levels of gamma radiation for test series, estimates dosages near test site under certain assumptions. Discusses beta burn hazard and danger from retention of particles in lungs. Concludes only particles from 0.5 to 5 micron diameter are potentially hazardous to lungs, and fraction in this size range will be small. Also discusses long-lived fallout and potential hazard from Sr–90 and genetic effects. Concludes weapon testing program justified by defense effort and that radiological hazards have been minimized under the well-controlled conditions of weapon testing.

Answer sought to fallout hazards. Chemical and engineering news, v. 33, April 25, 1955: 1774.

Baker, W. K., and Von Halle, E. Production of dominant lethals in drosophila by fast neutrons from cyclotron irradiation and nuclear detonations. Science, v. 119, January 1, 1954: 46–49.

Bell, Carlos G., and others. Passage of nuclear detonation debris through water treatment plants. Journal of the American Water Works Association, v. 46, October 1954: 973–986.

After nuclear detonations, 2,200 samples taken from 3 water plants, were measured for beta-gamma radioactivity; within 2 weeks after detonation 45

percent of fallout radioactivity passed through rapid sand filtration plants; from 2 to 10 weeks after detonation, percentage increased to 53 percent, and 10 weeks after detonation all radioactivity passed through plant.

Blifford, Irving H. Collection of atomic bomb debris from the atmosphere by impaction on screens. Science, v. 123, 1956: 1120–1121.

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Campbell, Charles. Radiostrontium fallout from continuing nuclear tests. Science, v. 124, November 2, 1956: 894–895.

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Caster, W. O. Strontium-90 hazard: relationship between maximum permissible concentration and population mean. Science, v. 125, June 28, 1957: 1291.

Chow, Tsaihwa J., and Thompson, Thomas G. Flame photometric determination of strontium in sea water. Analytical chemistry, v. 27, January 1955:18–21.

Clark, H. M. The occurrence of an unusually high-level radioactive rainout in the area of Troy, N. Y. Science, v. 119, May 7, 1954: 619–622.

Thirty-six hours after detonation of a nuclear bomb at the Nevada Proving Ground, an unusually violent electrical storm hit Troy, N. Y., 2,300 miles distant. The storm left in its wake an exceptionally high, though not hazardous, deposition of radioactive material.

Clark, Stanley H. Genetic radiation exposures in the field of medicine. Bulletin of the atomic scientists, v. 12, January 1956: 14–18.

Cohn, S. H., and others. Nature and extent of internal radioactive contamination of human beings exposed to fallout material in Operation Castle. Radiation research, v. 3, October 1955. Presented before the Radiation Research Society, New York, May 16–18, 1955.

The first instance of exposure of human beings to significant internal contamination with fission products occurred as a result of the ingestion and inhalation of fallout material from a nuclear detonation in the spring of 1954. An evaluation of the nature and extent of these internal radioelements excreted by the exposed human beings with data obtained from radiochemical analysis of the tissues and excreta of animals contaminated in the same event.

Cole, L. C. Biological clock in the unicorn. Science, v. 125, May 3, 1957: 874–876.

Comar, C. L., and others. Thyroid radioactivity after nuclear weapons tests. Science, v. 126, July 5, 1957: 16–18.

Conger, Alan D. The relative biological effectiveness of radiation from a nuclear detonation on tradescantia chromosomes. Science, v. 119, January 1, 1954: 36–42.

Tests at Oak Ridge on the flowering plant tradescantia to determine quantitative relation between radiation dose and biological effects of radiation resulting from nuclear detonation.

Coven, A. W. Evidence of increased radioactivity of the atmosphere after the atomic bomb test in New Mexico. Physical review, v. 68, 1945: 279.

A G.-M. counter and circuit (Rev. Sci. Instr. 13, 188 (1942)) was tested for background count near Annapolis, on July 12, 14, 15, 19, August 16 and October 31–November 3, all in 1945. The background count was 5.5 per minute. On July 16–18 the background count was 6.3, 7.7, and 10.7 per minute, respectively.

Cronkite, Eugene P., and others. Biological effect of atomic bomb gamma radiation. Science, v. 122, July 22, 1955: 148–150.

Mice were exposed to atomic bomb radiation at 28 stations no both sides of the established L. D. $_{50}$ distance to compare the biological effect with that produced by laboratory gamma and X-radiations. At doses up to 620 r, mortality was consistently at 3 percent but above this dosage, mortality increased rapidly to 950 r, which was approximately the absolute lethal dose. The established L. D. $_{50}$ was 759 r. The relative biological effects of X-ray to bomb radiation closely approached unity.

Cronkite, Eugene P., and others. The characteristics of fallout material and the effects of fallout radiation on human beings. Radiation research, v. 3., No. 2, 1955. Presented before the Radiation Research Society, New York, May 16–18, 1955.

Human beings were accidentally exposed to fallout radiation commencing approximately 5 hours after explosion of a large nuclear device. The fallout material was visible (snowlike). It contaminated skin, clothes, and surroundings, producing skin lesions, whole-body effects, and internal deposition of small amounts of radionuclides. Whole-body exposure occurred at four levels: 175 r, 78 r, 69 r, and 14 r midline dose of radiation.

Damon, P. E. and Kuroda, P. K. Artificial radioactivity of rainfall. Nucleonics, v. 11, December 1953: 59.

Fission products found in rain water during the period June 5–July 23, 1953, correlated with the origin of the prevailing air mass. The total fission activity fallout in the vicinity of Fayetteville, Ark., during this period was in excess of 10^{-8} curies per square mile.

Disposal and dispersal of radioactive wastes. Science, v. 124, July 6, 1956: 17–19. Part of National Academy of Sciences report.

Dunn, L. C. Radiation and genetics. Scientific monthly, v. 84, Jan. 1957: 6–10.

Dunning, Gordon M. Effects of nuclear weapons testing. Scientific monthly, v. 81, December 1955: 265–270.

Presents a summary of blast, thermal and radiation effects of atomic testing. Discusses internal radiation hazard from radioactive iodine, strontium and carbon hazard from contaminated foods. Concludes hazard negligible to date. Discusses genetic effects and possible increase in mutations. Computes the average radiation exposure to people in the United States from all nuclear detonations to date is 0.1 roentgen. Discusses possible effects on weather and nitric acid formation and concludes effect is negligible.

———. Thyroid dose from radioiodine in fallout. Nucleonics, v. 14, 1956: 38–41.

Eisenbud, Merril. The AEC fallout monitoring network. Journal of the Air Pollution Control Association, v. 6, November 1956: 144–146.

———. Global distribution of strontium-90 from nuclear detonations. Scientific monthly, v. 84, May 1957: 237–244.

———. Monitoring network for measuring radioactive fallout. Journal of the American Water Works Association, v. 48, 1956: 659–664.

The basic mechanics of radioactive fallout are discussed. The principal isotopes are listed, collection stations are tabulated, and methods of analysis are briefly outlined. The isotope Sr^{90} is considered the constituent of prime biological significance.

——— and Harley, John H. Radioactive dust from nuclear detonations. Science, v. 117, February 13, 1953: 141–147.

A network of 121 monitoring stations has been established in the United States to collect airborne and settled dust samples for radioactive assay. The results from 30,000 samples collected in conjunction with the 8 detonations in Nevada between April 1 and June 4, 1953, are given. For brief periods following an explosion the radioactive background can be increased in distant areas by fallout of airborne dust.

——— and Harley, John H. Radioactive fallout in the United States. Science, v. 121, May 13, 1955: 677–680.

Summarizes fallout in the United States from early in 1951 through 1954. Accumulated fission product activity in the United States, exclusive of the area within 200 miles of the Nevada test site, was 61 millicuries per square mile. Gamma radiation from this is of the order of 0.0010 mr/hr compared with normal background of 0.005 to 0.05 mr/hr. Measuring technique detects increase of 10^{-8} roentgens per hour, natural background 5×10^{-6} to 5×10^{-5} r/hr.

——— and Harley, John H. Radioactive fallout through September 1955. Science, v. 124, August 10, 1956: 251–255. Bibliography.

Eliassen, Rolf and Lauderdale, Robert A. Radioactive fallout in water supply at Portland, Maine. Journal of the American Water Works Association, v. 48, 1956: 665–670.

This work was done to determine any radioactive increase following atomic weapons tests during 1955. Nine objectives are outlined. Analytical procedures are listed, and the results discussed. Materials of all kinds were

found. It was found that Sr^{90} in the tap water could be increased by a factor of about 70,000 before exceeding the limits specified by the National Bureau of Standards.

Fafarman A. and Shamos, M. H. Effect of fallout from atomic blast on background counting rate. Nucleonics, v. 11, June 1953: 80–81.

Background measurements with a sodium-iodide scintillation detector at New York University show normal variation of ±5 percent. At 1030, March 19, 1953, the rate increased to 3850 cpm, 600 percent above background. A heavy rain preceded the measurement and presumably contained debris from the March 17 Nevada test. Removal of rainwater dropped rate to 1470 cpm. Curves of energy versus count before and after fallout are given.

Fallout detector. Military review, v. 35, March 1956; 69.

Fallout; new H-bomb peril? Chemical and engineering news, v. 33, February 28, 1955: 842–843.

Fallout warning signal blankets United States. Signal, v. 10, March–April 1956: 70.

Fearson, R. E., and others. Results of atmospheric analyses done at Tulsa, Okla., during the period neighboring the time of the second Bikini atomic bomb test. Physical review, v. 70, October 1 and 15, 1946: 564.

Radioactive concentrates were prepared from the atmosphere. Data of July 26 and August 30, 1946, represent the active deposits of Rn and Tn. The data of July 28, based on two samples with initial intensities of $\sim 5 \times 10^{-10}$ curie, are explained by assuming that the concentrate is the active deposit of a new rare radioactive gas of at. No. 86, with a halflife of 82 min.; it corresponds with at least two members of an unreported radioactive series.

Fields, P. R., and others. Transplutonium elements in themonuclear test debris. Physical review, v. 102, 1956: 180–182.

The isotopes of curium, berkelium, and californium found in the thermonuclear debris of the November 1952 thermonuclear test are discussed. The instantaneous buildup of the heavy elements in the thermonuclear device is compared with the buildup during pile irradiation. The alpha-particle energy (5.4 Mev) and the spontaneous fission half-life ($<1.2 \times 10^7$ years) of Cm^{246} are reported. The spontaneous fission halflife of Cf^{254} was found to be 55 days. No other mode of decay was observed for this isotope.

Genetic effects of atomic radiation. Science, v. 123, June 29, 1956: 1157–1164.

Text of the summary report of the Committee on Genetic Effects of Atomic Radiation, one of six reports prepared for the Study of the Biological Effects of Atomic Radiation by the National Academy of Sciences.

Genetics and the atom. Bulletin of the atomic scientists, v. 11, November 1955: 314–343.

Issue contains an editorial on genetics in Geneva, glossary of genetic terms and articles on the subject.

Greenfield, S. M. Ionization of radioactive particles in the free air. Journal of geophysical research, v. 61, 1956: 27–33.

In order to evaluate the possible role of radioactive particles from an atomic cloud as condensation nuclei, an analysis has been made to determine their degree of ionization. Individual radioactive particles become ionized owing to β-emission, and an estimate of the half-life of these ions has been made for various times in the life history of an atomic cloud. It is concluded that while there is a transient charge on these particles, its half-life is small compared to the disintegration rate, with the result that for all practical purposes radioactive particles in the free air are not necessarily preferred condensation nuclei.

——— Rain scavenging of radioactive particulate matter from the atmosphere. Journal of meteorology, v. 14, April 1957: 115–125.

Hahn, Richard B. and Straub, Conrad P. Determination of radioactive strontium and barium in water. Journal of American Water Works Association, v. 47, April 1955: 335–340.

Haldane, J. B. S. Genetical effects of radiation from products of nuclear explosions. Nature, (London) v. 176, July 16, 1955: 115.

The serious nature of genetical effects of radiation is argued. Upper and lower limits of radiation-induced human mortality are estimated.

Harris, D. Lee. Effects of atomic explosions on the frequency of tornadoes in the United States. Monthly weather review, v. 82, December 1954: 360–369.
The increase in tornadoes reported in the United States during the past few years is ascribed to better reporting procedures rather than the presence of atomic debris. Maps showing the distribution of fallout in the United States during the first and second halves of May 1953 are given.

Harris, William B., and LeVine, Harris D. Sampling and measurement of radioactive atmospheric pollution. Proceedings of the Air Pollution Control Association, 1953: 17–21.
Apparatus is described for continuously monitoring the alpha and gamma radiation from stack effluents. A storage-battery-driven dust collector can be used to collect dust away from power sources. An adhesive-coated film can be used to collect "fallout," at a distance from the source. An apparatus is shown for collecting samples nearer the source of contamination by the use of impactors, cyclones, etc.

Harris, D. Lee. Effects of radioactive debris from nuclear explosions on electrical conductivity of lower atmosphere. Journal of geophysical research, v. 60, March 1955: 45–52.
An increase in the ionization near the ground due to the fallout from a radioactive cloud formed by a nuclear explosion will increase the conductivity and lower the potential gradient in the lower atmosphere. Records of atmospheric conductivity and potential gradient from the Tucson Magnetic Observatory are compared with records of the deposition of atomic debris on the ground following the Nevada tests.

Herzog, G. Gamma-ray anomaly following the atomic bomb test of July 1, 1946. Physical review, v. 70, 1946: 227–228.
A recording gamma-ray meter (in Houston, Tex.) indicated a peak in atmospheric gamma rays, from 8 p. m., July 4 to 7 p. m., July 5, with a maximum at 3 a. m., July 5. The maximum increase was 77 percent of the background count.

Heslep, J. M., and Bellamy, A. W. Sampling for airborne radioactivity. Air repair, v. 5, May 1955: 1–4.
Potential sources of radioactive aerosols are discussed, and special attention is given to widespread contamination as might be expected from atomic weapons. There is no perfect sampling method, but rather good results have been secured by use of con. vacuum cleaners and Hollingsworth and Vose H–70 filter paper. Particle-size determinations are best made by the cascade impactor—this gives good characterization of particle size down to 0.3–0.6 μ

Hess, Victor F. and Luger, Paul. The ionization of the atmosphere in the New York area before and after the Bikini atom-bomb test. Physical review, v. 70, 1946; 564–565.
From June 29 through July 10, 1946, no atmospheric ionization due to the atomic bomb was observed.

Hollaender, Alexander. Modification of radiation response. Bulletin of the atomic scientists, v. 12, March 1956: 76–80.

Holzman, B. The effects of atomic bomb explosions on weather. Weatherwise, v. 4, February 1951: 3–4 f.

Holter, N. J. and Glasscock, W. R. Tracing nuclear explosions. Nucleonics, v. 10, August 1952: 10–13.
Airborne radioactivity precipitated in rain and snow has been measured by counter observations on samples concentrated by evaporation or filtration through cotton. A maximum half life of 10.6 hours (Pb^{212}) is associated with natural atomspheric activity. A number of samples collected (at Helena, Mont.) revealed activities of much longer decay periods. These are attributed to atomic explosions in Nevada and Russia. It is considered possible to assign a date to the occurrence of the explosion from observations of the decay curve.

Humphrey, Andrew J. Radiation injury: a technical and legal survey. Cleveland—Marshall law review, v. 6, May 1957: 171–188.
Examines the relations between various types of radiation as to sources and effects.

Hunter, H. F. and Ballou, N. E. Fission-product decay rates. Nucleonics, v. 9, November 1951: C-2–C7.

Jaffee, Gilbert, and others. Radioactive hailstones in the District of Columbia, May 26, 1953. Bulletin of the American Meteorological Society, v. 35, June 1954: 245–249.

At 2030 Greenwich civil time on May 26, 1953, hailstones ranging to the size of tennis balls fell in the District of Columbia, 29 hours after an atomic test in Nevada. Activity of 620 c/m as compared to a background of 20 c/m was measured in the stones. Meteorological analysis and decay curves confirmed the origin of the radioactivity as being from the atomic test.

Kellogg, D. A. Atomic defense in oil refinery. Petroleum engineering, v. 27, October 1955: C6–C8.

Methods of preventing continuous effects of radioactive fallout.

Kellogg, W. W., and others. Close-in fallout. Journal of meteorology, v. 14, February 1957: 1–8.

Keosian, John. Speculation on hazards of exposure to radiations. Science, v. 122, September 30, 1955: 586–587.

Question of maximum tolerance dose of radiation for man has not been satisfactorily determined. It may turn out that all high energy radiation, even of low intensity and brief duration must be considered as potentially dangerous to the exposed individual.

Kilcawley, E. J. Measurement of radioactive fallout in reservoirs. Journal of the American Water Works Association, v. 46, November 1954: 1101–1111.

An extensive survey of radioactivity in the Troy, Albany, Schenectady water supply system was made in an investigation following the heavy rainout which occurred in that area on April 26, 1953. Samples of water, soil, algae, plants, etc., were measured for radioactivity. Decay rates, effectiveness of filtration, rate of disappearance, etc., were studied. The general contamination of the ground at arrival time was 1μc/ft^2. Surface water at Rensselaer Polytechnic Institute, Troy, N. Y., had 2.7 x 10^{-2} $\mu\mu$c/ml. Measurements were also made for subsequent bursts. Corrected to time of rainout, highest rainwater activity was 25.0 $\mu\mu$c/ml on June 9, 1953. Highest stream samples, 0.10–.13 $\mu\mu$c/ml. Alpha activity of same samples was also investigated.

Kirby-Smith, J. S. and Swanson, C. P. The effects of fast neutrons from a nuclear detonation on chromosome breakage in tradescantia. Science, v. 119, January 1, 1954: 42–46.

Physical determination of the fast neutron dose in nuclear explosions; supplementary experiments to those of Conger.

Krumholz, Louis A. Observations of the fish population of a lake contaminated by radioactive wastes. Bulletin of the American Museum of Natural History, v. 110, article 4, 1956: 281–367.

Kulp, J. Laurence, and others. Strontium–90 in man. Science, v. 125, February 8, 1957: 219–225.

To determine amount of radioactive strontium in human bones today, three scientists from Columbia University analyzed about 500 autopsy samples obtained from 17 stations in a worldwide network. Concludes that if bomb tests continue at their present rate the average worldwide concentration in 1970 will be 4 to 8 micromicrocuries of strontium–90 per gram of calcium. The upper figure approaches the significant level established by the British Medical Council in its June 1956 report.

Lacassagne, A. The risks of cancer formation by radiations. Bulletin of the atomic scientists, v. 13, April 1957: 135–136f.

Langham, Wright H., and Anderson, F. C. Strontium–90 and skeletal formation. Science, v. 126, August 2, 1957: 205–206.

Lapp, Ralph E. Strontium–90 in man. Science, v. 125, May 10, 1957: 933–934.

Commentary on Kulp article in Science, February 8, 1957.

Reply by Kulp and others: 934.

Lewis, E. B. Leukemia and ionizing radiation. Science, v. 125, May 17, 1957: 965–972.

Discusses incidence of leukemia in Hiroshima and Nagasaki, also cases among radiologists. Applied to radiostrontium exposures.

Libby, Willard F. Current research findings on radioactive fallout. Proceedings of the National Academy of Sciences, v. 42, December 1956: 945–962.

Speech given before American Association for the Advancement of Science, October 12, 1956.

———. Dosages from natural radioactivity and cosmic rays. Science, v. 122, July 8, 1955: 57–58.

Reprinted in Congressional Record (Daily ed.) July 14, 1955: A5165–A5166.

———. Genetic effects of atom bombs. Metal progress, v. 68, October 1955: 130–131.

Libby, Willard F. Radioactive strontium fallout. Proceedings of the National Academy of Sciences, v. 42, June 1956: 365–390.

Based on a speech given before Annual General Meeting of the American Philosophical Society, April 20, 1956.

Hazards from Sr^{90} deposited in fallout following nuclear explosions are reviewed. $Strontium^{90}$ is of particular interest among the fission products because of chemical similarity to Ca, an average life of about 40 years, and a low rate of skeletal elimination. The maximum permissible average concentration of Sr^{90} in the adult skeleton is calculated to be 1μc/1,000 gm of Ca.

Data are summarized on Ca, Sr, and Sr^{90} concentration in samples of soil, animal, and plant material collected throughout the world before and after the thermonuclear explosions during Operation Castle. A Sr^{90} fallout probably derived from megaton weapons are nearly uniform over the world except for local effects due to rainfall variations and to fallout from submegaton weapons, was found to occur at least 1.7 years after the megaton test series. The average world-wide Sr^{90} fallout rate in the fall of 1954 and the spring and summer of 1955 was 1.2 mc/mi^2/yr. An estimate is presented of fallout rate of Sr^{90} to be expected from weapons tests up to and including the Castle series. Factors influencing the transfer of Sr^{90} from soil to plants, to animals and milk produced by them, and finally to the human skeleton are discussed.

Lieberman, Joseph A. Disposal of radioactive wastes—a growing problem. Civil engineering, v. 25, July 1955: 44–47.

List, Robert J. On the transport of atomic debris in the atmosphere. Bulletin of American Meteorological Society, v. 35, September 1954: 315–325.

Describes results of 91 gummed paper and 51 air filter monitoring stations in the United States during Nevada tests in the spring of 1952. Detailed meteorological trajectories of each of the bursts of the series are given, together with a discussion of the meteorological aspects of transport and fallout. Excluding area within 200 miles of test site, highest gummed paper fission product beta activity was 8×10^6 d/m/ft^2/day at station 330 miles from test side. At distances over 2,000 miles, maximum activity was 1.7×10^5 d/m/ft^2/day, on sampling day. The two highest air filter activities were 1.3×10^5 and 6.8×10^3 d/m/meter^3. Detailed discussion of fallout from 8 of the 8 bursts given, with daily maps showing isolines of activity and areas of precipitation for several days following the bursts.

———. On the transport of atomic debris in the atmosphere. Journal of the Air Pollution Control Association, v. 5, 1955: 153–156f.

The author correlated the meteorological trajectories of bomb debris following each of the eight nuclear detonations at the Nevada test site in 1952 with fallout in the United States. In most instances the predicted pattern of fallout was in agreement with fallout data.

Luntz, Jerome D. Radiation safety for a weapons test. Nucleonics, v. 10, May 1952: 10–13.

An eyewitness report on the elaborate system used to obtain detailed data on distribution of radioactivity from the atomic explosion on April 22, 1952.

Machta, L. and Harris, D. L. Effects of atomic explosions on weather. Science, v. 121, January 21, 1955: 75–81.

A study of temperature and rainfall for the United States does not indicate any departures from normal that are related to atomic explosions.

Machta, L., and others. Worldwide travel of atomic debris. Science, v. 124, September 14, 1956: 474–477.

Machta, L., and others. Airborne measurements of atomic debris. Journal of meteorology, v. 14, April 1957: 165–175.

Margolis, Emanuel. The hydrogen bomb experiments and international law. Yale law journal, v. 64, April 1955: 629-647.

Detailed consideration of influence of such international law doctrines as freedom of the seas and the illegality of the "pollution" of international waters on the H-bomb test in the Pacific.

McDougal, M. S. Hydrogen bomb tests and the international law of the sea. American journal of international law, v. 49, July 1955: 356.

——— and Schlei, N. A. Hydrogen bomb tests in perspective: lawful measures for security. Yale law journal, v. 64, April 1955: 648–710.

Discusses conflicting claims of the security of the United States and its allies and the principles of international law as interpreted in some quarters.

Meinke, W. Wayne. Observations on radioactive snows at Ann Arbor, Mich. Science, v. 113, May 11, 1951: 545–546.

Rigorous chemical separations performed on radioactivities found in snows around Ann Arbor, Mich., after the Las Vegas atomic test explosions on January 27 to February 6, 1951, have definitely established the presence of radioactive rare-earth isotopes and Ba and/or Sr isotopes and have shown the possible presence of I isotopes. The tests conducted on the samples are described. Because of the chemical distribution of the activities found in the Ann Arbor snows, these activities undoubtedly originated in the Las Vegas atomic test explosions.

Mesler, Russell B. and Widdoes, Lawrence C. Evaluating reactor hazards from airborne fission products. Nucleonics, v. 12, September 1954: 39–41.

Meteorological aspects of atomic radiation. Science, v. 124, July 20, 1956: 105–112.

One of six reports prepared for the Study of the Biological Effects of Atomic Radiation by the National Academy of Sciences.

Miller, C. E. and Marinelli, L. D. Gamma-ray activity of contemporary man. Science, v. 124, July 20, 1956: 122–123.

Moloney, William C. and Kastenbaum, Marvin A. Leukemogenic effects of ionizing radiation on atomic bomb survivors in Hiroshima City. Science, v. 121, February 25, 1955: 308–309.

Incidence of leukemia is "high" at distances close to the hypocenter, regardless of presence or absence of severe radiation complaints.

Morgan, K. Z. Maximum permissible internal dose of radionuclides: recent changes in values. Nuclear science and engineering, v. 1, December 1956: 477–500.

Muller, Hermann J. After effects of nuclear radiation. National safety news, v. 74, August 1956: 43–48. Bibliography.

———. Genetic damage produced by radiation. Science, v. 121, June 17, 1955: 837–840.

———. The genetic damage produced by radiation. Bulletin of the atomic scientists, v. 11, June 1955: 210 ff.

Article based upon Japanese analyses of fallout from the March 1, 1954, superbomb.

———. How radiation changes the genetic constitution. Bulletin of the atomic scientists, v. 11, November 1955: 329–338.

———. Radiation and human mutation. Scientific American, v. 193, November 1955: 58–68.

———. Radiation damage to genetic material, Parts I and II. American scientists, v. 38, 1950: 33, 399.

Nader, J. S., and others. Radioactive fallout in rain in the Cincinnati area. Journal of the American Water Works Association, v. 46, November 1954: 1096–1100.

Precipitation in the Cincinnati area was measured for suspended and soluble radioactivity from March 1953 to March 1954. "Background" level in precipitation was 0.03 to 0.08 $\mu\mu$c/ml. Maximum concentration, on April 29, 1953, 319 $\mu\mu$c/ml (500 $\mu\mu$c/ml corrected for decay). Maximum fall-out from a single rain on May 22, 1953, 1.55 inches containing 85.7 $\mu\mu$c/ml, giving 8.75 curies per square mile. Soluble activity averaged 60–80 percent of total before and after tests, fell to 30 percent during tests. Samples from creeks and tapwater show most of activity removed by natural purification. Accumulated and decayed rain activity on June 10 and December 10, 1953, was 2.4 and 0.15 curies per square mile, respectively.

A Navy medical team studies fallout effects. Bulletin of the atomic scientists, v. 12, February 1956: 58–59.

Reprint of article on radiation research performed by doctors from Naval Medical Research Institute after 1954 nuclear tests in Marshalls. Original article in Research Reviews, November 1955. Also summarized in Science, v. 122, December 16, 1955: 1178–1179.

Neher, H. V. Gamma rays from local radioactive sources. Science, v. 125, May 31, 1957: 1088–1089.

New research facts on how foods weather A-bombing. Food engineering, v. 28, November 1956: 59f.

Oceanography, fisheries and atomic radiation. Science, v. 124, July 6, 1956: 13–16.

Part of a continuing study on the Biological Effects of Atomic Radiation conducted by the National Academy of Sciences.

Ophel, I. L. Fallout and the strontium-90 hazard. Science, v. 125, March 1, 1957: 399.

Plough, H. H. Radiation tolerance and genetic effects. Nucleonics, v. 10, 1952: 16–20.

Radiation and health (editorial). Science, v. 125, April 19, 1957: 719.

Questions necessity for establishing a National Radiation Health Institute in the Public Health Service. Points out that any radiation health agency should deal with radiation from all sources, not just atomic radiation.

Randolph, M. L., and others. Effect of bone-marrow treatment on mortality of mice irradiated with fast neutrons. Science, v. 125, May 31, 1957: 1083–1084.

Rediske, J. H. and Selders, A. A. The absorption and translocation of strontium by plants. Plant physiology, v. 28, 1953: 594–605.

Rosenfeld, A. H., and others. Fallout: some measurements and damage estimates. Bulletin of the atomic scientists, v. 11, June 1955: 213–216.

Rotblat, Joseph. The hydrogen-uranium bomb. Bulletin of the atomic scientists, v. 11, May 1955: 171–172, f.

Speculation on the composition and possible radiological effects of the superbomb tested in the Pacific in 1954.

Russell, W. L. Comparison of X-ray induced mutation rates in drosophila and mice. American naturalist, v. 90, January–February 1956: 69–80.

Until recent years, estimates of genetic hazards of ionizing radiation in man were based primarily on information obtained from drosophila. Investigations on radiation-induced mutation rates in mice showed a higher mean rate in the mouse and led to conclusion that estimates of human hazards based on drosophila mutation rates may be too low.

———. Radiation in mice—the genetic effects and their implications for man. Bulletin of the atomic scientists, v. 12, January 1956: 19–20.

———. Shortening of life in the offspring of male mice exposed to neutron radiation from an atomic bomb. Proceedings of the National Academy of Sciences, v. 43, Apr. 1957: 324–329.

Schubert, Jack. Radioactive poisons. Scientific American, v. 193, August 1955: 35–39.

The biological effects of nuclear radiation are as yet imperfectly understood. Maximum permissible limits of exposure must be determined more precisely if byproducts of nuclear technology are to be safely handled.

Setter, L. R., and Goldin, A. S. Radioactive fallout in surface waters. Industrial and engineering chemistry, v. 48, February 1956: 251–255.

Slatis, Herman M. Current status of information on the induction of mutations by irradiation. Science, v. 121, June 10, 1955: 817–821.

Some biological effects of radiation from nuclear detonations. American naturalist, v. 88, 1954: 209–314.

Stefannizzi, A. Radioactivity of atmospheric precipitates. Journal of geophysical research, v. 55, 1950: 373–378.

The radioactivity of snow and rain was detected in 33 cases. It was found that snow usually has a greater activity than rain; that rain in thundershowers is more active than ordinary rain; and that at least a certain amount of activity is acquired by precipitates during their fall from the clouds to the ground level.

Strauss, Lewis L. Fallout from an H-bomb. Metal progress, v. 67, May 1955; 98–99.

———. Radioactive fallout (statement released February 15, 1955) Armed Forces chemical journal, v. 9. March–April 1955: 43–44.

Sturtevant, A. H. The genetic effects of high energy irradiation of human population. Engineering and science, v. 18, January 1955: 9–12.

———. Social implications of the genetics of man. Science, v. 120, September 10, 1954: 405–407.

Tajima, Eizo and Doke, Tadayoshi. Airborne radioactivity. Science, v. 123, February 10, 1956: 211–214.

Twenty-four hour air filters in Tokyo from March 16 to May 31, 1955, were measured for artificial and natural radioactivity. The samples consisted of 650 cubic meters per day and assuming a 10 percent collection efficiency of the Whatman No. 14 filter papers for small particles, the peak activity from fission products, observed on April 12, was $3 \times 10^{-15} \sim 1.2 \times 10^{-14}$ curies per liter, which is comparable to the concentration of natural activity. No consistent correlation of fission product activity with temperature was found and only a slight tendency for rainfall to clean the air was noted. A better correlation was observed with trajectories of high pressure areas.

Thomas, Harold A., and others. Radioactive fallout in Massachusetts surface waters. Journal of the American Water Works Association, v. 45, June 1953: 562–568.
Following the Nevada nuclear-weapons tests, tests were made in Massachusetts which showed a noticeable increase in radioactivity in rain falling. Results are tabulated and the presence of natural radioactivity is considered. On standing there seems to be a settling out of radioactive products.
Thompson, Raymond. They play tag with atomic clouds. Science digest, v. 38, August 1955: 9–12.
Work of planes and crews from Air Force's Air Research and Development Command in tracking radioactive clouds after test atomic explosions.
Toombs, Alfred. Radioactive "garbage"—newest threat to man and nature. Natural history, v. 65, September 1955: 344–349.
Tsivoglov, E. C., and Towne, W. W. Sources and control of radioactive waste pollutants. Sewage and industrial wastes, v. 29, Feb. 1957: 143–156. Bibliography.
Turekian, K. K. and Kulp, J. Lawrence. Strontium content of human bones. Science, v. 124, August 31, 1956: 405–407.
U. N. appoints committee to study effects of ionizing radiation. Bulletin of the atomic scientists, v. 12, Jan. 1956: 13.
Uses and effects of atomic radiation. Scientific monthly, v. 84, January 1957: 3–25.
Contents: Radiation and the human body. Radiation and genetics. Uses of atomic radiation and energy. What we most need to know.
Van Middlesworth, L. Radioactivity in thyroid glands following nuclear weapons tests. Science, v. 123, June 1, 1956: 982–983.
Radioactivity reported and confirmed in thyroid glands of cattle, presumably from fallout.
———. Impact of atomic energy on the life sciences. Technology review, v. 57, July 1955: 471–472.
Primarily concerned with detrimental effects of radiation on human beings.
Warren, Shields. Antipersonnel effects of nuclear weapons. Confluence, v. 5, July 1956: 131–138.
———. Radiation and the human body. Scientific monthly, v. 84, January 1957: 3–6.
Symposium paper presented at all-day scientific program on the uses and effects of atomic radiation held by the American Association for the Advancement of Science, October 12, 1956 at the Carnegie Institution of Washington.
Webb, J. H. The fogging of photographic film by radioactive contaminants in cardboard packaging materials. Physical review, v. 76, August 1, 1949: 375–380.
After the detonation of the experimental atom bomb at Alamogordo, N. Mex. on July 16, 1945, a radioactive contaminant was encountered in strawboard material used by the Eastman Kodak Co. for packaging photographic sensitive films. This paperboard was manufactured in a mill situated at Vincennes, Ind., on the Wabash River. A run of strawboard, produced on August 6, 1945, showed this new and unusual type of radioactive contaminant.
Weiss, H. V. and Shipman, W. H. Biological concentration by killer clams of cobalt-60 from radioactive fallout. Science, v. 125, April 12, 1957: 695.
Woollam, D. H. M. and Millar, J. W. Why are children born deformed? Science news, v. 41, 1956: 27.
Wright, J. H., and others. High-speed computer for predicting radioactive fallout. Journal of research of the National Bureau of Standards v. 58, February 1957: 101–109.
Yoshii, G., and others. Biological decontamination of fission products. Science, v. 124, August 17, 1956: 320–321.

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Atomic bomb casualties; general effects of exposure to radiation. Cancer bulletin (Texas ed.) v. 7, May–June, 1955: 55–58.
Bergsma, Daniel. Public health aspects of atomic energy in peacetime. Public health reports, v. 71, January 1956: 43–46.
Bond, V. P., and others. Hematological changes in human beings exposed to fallout radiation. Radiation research, v. 3, October 1955.
Presented before the Radiation Research Society, New York, May 16–18, 1955.

All individuals exposed to fallout radiation showed depression of the total white, lymphocyte, neutrophil, and platelet counts, more marked in the higher exposure groups. The total white count was consistently lowest during the sixth and seventh postexposure, followed by an upward trend. Six months after exposure, values for the highest exposure group had not returned to the levels of a control population comparable with respect to age and sex distributions.

Clark, Dwight E. Association of irradiation with cancer of the thyroid in children and adolescents. Journal of the American Medical Association, v. 159, 1955: 1007–1009.

Cowan, F. P., and others. Health physics and medical aspects of a strontium 90 inhalation incident. American journal of roentgenology, radium therapy and nuclear medicine, v. 67, May 1952: 805–809.

Cronkite, Eugene P. Radiation injuries of atomic warfare; pathogenesis and therapy. Journal of the Omaha Midwest Clinical Society, v. 13, January 1952: 6–13.

———. and Brecher, G. Radioactivity; effects of whole body irradiation. Annual review of medicine, v. 3, 1952: 193–214.

———. and others. Response of human beings accidently exposed to significant fallout radiation. Journal of the American Medical Association, v. 159, October 1955: 430–434.

After detonation of a thermonuclear device in the Marshall Islands in the spring of 1954, radioactive fallout occurred over an area of thousands of square miles beyond the range of thermal and blast injury. Marshallese and Americans were accidentally exposed on Islands in the area, receiving whole-body radiation, beta radiation injury to skin, and minimal internal contamination.

DeCoursey, E. Ionizing radiation injury from atomic bombs. Journal of the Kentucky Medical Association, v. 50, September 1952: 391–393.

Dunning, Gordon M. Criteria for evaluating gamma radiation exposures from fallout following nuclear detonations. Radiology, v. 66, April 1956: 585–594.

———. Protecting the public during weapons testing at the Nevada test site. Journal of the American Medical Association, v. 158, July 16, 1955: 900–904.

Folley, J. H., and others. Incidence of leukemia in survivors of the atomic bomb in Hiroshima and Nagasaki, Japan. American journal of medicine, v. 13, September 1952: 311–321.

The genetic effects of the atomic bomb. Cancer bulletin (Texas ed.) v. 7, July–August 1955: 70–72.

Genetics and radiation. Eugenics quarterly, v. 3, September 1956: 131–138; December 1956: 201–208.

Contents: Genetics in the atomic age, by Curt Stern. The estimation of spontaneous and radiation-induced mutation rates in man, by James F. Crow.

Glass, Bentley. The hazards of atomic radiations to man. Journal of heredity, v. 47, 1956: 260.

Greulich, W. W., and others. Physical growth and development of children who survived bombing of Hiroshima or Nagasaki. Journal of pediatrics, v. 43, August 1953: 121–145.

Hamilton, Joseph G. Metabolism of radioactive elements created by nuclear fission. New England journal of medicine, v. 240, 1949: 863–870.

Data are presented on the oral absorption, principal organ of retention, and accumulation in and rate of elimination from the organ of Pu^{23}, Np^{233}, and the long-lived fission products.

Haymond, Herman R. Radiation hazards in medical practice. University of Southern California medical bulletin, v. 9, Winter 1957: 10–15.

Henny, G. C. Radiation protection problems in diagnostic roentgenology. American journal of roentgenology, radium therapy and nuclear medicine, v. 73, 1955: 649.

Henshaw, P. S. and Hawkins, J. W. Incidence of leukemia in physicians. Journal of the National Cancer Institute, v. 4, February 1944: 339–346.

Heustis, Albert E. and Van Farowe, Donald. X-ray radiation study shows hazards can be controlled. Hospitals, Journal of the American Hospital Association, v. 25, August 1951: 72–80.

Kirsh, I. E. Radiation dangers in diagnostic radiology. Journal of the American Medical Association, v. 158, 1955: 1420.

Lange, R. D., and others. Refractory anemia occurring in survivors of the atomic bombing in Nagasaki, Japan. Blood; the journal of hematology, v. 10, April 1955: 312–324.

Laug, E. P. Radioactive fallout and our food supply. Antibiotic medicine, v. 2, February 1956: 89–91.

Leukemia in atomic bomb survivors. I. General observations, by R. D. Lange and others. Blood; the journal of hematology, v. 9, June 1954: 574–585. II. Observations on early phases of leukemia, by W. C. Moloney and R. D. Lange. Ibid., July 1954: 663–685.

Looney, W. B. Late effects (25 to 40 years) of the early medical and industrial use of radioactive materials. Journal of bone and joint surgery, v. 37–A, December 1955: 1169–1187; v. 38–A, January 1956: 175–218; v. 38–A, April 1956: 392–407.

Lough, S. Allan. Radiological health—an over-all view. American journal of public health, v. 45, November 1955: 1447–1453.

Macht, S. J. and Lawrence, P. S. National survey of congenital malformations resulting from exposure to roentgen radiation. American journal of roentgenology, radium therapy and nuclear medicine, v. 73, March 1955: 442–466.

Maher, Thomas F. Nature and significance of radioactive fallout. New England Journal of medicine, v. 252, 1955: 740–741.

Principal hazards of radioactivity produced by detonations of atomic weapons are I (half-life 11.5 days) and Sr (Half-life approximately 30 years). General protective measures that can be taken against radioactive fallout are shelter and decontamination, and the avoidance of ingestion or inhalation of contaminated mediums from the environment. Persons who have been exposed to radioactive fallout may show burns, hematologic and bone-marrow changes, and infection.

Maximum permissible radiation exposures to man; a preliminary statement of the National Committee on Radiation Protection and Measurement. A special report. A. M. A. Archives of industrial health, v. 15, April 1957: 350–355.

Also in Radiology, v. 68, February 1957: 260–261.

Maxwell, C. L. The thermonuclear fallout problem in the State of Illinois. Illinois medical journal, v. 108, November 1955: 269–273.

Mewissen, D. J. and others. A formula for chronic radiation dosage versus shortening of life span: application to a large mammal. Radiation research, v. 6, April 1957: 450–459.

Miller, Robert W. Delayed effects occurring within the first decade after exposure of young individuals to the Hiroshima atomic bomb. Pediatrics, v. 18, July 1956: 1–8.

Moeller, Dade W., and others. Radiation exposures in the United States. Public health reports, v. 68, January 1953: 57–65.

Muller, Hermann J. Comments on the genetic effects of radiation on human population. Journal of heredity, v. 46, 1955: 199–200.

———. Damage to posterity caused by the irradiation of the gonads. American journal of obstetrics and gynecology, v. 67, 1954: 467–483.

Powell, Clinton C. Medical aspects of the control of radiologic health problems. American journal of public health, v. 47, February 1957: 179–183.

Reynolds, Mardelle L. and Lynch, Francis X. Atomic bomb injuries among survivors in Hiroshima. Public health reports, v. 70, March 1955: 261–270.

Rolleston, H. Critical review of the harmful effects of irradiation (X-rays and radium). Quarterly journal of medicine, v. 23, 1930: 101–132.

Russell, L. B. and Russell, W. L. Radiation hazards to the embryo and fetus. Radiology, v. 58, 1952: 369–376.

Salvin, M. Effect of atomic radiation on the incidence of sterility and mutation. Journal of the National Medical Association, v. 48, March 1956: 79–83.

Stoll, B. A. New drugs for irradiation sickness. Journal of radiology, v. 68, March 1957: 380–385.

Stone, R. S. The concept of a maximum permissible exposure. Radiology, v. 58, 1952: 639–661.

Storer, J. B., and others. Relative biological effectiveness of various ionizing radiations in mammalian systems. Radiation research, v. 6, February 1957: 188–288.

Straub, C. P. Limitations of water treatment methods for removing radioactive contaminants. Public health reports, v. 70, September 1955: 897–904.

Sutow, Wataru W., and West, Emory. Studies on Nagasaki (Japan) children exposed in utero to the atomic bomb. A roentgenographic survey of the skeletal system. American journal of roentgenology, radium therapy and nuclear medicine, v. 74, September 1955: 493–499.

Taliaferro, William H. and Taliaferro, Lucy G. Effect of X-rays on immunity: a review. Journal of immunology, v. 66, February 1951: 181–212.
Review documented by 201 references.
Taylor, Lauriston S. Education in radiation protection. American journal of roentgenology, radium therapy and nuclear medicine, v. 73, February 1955: 193–202.
———. Legislative control of radiation. Radiology, v. 66, February 1956: 246–252.
"Two States already have radiation-control regulations, and several others are in the process of developing them * * * The National Committee on Radiation Protection feels that legislation should cover both occupational and non-occupational exposure and all kinds of radiation * * * The Committee believes that the maintenance of the maximum degree of uniformity between the regulations in all States is of prime importance."
———. State control of protection against ionizing radiation. American journal of roentgenology, radium therapy and nuclear medicine, v. 71, April 1954: 691–702.
Thomas, Harold A. The public health implications of radio-active fallout in water supplies. American journal of public health, v. 46, October 1956: 1266–1274.
Warren, Shields. Public health aspects of atomic power development. A. M. A. Archives of industrial hygiene and occupational medicine, v. 9, March 1954: 183–185.
———. Longevity and causes of death from irradiation in physicians. Journal of the American Medical Association, v. 162, September 29, 1956: 464–468.
Williams, E. G., and Ingraham, S. C. Biological-medical considerations in atomic defense. Public health reports, v. 71, February 1956: 173–180.
Wilson, Robert R. Nuclear radiation at Hiroshima and Nagasaki. Radiation research, v. 4, 1956: 349–359.
Use of X-rays and other ionizing radiation; U. N. Scientific Committee on Effects of Atomic Radiation. Armed Forces medical journal, v. 8, March 1957: 358–360.
Vorder Bruegge, C. F. Radiation injury following A-bomb explosion. Annals of internal medicine, v. 36, June 1952; 1444–1458.
Ward, L. J. Lethal radiation from exploded atomic bomb. Mississippi Valley medical journal, v. 74, November 1952: 191–195.
Yamazaki, J. N., and others. Symposium on effects of radiation and other deleterious agents on embryonic development; study of outcome of pregnancy in women exposed to atomic bomb blast in Nagasaki. Journal of cellular and comparative physiology (supp. 1) v. 43, May 1954: 319–328.
Zavon, M. R. Radiation—helpful or harmful? Journal of the American Medical Association, v. 162, 1956: 533.

Technical Periodical Articles, Foreign

BRITISH

Auerbach, Charlotte. Biological hazards of nuclear and other radiations. Nature (London) v. 178, September 1, 1956: 453–454.
A comparison is made of British and United States reports on radiation hazards. Both reports show that the present dangers arise much more from excessive use of X-rays than from fallout or atomic energy establishments.
Biological effects of radiation. Nature (London) v. 179, April 13, 1957: 755–756.
Blifford, Irving H., and others. Relation between air concentration of radioactive fission products and fallout. Nature (London) v. 177, 1956: 990–992.
The daily atmospheric concentration and fallout of radioactivity due to fission products was measured at Washington, D. C., during December 1954–May 1955 by air-filter and gummed-paper techniques, respectively. The apparent rate of decay was used to distinguish between natural Th B and fission products. There was no correlation of individual daily measurements of air concentration and fallout of this material on the ground. Despite many variables the concept of fallout rate may be useful in arriving at some correlation.
Chatterjee, Santimay. Radioactive ashes over Calcutta and a method of dating a nuclear explosion. Atomic scientists journal (London) v. 4, 1955: 273–278.

Cockcroft, John D. Biological effect of nuclear explosions. Pharmaceutical journal (London) v. 174, 1955: 387–388 f.

———. Radiological hazards from nuclear explosions and nuclear power. Nature (London) v. 175, May 21, 1955: 873–875.

Reprint of address to the Parliamentary and Scientific Committee at the House of Commons, London, April 20, 1955.

Average concentration of radioactivity in the air at ground level over the last 3 years due to bomb explosions is about 1 percent of the natural radioactive dust content. Accumulated dose to completely unprotected people from fallout in England is about 0.01 R. Further fallout from airborne debris should bring it to 0.03 R. In the United States, average dose is about 0.1 R. Average dose in England to average person over a generation (30 years) will be about 0.003 R, natural radioactivity gives 3 R. Discusses other radiological hazards and concludes that probably at least 1,000 times the present level of contamination would be needed to give rise to serious harmful effects.

Howard, Alma. The hazards from the increasing use of ionizing radiations: A symposium. III. An attempt to assess the genetic changes resulting from the irradiation of human populations. British journal of radiology, (London) v. 29, June 1956: 270–273.

Leukaemia and natural background radiation. British medical journal, issue 5021, March 30, 1957: 760.

Nishiwaki, Yasushi. Bikini ash. (Letter to the editor.) Atomic scientists journal (London) v. 4, November 1954: 97–109.

Account of the results of the shower of radioactive ash that followed the explosion of the H-bomb at Bikini. Effects of the fallout on the Japanese fishermen, on the fish in the sea, etc., are considered.

———. Effects of H-bomb tests in 1954. Atomic scientists journal (London) v. 4, May 1955: 279–288.

Preston, R. L. and Hogg, B. G. Radioactive fallout in Kingston, Canada. Nature (London), v. 176, 1955: 459.

The monitoring of the radioactive fallout in Kingston, Ontario, during the period February 15 to May 28, 1955, from the nuclear tests conducted by the AEC is reported. Meteorological factors seem to outweigh the magnitude of the bombs at such a great distance from the explosion.

Poyce, M. H. L. Global thermonuclear explosions are impossible. Discovery (Norwich, England) v. 16, December 1955: 495–497.

Some people are afraid that a thermonuclear explosion might set off a reaction in the earth's crust and in the seas, resulting in the earth's destruction. The author of this article attempts to show why this is not possible.

Radiation hazards. Lancet (London), v. 270, June 23, 1956: 999–1000.

Radiation hazards of experimental nuclear explosions. British medical journal (London) No. 4916, March 26, 1955: 775–776.

Read, John. The approach of the physicist to radiation biology. Physics in medicine and biology (London) v. 1, January 1957: 209–224.

Rotblat, Joseph. The hydrogen-uranium bomb. Atomic scientists journal (London) v. 4, March 1955: 224–228.

This British scientist feels that the various bombs that have been detonated throughout the world have resulted in more dangers from radioactivity than officials admit. He believes that genetic aberrations may well result from present radioactivity in the atmosphere.

Sevitt, S. The bombs. Lancet, v. 269, July 23, 1955: 199–201.

———. The case of Mr. Kuboyama; a clinical and pathological study of the first victim of the atomic bomb. Medical world (London), v. 84, May 1956: 385–390.

Spiers, F. W. Radioactivity in man and his environment. British journal of radiology (London), v. 29, August 1956: 409–417.

———. and Haldane, J. B. Genetical effects of radiation from products of nuclear explosions. Nature (London) v. 177, February 4, 1956: 226–227.

Relative radiation dose-rates to man and to Drosophila are discussed. Data previously presented by Prof. J. B. S. Haldane on the genetical effects of radiation resulting from nuclear explosions are reviewed. A reply from Professor Haldane presents revised calculations of radiation dose rates.

Stanford, R. W., and Vance, J. The quantity of radiation received by the reproductive organs of patients during routine diagnostic X-ray examinations. British journal of radiology, v. 28, May 1955: 266–273.

Strontium-90 in man. (Letters to the editor.) British medical journal, issue 5024, April 20, 1957: 943–944.

Strontium-90 in man. British medical journal issue 5021, March 30, 1957: 752–753.

Sutton, G. Thermonuclear explosions and the weather. Nature (London) v. 175, February 19, 1955: 319–321.

Available evidence points to conclusion that atomic tests cannot be held responsible for any worldwide extremes of weather encountered in 1954.

FRENCH

Abribat, Marcel and Pouradier, Jacques. Evolution of the amount of artificial radioactive elements in the atmosphere of Paris. Comptes rendus, v. 237, 1953: 1233–1255 (in French).

The β-activities of airborne dust particles, rain waters, and natural waters were assayed daily between January and September 1953. The dust-particle activity showed 2 maximum March 24–June 25 and August 30–September 17, which probably resulted from atomic explosions in the United States and Russia, respectively. Rainwater activity gave only the earlier maximum while no changes in the activity of natural waters were found.

———, and others. Artificial radioactivity in rainwater of the Paris area. Comptes rendus v. 234, March 10, 1952: 1161–1163 (in French).

Considerable radioactivity was observed 8–15 days after the explosion of atomic bombs in Nevada.

——— ———. Evolution of atmospheric radioactivity in Paris region. Comptes rendus v. 240, 1953: 2310–2312 (in French).

Daily measurements of radioactivity have shown the passage of many atomic clouds, and particularly the series of explosions in the United States of America and Russia, while those in the Pacific and Australia have been identified in Milan. For the Australian explosion in October 1953 there was no radioactive increase in the air in the Paris region, while for the Pacific explosion there were measurable fluctuations but very feeble. For the Russian explosions in August 1954 the fluctuations were much greater than for the Pacific ones.

——— and others. On the artificial radioactive products present in the atmosphere in the region of Paris. Comptes rendus hebdomadaires des séances de l'Académie des Sciences (Paris) v. 235, July 16, 1952: 157–159(in French).

The radioactivity of solid matter in rain water and air collected near Paris in November 1951, and in April and May 1952, follows the same decay law as that observed for fission products after a nuclear detonation in Nevada in November 1951.

Besson, A. and Pelletier, J. Radioactivity and air pollutions. Bulletin de L'Académie Nationale de Médecine (Paris), par. 140, January 24–31, 1956: 43–45.

Bouquiaux, J. Ionizing radiations and public health problems. Archives Belges de médecine sociale, hygiéne, médecine du travail et médecine légale (Brussels). v. 14, May 1956: 230–268 (in French).

Garrigue, Hubert. The abnormal radioactivity of the atmosphere. Comptes rendus, v. 235, 1952: 1489–1499 (in French).

Further measurements on the radioactivity of the atmosphere near Puy de Dome are tabulated. The activity in April 1952 with a mean life of 25 hours was ascribed to the complex particles A. In June 1952 an activity with a mean life of 100–400 hours was attributed to the previously observed A″ particles. The activity associated with the A″ particles was also observed in the soil and air.

———. Atmospheric radioactivity of atomic origin. Comptes rendus, v. 237, November 16, 1953: 1232.

Aircraft measurements in October 1953, show at most, traces of radioactivity. Snow sample (7 kg) on November 3, 1953, yielded 0.1 x 10^{-13} curies/cm^3 (radon equivalent) in the residue, about 20 percent above background. Only β—activity was found.

———. Atmospheric radioactivity of atomic origin. Comptes rendus, v. 240, January 10, 1955: 178–180 (in French).

An improved collector has been installed in the aerial laboratory, but in recent months little activity caused by atomic explosions has been detected.

Garrigue, Hubert. Establishment of a flying laboratory and the improvement of apparatus for the study of weak radioactivity in the atmosphere. Comptes rendus, v. 230, June 26, 1950: 2279–2280 (in French).

Describes in general terms the equipment of a plane to measure the radioactivity of the air. The first flight, on May 13, 1950, showed a high concentration of natural activity and a measurable amount of activity of unknown origin with a long half-life.

———. Fresh outbreak of activity of atomic origin in the atmosphere. Comptes rendus, v. 240, 1955: 1453–1455 (in French).

During the period December 1954 to February 1955 values of the order of 1 β-emitting atom per cm^3 of air have been obtained for observations on the ground and in the air in Puy de Dôme region.

———. The invasion of radioactive air of atomic origin and its influence on atmospheric precipitation. Comptes rendus, v. 232, March 5, 1951: 1003–1004 (in French).

Reports on aircraft observations in June 1950 and in January and February 1951. On February 3, 1951, at 4,100 meters, a maximum concentration of 0.15×10^{-18} curies/cm^3 radon equivalent of material of 30–50 hour apparent half-life. Attempts to show that precipitation cleanses the tropospheric air.

———. Observations on the impurities in free air. Comptes rendus, v. 236, 1953: 2309–2311 (in French).

Results of analysis of atmospheric radioactivity and pollution from measurements on free air and on atomic precipitation (snow) at heights from ground level to the summit of the Puy de Dôme (1450 m) during January-February 1953. The radioactivity was probably of distant origin (from nuclear fission explosions); the dust and soot of local origin.

———. On the radioactivity of the atmosphere. Comptes rendus, v. 228, May 16, 1949: 1583–1584 (in French).

A radioactive substance with a half-life of 25 hours was detected in aircraft flights at about 6,000 meters in the summer of 1946 and again, in much weaker concentration, in the summer of 1948. Highest value reported, August 1, 1946, at 6,000 meters, 2.0×10^{-18} curies/cm^3. Observations at Puy-de-Dôme, 1,500 meters, gave no definite positive results. Speculation that material is from Bikini tests or possibly of meteoric or cosmic ray origin.

———. Prospecting the radioactivity of the air. Comptes rendus, v. 237, October 12, 1953: 802–803 (in French).

Measurements aboard an airplane (3,000 meters altitude) indicated a sudden influx of radioactive particles "A" with a mean life of 25 hours on August 8 (traces) and, in appreciable amounts, on August 15, 28, and September 5, 1953. The maximum of intensity of about 0.1×10^{-18} equivalent curie of Rn activity/cc. probably prevailed around August 15. No activity could be detected prior to these dates in air or in snow on the summit of Puy-de-Dôme (1,460 meters), nor did the evaporation of hail (collected on the ground), precipitated during a "microcyclone," which occurred on August 8, disclose any abnormal activity.

———. Radioactivity of the atmosphere of atomic origin. Comptes rendus, v. 237, 1953: 1232–1233.

Recent measurements at the summit of Puy-de-Dôme indicate the radioactivity of the snow may be attributed to atomic explosions.

———. Research on atmospheric radioactivity. Comptes rendus, v. 238, 1954: 2074–2075 (in French).

In flight at 2,800 meters, traces of radioactive particles were gathered by means of a corona effect similar to that of Sella, and of long period, on April 24, 1954. At Puy-de-Dôme (1,465 meters) after a fall of rain and snow on May 3–4, 1954, samples of the fallen residue showed very feeble radioactivity of period 10 days or more. This radioactivity of the airborne particles was independent of their probable natural electrical charge and mobility and was indicative of an atomic cloud.

———. Researches in radioactivity at the top of Puy-de-Dôme. Comptes rendus, v. 233, December 3, 1951: 1447–1448 (in French).

Snow collected at Puy-de-Dôme which fell on November 19–20, 1951, showed β-activity about 43 percent above background with an apparent half-life of 10 days. Speculation on role of particles as condensation nuclei.

———. Studies on the radioactivity of the atmosphere. Comptes rendus, v. 233 Oct. 15, 1951: 860–862 (in French).

The existence in the atmosphere of a radioactive substance, labeled "A," of several hours half life has been confirmed in flights at 3,300 meters, and at a ground station at 1,460 meters. Concentrations of Rn, Tn, "A," and "A'" observed from March 15 to August 14, 1951, are tabulated. The concentration of substance "A," of 20- to 30-hour half life, is related to atomic explosions and precipitation.

Lacassagne, A. Medical consequences of atomic bomb explosions. Bruxelles-Médical, (Brussels) v. 35, September 11, 1955: 1821–1833 (in French).

Martin, Charles Noel. Accumulative effects on the global surface caused by thermonuclear explosions. Comptes rendus, v. 239, 1954: 1287–1289.

HNO_3 is formed which locally lowers the pH of rain water. This can affect plant metabolism. C^{14} production and its absorption by living things is discussed.

Nahmias, M. E. Detection at a distance of atomic bomb tests. Mem. Artillerie Franc. 28, 1954: 393–402.

Nahmias describes the radioassay of atmospheric air, rain, and snowfall as the basis for detecting atomic bomb tests. Tables and graphs give time versus Ra equivalents, distance versus Roentgens/hours of radiation, and distribution of radio-elements which can be expected.

Ravina, A. The first known effects of hydrogen bomb on man. Presse médicale (Paris), v. 62, June 5, 1954: 881 (in French).

Tanaevsky, Olga and Vassy, Etienne. Variations of the natural and artificial radioactivity of the atmosphere. Comptes rendus, v. 241, 1955: 38–40.

The natural activity of the atmosphere, presumably Rn, Tn and their decay products, was most evident during periods of weak-winds at the Val Joyeux Scientific Station. In 30 of the 66 cases of high activity, the winds were from the southwest or west southwest, indicating a Rn source in that direction. The artificial radioactivity, detected in rains and snows, was strongest at the beginning of the precipitation. The highest activity measured was 0.724 microcuries/1.

GERMAN AND AUSTRIAN

Gerlach, Walther. The hazards of radiation and its danger to life. Universitas (Stuttgart, Germany), v. 2, 1957: 125–131.

Haxel, O., and Schumann, G. On the radioactive contamination of the atmosphere. Naturwissenshaften (Berlin), v. 40, 1953: 458 (in German).

Beginning on March 19, 1953, radioactivity in the air near Heidelberg was measured continuously by means of air filtering, using, in general, 48-hour exposures. The long-lived activity, presumably from atomic explosions, showed several peaks in the period from mid-March to mid-June, reaching a maximum of 2.5 curies/m^3 in mid-April. An examination of the decay rates allows the determination of the time of explosion. It was found that fission products reached Heidelberg in as little as 7 days from the Nevada test site.

Herbst, W., and others. Considerations of the suitability of radioactive atomic aerosols as tracers in meteorological flow investigations. Naturwissenschaften (Berlin) v. 41, 1954: 156–160 (in German).

A large increase in radiation from the ground at Wittental (Brunswick), October 18–20, 1951, led to an attempt to trace the increased radioactivity to atom bomb explosions in the United States and the corresponding measurements by Holter and Glasscock at Helena, Mont. For the explosions on October 6, 7, and 14 the probable track of air at the 500 mb level from Helena to Wittental was determined in each case and shown on a chart.

——— and Philipp, K. The path of an atomic explosion aerosol. Naturwissenschaften (Berlin), v. 40, 1953: 54 (in German).

Experiments at Wittental show that during October 16–24, 1951, a high value of the radioactive background was discovered. A similar high value had been reported at Helena, Mont., between October 6–16, 1951. It is suggested that the same air mass was at these two places at the different times and that radioactivity measurements permit the path of the air mass in which the explosion occurred to be plotted.

Sittkus, A. Observations on radioactive vapour from atomic experiments in the years 1953/54. Naturwissenschaften, (Berlin) v. 42, 1955: 478–482 (in German).

Records data from rainfall at Freiburg, Germany, on the radioactivity of the atmosphere and describes a method of deducing the time of the explosion from the observations.

———. The path of an atomic explosion aerosol. Remarks of Mr. Sittkus to W. Herbst. Naturwissenschaften, (Berlin) v. 40, 1953: 198.

No comparable effect was detected on other geiger counters in the same neighbourhood at the time when Herbst and Philipp recorded the effect they believe due to an atomic bomb. In reply these authors pointed out that they used thin-walled counters to detect β-radiation, whereas Sittkus used tubes suitable for detecting the penetrating component of cosmic rays.

Steinhauser, F. Atomic bomb explosions and weather events. Universitas (Stuttgart) 1954: 1189–1196 (in German).

———. Atomic energy and world weather. Universum natur and technick (Vienna) v. 16, 1954: 481.

———. Atomic energy experiments and weather. Osterreichische Hochschulzeitung (Vienna) 6 jg., Wien 1954 (in German).

Tsuzuki, M. Radioactive damage of Japanese fishermen caused by Bikini ashes. Münchener medizinische wochenschrift (Munich), v. 97, August 5, 1955: 988–994 (in German).

Short description of clinical experiences with radioactive injuries of the 23 fishermen during 1 year. All of the 23 fishermen in the boat were afflicted with acute radioactive-sickness as a result of contact with radioactive rain and ashes. They were injured through the combination of external as well as internal radiation.

INDIAN

Bandopadhyay, K. G., and others. Radioactive nuclei in rains over Calcutta. Science and culture (India), v. 21, 1955: 273–275.

The radioactivity in dusts carried down by rains between March and September 1955 were determined by β-ray assay. Histograms of relative activities as a function of specific rainfalls are given.

Chatterjee, Santimay, and others. Dating a nuclear explosion. Science and culture, v. 20, 1955: 403–404.

A method is proposed for dating nuclear explosions from the composite beta-decay curves of the radioactive dusts.

——— ———. Measurements on radioactive dusts over Calcutta. Science and culture, v. 20, 1955: 399–401.

A brief report of the measurements of the radioactivity of rain water samples collected in Calcutta from April 29 to the middle of July 1954, is presented. Measurements of energy and half lives indicated that the dusts originated from nuclear explosions.

——— ———. Presence of radioactive dusts over Calcutta. Science and culture, v. 19, May 1954: 570–571.

Beginning April 4, 1954, settled dust in Calcutta was analyzed for radioactivity; none was found until the first rain occurred on April 29.

ITALIAN

Neuwirth, R. Meteorological utilization of measurements of the artificial radioactivity of the air and precipitation. Geofisica pura e aplicata (Milan, Italy) v. 32, 1955: 147–158. (In German.)

German, French, and American measurements of the rainfall and air activity are being evaluated. For that purpose, trajectories from the experimental grounds for bomb tests in Nevada to Western Germany are drawn. By means of intermediate values, the test possibilities of air paths—first only scheduled—are given. The so-called deposit spaces and meridional circulations, which are significant particularly in divergence regions, prove to be of especial importance. The mechanism of activation of precipitation is discussed. A connection between the activity of precipitation and air masses could only be found in individual cases. But it seems that semitropical air air masses dispose of a higher specific activity in comparison with the polar air masses.

Radioactive precipitations caused by the experimental explosion of atomic weapons. Minerva medica (Turin) v. 46, March 31, 1955: 640–642 (in Italian).

Santomauro, L. and Cigna, A. First measurements of the radioactivity in atmospheric precipitations. Annali di geofisca (Rome), v. 6, 1953: 381–387.

Measurements conducted between February 1951 and November 1952 showed that nuclear-weapon tests at Las Vegas, Eniwetok, and Montebello were followed, 1, 2, and 3 weeks later, respectively, by an increase in the radioactive content of rain and snow falling in Italy.

Spena, A. The genetic problem in its relation to the use of atomic energy. Annali di medicina navale e tropicale (Rome), v. 61, July–August 1956: 569–582.

JAPANESE

Arakawa, Akio, and others. Climatic abnormalities as related to the explosions of volcano and hydrogen-bomb. Geophysical magazine (Tokyo) v. 26, 1955: 231–255.

The effects of volcanic explosion on climatic abnormalities are investigated statistically and synoptically. The abnormal weather during the summer season of 1954 is found to have features similar to climatic abnormalities caused by volcanic dust. The distribution of temperature anomalies and its annual variation are discussed in relation to the tropospheric circulation.

Arakawa, H. Abnormal weather caused by the H-bomb. Astronomy and meteorology, (Tokyo) v. 20, 1954.

———. Possible atmospheric disturbances and damages to the rice-crops in northern Japan that may be caused by experimentation with nuclear weapons. Geophysical magazine (Tokyo) v. 26, 1955: 125–134.

———, and Tsutsumi, K. A decrease in the normal incidence radiation values for 1953 and 1954 and its possible cause. Geophysical magazine (Tokyo) v. 27, 1956: 205–208.

Arizumi. On the distribution of ash-fall (meteorological investigation on the H-bomb experiment at Bikini Island—II) Journal of the Meteorological Society of Japan (Tokyo), Nos. 9–10, 1954.

Egawa, Tomoji, and others. Investigations on the contamination of field crops by artificial radioactivities as a result of the H-bomb tests at Bikini Atoll. Soil and plant food, v. 1, 1955: 19–20.

Crop samples taken between June and October 1954 were analyzed for radioactivity. Rare earth elements contributed the greater part of the activity. Polished rice showed no activity.

Horie, Kuniko. Damping of radioactivity of the Bikini ashes. Kagaku (Science) (Tokyo), v. 25, 1955: 636–637.

The radioactivity (β- and γ-radiation) of the H-bomb ashes was measured over a period of 600 days by means of an electroscope and a Geiger-Muller counter. Absorption by Al foils shows that the half-life is shorter for radiation of lower energy.

Ito, Gakuro and Moriuchi, Yasuyuki. Some problems on the radiological protection: especially concerning with the maximum permissible dose. Oyô Butsuri (Tokyo?), v. 24, January 1955: 3–17 (in Japanese).

Kakehi, H. Ash of Bikini and its effect on human body. Journal of Japan Physicians' Society, v. 31, May 1, 1954 (in Japanese).

Discusses physical and chemical composition of radioactive ashes which fell on the fishermen of the Fukuryu Maru and gives a clinical study of its effects. Estimated radiation received by fishermen in 2-week stay on ship as 200 r. Discusses hazard from contaminated tuna.

Kaneshige, Kankuro. Japan-United States radiobiological conference. Contemporary Japan (Tokyo), v. 23, Nos. 4–6, 1955: 296–310.

Kawabata, Toshihuri. Studies on the radiological contamination of fish. Japanese journal of medical science and biology (Tokyo), v. 8, October 1955: 337–372.

The decay rate of the radioactivity of the organs of fish caught by the crew of the "Shunkotsu Maru" was measured. After 3 months, the spleens, kidneys, and gonads retained considerable radioactivity, suggesting the presence of long-lived radioactive elements. At the end of this time, the liver and other organs retained only about 10 percent of the initial radioactivity and the bile had lost most of its activity. The retention by the pyloric caeca and the intestinal contents varied considerably and was probably dependent on the food consumed. Absorption of the ash of the organs on Dowex 50 and elution located most of the activity in the fractions eluted with 0.5 percent oxalic acid and 5 percent citrate buffers of pH 4.1 and 4.6. Qualitative separation with carriers identified the main radioactive element of the citrate buffer, pH 4.1, eluate as Zn^{65}.

Kimura, Kenjiro. Radioactive ashes on the fifth Fukuryu-Maru, the fishing boat that suffered from the hydrogen bomb test on March 1, 1954. Kagaku, (Tokyo) v. 24, 1954: 300–302.

By ordinary procedures with carriers and by separation with cation-exchange resins, the ashes were analyzed and the following radioactive nuclides were detected, Zr^{95} (65 days), Nb^{95} (35 days), I^{32} (2.4 hours), Te^{132} (77.7 hours), Nb^{95m} (90 hours), I^{131} (8.141 days), Ba^{140} (12.8 days), La^{140} (40.0 hours), Sr^{89} (53 days), Sb^{127} (93 hours), Ru^{103} (39.8 days), and Ru^{106} (1.0 year) etc.

———, and others. Detection of rhodium–103m in the "Bikini ashes." Bulletin of the Chemical Society of Japan, v. 29, 1956 (in English) 395–398.

The radiochemical analysis of the so-called Bikini ashes which fell on a Japanese fishing boat, the No. 5 Fukuryu Maru on March 1, 1954, are described as of some 25 days after detonation of the bomb. The collected sample (10^{-7} counts/minimum) was ignited and dissolved in 6N HC1, insolubles were filtered off, and the activity of small aliquots of the filtrate was measured. Total activity was estimated about 10^{-6} counts/minimum. Ru (10 mg.) was added to the filtrate as a carrier, the acidity of solution was adjusted to 2N, H_2S was passed through to precipitate R_u as sulfide, and the precipitate was dissolved with HNO_3; H_20, KM_n0_4, and concentrated H_2O_2. The appropriate aliquot portion of the distillate was taken up in a counting dish and evaporated to dryness; the activity was measured and found to be 1.5 x 10^{-5} counts/minimum.

Kosaka, Takao, and others. Radioactive rain and contaminated atmosphere observed in Niigata City (Japan). First report: the effect on environment and human body. Niigata Medical Association journal, v. 69, 1955: 1–6.

Koyama, Y. and others. Clinical course of the radiation sickness caused by Bikini ashes: intermediate Report. Iryo (Tokyo) v. 9, January 1955: 5–45 (in Japanese).

Clinical observations are summarized covering a 5-month period on 16 patients exposed to fallout from the Bikini explosion on March 1, 1954.

——— ———. Conference of the radioactive disease caused by the atomic bomb explosion in the central Pacific. Iryo (Tokyo) v. 9, January 1955: 56–68 (in Japanese).

Mitsui, Shingo, and others. Investigations on the radioactive contamination of crop plants as a result of hydrogen-bomb detonation. Soil and plant food, v. 1, 1956: 15–18.

I. Radioactive contamination of crop plants and soil. II. Root and foliage uptake of Bikini ash.

Miyake, Y. Artificial radioactivity in rain water observed in Japan, from autumn 1954 to spring 1955. Papers in meteorology and geophysics, Meteorological Institute, Tokyo, v. 6, May 1955: 26–32.

At about midnight of September 18, 1954, a typhoon (No. 14, 1954) ran away toward the sea after attacking Japan. In place of the typhoon a colder and less moist air flowed in from the north. Just at the time, a new activity of artificial origin was detected in rain water at Niigata and Hirosaki, both situated along the Japan sea coast of the northern part of the main island. From 22d to 24th of September, the activity increased rapidly, spreading over a wide area in Japan and finally an activity as strong as 0.3 x 10^{6} curie/liter was counted in rain water at Yamagata.

———. The artificial radioactivity in rain water observed in Japan from May to August 1954. Papers in meteorology and geophysics, Meteorological Institute, Tokyo. v. 5, September 1954: 173–177 (in English).

Radioactivity in rainfall was measured at several places in Japan after the spring, 1954, Pacific tests. The maximum activity, 0.5 x 10^{-6} c/1, was observed at Kyoto on May 16, 1954. Meteorological trajectories indicate air that was over Bikini on May 8 reached Japan on the 16th and it is speculated that an explosion on May 5 is responsible for the activity. On August 3, airplane measurements with a dust impinger indicate 0.8 ~ 2.0 x curie/cc on the average from 1000–3000 meters over Tokyo.

Miyake, Y. Rain from south and snow from north. Kayaku Asahi, (Tokyo) December 1954 (in Japanese).

Discusses detection of nuclear explosions by various methods including observations of fission product activity in the atmosphere. Deposition of 750 cpm on a vaseline coated paper (30×30 cm) on May 13–16, 1954. Eighty-six thousand cpm/l observed in rain at Kyoto on May 14, apparently from May 5 test at Bikini. Thereafter, strong contamination of rain observed at many places on Pacific Coast of Japan. Since May 1954, activity of rain on Pacific side about an order of magnitude greater than on Japan Sea side of Japan. On September 22, 1954, a record-breaking 124,000 cpm/l from a Russian test was observed in rain at Yamagata, associated with a cold front advancing from Siberia, almost no activity in warm front rain on the Pacific coast. Discusses possible hazard from contaminated snow.

——— and Sugiura, Y. Radiochemical analysis of radio-nuclides in sea water collected near Bikini Atoll. Papers in meteorology and geophysics, Meteorological Institute, Tokyo, v. 6, 1955: 33–37.

A radiochemical analysis of sea water containing fission materials collected near Bikini Atoll in June 1954, was performed. The sea water was boiled with hydrochloric acid; iron and lanthanum salts each 5 mg as Fe and La were added to it. They were precipitated as hydroxide, which was dissolved in hydrochloric acid and ferric chloride was extracted with ethyl ether. The remaining solution was evaporated to dryness and the residue was dissolved in hydrochloric acid. Using the latter solution the group separation was done with cation exchanger resins.

———, and others. Artificial radioactivity in the sea near Japan. Papers in meteorology and geophysics, Meteorological Institute, Tokyo, v. 6, May 1955: 90–92.

Sea water collected around the Bikini Atoll from July to September 1954, was analyzed for total radioactivity by adding 2 g. solid NH_4Cl, 1 ml. of an aqueous solution of Ferric alum (86.3 g./l.), and 1 ml. of $BaCl_2$ solution (17.8 g./l.) to 1 l. of H_2O heated to 60–70 while being stirred. NH_4OH was added until the solution was faintly pink to phenolphthalein. After 2-minutes boiling the precipitate settled on standing for several hours at room temperature before being filtered on a filter disk lain above a glass filter. Counting rates of 2.1 ± 1.6 to 140.8 ± 6.8 counts/minute/l. were obtained.

——— ———. On the distribution of radioactivity in the sea around Bikini Atoll in June 1954. Papers in meteorology and geophysics, Meteorological Institute, Tokyo, v. 5, January 1955: 253–263.

Report of an oceanographic survey in the late spring of 1954 in the Marshall Islands area to investigate the radioactivity of the waters following the Castle tests. Maximum value was 7025 cpm/1, 450 km west of Bikini at a depth of 75 m (1,000 cpm $\approx$ 5.9 mμc). Almost all radioactivity was in solution, a filter of pore size 0.5μ passed 99 percent of the activity. Distribution of the radioactivity and its relation to ocean currents is shown. Vertical cross sections show marked decrease in activity below thermocline, about 150 m. Estimate flow of radioactivity through cross-section 150 km west of Bikini was 10^6 curie/hour. Coefficient in mixed fission product decay law ranged from −1.3 to −1.6, mean −1.5.

——— ———. Radiochemical analysis of fission products contained in the soil collected at Tokyo, May 1954. Papers in meteorology and geophysics, Meteorological Institute, Tokyo, v. 6, 1955: 93–94.

Soil (300 g.) was leached with 50 ml. 6N HCl on a steam bath and the filtered solution evaporated to dryness. The residue was dissolved in distilled water and an aliquot of the solution was subjected to chemical analyses, in which the sample was dried on a stainless steel planchet and its β-rays were counted. Group separation of the extract was made after addition of carriers of Ce, Ba, and Sr. Precipitation with H_2S showed very weak activity which was only a few percent of the total. The hydroxide group contained an appreciable amount of radionuclides, but most of them were insoluble when changed into fluoride forms. The filtrate of fluoride solution also showed a weak activity. The radionuclides obtained in the carbonate fraction were separated into Ca, Sr, and Ba and the Ca fraction was separated by concentrated HNO_3 and the Ba fraction obtained by precipitation as chromate. Results show radionuclides of rare earths $=9\times10^{-13}$ curie/g. $Sr^{89}=3\times10^{-13}$ curie/g., and $Ba^{140}=7\times10^{-13}$ curie/g.

Ohashi, S., and others. Pathological findings in the fatal case (the late Mr. Kuboyama) of the radiation sickness caused by Bikini ashes. An intermediate report. Iryo (Tokyo), v. 9, January 1955: 46–55 (in Japanese).

Autopsy findings and the case history are summarized from a case diagnosed as radiation sickness caused by exposure to fallout from a thermonuclear explosion. The patient died 207 days following exposure while on a fishing boat said to be located about 100 miles east of Bikini at the time of the explosion. Evidence was also found of a secondary virus hepatitis and aspergillus fumigatus pneumonia.

Otsuka, R. and Shimada, K. On the upper air current in lower latitude of the north western Pacific ocean at the beginning of March 1954. Meteorological investigation on the H-bomb experiments at Bikini Island. Journal of the Meteorological Society of Japan, v. 32, No. 7–8, 1954.

Ota, Michio, and others. Contamination of grapes by radioactive substances. Soil and plant food (Tokyo) v. 1, 1955: 43–44.

The K content of grapes was determined by measuring K^{40} content from 1951 to 1954. After the radioactive fallout in 1954, the grapes were shown to be contaminated by radioactivity.

Obo, Fujio. Radioactive rains and fishes in the Kagoshima area. Medicine and biology (Tokyo) v. 33, 1954: 19–23 (in Japanese).

Results are given of radioactivity determinations of rains, well and city water, vegetables, domestic animals, milk, and fishes. Radioactivity was determined in a radiation counter (Scientific Research Lab. model 32), at a distance of 1 cm. for 10 minutes. Samples were obtained during 18–27 May, 1954. The highest and lowest values obtained were: 4000–80 counts/min/1/ (c. p. m.) for rains, 20–0 c. p. m./cc. for well water, and 71–0 c. p. m./100 cc. for city water.

Radioactivity in the pelagic fish. Bulletin of the Japanese Society of Scientific Fisheries (Tokyo) v. 20, 1955: 907–926.

I. Distribution of radioactivity in various tissues of fish. Bulletin of the Japanese Society of Scientific Fisheries, v. 20, 1955: 907–915.

Pelagic fishes caught after atomic explosion experiment at Bikini Atoll in the Pacific were examined by radiochemical techniques. Generally the radioactivity was large in liver, kidney, gall bladder, and heart, and then in pyloric caeca, stomach, intestine, and gonad: there was little activity in skin, bone, and muscle. This order varied with species. Large radioactivity of the stomach contents did not necessarily mean large activity in the tissues, indicating considerable participation of diffusion of sea water into the fish body. Muscles from various sites showed slight difference in the activity. The dark muscle, however, showed several times as large activity as ordinary muscle.

II. Group separation of radioactive elements in fish tissues: p. 916–920.

Analytical group separation was performed with various ashed tissues of some fishes exposed to radioactive ash. The radioactivity was particularly large with element belonging to the third group, both A and B subgroups The second group showed considerable activity in pyloric caeca and kidney of skipjacks. The radioactivity of the first and fourth groups was detected in some tissues; the fifth group showed slight activity.

III. Separation and identification of zinc 65 in the muscle of skipjack.

Muscles of skipjack caught in the vicinity of the Bikini Atoll after the explosion were ashed, treated with Dowex 50, and eluted with various solvents. A fraction obtained with 0.5 percent oxalic acid and ammonium citrate (pH 4.18) contained Zn^{65}.

Saiki, Masamichi, and others. The radioactive material in the radiologically contaminated fishes caught in the Pacific Ocean in 1954. Bulletin of the Japanese Society of Scientific Fisheries (Tokyo) v. 20, 1955: 902–906.

The radioactivity of several samples of Coryphaena Hippurus caught in the southern Pacific in May 1954, after the atomic explosion at Bikini, was found, in decreasing order, in spleen, kidney, liver, pyloric caeca, heart, gill, intestine, gastric wall, ovary, testis, gastric content, red muscle, skin, vertebrae, and muscle. The red muscle of Neothunnus Macropterus showed 54.8 counts/min./0.20 g. activity on dry basis; the activity was decreased to 27.6 by soaking 25 g. muscle in 25 cc. water, and to 14.1 by soaking in 0.5 percent Na ethylenediaminetetraacetate solution. The radioactive substances in these fish tissues were found, upon analysis, to belong to the III group, particularly to III–B group. Examination of synchroscope patterns by scintillation counter indicated the presence of Zn^{65} among the radioactive substances. Sr^{90} was suggested to be present in very small amount.

Shimizu, Kentaro. Hiroshima and Bikini. Oriental economist, (Tokyo) v. 22, July 1954: 344–346.

A report by the doctor who treated the Japanese fisherman injured by the Bikini test.

Shinjiro, T. Death ash, experience of 23 Japanese fishermen. Japan quarterly, v. 2, 1955: 37.

Tajima, Eizo. Why fishing boats were contaminated by radiation. Shizen, December 1954 (in Japanese).

Many Japanese fishing boats were examined with a G–M counter following the Bikini tests of 1954. Decks and other washable parts were weakly irradiated, lamps and other unwashable parts were strongly irradiated. Directional relationships of contaminants on individual ships coincided with those of the prevailing winds. Ships to the west of Bikini averaged 123 cpm; those to the east, 1,800 cpm. Activity to the east shows sharp rises on test dates, sharp drops on other dates. To the west of Bikini, a strip of water from 15° N. to about the equator is contaminated, and the contamination of the boats may have been due to this.

Takase, Akira. Distribution of radioactivity in various tissues of fish and group separation of radioactive elements in them. Bulletin of the Institute of Public Health (Tokyo), v. 4, 1955: 27.

Radiological studies were made of several kinds of fish which had been caught in the fishing ground including the area from longitudes 128° East to 162° East and from latitudes 3° North to 33° North during the period from April 25 to July 7, 1954, and which had been rejected as highly contaminated radiologically at the time of landing.

———. Separation of the radioactive elements in the muscle of skipjack by ion-exchange resin, and confirmation of the presence of radioactive zinc. Bulletin of the Institute of Public Health (Tokyo), v. 4, 1955: 22–26.

An ashed sample of skipjack muscle caught in June 1954, near Bikini Atoll was analyzed for elements separated by an anion-exchange method (Dowex 50) with the use of 0.2N HCl, 0.5 percent oxalic acid, and 2 percent NH_4 citrate as eluents at each pH value of 3.53, 2.18, 4.60, 5.02, 5.64, and 6.42.

Yamada, Yoshio, and others. Measurement of radioactivity in contaminated crops. Soil and plant food (Tokyo), v. 1, 1956: 25–26.

A method called the direct method was developed to correct for natural K^{40} radiation in plant samples. The K_{20} content of the ashed sample is determined by flame-photometry. The radioactivity in a 100-mg. sample is measured and the natural radioactivity from K^{40} determined by calculation subtracted. Tea samples tested gave evidence of contamination by radioactive fallout.

Yamamoto, Ryozaburo. Atmospheric oscillation caused by the H-bomb. Astronomy and meteorology (Tokyo), v. 20 No. 8, 1954.

Yamasaki, F., and Koneko, H. On the artificial radioactivity in rainwater. Journal of the Scientific Research Institute (Tokyo), v. 49, June 1955: 137–143.

Rainwater in Tokyo was examined for artificial radioactivity from April to December 1954. The most active rain occurred on May 17, as reported by Miyake. Wide variability in activity was noted from sample to sample, even in the same rainfall. In a single rainfall, the specific activity appeared to be negatively correlated with rainfall intensity. Rainwater collected in the early stage of a single rainfall did not always show the strongest radioactivity. Exponent in the decay law for mixed fission products ranged from −0.9 to −1.4 in five rain samples investigated.

Yatazawa, Michihiko, and Ishihara, Takashi. Radioactive contamination of plants in Japan covered with fallout from H-bomb detonations in March–May 1954 at Bikini Atoll, Marshall Islands. I. Distribution of deposited radioactivity. Soil and plant food (Tokyo), v. 1, 1955: 21–22.

In May 1954 rains contained radioactivity up to 0.2 muc. per liter. The provisional permissible level of unknown radioisotopes in H_2O is given as 10^{-7} muc./ml. for β— or γ— emitters. The safety factor for these values is at least 100. From these values the permissible level for foods was calculated as 0.22 muc./day. Food plants tested ranged 0–1.25 muc./10 g. dry matter. It is concluded that serious radioactive contamination of plants was probable.

RUSSIAN

Marei, A. N. Radioactive wastes and public health problems. Meditsinskaia radiologiia (Moscow) v. 1, July–August 1956: 3–7 (in Russian).

Yakobson, I. I. Initial radioactive investigations in Russia. Akademiya Nauk Uzbek S. S. S. R., v. 5, 1953: 118–135.

SWEDISH

Muller, Hermann J. The manner of dependence of the permissible dose of radiation on the amount of genetic damage. Acta radiologica (Stockholm), v. 41, January 1954: 5–20.

Morgan, K. Z. Maximum permissible concentration of radioisotopes in food, water and air and maximum permissible equilibrium amounts in the body. Acta radiologica v. 41, January 1954: 30–46.

Warners, C. J. Note on radioactive compounds in the atmosphere. Tellus (Stockholm) v. 7, August 1955: 403–404.

Since January 1, 1955, daily measurements of the radioactivity of the air have been made by the Royal Netherlands Meteorological Institute. The highest values noted were in the period April 28–30, in tropical air transported from the south, and reached 27.4 x 10^{-10} μc per liter.

OTHER NATIONS

Akpinar, S. and Akpinar, R. Radioactive precipitations in Istanbul and Uludag (Turkey) Istanbul, v. 20C, 1955: 287–302 (in English).

From the decay curves of fission products in rain and snow, it was possible to determine the date of announced United States atomic tests and unannounced Russian tests with a probable maximum uncertainty of ± 8 days.

Gabites, J. F. Drift of radioactive dust from the British nuclear bomb test in October 1953. New Zealand Journal of science and technology (Wellington, N. Z.) v. 36B, September 1954: 160–165.

To account for the second wave of radioactivity which reached Wellington 38–54 hours after the blast in Woomera, Australia, it is apparently necessary to assume the material started from a height of 22,000–25,000 feet. The dust was dispersed downward by eddy diffusion and carried by lower level atmospheric circulations to New Zealand. The first wave which occurred 30 hours after the blast remains unexplained.

Holubec, K. Lesson of Hiroshima and Bikini. Časopis lékařů českých (Prague) v. 95, May 11, 1956: 518–525 (in Czech).

Levi, H. Natural background radiation and radioactive fallout. Ugeskrift for laeger (Copenhagen) v. 117, December 15, 1955: 309–311 (in Danish).

Piédrola, Gil G. and Amaro, Lasheras J. Precipitation of radioactive dust (fall-out); necessity for a national detection and defense organization. Medicina colonial (Madrid) v. 28, October 1, 1956: 229–231 (in Spanish).

Rose, D. C. and Katzman, J. Radioactive deposits found at Ottawa after the atomic explosions of January and February 1951. Canadian journal of physics, v. 30, March 1952: 111–116.

Gamma-ray measurements indicated that considerable radioactive matter, apparently consisting of fission products, fell on January 29, and on February 7, 1951. The earlier fall came from an explosion in Nevada on January 27, and the February 7 material appeared to be 3 to 5 days old. The quantity was estimated to be equivalent to 1 γ Ra per square mile.

Ryder, N. V., and Watson-Munro, C. N. The detection of radioactive dust from the British nuclear bombs of October 1953. New Zealand journal of science and technology (Wellington, N. Z.), v. 36B, September 1954: 155–159.

Thirty hours after the nuclear explosion near Woomera, Australia, in October 1953, a peak in β-activity of 50 cpm was observed in Wellington, New Zealand, on an air filter. A second peak of 6 cpm was observed a few hours later. Decay curves indicated the activity was fission product activity.

Szalay, A. Unusual radioactivity observed in the atmospherical precipitation in Debrecen (Hungary) between April 22–December 31, 1952. Acta physica (Budapest, Hungary) v. 5, 1955: 1–14.

The authors investigated, by means of an end-window β-counter tube equipment the activity of precipitation fallen in Debrecen between April 22 and December 31, 1952. At times the precipitations showed radioactivity which proved to be due to fission products deriving from atomic explosions. These anomalous activities, with a lag of a few days, were in correlation to time with the atomic explosions published during the same period.

Popular Periodical Articles

A-bomb survivors due to lose quarter of life. Science news letter, v. 69, March 31, 1956: 201.

All about A-bombs, fallout, dangers in the future. U. S. news and world report, v. 38, April 29, 1955: 96ff.

Extracts from transcript of hearings held by the Joint Committee on Atomic Energy, April 15, 1955.

Amrine, Michael. Atomic clouds over America. Science digest, v. 33, June 1953: 23–30.

———. Fallout, can man survive? Progressive, v. 21, February 1957: 6–10.

The author is deeply concerned about the dangers of radioactive fallout from bomb tests, and questions the administration's apparent lack of concern.

Armagnac, A. P. How the H-bombs spread radioactivity. Popular science, v. 166, April 1955: 144–145.

———. Will bomb dust endanger your health? Popular science, v. 170, February 1957: 163–167.

Arnold, James R. Effects of the recent bomb tests on human beings. Bulletin of the atomic scientists, v. 10, November 1954: 347–348.

"The death of a Japanese fisherman on September 23 [1954] not only shocked the world but made the radioactive fallout seem the most fearful consequence of the H-bomb."

As the bomb goes off and the cloud heads east * * * science tackles radiation peril. Life, v. 38, March 21, 1955: 32–39.

Ascoli, Max. There must be an end to it (editorial). The Reporter, v. 16, May 16, 1957: 8–9.

Urges end of bomb testing.

Reprinted in extension of remarks of Charles O. Porter, Congressional record [daily edition] v. 103, May 13, 1957: A3637–A3638.

Atom outgrows its proving ground. U. S. news and world report, v. 36, March 26, 1954: 45–47.

"Thousands of square miles now can be damaged by radioactive particles, from one H-bomb, carried by the wind."

The atom: "Unpleasant debate." Newsweek, v. 48, November 26, 1956: 64–66.

Various experts in the atomic energy field comment on dangers of strontium 90.

Atomic radiation: the r's are coming. Time, v. 58, June 25, 1956: 64f.

Ban the "dirty bomb," (editorial). New Republic, v. 136, April 29, 1957: 3–4.

Proposal that the United States should seek an international agreement to limit the radioactive debris from bomb tests.

Bengelsdorf, Irving. Can the atom change the weather? Saturday review, v. 39, July 7, 1956: 31–37.

Berninger, Karl. The bomb and the weather. Contemporary issues (London) v. 6, March-April 1955: 114–116.

Maintains that atomic tests do influence weather conditions; points out meteorological phenomena to bolster this thesis.

Berrill, Norman J. A Canadian scientist asks have we gone too far with the atom tests? Maclean's magazine (Toronto) v. 68, July 9, 1955: 7–9 ff.

"Every time an atomic explosion occurs anywhere in the world more highly dangerous radioactive particles are set free. Professor Berrill says it's time our leaders told us the truth—that even if we survive we may breed a future race of morons or monsters."

———. The menace of radiation. Atlantic monthly, v. 196, October 1955: 49–54.

"Radioactive fallout from test explosions of atomic bombs has made clear to Americans that nuclear warfare would mean annihilation of large areas. Less well understood is the fact that leakage of radioactive materials resulting from careless operation of atomic powerplants and other peacetime uses of nuclear energy can be just as deadly."

Bishop, R. Can fish survive the atom? Field and stream, v. 61, June 1956: 70–71.

Blifford, Irving H. Total radioactive fallout. Science news letter, v. 69, April 28, 1956: 267.

Bomb watchers; radioactive dust in Japan. Time, v. 67, April 16, 1956: 56 f.

Bombs and the species. Economist, (London) v. 175, May 14, 1955: 557–558.

Cattle caught in fallout cancerless. Science news letter, v. 71, April 20, 1957: 248.

Danger, strontium 90. Newsweek, v. 48, Nov. 12, 1956: 88 f.

Data on atomic radiation transmitted to U. N. committee. United States Department of State bulletin, v. 35, October 29, 1956: 687.

Davis, Helen M. Hazards of smog. Science news letter, v. 67, May 7, 1955: 298–299f.

"The problem of preventing harmful radioactive fallout is like that of smoke control. Filters and precipitators can reclaim valuable wastes from industrial chimneys."

De Roos, Robert. What are we doing about our deadly atomic garbage? Collier's, v. 134, August 20, 1954: 28–34.

Espinasse, Paul G. Biology and the bomb. Nation, v. 180, June 25, 1955: 579–581.

Despite the comforting statements of AEC on the background radiation increase caused by atomic tests, this author points out that a very little upset in the "balance of nature" might have very serious ramifications.

Facts about A-bomb fall-out. U. S. news and world report, v. 38, March 25, 1955: 21–26.

"Not a word of truth in scare stories over tests."

Abridged in Reader's digest, v. 66, June 1955: 22–24.

Fallout detector developed by Navy, Army-Navy-Air Force register, v. 76, December 10, 1955: 6.

Fallout hazard to grow; strontium–90. Science news letter, v. 71, February 23, 1957: 115.

Ferreus. Courage or perdition? The 14 fundamental facts of the nuclear age. Review of politics, v. 16, October 1954: 395–411.

Each fact is discussed in the light of its relationship to future policy.

Foulks, J. G. Do H-bomb tests threaten human survival? Canadian chemical processing, v. 39, April 1955: 29–30 f.

Glass, Bentley. The hazards of atomic radiations to man, British and American reports. Bulletin of the atomic scientists, v. 12, October 1956: 312–317.

Discussion of reports issued by National Academy of Sciences and British Medical Council on effects of radiation.

H-bomb contamination. Science news letter, v. 67, February 26, 1955: 134.

H-bomb such as that exploded in Marshall Islands would contaminate 7,000 square miles with radioactivity.

Turbulence in the atmosphere could make the area threatened twice as wide as the cigar shape (up to 40 miles) described in official AEC report.

H-bombs without fallout (editorial). Bulletin of the atomic scientists, v. 12, September 1956: 234ff.

Claims that statements on minimum fallout from H-bombs confuses public thinking.

Hayasaka, I. Appeal to the western evolutionists against the hydrogen bomb by the Japanese Society for the Study of Organic Evolution. Science, v. 120, October 8, 1954: 9A.

Same: Bulletin of the atomic scientists, v. 11, January 1955: 34.

Hicks, C. B. How dangerous is fallout? Popular mechanics, v. 106, November 1956: 198–202ff.

Holmes, Robert H. Latest about after-effects of A-bomb. U. S. news and world report, v. 38, May 13, 1955: 60–68.

The director of the Atomic Bomb Casualty Commission describes the present situation of the Hiroshima survivors, the effects of the bombing upon their offspring, and the present condition of Hiroshima itself.

Holsman, B. Have atom bomb tests fouled up the weather. Look, v. 17, August 11, 1953: 32–33.

Hot stuff: big hurdle for atom power. Business week, July 23, 1955: 72–82.

Problem of radioactive wastes discussed.

How dangerous are the bomb tests? Time, v. 69, June 3, 1957: 62, f.

How radiation can affect our children. Challenge, v. 5, October 1956: 54–59.

Interview with Warren Weaver, chairman, Committee on Genetic Effects of Atomic Radiation, National Academy of Sciences.

How safe humans? Newsweek, v. 49, February 18, 1957: 71.

Humanitarian bombs; minimum widespread fallout. New republic, v. 135, July 30, 1956: 3–4.

Inglis, David R. Prospects for stopping nuclear tests. Bulletin of the atomic scientists, v. 13, January 1957: 19–23.

A fervent plea for the ending of thermonuclear tests. The author believes the chief reason for ending them is not the radioactive fallout problem as much as it is the vital need to get on with some sort of disarmament.

Jacobs, Paul. Clouds from Nevada; a special report on the AEC's weapons-testing program. The Reporter, v. 16, May 16, 1957: 10–29.

Lengthy article describing the hazards to people living near the atomic testing grounds in Nevada.

Katz, Sidney. How serious is the threat of radiation? Maclean's magazine (Toronto), v. 69, December 8, 1956: 18–19, 115–118.

Knapp, H. A. South Woodley looks at the H-bomb. Bulletin of the atomic scientists, v. 10, October 1954: 306–311.

Lang, Daniel. Fallout. New Yorker, v. 31, July 16, 1955: 31–41.

Lapp, Ralph E. Civil defense faces new peril. Bulletin of the atomic scientists, v. 10, November 1954: 349–351.

Assessment of fallout and its impact upon civil defense. Urges that the Federal Government release classified data on fallout to provide guidance to civil defense organizations.

———. Confused alert. Life, v. 38, June 27, 1955: 48–49.

———. Fallout and candor. Bulletin of the atomic scientists, v. 11, May 1955: 170f.

———. Global fallout. Bulletin of the atomic scientists. v. 11, November 1955: 339–343.

Following a review of the fallout literature, the author concludes that, although many in the Pentagon maintain that blast and heat are the real military effects of nuclear weapons, it appears that local fallout from the uranium superbomb is a potent weapon. Remote fallout from even a large scale nuclear bombing, however, does not appear to be a global hazard to humanity.

———. Humanitarian H-bomb. Bulletin of the atomic scientists, v. 12, September 1956: 261–264.

"The superbomb can be designed to be relatively clean [by giving most of the energy to fusion, which produces nonradioactive helium] or very dirty [by maximizing fission]. The former would be desirable in the test series whereas the latter would seem to fulfill the requirements of a strategic weapon.

———. Radioactive fallout. Bulletin of the atomic scientists, v. 11, February 1955: 45–51; June 1955: 206–209.

———. Radioactive fallout. New Republic, v. 132, February 14, 1955: 8–12. Discussion. *Ibid.*, v. 132, April 4, 1955: 23.

———. Strontium limits in peace and war. Bulletin of the atomic scientists, v. 12, October 1956: 287–289, ff.

Questions whether MPC figures released by AEC are in fact safe.

Reply: New republic, v. 135, October 15, 1956: 5–6.

Laurence, William L. Promise of tomorrow. Colliers, v. 136, December 23, 1955: 46, ff.

Libby, Willard F. The radioactive fallout. Civil defender, v. 1, December 1955: 16.

Speech given at Annual Meeting of National Association of State Civil Defense Directors, November 3, 1955.

———. Radioactive fallout. National safety news, November 1955: 60, ff.

Remarks prepared for delivery at the fourth annual conference of the United States Civil Defense Council, Hotel Statler, Boston, Mass., September 29, 1955.

———. Radioactive fallout, with editorial comment. Bulletin of the atomic scientists, v. 11, September 1955: 256–260.

Speech given before Alumni Reunion at the University of Chicago, June 3, 1955.

———. Radioactive fallout and radioactive strontium. Science, v. 123, April 20, 1956: 657–660.

Based on a speech given at Northwestern University, Evanston, Ill., January 19, 1956.

The radioactivity produced in magaton weapons is placed largely and immediately in the stratosphere while the smaller kiloton weapons produce clouds which do not reach into the stratosphere, and the bulk of their radioactivity is left in the troposphere. In the troposphere where rain occurs, particulate matter will be washed down in an period of days or weeks. It is easy to show that one-tenth inch of ordinary rainfall will probably remove almost completely all particulate matter except that which is so small as to be nearly of molecular dimensions.

Also in Ordnance, v. 41, July–August, 1956: 40–43, with title: Fallout from the A-bomb.

Reprinted in Congressional Record [daily edition] February 20, 1956: A1577–A1579.

Libby, Willard F. What the atom can do to you and for you. U. S. news and world report, v. 42, May 17, 1957: 64–75.
A member of the Atomic Energy Commission gives "authoritative" answers to such questions as: Should we worry over fallout from atomic tests? When will we get usable atomic power? Are new atomic weapons in sight?

———, and others. Forum on the Schweitzer declaration. Saturday review, v. 40, May 25, 1957: 8–13, 35–36.
There are four articles in this forum concerning atomic tests and the dangers of the resulting radiation. One is Dr. Libby's answer to the protest of Dr. Albert Schweitzer.

Lodge, Henry C. Data on atomic radiation. United States Department of State bulletin, v. 33, July 11, 1955: 54.

Lorentz, Pare. Fight for survival. McCalls, v. 84, January 1957: 28–29.
Dangers of radioactive poisons from bomb tests and radioactive waste.
Reprinted in Congressional Record [daily edition] January 28, 1957: A498–A500; February 7, 1957: A860–A862.

MacLeod, Iain. Nuclear explosions. Vital speeches, v. 21, April 15, 1955: 1165–1168.
House of Commons discussion on the effects of increased atmospheric radioactivity on mankind's hereditary constitution.

Mansfield, Michael J. From A-bomb to U-bomb. New leader, v. 38, May 23, 1955: 16–20.
Senator Mansfield, concerned about the dangers to life and health caused by atomic and hydrogen bomb tests, suggests the possibility of ceasing such experiments.

McWilliams, Carey. Perils unknown. Nation, v. 180, April 9, 1955: 302–306.
Severe indictment of the administration for its failure to report to the people on the dangers of atomic war, and for its failure to become concerned by the increased atmospheric radioactivity caused by repeated atomic tests.

Measured fallout; control of fallout. Time, v. 68, July 30, 1956: 61.

Miller, Harold. Nuclear weapons and genetics. Fellowship, v. 21, September 1955: 5–9.
Reprint of article originally written for British magazine Reconciliation by a medical physicist of Sheffield, England.

Miller, Robert W. Safeguarding children from radiation risks. Children, v. 3, November–December 1956: 203–207.

Muller, Hermann J. Race poisoning by radiation. Saturday review, v. 34, June 9, 1956, 9, ff.
Article on "local" and remote fallout and possible effects. Discussion of mutations produced by radiation.
Reprinted in Congressional Record [daily edition], v. 103, May 15, 1957: 6222–6225.

Next to last words. New republic, v. 136, May 6, 1957: 4.

New dangers of H-bomb. Science news letter, v. 67, March 5, 1955: 147.
Discussion of AEC's report on the Effects of High-Yield Nuclear Explosions.
Dreaded fission products come from A-bomb material which is present in only small amounts in H-bombs, if at all. Therefore, fallout problem probably less serious with H-bombs, except for their greater size.

Nishiwaki, Yasushi. Death in the rain. Nation, v. 180, August 6, 1955: 111–114.

Nordheim, L. W. Tests of nuclear weapons. Bulletin of the atomic scientists, v. 11, September 1955: 253–255, f.
Testing of atomic weapons is essential to their development. On the other hand such testing may prove dangerous because of radioactivity. The author examines the evidence on both sides and concludes that the dangers of the tests are much less than the dangers of being caught with inadequate weapons.

Now there's a warning about too much X-ray; scientists say it's far more dangerous than "Fallout." U. S. news and world report, v. 40, June 22, 1956: 60–70.
The National Academy of Sciences' report, June 13, 1956, contains estimate that the average person receives 30 times as much radiation from X-rays as from fallout.

Nuclear weapons tests; statements by scientists. Science, v. 124, November 9, 1956: 925–926.

The peril of strontium-90. Time, v. 69, May 6, 1957: 24.
Willard Libby's answer to Dr. Schweitzer on dangers of fallout.
Passin, Herbert. Japan and the H-bomb. Bulletin of the atomic scientists, v. 11, October 1955: 289–292.
This article tells how the accident in which several Japanese fishermen became victims of fallout has poisoned relations of Japan with the United States.
Poling, J. Men who really know about bomb-dust radiation! Better homes and gardens, v. 35, May 1957: 71f.
Radiation dangers. New republic, v. 136, May 20, 1957: 8–9.
"What is known about the hazards of bomb tests? What risks is it reasonable to run?"
Radiation hazards accumulate. F. A. S. newsletter, No. 57–4, April 15, 1957: 1 f.
Summary of recent news articles on nuclear testing and fallout.
Radioactive man. Economist (London) v. 180, July 21, 1956: 206.
Real power of the super-bomb; radioactive fallout. New republic, v. 131, November 8, 1954. 3–4.
Report on Hiroshima: Thousands of babies, no A-bomb effects. U. S. news and world report, v. 38, April 8, 1955: 46–48.
"From medical records of the only people who have lived through an atomic blast. Their children are normal. Radiation burns have healed. There's no radiation blindness."
Robinson, Donald. If H-bombs fall. Saturday evening post, v. 229, May 25, 1957: 25, 105–113.
"Exactly how would Americans behave under a thermonuclear attack? A special disaster-research team has come up with these surprising answers to an urgent question."
Rotblat, Joseph. The atomic challenge; radiological hazards. The new statesman and nation (London) v. 50, August 13, 1955: 177–178.
Discussion of (1) radioactivity, and its distribution resulting from fission; (2) radiation damage in a fallout area; (3) radiological hazards involved in peacetime uses of atomic energy.
Round the world tracer; tracing radioactive air masses. Time, v. 67, March 12, 1956: 73.
Rovere, R. H. Letter from Washington, perils of thermonuclear warfare. New Yorker, v. 31, February 26, 1955: 98–100.
Rs from the sky; fallout from test, March 1, 1954. Time, v. 65, June 20, 1955: 66.
Russell, Bertrand, Man's peril from the hydrogen bomb. The Listener (London) v. 52, 1954: 1135.
Safer H-bomb. U. S. news and world report, v. 41, July 27, 1956: 8.
Schweitzer, Albert. A declaration of conscience. Saturday review, v. 40, May 18, 1957: 17–20.
Complete text of Dr. Schweitzer's plea for control over the testing and military uses of atomic weapons. Stresses the radiation hazards involved.
Reprinted in Congressional Record [daily edition], v. 103, May 15, 1957: 6217–6221.
Sherrod, Robert. The grim facts of the H-bomb accident. Saturday evening post, v. 227, July 17, 1954: 20–21 ff.
"What really happened to those Japanese fishermen who wandered too close to the hydrogen blast at Bikini."
Stapleton, Bill. Navy vs. the H-bomb. Collier's, v. 134, July 23, 1954: 19–25.
"The heretofore secret story of 10 warships that were unexpectedly trapped in radioactive dust, but saved themselves with a shower of sea water."
Stevenson, Adlai E. Why I raised the H-bomb question. Look, v. 21, February 5, 1957: 23–25.
"The defeated Democratic candidate warns that H-bomb tests are still exposing all of us to a substance so lethal that just one spoonful would poison everyone on earth."
Straight, Michael. The ten-month silence. New republic, v. 132, March 7, 1955: 8–11.
Why did the administration wait 10 months after the H-bomb tests to tell the people of the lethal effects of fallout. Author criticizes AEC for its emphasis on secrecy.
Strauss, Lewis L. Truth about radioactive fallout. U. S. news and world report, v. 38, February 25, 1955: p. 35–38.
Strontium 90. (Editorial) Commonweal, v. 65, March 1, 1957: 556.

Teller, Edward. The nature of nuclear warfare. Air force, v. 40, January 1957: 43–47.
Main thesis: "If we so prepare ourselves that a terrible attack could hurt us but could not destroy us, then such an attack, I believe, will never come."
This dangerous planet. Newsweek, v. 45, January 17, 1955: 52–54.
This article is a consideration of the following question: "Is the lingering radiation spread around the earth by past or future H-bombs capable of generating [a] biological catastrophe, an insidious weakening of the human race, perhaps its extinction?"
Tunnel to nowhere. Atlantic, v. 199, April 1957: 6, f.
Article on radioactive waste disposal.
Unpleasant debate. Newsweek, v. 48, November 26, 1956: 64-66.
Describes debate among scientists on amounts of strontium 90 that can safely be absorbed by humans.
Same article, abbreviated with title, Will strontium 90 poison the world? Science Digest, v. 41, February 1957: 29–33.
Unseen cloud; Sir Anthony's Bikini. Economist (London), v. 179, June 16, 1956: 1077–1078.
Waddington, C. H. Atoms and genes. Nation, v. 183, August 18, 1956: 137–140.
Discussion of reports issued by National Academy of Sciences and British Medical Council on effects of radiation. Emphasizes dangers of fallout.
———. Peril from A-dust; with editorial comment. Nation, v. 180, February 19, 1955: 155–157.
War on the unborn. Economist (London), March 26, 1955: 1072.
Westergaard, M. Man's responsibility to his genetic heritage. Bulletin of the atomic scientists, v. 11, November 1955 : 318, f.
What the H-bomb fuss is all about. U. S. News and World Report, v. 41, October 26, 1956: 126–134.
Statements by Stevenson, Dewey, Dulles, Nixon, and Kefauver.
What will radioactivity do to our children? U. S. News and World Report, v. 38, May 13, 1955: 72–78.
Interview with Dr. H. J. Muller. Biggest danger is in the careless use of X-rays.
What you should know about danger from X-rays. U. S. News and World Report, v. 40, June 29, 1956: 44–48.
What's back of the "fallout" scare. U. S. News and World Report, v. 42, June 7, 1957: 25–28.
"Official position is that United States cannot afford to give Russia a monopoly on nuclear experiments.
Why is all the agitation directed against British and American bomb tests—and not against the tests conducted by Soviet Russia?"
White, E. B. Letter from the East. New Yorker, v. 32, November 3, 1956: 198–202 ff.
Wilson, E. Raymond. Japan's atomic fears. Christian century, v. 74, May 1, 1957: 553–554.
This article tells of Japan's efforts to be the "conscience of the world" regarding atomic and hydrogen bomb tests. The author feels that Japan's efforts toward leadership in abandoning such tests and in disarmament are not as effective as they might be.

Newspaper Articles

Atomic peril cut in fallout data. New York Times, October 13, 1956: 3.
Report of Dr. Libby's speech on radiation at dedication of new building of American Association for Advancement of Science.
Baldwin, Hanson W. A military atom problem. New York Times, June 14, 1956: 14.
An analysis of the National Academy of Sciences report and the possible effect of its findings on defense policies.
Cahan, William G. Effects of radioactivity. New York Times, October 31, 1956: 32.
A physician writes a letter to New York Times on possible role of radioactivity in inducing cancer.
Cowen, Robert C. Atomic radiation: new challenge for humanity? Christian Science Monitor, August 3, 1956: 9.
Review of reports issued by National Academy of Sciences and British Medical Council.

Cutler, Robert. Reply to Finletter on the bomb. New York Herald-Tribune, October 12, 1956: 1, f.

Danger level of strontium. New York Times, October 28, 1956: 10E.

Letter to the editor from eleven Professors of Chemistry, Physics and Biology.

"The question of what is to be considered as the maximum permissible concentration (M. P. C.) of radioactive strontium in human bone is one which probably will not receive a definite answer for many years. It is the answer to this question that is the basis of most of the disagreements that are not of political origin. * * *"

Engle, William. Radiation—your friend or foe? The American Weekly, January 6, 1957: 4–6.

Fallout of a Soviet bomb. The Times (London) April 22, 1957: 6.

Recent Soviet nuclear explosion was of a kind calculated to release the maximum quantity of radioactive fission products into the atmosphere, observes Professor Shiokawa, of Shizuoka University, Japan.

Fallout tests at Harwell. The Times (London) May 30, 1956: 7.

Report on measurements of radioactivity caused by thermonuclear explosions.

Fallout: the peril point? Washington Sunday Star, June 2, 1957: A23.

Gallup, George. Public favors H-tests' halt, if—Washington Post, May 19, 1957: E5.

Germans cite gain in hydrogen fusion. New York Times, March 3, 1957: 22.

Team of German nuclear physicists working in Hamburg report solution of problem of hydrogen fusion without an atomic bomb trigger, a method which does not produce radioactive fallout.

Harsch, Joseph C. Dangers of fallout. Christian Science Monitor, April 25, 1957: 1.

Henry, Thomas R. Does radiation exposure hasten aging? Washington Star, September 3, 1956: A8. (Vistas in Science).

Henry, Thomas R. Scientists warn world of genetic atom peril. Washington Star, April 23, 1957: A5.

Account of symposium on genetics at 94th annual meeting of the National Academy of Sciences.

Increased radioactive fallout on Britain. Manchester Guardian Weekly, May 31, 1956: 3.

Report of Manchester Guardian's scientific correspondent of a tour of Britain's Atomic Energy Authority's Research Establishment at Harwell. Mentions methods used to measure fallout.

Is fallout good? (editorial). Washington Post, April 27, 1957: A12.

Questions Dr. Libby's statements on relative harmlessness of fallout.

Kaempffert, Waldemar. How fallout from atomic explosions could be controlled is an unanswered question. New York Times, July 29, 1956: E9. (Science in review.)

Krock, Arthur. H-bomb issue raises problem for voters. New York Times, October 21, 1956: E3.

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1. Japan, Canada and Norway: Called for advance registration of all nuclear tests and for observation of the radiation results by U. N. experts.

2. Sweden: Proposed that all nuclear explosions be banned until the U. N. Scientific Committee on the Effects of Radiation had completed studies now in progress. This would amount to a 2-year moratorium.

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activity in residue, 72.5 percent in filtrate. Using Whatman-41 and HA molecular filter (efficient down to 0.2μ) 83.4 percent of activity in residue, 16.6 percent in filtrate, indicating significant portion of activity on particles between 0.2 and 0.7 μ. Other studies on solubility of fallout and on variation in rainout during course of storm, also gamma energy spectrum of rainout.

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	Curies
Settled dust, solid residue	.04
Settled dust, filtrate	.14
Precipitation, solid residue	.22
Precipitation, filtrate	.05
Total	.97

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In 3 out of 23 flights at 6,000 meters over France in July 1946, an unknown radioactive material with a half-life of 25 ± 5 hours was detected. Average concentration 0.013×10^{-16} curies/cm³ radio equivalent. The first detection was on July 20, 1946, 20 days after the first explosion at Bikini. Similar material was detected in 6 out of 15 flights in July, 1948, the first on July 2, 75 days after the Eniwetok test.

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Analysis of the "Bikini ash." (Special collection of papers). Tr. from Japan Analyst (Tokyo) v. 3, 1954: 333–368, 75 pp. (AEC–tr–2104).

Kimura, Kenjino. Introduction to special collection of papers: 1–6.

Incident of the Bikini ashes and the fishing boat is reported. Experiences on the boat are recorded, and fallout analyses are compared with those of Nagasaki and Hiroshima.

———. Radiochemical analysis of "Bikini ashes" fallen on board the No. 5 Fukurya Maru on March 1, 1954: pp. 7–27.

Comprehensive analysis was done in order to find the proper method of medical treatment for the victim fishermen on board. Analysis was started on March 18, and ash was found which consisted mostly of $Ca(OH)^2$, activity of which was 0.37 mc./g. on April 23. Cations of the third group (especially rare-earth metals) and fifth group were found to have strong activity by chemical separation. Fractions of each group, anions, Zr and Nb fraction, and U fraction were separated by an ion-exchange method.

Shiokawa, Takanobu, and others. Radiochemical studies on "Bikini ashes" (March 1, 1954): pp. 28–42.

Decay characteristics of the ashes which were brought back by the crew of the Fukuryu Maru No. 5 were; untreated ash $I = ct^{-1.81}$, water solution part $t^{-2.71}$ insol. part $t^{-1.68}$. Radioactive species separated by chemical method with carrier or collector were; nuclide, activity of nuclide (counts/min.) /activity of original sample (counts–min.), and the date of separation, Sr^{89} $6{,}000/80 \times 10^4$, April 24; Zr^{95}, $280/80 \times 10^4$, –; Ag^{111}, $200/200 \times 10^4$, April 14; Ru^{103}, $2.300/25 \times 10^4$ etc.

Yamatera, Hideo, and others. Radioactive dust from No. 5 Fukuryu Maru: pp. 43–54.

Analyses of radioactive dust collected on board No. 5 Fukuryu Maru were done by chemical separation and measurement of γ-ray energy and half-life of each species. Results are summarized as follows, radioactive nuclide and approximate percent of radioactivity given: Ru^{103}, 4.3–57; Ru^{106}, 1.4; Te^{129}, 1.3; I^{131}, 4.5; I^{132}, 1.0; Te^{132}, 1.0; etc.

Kiba, Toshiyasu and others. Radioactive substances found on the contaminated fish: pp. 55–60.

Radiochemical investigation was done on the substance collected from the surface of tuna fish which were brought back by the No. 5 Fukuryu Maru. Most of radioactivity was found on the scales, which could not be decontaminated by treating with H_2O; 80 percent of activity was removed by washing dried scales with 3N HCl. Paper chromatographic separation of the HCl fraction showed the presence of Ba^{140}, Sr^{89}, Te^{132}, and probably Zr^{95}, La^{140}, and rare earths.

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Shimizu, S., and others. Radioactive dust from nuclear detonation. Survey of the radioactive contamination of the No. 5 Fukuryu Maru: pp. 1–3.

A collection of reports on investigations on No. 5 Fukuryu Maru, a fishing ship which was in the vicinity of the Bikini atoll when nuclear detonation occurred on March 1, 1954. The radiation dosage rate of contamination observed for combined β- and γ-radiation at every part of the ship on March 19, April 21, and May 16 is recorded. The av. value of total γ-dosage for the crew was supposed to lie between 200 and 500 r.

Kikuchi, Takehiko, and others. Properties and size of the radioactive ashes obtained from the No. 5 Fukuryu Maru: pp. 4–11.

Size and radioactivity of the ashes collected from the ship have been measured. Ashes consist of particles which appeared dark when observed through an occular microscope. When observed by side illumination the particles appeared white and several black spots were seen on the surfaces.

———. Radioautographic studies of the radioactive ashes obtained from the No. 5 Fukuryu Maru: pp. 12–17.

Radioautographic studies have been made of the radioactive ashes obtained from the ship by the use of X-ray film, radioautographic stripping plates, and plates of α-emitters. The radioactivity was found not proportional to the size of the particle, and the distribution of radioactivity in each particle was not uniform.

———. Radioautographic studies of the materials obtained from the No. 5 Fukuryu Maru contaminated by radioactive ashes: pp. 29–34.

The contamination was associated with the presence of small radioactive particles. Although these particles were easily scattered, it was difficult to remove them completely. The particles did not penetrate into the interior of clothes of fine meshes. Decontamination by washing with sea water was not perfect.

———. The contamination of the fishes caught by the No. 5 Fukuryu Maru and the foods manufactured from these fishes: p. 35–38.

The radio-contaminated tunas and other fish caught by the ship in the vicnity of Bikini Atoll were studied. The contamination was caused directly by radioactive ashes and was limited to the surface of the fish. No radioactivity was detected in muscles and bones. The contamination of tuna expressed as Co^{60} was 10^{-2}—10^{-3} microcurie per square centimeter of skin and 10^{-1} microcurie per g. scales.

Ishibashi, Masayoshi, and others. Radiochemical analysis of the Bikini ashes: pp. 35–39.

The following nuclides were detected in the Bikini ashes by radiochem. procedures: Ca^{45}, Sr^{89}, Y^{91}, Zr^{95}, Ru^{103}, Nb^{95}, Rh^{103m}, Ru^{106}, Te^{129}, I^{131}, Ba^{140}, La^{140}, Ce^{144}, Pr^{144}, and U^{237}. The ion-exchange method was used for analysis of contaminated rain water which fell on the Kyoto area on May 16, 1954 from which the presence of Sr^{89}, Zr^{95}, and Ba^{140}, was detected. Rare earths seemed also to be present.

———. Analysis of carrier-free radioisotopes by paper chromatography: pp. 60–74.

Rf values of Ru-Rh, Zr, Nb, Y, Ce-Pr, I, Ca, and Sr, are listed. Zr and Nb were separable only when they were developed with mandelic acid of pH 5.2 and 7.9 Elements of the Ce group seemed to be separated when developed with acetylacetone-BuOH.

Kikuchi, Takehiko, and others. The metabolism of fission products. I. The metabolism of the radioactive ashes obtained from the No. 5 Fukuryu Maru: pp. 75–83.

When the radioactive ashes were administered by mouth, the radioisotopes which were chiefly absorbed were alkaline earths, and were deposited mainly in the bones. When, after the removal of the alkaline earths, the radioisotopes contained in the radioactive ashes were administered by mouth in the form of chloride or citrate, the radioisotopes chiefly absorbed were heavy metals such as Ru and Rh.

———. I. Metabolism of the radioisotopes contained in the radioactive ashes obtained from the No. 5 Fukuryu Maru: pp. 84–90.

Among the radioisotopes obtained by separation from ashes on the ship, i. e., Y^{91}, $C^{141, 144}$, Pr^{144}, Ca^{45}, $Sr^{89, 90}$, $Ru^{103, 106}$, Rh^{106}, Zr^{95}, Nb^{95}, and I^{131}, Sr, Ca, and Y were accumulated chiefly in the bones of adult mice, and the elimination of radio-Sr from there was very slow. When administered by mouth, radio-Sr and Radio-Ca were readily absorbed from the digestive tract, while the absorption of radio-Y from the tract was poor.

———. Paper XI, Studies on the metabolism of fission products III. Radioautographic studies on the localization of radiostrontium and radiocalcium in the bones: pp. 99–105.

———. The effects of EDTA–Na (Na Ethylenediaminetetraacetate) upon the metabolism of radiostrontium and radioyttrium in mice: pp. 106–111.

The toxicity of EDTA–Na, inert Sr $(NO_3)_2$ and Ba $(NO_3)_2$ has been examined. Simultaneous injection of EDTA–Na showed no significant effect upon the distribution of radio-Sr in the bones of mice. The distribution of Radio-Y in the bones of mice tended to decrease following the simultaneous subcutaneous injection of Y^{91} and EDTA-Na.

Japanese Committee for Compilation of Report on Research in the Effects of Radioactivity. Research in the effects and influences of the nuclear bomb test explosions. Tokyo, Japan Society for the Promotion of Science, 1956: 1824 pp. in 2 v.

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Appendix A: A report by the United States Atomic Energy Commission on the effects of high-yield nuclear explosions, 8 pp.

"It is important to note that information relating to high level winds is very sparse, and that no information is available on the distribution of particle size and radio-activity within the nuclear cloud resulting from high yield explosions. Any conclusions on results presented in this paper must therefore be considered tentative."

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Paper 244: A formulation of the relation between radiation dose and shortening of life span, by H. A. Blair (USA): pp. 118–120.

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Paper 88: The deposition of radioactive substances in bone, by Frank E. Hoecker (USA): pp. 138–145.

Paper 899: Biological damage resulting from exposure to ionizing radiation, by L. H. Gray (UK): pp. 209–212.

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APPENDIX 10

OAK RIDGE NATIONAL LABORATORY,
Oak Ridge, Tenn., August 21, 1957.

Mr. JAMES T. RAMEY,
Executive Director, Joint Committee on Atomic Energy,
Washington, D. C.

DEAR MR. RAMEY: Enclosed please find a copy of the material concerning topic VIII D of the outline, fallout and water decontamination, requested by Congressman Holifield for the Joint Committee on Atomic Energy Report.

Enclosed is the biographical sketch also requested in your letter of June 19, 1957.

If I can be of any further assistance to you and the committee, please feel free to write.

Thank you.

Very truly yours,

WILLIAM J. LACY,
ERDL Representative at ORNL.

Enclosures: 1. Report on Fallout. 2. Biographical sketch.

Cc: Commanding Officer, Engineer Research and Development Labs, Fort Belvoir, Virginia; Harry N. Lowe, Jr., Chief Sanitary Engineering Branch, Fort Belvoir, Virginia; Dr. Karl Z. Morgan, Director, Health Physics Division, Oak Ridge National Laboratory, Oak Ridge, Tennessee.

BIOGRAPHICAL SKETCH

William J. Lacy was born in 1928 in Wallingford, Connecticut, attended Lyman Hall High School where he won the prizes in science and chemistry, then he obtained a B. S. degree in 1950 from the University of Connecticut where he majored in Chemistry. He entered graduate school at New York University in September of 1950 and worked as a research associate on an AEC research contract. In May of 1951 he joined the staff at the Engineer Research and Development Labs of Fort Belvoir, Virginia, and immediately was transferred to the Oak Ridge National Laboratory to work on the water decontamination research project.

He has had seven (7) articles published, presented numerous papers and is a member of the American Chemical Society, American Association for the Advancement of Science and the Scientific Research Society of America.

Mr. Lacy is married and has two (2) sons, 2½ and six months, he resides in Oak Ridge, Tennessee.

[Material for Joint Committee on Atomic Energy Topic VIII D]

REMOVAL OF RADIOACTIVE FALLOUT FROM CONTAMINATED WATER SUPPLIES

William J. Lacy, Chemist,* Sanitary Engineering Branch, Engineer Research and Development Labs, Fort Belvoir, Va.

There are two possible sources of radioactive contamination of public water supplies, (1) the result of direct discharge into the environment from reactor processing plants, research center using radioisotopes and others and (2) deposition of radioactive material by fallout or wash-in due to weapon's test activities.

Most of the radioactive materials in item one are in solution, fallout, however, may be in the form of insoluble oxides, and its removal may differ from the removal of ionic material.

Studies have been reported on the subject of fallout in particular areas (1), (2), (3), (4). It was reported that 35 percent of the fallout activity was removed by the Albany, New York, water treatment plant, an alum coagulation, settling and filtration plant. Thomas and his coworkers at Harvard (2) (3) working at the Lawrence, Massachusetts, water plant obtained 80 percent removal by coagulation, settling, and filtration. Bell (4) compared the fallout removal results from Cambridge and Lawrence, Massachusetts, and Rochester, New York, with pilot plant results obtained by Straub (5) (6) who used a simulated bomb blast mixture with an age about one month after detonation.

*On loan to Health Physics Division, Oak Ridge National Lab., Oak Ridge, Tennessee.

The comparison indicated the three treatment plants show much lower removals of fallout than Straub obtained on chemical processed radioactive material even though the same procedure was used in both cases. The U. S. P. H. S. reported the analysis of rain water samples containing fallout showed 50 to 100 percent of the "old" radioactive material to be soluble. However, the soluble fraction dropped to about 30 percent during the weapon's testing period.

For reactor made fission products, or a mixture of commercially available radioisotopes, the efficiency of removal would be a function of the various radioelements comprising the mixture. Results in laboratory studies and pilot plant scale investigations by the author indicates removals of about 70 to 85 percent using either alum and soda ash or ferric chloride and limestone coagulants. A series of studies (7) reported that removals of 99 percent could be obtained using a serial coagualtion procedure including an excess lime-soda ash softening or phosphate coagulation step, provided some clay material was added to remove radiocesium.

Conventional wastes treatment processes include coagulation, settling, and filtration, plus disinfection. Often additional treatment, such as fluoridation, aeration, softening, ion exchange, iron and manganese removal are employed.

During coagulation certain of the dissolved constituents are precipitated as insoluble hydroxides or carried along, scavenged, with the heavy metal hydroxides of alum or iron. Coagulation can have its radioactivity removal increased from about 75 percent to almost 90 percent by the addition of clay for cesium and copper sulfate for radioiodine.

It should be pointed out that different radioisotopes respond differently to removal by coagulation. Other factors to be considered include: (1) Chemical and physical form of the radionuclide, (2) concentration or the radioactive material, and (3) optimum pH of flocculation for the coagulant available and the water under treatment. Investigation by the author (8) indicates increase dosages of chemical generally yielded only slightly higher removals while higher pH usually resulted in proportionately higher removals.

Softening using lime-soda ash is one of the more effective chemical methods for the removal of radiostrontium and barium. However, it is necessary to use excesses quantities, over the stoichiometric dosage, for satisfactory results. Studies at MIT (9) (10) have indicated that the radiostrontium is removed by coprecipitation with the hardness or calcium carbonate in a mixed crystal formation.

Ion exchange is another method used by some municipal water treatment plants. Removal of ionic radionuclides by this process is not only technically possible (11), but very satisfactory. The most effective method employes either a mixed bed principal or separate cation-anion exchange columns. Ion exchange units such as home-type water softeners are very effective for removal of 99+ percent of the radioactive fallout or reactor originated radionuclides from contaminated water. Also ion exchange resins (mixed) can be used with, good results, as slurries for the removal of a vaniety of radioactive contaminants from water solutions (12)

Other methods, such as, the use of clays, powdered metal, charcoal, flotation and various adsorbents all have some merit for the removal of specific radioisotopes or under a given set of condition result in good removals. (13) However, clay seems to have the most practical and over advantage of being (1) available, (2) cheap, (3) effective, (4) simple to use, (5) easy to remove both absorbent and absorber and the radioactive material will not be easily leached once it is attached to the clay particle. Distillation although not a usual municipal water treatment method is used extensively by the military on island bases and where a high quality of water is required. Distillation results in the best single treatment of a contaminated water removing 99.9+ percent. (14) The major objection to distillation as a water treatment procedure is cost.

As indicated by the literature cited most of the above studies have been made on chemically processed, radiochemically pure radioisotopes and not true fallout from a nuclear detonation. Therefore, it was expected that the actual fallout material not being entirely in the same physical and chemical form could not be as readily removed from contaminated water. However, recent tests by the Corps of Engineers at the AEC Nevada Proving Grounds on some very low level fallout indicated (1) close agreement with laboratory results on removal by coagulation and softening using lime-soda ash and precipitation with trisodium phosphate at a high pH, (2) the ion exchange procedures resulted in 99 to 100 percent removal of the bomb fallout material, (3) the material that was not

a true solution could be removed physically and the material in solution treated chemically and (4) radionuclide once adsorbed on clays were not appreciably leached by tap water.

Many other experiments have been made by myself and others, some are still in progress, which have not been cited here. It is felt that this brief general review plus the six tables showing detailed data, will give the committee a review of the field on water decontamination.

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TABLE I.—*Coagulation for removal of radioactivity*

Contaminant	Dosage, p. p. m.	Percent removal	
		$FeCl_3$-$CoCO_3$	Alum-soda ash
Ce^{144}-Pr^{144}	50	99.2	96.1
	100	99.4	96.5
Ba^{140}-La^{140}	50	67.4	58.4
	100	70.7	58.0
Zr^{95}-Nb^{95}	50	98.1	76.4
	100	98.8	78.6
I^{131}	50	45.0	26.3
	100	63.0	35.7
P^{32}	45.7	93.3	94.1
MFP-1 [1]	29–58	60–83.7	
MFP-2 [2]	50	70.1	72.6

[1] MFP-1—ORNL waste containing mixed fission products.
[2] MFP-2—Simulated 30-day atomic-bomb blast mixture.

TABLE II.—*Results of lime-soda ash treatment for removal of strontium*

Treatment	Percent removal of activity
Stoichiometric amounts	75.0
20 ppm excess lime-soda ash	77.0
50 ppm excess lime-soda ash	80.1
100 ppm excess lime-soda ash	85.3
150 ppm excess lime-soda ash	97.3
200 ppm excess lime-soda ash	99.4
300 ppm excess lime-soda ash	99.7

TABLE III.—*Ion exchange column for water decontamination*

Run No.	Resin*	Contaminant	Resin capacity gal./ft.^{3}	Percent removal until breakthrough
1	Cation	MFP-1	5,700	71-82
2	Mixed bed	MFP-1	3,150	93-99+
3	Cation	MFP-2	6,000	88-96
4	Mixed bed	MFP-2	2,890	96-99
5	Cation	Zr^{95}-Nb^{95}	6,750	85-88
6	Mixed bed	Zr^{95}-Nb^{95}	2,600	92-97
7	Cation	MFP-3	3,270	85-90
8	Mixed bed	MFP-3	6,150	92-99

*Cation resin was a high capacity nuclear sulfonic acid type and mixed bed was amberlite MB-3.

NOTES

MFP-1—ORNL liquid waste material.
MFP-2—Simulated 30-day atomic-bomb debris.
MFP-3—Three year old dissolved reactor fuel element.

TABLE IV.—*Removal of radioactive contaminants from water—Resin-jar test studies (stirring time, 90 minutes, samples filtered)*

Contaminant	Initial pH	Initial activity c/m/ml	Percent removal mixed ion exchange resin, p. p. m.			
			450	900	1,800	2,700
P^{32}	8.2	5,560	47.4	74.5	96.2	99.8
Cd^{115}	8.0	7,880	37.9	45.6	91.1	99.99
Cs^{137}-Ba^{137}	8.2	8,200	15.1	14.6	69.1	99.99
Zr^{95}-Nb^{95}	8.1	6,700	98.3	98.4	99.2	99.4
I^{131}	7.5	8,200	84.5	95.5	95.6	98.1
$Ce^{141, 144}$-Pr^{144}	7.9	4,150	98.7	99.2	99.8	99.98
Ba^{140}-La^{140}	7.6	3,490	85.1	94.5	98.8	99.9
FPM-4	8.3	13,660	82.7	90.5	97.3	99.2
FPM-5	2.7	8,400	88.4			

NOTES

FPM-4—Iodine dissolver solution aged 30 days.
FPM-5—Mixed fission product waste containing mainly Cs^{137}-Ba^{137} and Ru^{106}-Rh^{106}.

TABLE V.—*Decontamination of radioactively contaminated water by slurrying with clay*

Contaminant	pH	Clay concentration, p. p. m.	
		1,000	3,000
		Percent removal	
Ru^{106}-Rh^{106}	5.2	50.5	61.5
Zr^{95}-Nb^{95}	7.5	98.0	99.4
Sr^{90}-Y^{90}	7.7	83.4	92.9
I^{131}	7.5	4.9	3.4
$Ce^{141,144}$-Pr^{144}	8.0	99.7	99.9
Ba^{140}-La^{140}	7.8	88.8	94.3
MFP-1	8.8	82.0	86.3
MFP-2	9.0	70.0	72.8
MFP-3	7.7	79.0	83.6

TABLE VI.—*Removal of radioactive material by distillation (60 gallon/hr thermocompression unit)*

Run No.	Contaminant	Activity of feed, d/m/ml	Removal of activity expressed as decontamination factor	Percent
1	MFP-1	22,060	4.10×10^3	99.98
2	MFP-2	97,400	4.97×10^3	99.98
3	MFP-3	31,150	3.59×10^3	99.97
4	MFP-4	62,400	3.52×10^2	99.72
5	Pa^{233}	41,030	2.31×10^3	99.96
6	I^{131}	60,900	7.04×10^2	99.86
7*	MFP-5	38,910	1.09×10^3	99.91
8*	MFP-4	69,700	1.00×10^4	99.99
9*	MFP-1	12,020	1.70×10^4	99.99
10*	I^{131}	45,600	1.28×10^3	99.92
11*	Pa^{233}	25,300	5.80×10^3	99.98

*Glass wool reflux condenser used.

NOTES

MFP-1 was 3-year-old fission product mixture.
MFP-2 was a 2-week-old mixture from disolution of a reactor slug.
MFP-3 was composite sample or ORNL liquid waste.
MFP-4 concentrate from ORNL liquid waste evaporator.
MFP-5 mixture to simulate the material expected 10 days after atomic detonation.

APPENDIX 11

UNITED STATES ATOMIC ENERGY COMMISSION,
Washington 25, D. C., August 20, 1957.

Hon. CHET HOLIFIELD,
Chairman, Special Subcommittee on Radiation of the Joint Committee on Atomic Energy, House of Representatives, Congress of the United States.

DEAR MR. HOLIFIELD: At the suggestion of your Committee, the Division of Biology and Medicine, U. S. Atomic Energy Commission, invited the principal participants in the discussions involving predictions of future skeletal concentrations of strontium 90 in humans which took place at the recent Congressional Hearings on fallout to meet once again in an attempt, insofar as present information permitted, to reduce the degrees of uncertainty in these predictions.

This meeting took place on July 29, 1957 and I am pleased to transmit a summary report of the meeting based on the stenographic transcript and consultation with the principal participants. This report was prepared by Dr. Forrest Western, of the Division of Biology and Medicine. It is my opinion this report honestly and clearly reflects the views of the participant scientists with respect to this problem. This document, then, would appear to reflect the thinking of those scientists who have worked hardest and thought most on the subject of these predictions, and should, therefore, be a useful addition to the text of the very important and

informative Hearings which your Committee held in May and June of this year on the whole matter of fallout from weapons tests.

Sincerely yours,

CHARLES L. DUNHAM, M. D.,
Director, Division of Biology and Medicine.

PREDICTED SKELETAL CONCENTRATIONS OF STRONTIUM 90

INFORMAL DISCUSSION OF JULY 29, 1957

At the suggestion of the Joint Committee on Atomic Energy, the Division of Biology and Medicine, U. S. Atomic Energy Commission, invited the principal participants in discussion of this subject at the recent Congressional Hearings, May 27–June 6, 1957, to meet in Washington, D. C., July 29, to try to reduce the degrees of uncertainty involved in various predictions of future skeletal concentrations of strontium 90 in humans. These persons were: Mr. Merril Eisenbud, Dr. J. Laurence Kulp, Dr. Wright H. Langham, Dr. Willard F. Libby, Dr. Lester Machta, Dr. William F. Neuman and Dr. Walter Selove. In addition to these participants were: Dr. Charles L. Dunham who alternated with Dr. Libby as Chairman of the discussion, Dr. Lyle T. Alexander, Mr. Hal Hollister, Dr. J. Calvin Potts, Dr. Robert Reitemeier, and the author of this summary, Forrest Western. This summary is based on a stenographic transcript and has had the benefit of comments by most of the participants.

It was generally agreed that the extensive measurements by Dr. Kulp of concentrations of strontium 90 in human skeletons established one base from which one may extrapolate skeletal concentrations of strontium 90 to be expected in the future. Dr. Kulp stated that the average concentration in children of the northeastern United States in the fall of 1956 was about 0.8 micromicrocuries of strontium 90 per gram of calcium. In discussion of relationships between concentrations in the United States and other parts of the world, Dr. Kulp indicated that concentrations in children of northeastern United States are consistent with those of two widely separated areas in other parts of the world from which he has been able to obtain a considerable number of samples. Dr. Selove discussed reports of relatively high local concentrations of fallout in an Asiatic area and suggested that possible fluctuations in local patterns of tropospheric fallout in the periods shortly following tests explosions might result in considerably higher skeleton concentrations in some areas of the world. Although our world sampling program to date has failed to disclose areas in which skeletal concentrations are higher than in the United States, individual comments endorsed the desirability of continued search for such areas as a part of our world-wide study of the distribution and uptake of fallout from nuclear detonations.

It was agreed that, *even if fallout had ceased at the end of 1956*, skeletal concentrations of strontium 90 would be expected to increase until they came into equilibrium with the strontium 90 in the environment. Somewhat independent estimates of the equilibrium value may be made (1) from a knowledge of *changes* in environmental (specifically, *dietary*) concentrations during the growth of the skeletons assayed, and (2) by application of factors of discrimination between calcium and strontium in estimating the uptake of strontium 90 from existing concntrations of strontium 90 (a) the soil, (b) the over-all diet, or (c) milk. Each of these methods involves some degree of uncertainty. After discussion of the uncertainties involved, it was agreed that these various considerations make it appear probable that average skeletal concentrations to be expected in young persons of northeastern United States as a result of strontium 90 actually deposited on the earth's surface up to the end of 1956, fall between 1.5 and 2 micromicrocuries per gram of calcium.

Because some of the strontium 90 released to the stratosphere in past years has not yet been deposited on the earth's surface, actual skeletal concentrations from tests performed before the end of 1956 may be expected to become greater than the values estimated under the conditions assumed above. The increase in concentration to be expected from additional fallout of this material depends upon a number of factors; (1) the additional activity reaching the surface of the earth, (2) the fact of delay in the appearance of additional strontium in the diet, (3) the relative importance of total soil content and rate of fallout (specifically, rate of retention on surfaces of vegetation), (4) radioactive decay and (5) decrease, with time after fallout, in the percentage of strontium 90 in the soil which is available for uptake by plants. It appeared from the discussion that the greatest uncertainty involved here is in predicting the distribution and time of fallout to be expected from residual stratospheric content.

It was estimated that, in addition to the range of uncertainty by a factor of 1.25 represented in the above estimate of the average skeletal concentrations to be expected from strontium 90 actually deposited on the earth's surface by the fall of 1956, there might be an additional range of uncertainty, by a factor as large as two, in estimates involving the ratio of the quantity of strontium 90 in the stratosphere to that on the ground. In estimating total skeletal concentrations in the northeastern United States which might be expected when fallout of strontium 90 produced prior to 1957 is essentially complete, these two uncertainties alone would result in a range of a factor of 2.5 between minimum and maximum estimates.

It was agreed that the first effort of the group would be to estimate average skeletal concentrations to be expected in the age group of maximum concentration in 1975, assuming that there were no nuclear detonations after 1956. Dr. Kulp estimated that, because of radioactive decay and decrease in availability, additional fallout of strontium 90 produced before 1957 would not make environmental concentrations of strontium 90 in the northeastern United States in 1957 significantly greater than in 1956. After discussion, this led to the estimate that, if the residual stratospheric content were to be deposited with uniform distribution over the surface of the earth, skeletal concentrations in the age group of maximum concentration in 1975 would fall in the range of from 1.5 to 3.5 micromicrocuries of strontium 90 per gram of calcium.

Mr. Eisenbud, taking a different approach, first estimated quantities of strontium 90 on the surface in the northeastern United States at the end of 1956 due to tropospheric and stratospheric fallout, respectively, and the fraction of strontium 90 previously injected into the stratosphere which had fallen out by the end of 1956. Maximum future environmental levels were then related to those of 1956, using the assumption that fallout of the residual (1956) stratospheric content would have the same geographic distribution as the previous stratospheric fallout. Discussion of this approach led to an estimate of skeletal concentrations, in 1975, in the range from 2 to 5 micromicrocuries of strontium 90 per gram of calcium.

Dr. Machta discussed the possibility that strontium 90 injected into the stratosphere near the equator may be moving northward and entering the troposphere preferentially above 30° N., in such a manner that the fraction of the stratospheric content falling out in these latitudes is increasing with time. It was estimated that such "banding" of fallout might result in skeletal concentrations two times as high as those estimated on the basis of Mr. Eisenbud's assumption of a constant stratospheric fallout pattern; i. e., an average skeletal concentration in children of from 4 to 10 micromicrocuries per gram of calcium.

It was agreed that at this time our knowledge of atmospheric transport is too limited to reduce the range of uncertainty on this point represented by the ranges of estimated concentrations described in the preceding three paragraphs.

It was estimated that, if testing of weapons were continued up to about 1965 in such a manner as to produce strontium 90 in the same total quantity and with the same distribution (i. e., with tests held at the same geographical locations and with the same distributions of tropospheric and stratospheric fallout) as were produced prior to 1957, the average total skeletal content in young persons in about 1975 might be approximately 2.5 times that to be expected if there were no further testing. This factor is considered to apply equally to the three estimates discussed in the preceding paragraphs.

The estimates described above are summarized in the following table:

Predicted average skeletal levels of strontium to be expected in young persons in northeastern United States under various conditions of testing of nuclear weapons

[All levels are in micromicrocuries of strontium 90 per gram of calcium]

Concentrations in children, fall 1956	Predicted future concentrations from strontium 90 on ground before 1957	Predicted concentrations in 1975 from all strontium 90 produced before 1957	Predicted concentrations in 1975 if past tests or equivalent were repeated before 1965
0.8	1.5 to 2	1.5 to 3.5*	3.5 to 9.*
		2 to 5	5 to 12.
		4 to 10	10 to 25.

*The considerations leading to each of these 3 estimates are discussed in the text of this summary.

It was the consensus of the group that within one or two years the confidence with which predictions of future concentrations can be made will be so greatly increased that it is unprofitable for the present purpose to extend predictions beyond those given in the above table.

APPENDIX 12

UNITED STATES ATOMIC ENERGY COMMISSION,
Washington 25, D. C., June 1, 1957.

Hon. CHET HOLIFIELD,
Chairman, Special Subcommittee on Radiation, Joint Committee on Atomic Energy, Congress of the United States.

DEAR MR. HOLIFIELD: Your letter of 31 May 1957 requests a letter on an unclassified basis giving: (a) an estimate as to year-to-year fission yield and total yield put into the atmosphere by atomic weapons from all sources; (b) a list of weapons test series conducted by each country; and (c) a listing of the number of explosions which have taken place, country by country and year by year.

As to total yield and total fission yield released by tests to date either in gross or by nation, this information has security implications which preclude my giving it as unclassified data. Various unclassified estimates which do not reveal such totals have been made as to the amount of fission debris making its way into the stratosphere. One such estimate is that given on pages 952 and 954 of the attached report, "Current Research Findings on Radioactive Fallout," by Dr. Willard F. Libby.

Information as to U. S. test series and the number of shots in each is contained in the following table:

Name of series	Date	Number of shots
Trinity	1945	1
Crossroads	1946	2
Sandstone	1948	3
Greenhouse	1951	4
Ranger	1951	5
Buster/Jangle	1951	7
Tumbler/Snapper	1952	8
Ivy	1952	2
Upshot/Knothole	1953	11
Castle	1954	(1)
Teapot	1955	14
Wigwam	1955	1
Redwing	1956	(1)
Plumbbob	1957	[2] 1

[1] The number of shots in Castle and Redwing is still classified.
[2] Only 1 shot to date.

The above list is exclusive of the two weapons detonated in combat during World War II.

With respect to the British series, the United Kingdom has announced the following series and number of shots:

Location	Date	Number of shots
Australia	1952	1
Do	1953	2
Do	1956	2
Do	1956	4
Christmas Island	1957	2

Of several Soviet references to their tests, they have: announced one on 20 August 1953, shortly before the U. S. announcement was issued; made a general statement on 18 September 1953 to the effect that they had conducted several tests in recent weeks which constituted confirmation of the U. S. announcement of 31 August 1953; and announced a test on 10 September 1956 about which the U. S.

made no comment. The United States has from time to time announced certain of the tests. Copies of these announcements are attached. From a review of these announcements it can be noted that the U. S. has announced 23 specific USSR detonations. A complete list of all detected shots cannot be provided on an unclassified basis. In this respect on August 26, 1956, the Chairman of the AEC stated: "Although there have been but 13 announcements by the U. S. regarding Soviet testing, several have noted a series of detonations and the actual number of Soviet detonations is significently higher than 13."

Should you desire a classified summarization of the information requested which cannot be given on an unclassified basis, I would be most pleased to provide this separately.

Sincerely yours,

K. E. FIELDS,
General Manager.

[From Congressional Record, June 18, 1957]

NUCLEAR WEAPONS EXPLOSIONS

The following table has been compiled principally from press releases of the United States Atomic Energy Commission. However, reports in the press as to the size and nature of various explosions have been included when available.

On April 12, the United States had announced 19 Soviet tests. The AEC has pointed out that this country does not disclose all of the U. S. S. R. shots of which it has knowledge but limits itself to statements about explosions of special interest. The actual number of Soviet detonations is therefore significantly higher than those announced.

As of June 17 the AEC had announced 68 tests by the United States. However, the total number of detonations made by this country has never been announced.

The United Kingdom has announced 11 tests to date, June 18, which is understood to be the total number of tests made by that country.

Year	United States		U. S. S. R.		United Kingdom		Remarks
	Date	Place	Date	Place	Date	Place	
1945	July 16	Alamogordo					1st test of A-bomb.
	Aug. 6	Hiroshima					Air burst, energy release about 20,000 tons of TNT.
	Aug. 9	Nagasaki					Do.
1946	July 1	Operation Crossroads, Bikini Lagoon.					Air burst, Nagasaki-type bomb.
	July 25						Underwater detonation.
1948 (spring)		Operation Sandstone, Eniwetok Atoll.					3 explosions announced May 17, no details given.
1949			Sept. 23	Soviet territory			1st atomic explosion in U. S. S. R. announced by President Truman.
1951 (winter)	Jan. 27 Jan. 28 Feb 1 Feb. 2 Feb. 6	Operation Ranger, Nevada Flats.					5 tests primarily for tactical information, Air bursts.
1951	April and May	Operation Greenhouse, Eniwetok.					4 tests described as "experiments contributing to thermonuclear research."
			Oct. 3	Soviet territory			2d explosion. Reported by the United States, later confirmed by the Kremlin.
			Oct. 22	do			President Truman announced evidence of a 3d nuclear explosion by the U. S. S. R.
1951	Oct. 22 Oct. 28 Oct. 30 Nov. 1 Nov. 5 Nov. 19 Nov. 29	Operation Buster Jangle, Nevada Flats.					7 air, tower, and surface or underground bursts. Low yield. Tactical.
1952	Spring and summer Apr. 1 Apr. 15 Apr. 22 May 1 May May 25 June 1 June 5	Operation Tumbler Snapper, Nevada Flats.					8 air and tower bursts. Troop participation in some tests.
	Fall	Operation Ivy, Eniwetok					2 tests. 1 was the 1st hydrogen bomb ever exploded. Yield 5 megatons.
					Oct. 3	Monte Bello Islands.	Nature of bomb not disclosed. Probably of Hiroshima type.

Year	United States		U. S. S. R.		United Kingdom		Remarks
	Date	Place	Date	Place	Date	Place	
1953	Mar. 17 Mar. 24 Mar. 31 Apr. 6 Apr. 11 Apr. 16 Apr. 25 May 8 May 19 May 29 June 4	Operation Upshot Knothole, Nevada Flats.					11 tests: Air, tower, and 1 shot from a 280-millimeter gun. The largest explosion, that of Apr. 18, was reportedly equal to 30,000 to 40,000 tons of TNT.
			Aug. 12	Soviet territory			Initial hydrogen bomb (nondeliverable).
			Aug. 31	do			Fission explosion in same range of energy release as Nevada Flats tests.
1954	Mar. 1	Operation Castle, Bikini					Hydrogen bomb reported to have had a yield of 15 megatons. Radioactive fallout over 7,000 square miles.
	Mar. 26						No information released concerning this test.
	Apr. 6	Eniwetok					3d test in the series. Presumably an H-bomb.
			Oct. 26	Soviet territory			Announcement made by the AEC of a series beginning in September.
1955	Feb. 18 Feb. 22 Mar. 1 Mar. 7 Mar. 12 Mar. 22 Mar. 23 Mar. 29 (2 tests) Apr. 6 Apr. 9 Apr. 15 May 5 May 15	Operation Teapot, Nevada Flats.					14 air, tower, and underground tests. Included atomic trigger for detonating H-bombs, also civil defense test and "air-killer" test.
	May 17	Operation Wigwam, off west coast.					Small underwater atomic weapon exploded, probable yield 1,000 to 5,000 tons of TNT.
			Aug. 4	Soviet territory			AEC announced that the Soviets had resumed testing of nuclear weapons.
			Sept. 24	do			Announcement by AEC of another Soviet nuclear explosion.
					Oct. 15	Woomera Rocket Range, Australia.	2 tests, the 1st equivalent to 20,000 tons of TNT. No data for the 2d test.
			Nov. 10	Soviet territory			Announcement by AEC of a further test in the 1955 series.

			Nov. 23	do			Hydrogen bomb in the range of megatons. Khrushchev announced on Nov. 27 that it was a hydrogen bomb and that it had been dropped at a great height from an airplane.
1956			Mar. 21	Soviet territory, probably central Asia.			AEC announced another Soviet nuclear explosion.
			Apr. 2				Another explosion in current series announced by AEC.
	May 5	Operation Red Wing, Eniwetok.					Relatively small device exploded, about 10 kilotons.
					May 16	Monte Bello Islands.	Test expected to supply data for triggering of H-bomb.
	May 21	Bikini					1st H-bomb dropped by U. S. Air Force. Yield at least 15 megatons.
			Aug. 24	Southwestern Siberia.			
			Aug. 30				A series of small explosions.
			Sept. 2				
					Sept. 27	Maralinga Proving Ground, South Australia.	4 tests, tower and air; new and cheaper kinds of A-bombs tested.
			Nov. 17	Southwestern Siberia.			Small explosion.
			Jan. 19	do			
			Mar. 8				
			Apr. 3				
			Apr. 6				Moscow announced these tests were designed to perfect atomic warheads for tactical purposes.
			Apr. 10				
			Apr. 12				
			Apr. 16				
					May 15	Christmas Islands.	H-bomb dropped from Vikers Valiant jet bomber. Energy release 1,000,000 tons of TNT.
	May 28	Beginning of Operation Plum Bob, atomic test site, Nevada. The tests are scheduled to continue into September.					Probably a warhead for small rockets or missiles. Yield about 10,000 tons of TNT.
					May 31	Christmas Islands.	2d hydrogen bomb exploded by the British.

[1] 1 of a series.

Prepared for the use of the Subcommittee on Disarmament by Janie E. Mason, of the subcommittee staff, on loan from the Library of Congress, June 6, 1957.

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THE NATURE OF RADIOACTIVE FALL-OUT AND ITS EFFECTS ON MAN

HEARINGS

BEFORE THE

SPECIAL SUBCOMMITTEE ON RADIATION

OF THE

JOINT COMMITTEE ON ATOMIC ENERGY
CONGRESS OF THE UNITED STATES

EIGHTY-FIFTH CONGRESS

FIRST SESSION

ON

THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN

MAY 27, 28, 29, JUNE 3, 4, 5, 6, AND 7, 1957

PART 3

INDEX

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INDEX TO HEARINGS ON THE NATURE OF RADIOACTIVE FALLOUT AND ITS EFFECTS ON MAN [1]

[1] This index was prepared by Miss Catherine S. Corry, under the supervision of Dr. Charles S. Sheldon, of the Legislative Reference Service of the Library of Congress. The Joint Committee on Atomic Energy wishes to acknowledge their assistance in the preparation of this index.

98299—58—pt. 3——2

98299—58—pt. 2——8

H

I

N

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P

Q

R

93299—58—pt. 3——8

98299—58—pt. 3——9

T

Y

Z

NAVAL REACTOR PROGRAM AND SHIPPINGPORT PROJECT

HEARINGS

BEFORE

SUBCOMMITTEES OF THE JOINT COMMITTEE ON ATOMIC ENERGY CONGRESS OF THE UNITED STATES

EIGHTY-FIFTH CONGRESS

FIRST SESSION

ON

PROGRESS REPORT ON NAVAL REACTOR PROGRAM AND SHIPPINGPORT PROJECT

MARCH 7 AND APRIL 12, 1957

Printed for the use of the Joint Committee on Atomic Energy

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LETTER OF TRANSMITTAL

CONGRESS OF THE UNITED STATES,
Washington, D. C., June 12, 1957.

Hon. CARL T. DURHAM,
Chairman, Joint Committee on Atomic Energy,
Washington, D. C.

DEAR MR. DURHAM: We would like to thank you for publishing the hearings on naval reactors development held on March 7 and April 12, 1957, by the Research and Development and Military Applications Subcommittees of which we are the chairmen, and ask if you would print this letter as a preamble to the record.

The efforts of the Naval Reactors Branch of the AEC together with its field offices and contractors have given our Nation world leadership in the development of atomic power for naval propulsion. In no other area of military endeavor, either nuclear or conventional, is our national leadership now so clear and pronounced. But today's leadership is not enough. We believe that a fleet of nuclear-powered underwater vessels capable of firing long-range missiles will ultimately decide the balance of world power and the maintenance of the peace.

The Naval Reactors Branch has also played a leading role in the development of civilian atomic power. With its industrial contractors it has been primarily responsible for developing the technology of pressurized-water power reactors. The country has, therefore, received a double dividend on money spent for naval nuclear propulsion. We have world naval leadership in atomic power and will soon have the civilian reactor at Shippingport, Pa. Unquestionably, the dollar value of these developments to American industry is many times greater than the Government's investment.

In our hearings we determined that the Naval Reactors Branch is enjoying the full support and cooperation of the Atomic Energy Commission, and the observations we make below should not be construed as criticism. They are offered in a constructive sense and for the most part call for the continuance of existing practices. We consider the remaining suggestions refinements of existing practices.

We feel confident that our colleagues on the Joint Committee join us in expressing every confidence in the Naval Reactors Branch under the direction of Rear Adm. H. G. Rickover, United States Navy.

In connection with our review of the role and activities of the Naval Reactors Branch, we believe the following observations are appropriate:

1. The wisdom of continuance of the placing of central direction of the program in the Atomic Energy Commission and the Navy in a single official has been amply shown. Our hearings consistently dem-

onstrated that Admiral Rickover has always accepted personal responsibility for the program under his direction—including responsibility for technical setbacks.

2. This unique organizational arrangement whereby the Office of the Chief of Naval Reactors Branch of the Atomic Energy Commission and the Assistant Chief of the Bureau of Ships for Nuclear Propulsion is combined in the same person has prevented much duplication and is in keeping with the purposes of the Atomic Energy Act of 1946 and 1954. The present organizational arrangement for naval reactors work has proved economical, efficient, flexible, and capable of doing the job, and should be maintained. Any suggestion of establishing duplicate functions in the Department of Defense should be rejected in the interests of economy of manpower and appropriated money. The system employed by the Naval Reactors Branch in the selection, training, and assignment of personnel should also be continued. Officers and civilians are assigned to jobs on the basis of ability and not rank. We are assured that the test of selection is—who can best perform the job?

3. The research and development philosophies which have been employed by the Naval Reactors Branch should be maintained. We observed that this approach is neither attached to "beyond the horizons" concepts nor affixed to mere reproductions of successful models. A proper balance has been found in that the developmental goals are fixed at a level above present anticipations of success but not so far beyond them that accomplishment is not possible.

4. The Atomic Energy Commission should consider the immediate assignment to the Naval Reactors Branch of the development and construction of a new civilian reactor concept. With the prospective completion of the pressurized water reactor at Shippingport, additional time and attention could be given to a new reactor type. We specifically call attention to the desirability of assigning the development of an advanced type gas cooled reactor, and point out that some years from now the knowledge acquired in the development of a gas system may have application for ship propulsion.

We were pleased with the assurance we received from the Chief of Naval Operations that Admiral Rickover has direct access to him in connection with the naval reactors effort. We feel sure that in light of the importance of this work that this same access is available to him in all departments in the executive branch of the Government.

We are confident that the Commission and the Navy recognize the importance of these programs in furthering the interest of the national defense and security and will not permit doctrinaire administrative practices to interfere with the effort.

Quite often in carrying out our duties as Members of the Congress we must be critical of the activities of the agencies in the executive branch of the Government. We therefore take great pleasure in commending those who have been responsible for the success of the naval reactors program.

Sincerely yours,

MELVIN PRICE,
Chairman, Subcommittee on Research and Development.
HENRY M. JACKSON,
Chairman, Military Applications Subcommittee.

JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C., June 13, 1957.

Senator HENRY M. JACKSON,
Chairman, Military Applications Subcommittee.
Representative MELVIN PRICE,
Chairman, Subcommittee on Research and Development.

DEAR SENATOR JACKSON AND MR. PRICE: I have asked the staff of the Joint Committee to have your letter to me of June 12, 1957, on the naval reactors program printed with the record of your hearings on the subject.

I fully endorse your comments and observations, for the work under Admiral Rickover is most vital to the United States.

Sincerely yours,

CARL DURHAM,
Chairman, Joint Committee on Atomic Energy.

CONTENTS

(A brief subject index will be found on page 101)

PROGRESS REPORT ON NAVAL REACTOR PROGRAM AND SHIPPINGPORT PROJECT

THURSDAY, MARCH 7, 1957

CONGRESS OF THE UNITED STATES,
SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT
AND THE SUBCOMMITTEE ON MILITARY APPLICATIONS OF
THE JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The subcommittees met, pursuant to notice, at 2 p. m., in room F–88, the Capitol Building, Hon. Melvin Price presiding.

Present: Representatives Price, Durham, Holifield, Dempsey, Cole, Van Zandt, and Patterson; and Senators Jackson, Hickenlooper, Knowland, and Bricker.

Also present: Willard F. Libby, Commssioner, AEC; Thomas E. Murray, Commissioner, AEC; A. Tammaro, Assistant General Manager for Research and Industrial Development; L. H. Roddis, Jr., Deputy Director, Division of Reactor Development; Adm. H. G. Rickover, Chief, Naval Reactors Branch; Capt. James M. Dunford, Assistant Chief, Naval Reactors Branch; Bryan LaPlante, special assistant to the General Manager; Capt. John H. Morse, Jr., assistant to Mr. Strauss; Capt. W. B. Braun, representing Admiral Burke, United States Navy.

Staff members present: James T. Ramey, executive director; Messrs. Brown, Conway, Hollister, and Toll.

Representative PRICE. The committee will be in order. This is a joint meeting of the Subcommittee on Military Applications, of which Senator Jackson is chairman, and the Subcommittee on Research and Development, of which I am chairman.

The meeting today has been called for the purpose of hearing from the Atomic Energy Commission as to the current status of the projects under the Naval Reactors Branch and what progress we may expect in the future.

It is my understanding that Admiral Rickover is prepared to cover this matter for us and to answer any questions individual members may have.

Admiral Rickover, we would like to say how pleased we are to have you with us today. The committee has been very favorably impressed by the excellent contributions the AEC has made to the civilian power program through the naval reactors program. As Congressman Durham, the chairman of the Joint Committee, has publicly stated in the 202 hearings, the contribution has been outstanding.

We sincerely hope it will continue to be so in the future.

From the large number of models which you have brought with you and which we see in the room, it would appear that you intend

to cover a great deal of material. We will, therefore, turn the meeting over to you, Admiral Rickover, and you may proceed when ready.

STATEMENT OF ADM. H. G. RICKOVER, CHIEF, NAVAL REACTORS BRANCH, ATOMIC ENERGY COMMISSION

Admiral RICKOVER. Thank you, sir. This is the first time I have been at the committee since Carl Hinshaw died, and as a witness who has appeared here many times and who has been questioned by him very pointedly, I would like to express my sincere regrets that he is not with us here today so he could continue to ask so many pointed questions, as he always did.

Although I used to accuse him of being for the Air Force, he was really for everybody, and helped us in the Navy a great deal.

Representative PRICE. We certainly miss him on the committee in many ways.

Admiral RICKOVER. There are a number of activities in the naval program that have no names. I wonder if it would be desirable to name one of these activities after Carl Hinshaw?

Representative PRICE. I think it would be very desirable, and I hope you can find a suitable one to dedicate to his memory.

Admiral RICKOVER. We can find one. I talked to the chairman of the board of Combustion Engineering. They have their new plant at Windsor, Conn., and I think they would be very happy to name it the Carl Hinshaw Laboratory. There are other places too.

If the committee cared that this be pursued, I would be happy to do so.

Representative PRICE. I think the committee would be very happy to have you do it. I think also the first flying prototype of a nuclear aircraft would be a fitting memorial to Mr. Hinshaw.

Admiral RICKOVER. But an aircraft carrier might come first.

Representative PRICE. You may proceed, Admiral, with your presentation.

Admiral RICKOVER. It has been frequently said that the things that have been done in the naval program have been done by a combined industry, AEC, and Navy team. I consider the Joint Congressional Committee to be an additional member of that team. I know and you people know that you have been just as much behind our program as anyone else and I very sincerely feel that way, because I know that without all your help and your confidence during the time when there was not anything to show you, without your backing we would not have progressed as far as we have.

The models I have in this room today are some examples of that, because all of them have either been built or are under construction

I will touch briefly on the naval program and then tell you about some of the advanced things we are doing, as you requested, sir.

The *Nautilus* was refueled about a week ago. She will again be ready for operation in about a month. Her first core ran for 62,500 miles, of which about 37,000 miles were fully submerged. The core lasted longer than we expected.

The new core will last even longer. This additional core life is being obtained without any increase in cost of manufacture, so that in effect when we increase the life of the core we cheapen it; even though it may cost the same it cheapens it in that proportion.

Representative PRICE. Would you mind comparing that performance with the performance of a conventional submarine.

Admiral RICKOVER. Yes, sir. During this process of steaming 62,500 miles, a conventional submarine having equal horsepower would have burned about 2,170,000 gallons of oil, the amount contained in 217 tank cars, with a length 1.7 miles long.

Representative VAN ZANDT. How long?

Admiral RICKOVER. 1.7 miles long; 2,170,000 gallons. That is the equivalent amount of fuel oil a conventional submarine of that power and having the same displacement as the *Nautilus* would have burned. The new core should do better than that.

Representative COLE. What is the price of that?

Admiral RICKOVER. Diesel oil, I think, costs about 8 to 10 cents a gallon, something on the order of 9 cents. I am not absolutely certain of the cost.

Representative VAN ZANDT. Are you good at arithmetic?

Admiral RICKOVER. That would be about $200,000 worth of oil. It is not much in cost. I do not want to leave any impression with this committee that to run a nuclear-powered vessel is cheaper than a conventional one. It is not. Because the amount you save by the oil is insignificant compared to the cost of the reactor core on present prices.

However, the cost of cores is coming down.

I do not want to create any wrong impression on that score.

As I stated, the new core will have more life. We already have evidence of that. As I stated, the *Nautilus* will be ready for sea in April and her core should be good for more than 2 years without refueling.

Representative VAN ZANDT. What did you find on the inside of the reactor, as far as the metals that were employed? I am thinking now of the pipes and so forth.

Admiral RICKOVER. We looked inside the pressure vessel of the *Nautilus* after the core was removed and it was shiny and clean. We found nothing wrong. During the refueling operation no one was irradiated beyond 300 milliroentgens a week, which is the normal permissible by AEC standards—no more irradiation than is allowed in an AEC laboratory for a week. I think 250 milliroentgens was actually the maximum dose.

Representative VAN ZANDT. In the beginning you recall in the design of the *Nautilus* there was piping used that could not take the pressure. I am wondering what did you find in conection with that redesigning?

Admiral RICKOVER. There was no faulty pipe found. A mistake had been made during the building period in 1954. Some wrong pipe had been used in the steam system instead of the pipe which had been specified, but this was corrected before the ship ever went to sea.

At Arco—perhaps I had better talk for a few moments on what we are doing there. We are using that installation as a training facility and as a test facility for trying out new reactor ideas.

For instance, it was there we tried out fuel elements that gave us increased life.

We have trained about 60 officers and about 500 enlisted men on the submarine prototype at Arco. Just before I came up here today,

I heard that we are losing 6 percent per year of all the enlisted men in the nuclear program because they become officers; 20 times as many enlisted men in our program become officers as do in the rest of the Navy.

I am not happy about this loss, but it is good for morale, and it is good for the Navy.

Of course, not all of the credit belongs to our training program. Our selection procedure obtains a high class of people for the program. That helps account for it. It does pose a serious problem to lose so many men, but it results in overall good to the Navy.

Representative VAN ZANDT. What about the initial selection? Have you an unlimited reservoir to select from as far as enlisted personnel is concerned?

Admiral RICKOVER. Enlisted personnel for the program are selected by the forces afloat, but we have fairly strict requirements, such as 4 years obligated service, and graduation from high school. Also a good record and above-average ability and intelligence.

We have, first, a 6 months' course at the submarine school in New London. This is a special course where we teach physics and mathematics, and various nuclear courses. After the course at New London the men are sent to Arco for another half year. There they learn how to operate and service the plant. They are then assigned to nuclear-powered ships.

The officers for the nuclear propulsion program are designated by the Bureau of Naval Personnel. They are also given 6 months' theoretical training and 6 months at Arco. Their course is much more difficult than that the enlisted men. All of the officers and a number of the enlisted men qualify as chief operators before they complete the course. This is a very difficult thing to do, but it assures us of having competent people for our nuclear plants. Admiral Holloway, our Chief of Naval Personnel, has been very farsighted in understanding our problems, and has gone out of his way time and again to help us. Without his help we could not have accomplished what we have.

Representative COLE. How does the enlisted man get to be an officer?

Admiral RICKOVER. The Navy conducts examinations twice a year.

Representative COLE. What additional training does he have in order to become an officer, none?

Admiral RICKOVER. No, sir. When he is selected for officer he is then sent to an officer candidate's school. There he is given special training for about 6 months and, if he completes the course, he is commissioned.

Representative COLE. Is he a general officer, a line officer?

Admiral RICKOVER. Yes, he becomes a line officer, an ensign.

The year's training we give the sailors and officers in our program is very valuable. I have been told that to qualify as Chief Operator at our prototype at Arco is several times as difficult as it is to qualify for command of a submarine.

It requires about 1,000 hours of practical work for this qualification, and this is in addition to all of the study they do. They must become adept in all phases of reactor operation, particularly in everything that pertains to safety. Even the commanding officers work 16 hours a day, 7 days a week at Arco.

I will say no more on training, except I must add that the facility at Arco is most valuable from this standpoint. We have no better training facility in the Navy than we have there and it is absolutely essential for the future of nuclear power in the Navy that we train the people there, on a real plant, a live one, because we do not want any accidents to happen. We want them to know the plant inside out. I am proud of the fact that all of the officers and men in the program are outstanding. They are all a marvelous group of very fine people and I am sure that ultimately they will be the leaders in the Navy.

Senator HICKENLOOPER. May I ask a question of Admiral Rickover?

Do you have a rough approximation of the cost of the operation of the *Nautilus* so far as fuel is concerned? Take the 2,170,000 gallons of fuel oil. That cost compared with the cost of nuclear heat, the core, its replacements, and utility of the core.

Leave out the plant itself.

Admiral RICKOVER. It is rather meaningless to say we are saving money. What we are getting is a profound military advantage.

Senator HICKENLOOPER. I understand that, but I just wanted to get some approximation.

Admiral RICKOVER. This gets into the problem of the cost of atomic power. At the present time, and for the next 10 years, I do not believe atomic power can compete economically with conventional power. We are still in the development stage. We are still learning how to develop and manufacture nuclear cores.

At Shippingport the cost of the power will be between 55 to 65 mills per kilowatt.

I believe it is rather farfetched to expect nuclear power to be competitive with conventional power for the next few years. I do not say it will not happen, but not today, not for the next few years.

Representative VAN ZANDT. What was the cost of the *Nautilus* per kilowatt?

Admiral RICKOVER. In wartime the cost was about 8 cents per kilowatt-hour on a conventional submarine.

Representative VAN ZANDT. You said 8 cents?

Admiral RICKOVER. That is 80 mills. But if you took into account the cost of convoys and fueling stations and other factors, I am not so sure that from an overall standpoint you would find, taking all the other factors into account, that nuclear power on a warship is more expensive than conventional fuel. I am not talking now about the military advantages we get from the use of nuclear power. Nuclear power gives us ships that can go at high speeds and can stay submerged for days at a time.

One of the things we did with the second core at Arco was to run it for 66 days and nights continuously at full power. The lines on this map [indicating] show the distance a nuclear submarine would have steamed under the same circumstances, on a full-power nonstop run. It would have run once around the world and back to New London, and without stopping set out again and go around the world on a northern route. That is a longer continuous run at full power than a plant of any kind, land, sea or air, has ever made. In contrast, the Navy acceptance trial at full power for a new ship is 4 hours. This plant ran for 1,700 hours continuously at full power.

Representative PRICE. Which reactor is that?

Admiral RICKOVER. It is the one at Arco, and is similar to the one that has just been installed in the *Nautilus*.

Representative PRICE. Similar to the *Nautilus?*

Admiral RICKOVER. Similar to the one that has just been placed in the *Nautilus*.

Representative PRICE. The new *Nautilus* reactor?

Admiral RICKOVER. Yes, sir.

Representative VAN ZANDT. The *Nautilus* is equipped with auxiliaries?

Admiral RICKOVER. Yes, sir.

Representative VAN ZANDT. Both diesel as well as batteries?

Admiral RICKOVER. There are two 300-horsepower diesels used for supplying auxiliary power in port. They are capable of propelling the *Nautilus* on the surface at slow speed if the ship had to do so in an emergency.

Representative VAN ZANDT. At any time during her life so far have you had to fall back on your auxiliaries?

Admiral RICKOVER. Yes, sir; we have fallen back on storage batteries for a short time, but Admiral Watkins, who is commander of the Submarine Atlantic Fleet, has told me that the *Nautilus* has operated with as great a reliability as any submarine he has in his entire force. She has not once failed to meet a planned operating commitment.

I understand you are going to have Captain Wilkinson in here next week to testify.

Senator JACKSON. Monday.

Admiral RICKOVER. He will come before Senator Jackson's committee, and I hope you ask him some of these questions.

The *Nautilus*, of course, has operated much more than any conventional submarine since she has been in commission.

Senator JACKSON. In order to get a proper projection of costs on the operation of the *Nautilus*, you would have to project the *Nautilus* into, say, 50 such ships.

Admiral RICKOVER. Yes, sir.

Senator JACKSON. And compare it with 50 conventional submarines with all the supporting elements, would you not, in order to get a fair overall operating cost?

Admiral RICKOVER. Yes, but Senator, you have a lot of intangibles in there, too. A war machine cannot be judged by cost.

Senator JACKSON. No.

Admiral RICKOVER. Cost is one factor. But a more important factor is, What can she do?

Senator JACKSON. In other words, the contribution she can make to our national security.

Admiral RICKOVER. I do not know how you are going to equate the operating cost with the military value.

Senator HICKENLOOPER. My question did not go to holding you down to dollars-and-cents cost. I want to get an approximation of comparison.

Admiral RICKOVER. Would you want me to get this information and file it for the record? I will be glad to.

Senator HICKENLOOPER. If it is at all reliable. I realize there are many, many intangibles involved.

Admiral RICKOVER. Yes, sir.

Senator HICKENLOOPER. You also have an experimental ship and have done a lot of experimenting with this thing, and I presume it would be very difficult indeed to get down to any accurate cost accounting on this thing. I just wanted to get some idea.

Admiral RICKOVER. I think I have given you a rough idea.

Senator HICKENLOOPER. Yes.

Admiral RICKOVER. I think the best way to sum it up is to state that the *Nautilus* is not a new type submarine; she is really a new weapon. I think you just cannot compare it with any conventional submarine. You would be comparing two dissimilar things. It is very difficult for most people, including people in the Navy, to realize, that a nuclear submarine is really a new weapon. We are going to be faced with the same problem in the Navy when we get the first nuclear-powered surface ship.

The next class of submarine after the *Nautilus* was the 578 class. This model [indicating] shows the *Nautilus*. It is 320 feet long and makes over 20 knots. It displaces 3,200 tons on the surface.

There are five of the 578 class. Four will be attack submarines and the fifth one will be a guided-missile submarine which is now being built at the Mare Island Naval Shipyard. Keels of all five have been laid. The first one, the *Skate*, will be launched in May at Electric Boat, and should be at sea about the end of this year or early next year.

This one [indicating] the *Skipjack*, is the latest type, with an *Albacore*-type hull. It has a single propeller.

The reason we went to a faster submarine was because soon after the *Nautilus* went out on her trials it became evident that for a submarine to remain effective against the most modern type of antisubmarine methods, she would have to make higher speeds.

Therefore, we designed this ship and she should be in operation about June of 1958.

Incidentally, the *Sea Wolf* went to sea this morning at 8 o'clock. Perhaps you would want to hear about the *Sea Wolf?*

Representative PRICE. Yes.

Admiral RICKOVER. I will get on that story now. I hope you do not mind me jumping around on this testimony.

Representative DURHAM. I would like to hear about the *Sea Wolf*.

Admiral RICKOVER. As you know, the *Sea Wolf* reactor uses sodium as a coolant instead of the ordinary water used in the *Nautilus* reactor. Sodium becomes about 30,000 times as radioactive as water. Furthermore, sodium has a half-life of 14.7 hours, while water has a half-life of about 8 seconds. As we went on with the testing of the *Sea Wolf* we found that even a very small leak in the heat exchangers would cause serious trouble. We went to full power on the *Sea Wolf* alongside the dock on August 20 of last year. Shortly thereafter she developed a small leak. It took us 3 months, working 24 hours a day, to locate and correct the leak. This is one of the serious difficulties in sodium plants. When you do have trouble, a considerable amount of time and expense is involved in correcting it because of the high radioactivity. We found that the trouble was a type of corrosion of stainless steel called stress corrosion; stainless steel has a tendency to become corroded by sodium. This means that unless the heat exchangers are absolutely tight and never leak there will be trouble.

We managed finally to fix the heat exchangers on the *Sea Wolf*.

We cut out some of the heat exchanger capacity which reduced the power about 10 percent. We also cut the superheaters out of the system which reduced the power another 10 percent. The *Sea Wolf* went to sea on the 21st of January, and she has been operating since that time. With the reduced power she makes about 90-percent speed.

I was on her during her first sea trials. She steamed about 800 miles, half of which were submerged. After operating for 4 or 5 days she went into drydock for structural repairs. This had nothing to do with the atomic powerplant. She got out of drydock and was at sea again for 8 to 10 days.

If a leak develops in a sodium plant on board ship—I am not talking about shore sodium-cooled plants—then it is quite serious to repair. The radioactivity must be left to decay, and repairs are lengthy and expensive.

But if you had a thimbleful of leakage in a sodium plant you probably could not run.

Senator BRICKER. What is the reason for adopting the sodium coolant?

Admiral RICKOVER. At the time we started on the nuclear propulsion program, sir, we went to two equivalent approaches. At that time, in 1947, we did not know which one would work. As a matter of fact, at that time we thought sodium had a better chance of working than water. Sodium had been chosen by General Electric for their power breeder at the Knolls Atomic Power Laboratory. Later on, when the Commission found that the design of the sodium-cooled power breeder was not going along well and the expense to build it would be too great, it was changed to a naval submarine project and we in the Naval Reactors Branch took it over.

No one knew at that time which would work better, and since atomic power was extremely important for the Navy we decided to follow the two approaches, but we did not know then which would be better. In fact, we were not then definitely sure that either one would work. Now that we have had the chance to operate both the *Nautilus* with her water-cooled plant, and the *Sea Wolf* with her sodium-cooled plant, it is obvious that water is much better than sodium for naval plants. All other nuclear ships, submarines and surface vessels, are being designed for water-cooled plants.

Senator BRICKER. What did you say the half life of sodium is?

Admiral RICKOVER. 14.7 hours.

Representative VAN ZANDT. What percentage of pressure have you lost?

Admiral RICKOVER. We have not lost pressure. We have lost heat transfer capacity. We have lost about 20 percent. The bypassing of the superheaters and the plugging of some of the heat exchanger circuits have reduced the heat transfer by about 20 percent.

Representative VAN ZANDT. Then you are working with 80 percent of capacity today.

Admiral RICKOVER. Yes, sir. But if we get more leaks we will have to plug additional circuits, and reduce the capacity some more.

Representative VAN ZANDT. What has that done to the speed?

Admiral RICKOVER. It has cut the speed 2 to 3 knots so far. Of course, the speed of a ship is not cut in proportion to the loss in power.

Senator Bricker, I think you have to recognize that if you want to get ahead with any game of this kind you have to take a chance. You do not know if it will work and if something is important you have to go ahead on more than one. You went ahead fully, experimentally, on both sides.

Admiral RICKOVER. Yes, sir. I did not consider the *Nautilus* a success until it was demonstrated that it worked all right for a long period of time.

Senator BRICKER. Is there any advantage in the sodium coolant at the present time that you know of?

Admiral RICKOVER. Theoretically sodium has advantages which may pay off in shore plants. You can get much higher temperatures and steam pressures, which means greater efficiency. Also it is possible to use lower pressures to circulate the sodium in the primary system. This means less pumping power.

For example, in the *Nautilus* we use high pressure in the primary system in order to keep the water as a liquid instead of boiling over into steam. On the *Sea Wolf* we use low pressure, just enough to force the sodium through the system. For this reason the pumping power in the *Sea Wolf* is only one-fourth that in the *Nautilus*.

Sodium has the advantage that it does not rust away the surface of material as water does. For this reason small particles of radioactive material do not get into the system and remain there for longer periods of time, and make access and maintenance difficult.

There may be advantages for sodium for shore-based atomic powerplants but I cannot see it for a ship. It is too dangerous for a ship.

After the *Sea Wolf* returned from her first trials she was docked at the Electric Boat Co. We then moored a conventional submarine alongside her and flooded the latter's ballast tanks to see how much of the *Sea Wolf's* radiation would carry through the flooded ballast tanks and into the ship. Even with the ballast tanks full, enough irradiation came through to the conventional submarine to give her crew in that vicinity as much radiation in 4 hours as is normally permitted for a week. Of course, in a while the radiation would be reduced a great deal because it has a half-life of 14.7 hours.

These are some practical problems we have learned about sodium plants. I am not saying these all apply to shore-based sodium plants. I am confining myself to ships. It certainly does apply to ships. As a result of this situation on the *Sea Wolf* and because the Navy is not building any more sodium-cooled ships, the Commission has decided to shut down the Mark A prototype plant at West Milton, N. Y. In order to save money, we are shutting it down. There is a letter in process which states this, but which may not yet have reached the committee.

Representative DURHAM. The letter has been received today.

Admiral RICKOVER. I wanted to mention it.

Senator BRICKER. That will be the only sodium reactor, then, in the whole fleet of submarines.

Admiral RICKOVER. Yes, sir. We intend to keep on operating the *Sea Wolf* as long as we can. If we get another sodium leak we will analyze it and see how expensive it is going to be to repair it. If it is not too expensive we will repair it and keep on operating because we can get very valuable tactical information today from any nuclear-

powered ship. We are not planning to have any more sodium-cooled ships in the Navy at the present time.

Representative VAN ZANDT. Have you had any personnel problems as far as exposures?

Admiral RICKOVER. No, sir. The maximum exposure we have had in the *Nautilus* per year in 2 years of operation is about 2 roentgens. That is the total for the year. The average radiation of people in the *Nautilus* during the 2 years of operation has been about 200 milliroentgens a week, or two-thirds of what is permissible in AEC laboratories.

Representative DURHAM. That West Milton plant could be used for civilian power; could it not?

Admiral RICKOVER. Actually, since it first started up we have gotten out of that plant about 2¼ million kilowatts of electric energy, of which about three-fourths of a million was sent out over the Niagara-Mohawk system. The rest was used on the site. But there is not much income from the sale of this power, sir. The cost of operating the plant is pretty high. That is, you get a few thousand dollars for the power but it costs very much more to operate. That is not a good financial deal for the Government.

Representative PRICE. It is not a good prototype for a civilian reactor.

Admiral RICKOVER. No, sir; because a civilian reactor would use an entirely different type of heat exchangers and an entirely different reactor system. A civilian plant would also be much larger. We are expecting to use the sphere in which the *Sea Wolf* prototype is contained for a destroyer prototype. I will discuss this in a little while.

Senator JACKSON. While on reactors, what about the gas-cooled reactor; what is the situation there?

Admiral RICKOVER. For a naval vessel?

Senator JACKSON. For propulsion.

Admiral RICKOVER. We made studies of gas-cooled reactors in the early days before we decided on sodium or water or gas, and we came to the conclusion first that a gas plant would be heavier and take up more space, and second, there was no assurance that there would not be leakage of radioactive gas into the ship. You may be able to tolerate such leakage in a shore plant. Since we do not know yet how to make any gas system tight, I do not consider it practicable for a ship. You are always taking the chance that radioactive particles may carry over from the reactor into the propulsion system or that radioactive gas leaks into the atmosphere. You can irradiate the crew this way.

A plant using air as the coolant would be so large it is impracticable for a warship where space is limited. Therefore, you have to go to a closed cycle plant. This type of plant should be tried out ashore and on other types of ships before it is tried in a naval vessel.

The Maritime Commission and the Reactor Development Division are having studies made at the present time of gas-cooled reactors for marine application. My personal opinion is that it will take a number of years before there is a sufficient degree of reliability to permit us to go ahead with a closed cycle gas-cooled plant in a naval vessel.

You see, our program consists not alone of reactors but also of ships that are appropriated for by Congress and we have to meet a date, and

ships have to work. That is a difficulty I labor under. That is demonstrated by all of these models. Everything you see here either has been built or is being built. They are not pictures of reactors. They are items that have been or are being designed.

Representative PRICE. Admiral, it would be advisable if you would just specifically describe to us the different types of reactor programs you are engaged in at the present time and then we might direct our questions toward those various types of reactors that you are actually working on.

Admiral RICKOVER. Yes, sir.

Representative PRICE. And then discuss those you think might be valuable to put into future programs.

Admiral RICKOVER. I discussed the Nautilus and the Seawolf. I would like to go on because I have so much more to say. I have talked about the 578 class. There are four 578-class attack submarines and one guided missile submarine.

These are the 585 class [indicating], which are single screw. There is 1 in the 1956 shipbuilding program and 6 in the 1957 shipbuilding program.

Incidentally, it may interest you to know this: I just checked up the amount of kilowatts we are going to have in nuclear-powered ships in the Navy. Projecting the present rate of nuclear shipbuilding we would have by 1963 about 1½ million kilowatts of power in operation and an additional million kilowatts in ships under construction. So we might have either in operation or under construction by 1963 about 2½ million kilowatts of atomic power in the Navy.

Senator JACKSON. 600,000 more than Grand Coulee.

Admiral RICKOVER. Yes, sir.

Representative VAN ZANDT. What is the mission of the Albacore type?

Admiral RICKOVER. It is an attack submarine. It can carry standard torpedoes. But its nuclear plant can also be used for guided-missile submarines; there will be three in the 1958 program. There are also two other types of nuclear submarines.

One type, and the one I consider very important, is the radar picket submarine. The reactor plant is being designed by the Knolls Atomic Power Laboratory of the General Electric Co. It will have two reactors. The submarine will displace about 5,800 tons on the surface and 8,000 tons submerged. It will be by far the largest submarine ever built.

Representative DURHAM. Is there any difficulty in procuring the raw material?

Admiral RICKOVER. Yes, sir. You mean fissionable material?

Representative DURHAM. Not only that—zirconium.

Admiral RICKOVER. I am glad you mentioned that. I might as well say right now and get into that—in order to carry this naval program through we have had to build up an industry. The first type of industry we have had to build up is zirconium. We started several years ago when the Atomic Energy Commission made a contract under competitive bidding and selected the one which offered the best terms, Carborundum Co. They contracted to supply 325,000 pounds of sponge zirconium a year for 5 years at a cost of about $12½ a pound.

Last year in looking into our prospective program we decided that by late 1958 we would have to have enough additional zirconium

sponge-making facilities for 2 million pounds of sponge a year. An arrangement was worked out between the Navy Department and the Atomic Energy Commission whereby the Atomic Energy Commission made the contracts, the Navy supplied the money, and we brought three new organizations into the program. They were selected on the basis of competitive bidding to see which ones offered contracts which were to the best financial interests of the Government. One of those brought in was the National Distillers Corp. They are putting up their own plant and developing their own process to make a million pounds of zirconium sponge a year for us.

The National Research Corp., is another contractor with whom we made a 5-year contract. I think they are building facilities to make a minimum of 700,000 pounds a year for us by still another process.

We made another contract with the Carborundum Co. to supply us from another plant which would produce for us about 600,000 pounds a year for 5 years. In addition to these contracts we made one contract with the Wah Chang Corp. to operate the Government-owned pilot plant we had at the Bureau of Mines in Albany, Oreg. Still another source of supply is the Hugo Neu Co. from whom we expect to get Japanese zirconium. We expect to get zirconium sponge under our new contracts for about $6 to $7 a pound, instead of about $12 as at present.

The companies who have these contracts, in addition to putting in enough facilities for carrying out the naval program, are also putting in capacity to take care of anticipated civilian reactor needs. We paid $300 a pound for sponge when we started out in 1948. It was only available in gram quantities at that time.

Senator BRICKER. What are the civilian uses?

Admiral RICKOVER. In addition to reactor use, it is used in the electronic industry for clearing out air and gases and so on in electronic equipment. It certainly will be used in civilian atomic powerplants, because a number of the organizations who are designing atomic powerplants are using zirconium as a basic construction material.

It also should find considerable use in the chemical industry because zirconium is much less corrosive than stainless steel at higher temperatures. It is far better.

Representative DURHAM. Does your program take the entire production from the companies you mentioned?

Admiral RICKOVER. It is currently taking nearly all of the entire production. But, as I said, the new companies are installing capacity to meet the naval requirements, and also other reactor requirements, as well as various commercial uses. But we are paying for our requirements. The other people who are designing reactors which use zirconium have not put up any money, as far as I know, to get the zirconium they will need. The Navy has put up its own money for this. The money comes from naval construction funds.

Representative DURHAM. In other words, the civilian reactors cannot depend on this production.

Admiral RICKOVER. These companies are building more capacity than is needed for the naval plants, so there should be zirconium available for civilian reactor plants.

It might interest you to know that in every one of these cases the companies put up their own money for their plants. We did not finance their plants; we did not recommend that they get a certificate of necessity. The contracts were made on a strictly commercial basis. From here on out we expect to be able to buy zirconium in the regular market.

The next important thing we had to do was to get more facilities for making reactor cores.

Our practice right now is to have our laboratories develop and make the first core of a type, and thereafter to put it out on competitive bidding.

For example, take the core for the 578-class submarines. About 8 months ago we asked for competitive bids for 5 of these cores. We now have a number of companies that have equipped themselves at their own expense to manufacture cores on a competitive basis: Westinghouse Electric Corp.; Olin-Matheson Co. at New Haven, Conn.; Metals & Controls Corp. at Attleboro, Mass.; Combustion Engineering Co. at Windsor, Conn.; and Babcock & Wilcox Co. at Roanoke, Va. Each company has put in at least $3 million of their own money, again with no certificate of necessity, for the necessary facilities. They have taken contracts for these cores on a competitive basis. On this basis we were able to order these cores for less cost.

We have just put out proposals for seven cores for another type of submarine. These will all follow the design of the first one, which is a developmental job, and which was designed and built at the Bettis Laboratory. All will be made in commercial facilities and competitively. We are getting industry to make as many other components as possible for our nuclear plants.

So that even as we are going along with the design of different types of plants we are establishing the industry necessary to carry the production along.

Insofar as components for nuclear powerplants for these ships are concerned—I looked this up just the other day—we have placed orders for $29 million for nuclear parts competitively—this is about 86 percent of what we are buying now on competitive lump-sum bidding, and only 14 percent is bought on cost-plus basis. This consists of items such as controls where the development is still in progress and sources of supply have not yet been developed. I am not talking about the steam machinery. That is all bought competitively. I am talking about the nuclear part. Even for the nuclear parts of the plant 86 percent is being put out on competitive bidding right now.

Representative Holifield. Private companies are doing it?

Admiral Rickover. Yes, sir; private companies are doing it. We do not want laboratories or plants operated by the Government to do it. We do not want to keep on building up Government's facilities. Private industry is doing it. I think you would be amazed if you really knew how extensive this private manufacture has grown.

Representative Durham. Do you think they are qualified?

Admiral Rickover. Yes, sir; we have taught them and supervised them, and we have had them send their people into places such as Bettis and the Knolls Laboratory where they are taught how to do it. Our people teach them how to do it. We help them set up their factories. We start them off by having them make small pieces. Before we assign them contracts for large items we assure ourselves that

they are capable of doing it. I do not have much concern about this at all.

Representative HOLIFIELD. Do you have any trouble getting private companies to bid on these different components?

Admiral RICKOVER. Not on reactor cores. We have difficulty with things like valves because they, as well as many other items, are very special. They have to be made leak tight, and they require a great deal of engineering. As long as we have an economy where people can get lots of business by doing conventional stuff not too many want to get into this special field. More companies would rather use their engineering staffs on conventional items because they can make more money that way. But those who look to the future know that this is good business to learn the new techniques which are going to be necessary if they are to remain competitive tomorrow. It is these forward-looking ones who are getting into our business. We have really had to fight with people in many cases to take our orders. It has not been easy.

Representative PATTERSON. Who pays for tooling on special valves?

Admiral RICKOVER. They do. We are trying to get this program into industry as fast as possible—to buy it like a loaf of bread.

Representative PATTERSON. You could not expect any manufacturer to put a special gear into a plant where he is going to make only 200 or 300 or probably even 2.

Admiral RICKOVER. Yes, sir; I do.

Representative PATTERSON. You expect him to do that?

Admiral RICKOVER. Yes, sir.

Representative PATTERSON. And have them pay for the tooling?

Admiral RICKOVER. Yes, sir. Sure. Why not?

Representative PRICE. Are they doing it extensively?

Admiral RICKOVER. Yes, they are.

Representative HOLIFIELD. Do they do it at a loss to themselves?

Admiral RICKOVER. Look, next year we are going to have about $50 million worth of reactor core business alone. It is like a fuel business. Once you get a ship in the water with a core you have got it there 3 or 4 years, then you have to take the core out and put a new one in. It is like the oil business.

Representative PATTERSON. Then do you go back to the original vendor you did business with——

Admiral RICKOVER. Competitive bidding.

Representative PATTERSON. Again?

Admiral RICKOVER. Mr. Patterson, that is what I thought Congress wanted us to do.

Representative PATTERSON. I am just asking the question. If you can get them to do it, fine, but I do not see how you can get the manufacturer to do that.

Admiral RICKOVER. We do.

Representative HOLIFIELD. They include the cost of that.

Admiral RICKOVER. Sure. It is going to be charged off. They have to charge it off over a reasonable period of time just as they do on any other venture, but I think we have babied a lot of people in this country too long with the glamor of atomic energy, and I think as soon as possible we have got to get down to do it like any other business.

Representative VAN ZANDT. Can we talk about some dollars and cents here?

Admiral RICKOVER. Yes. May I take a few minutes for the other ship?

Representative PRICE. Did you cover each one of your individual programs that you are working on? Can you cover that specifically and then we will ask questions?

Admiral RICKOVER. Yes, sir, in only a short time.

I have covered these three types of submarines and the large radar picket submarine—which can operate either as a surface ship or as a submarine. It is designed to go along with a carrier task force.

Representative COLE. Why do you have two reactors?

Admiral RICKOVER. In the first place, if there were only one you would have quite a large monster, and if that one went out you would have a valuable ship which costs more than $100 million in a helpless condition.

It is like any other important ship; you install more than one plant. The two plants are exact duplicates.

Representative VAN ZANDT. Is that an exorbitant cost?

Admiral RICKOVER. It is a high cost, but it is for the first one. Follow ships will be cheaper. I will give you the cost of every one of these ships in a moment, sir.

The last type nuclear submarine plant we are designing is for a hunter-killer submarine, a type which we could use in large numbers.

This represents another departure from our previous practice. Combustion Engineering has put up their own money for all of their facilities at their Windsor Laboratory. Not a penny of Government money has been used in their laboratory facilities. They have built and paid for all their own critical experiments. They have not only done all these things at a cost of about $10 million, but they have also put up $3 million of their own money to assist in the research and development of this submarine project. The prototype for this plant is now being built on land which Combustion Engineering has let us use near their laboratory at Windsor, Conn.

The Electric Boat Co. is building the prototype for Combustion Engineering, and there is a corresponding submarine in the 1958 shipbuilding program. We are building as small a reactor as we can for this horsepower to keep the ship small. It will be as quiet as possible. I have now covered all of the nuclear submarine types.

There are three other types of ships—surface ships. First we have the aircraft carrier, for which Congress last year authorized the Navy to start buying the machinery—about $21 million to start manufacture of both the conventional and the nuclear machinery.

The nuclear-powered aircraft carrier will have 8 reactors, 2 reactors per shaft. The old battleships such as the *California* and the *West Virginia* developed 32,000 shaft horsepower or about 22,000 kilowatts altogether. This ship will have an auxiliary plant which will use more power than the main propulsion plants of the battleships of the last World War.

I am not going into the military characteristics of the carrier. I am sure Admiral Burke, when he testifies, will do a better job on that than I can. I will just talk about the technical aspects. As I mentioned before, the Navy expects to ask for one of these each year, starting with the 1958 shipbuilding program.

Another surface nuclear powered ship is the cruiser. This is the one which Secretary of the Navy Thomas recently named the *Long Beach*. It will displace about 14,000 tons. It will carry a considerable number of guided missiles.

It uses, in general, the same type of reactor as the aircraft carrier. There will be two reactors in this cruiser. The keel for the *Long Beach* will be laid in less than a year, and she should be completed late in 1960. This ship is being designed, and will be built at the Quincy, Mass., shipbuilding yard of the Bethlehem Steel Co. The reactors are being designed at the Bettis plant of the Westinghouse Electric Corp.

The last ship in the program is a frigate.

At the present time the Navy is mostly depending on destroyers for antisubmarine warfare, but our destroyers when they make high speeds, have to be refueled at short intervals. This creates an intolerable situation because they cannot carry enough fuel to operate for adequate periods of time at high speeds without refueling. We found that out when we sent the *Nautilus* out to be attacked by the destroyers. The destroyers would go out and operate with the *Nautilus* and have to go back to port to refuel if they ran at full speed too long a time.

The AEC has assigned a project to the Knolls Atomic Power Laboratory to develop a powerplant for a frigate. We are trying to design the machinery plant with as low a weight as possible. Work on the frigate reactor design has just been started. I am not sure whether the committee has as yet been notified about it.

Mr. RODDIS. I do not think the letter is here yet.

Admiral RICKOVER. Possibly it has not gone out of the Commission yet.

To sum up, the Navy's nuclear propulsion program consists of the *Nautilus* and the *Sea Wolf* which are experimental types. Then we have the 578 class plant. Following that one we have the *Albacore* hull type which can be used either in an attack submarine or in a guided-missile submarine. Next there is the radar picket submarine which can accompany a carrier task force, and finally there is the hunter-killer submarine. For surface ships there are the three types: aircraft carrier, cruiser, and frigate.

So with the types of nuclear propulsion plants we are now working on we envisage being able to power any important naval vessel. This can be accomplished by using one or more of the reactor plants I have described. We are right in the middle of a vast program for changing our Navy.

A most important submarine is the one with two reactors. Its propulsion plant can also be used for large submarines carrying ballistic missiles. In fact, we had this in mind from the very first. It was changed over from the original concept of a missile-carrying submarine to a radar picket submarine because the submariners did not like it. They were not smart enough then to see the possibilities of such a submarine, and so they did not back it. But the surface ship people saw its utility, and backed it. That briefly describes our program.

I have mentioned the fact that we are getting industry into our program, and I should like to elaborate on that.

Representative VAN ZANDT. The Congress appropriates, do they not, to the Navy and its shipbuilding program for this nuclear powered construction; is that right?

Admiral RICKOVER. Yes, sir.

Representative VAN ZANDT. Is that money positively spent on the nuclear powered ships?

Admiral RICKOVER. Yes, sir.

Representative VAN ZANDT. Can it be diverted?

Admiral RICKOVER. No. The Navy really wants atomic powered submarines and the Navy definitely does not divert any money from nuclear submarines.

Representative VAN ZANDT. A few years ago surely they had a tendency to divert some of this money to conventional type construction. Now is it different?

Admiral RICKOVER. It is different now. The thing that holds us up from going even further in nuclear power in the Navy is that we have got to prove some of these designs. You people always ask the question, "Why do you build any other ships, now that you have the nuclear power?"

You have to put yourself in Admiral Burke's place. He has commitments he must meet today in the Near East, and he has not got the fine nuclear-powered ships. He has got to have ships today; he cannot just obligate all his construction money on the nuclear-power program. Perhaps a generation from today everybody will say, "They were damn fools for not having done it," but the people who may say that will not have been faced with the problems that we are faced with, those of us who have some responsibility are faced with.

Representative VAN ZANDT. From an operational standpoint, when will the nuclear-powered ships start to phase into the fleet?

Admiral RICKOVER. The *Nautilus* and *Sea Wolf* are now at sea. The first one of the 578 class submarines will be completed about the end of this year. After that there will be a new ship about every 6 months. About 3 submarines should be out in 1958. As you know, a total of 15 submarines have been authorized for construction. I should mention that we have also spread the base for ship construction for nuclear-powered ships. We now have two more commercial submarine building yards besides Electric Boat.

The Ingalls Shipbuilding Corp., in Pascagoula, Miss., and the Newport News Shipbuilding & Dry Dock Co. have been awarded contracts for building nuclear-powered submarines. This is a very fine thing because we will now have three commercial shipyards to build nuclear-powered submarines. Since there will be a large number of nuclear-powered submarines, we will be able to increase the design talent available. It will also give us an opportunity to get competitive bidding on submarines instead of always going on a negotiated basis or on a cost-plus-fixed-fee basis. In fact, the first nuclear submarine contracts awarded to Ingalls and Newport News were on a fixed-price basis.

So, you see, we are trying, as far as possible to get out of the system we have been using of paying a man whatever it costs him to do the job; instead to give him an incentive for doing it efficiently.

Representative PRICE. Are you on a competitive-bid basis now on the allocation of submarine contracts?

Admiral RICKOVER. Yes, sir. The last 4 nuclear-powered submarines were awarded, 1 to Electric Boat, 1 to Newport News, and 2 to Ingalls. The three companies submitted bids, and the submarines were awarded on a fixed-price basis. It is not yet 100 percent pure competition, sir, but pretty close to it.

In a fixed price, the Navy, I think, paid roughly about $40 million per submarine.

Representative DURHAM. Has there been any cutback on the program?

Admiral RICKOVER. No, sir.

Admiral RICKOVER. There were six in the 1957 program. There will be four in the 1958 program.

Representative PRICE. I know only four are proposed in the 1958 program.

Admiral RICKOVER. Only four are proposed in the 1958 program. That is correct, sir.

Representative PRICE. Has there been any cutback financially to your program.

Admiral RICKOVER. Well, I have not gotten as much money as I could use, but I think any witness you get up here would tell you that.

Representative PRICE. Did you get what you asked for?

Admiral RICKOVER. I got what I asked for from the Commission. I did not from the Navy. I get only a small portion of my development money from the Navy. I think you understand that without the support of the Atomic Energy Commission, and the freedom with which we can operate in the AEC, we would not have any nuclear-powered naval vessels today. The AEC budget contains all that I asked for this year. However, the naval program is advancing so rapidly that it appears now we could use 9 to 9½ million dollars more of AEC funds.

I want to make it clear that the Commission has budgeted for the money I asked. I do not want any misunderstanding on that score.

Representative COLE. Why did you say the Navy would not?

Admiral RICKOVER. The Navy in the early days did not put much money into the program. They are still not putting in as much money as I need.

Representative COLE. So many ships in that many years does not look like that.

Admiral RICKOVER. I am talking about research and development funds, not construction funds. Today in fiscal 1958 I am getting $86 million from the Atomic Energy Commission in research and development. I am getting about $11 million from the Navy. I tried to get $20 million from the Navy and could not, and that set us behind.

Senator JACKSON. You were cut almost $10 million.

Admiral RICKOVER. I was cut about $7 million in the Navy. I mentioned this to Admiral Burke the other day. He is going to help us as much as possible. But you realize that our major support in research and development comes from the AEC. Furthermore, no one else has as fine a committee to deal with as you people. I mean that. I am not just joking. Your committee has been a great support. Without this committee the Atomic Energy Commission could not possibly fare as well as they have, nor could the naval program have fared as well.

Representative COLE. Was it because the Navy did not want ships?

Admiral RICKOVER. Not ships. I am talking research and development.

Senator JACKSON. I think for the record it should be said that 2 years ago as a result of the work of this committee, Admiral Carney dropped one of his conventional submarines and added an atomic one. But there has been a lag, there is no doubt about it. There has been hesitation to take on something new. I think it unfortunate that just 2 years ago we were building 4 conventional submarines or 5.

Admiral RICKOVER. The fifth one of these conventional guided missile submarines was changed over to nuclear power. We are using a reactor for a 578 class and we are installing it in what started out as a conventional guided missile submarine at the Mare Island Naval Shipyard.

Senator JACKSON. The proposition was the Navy had asked for funds at a time when you had not commenced the trial runs for the *Nautilus*.

Admiral RICKOVER. Yes, sir.

Senator JACKSON. Subsequently, the trial runs took place but apparently someone failed, down in the Navy, to take cognizance of those results and so we had before the committee the fact that they were asking money for conventional submarines without having taken into consideration the results of the *Nautilus*. It was one of those unfortunate things. Admiral Carney was decent enough to admit it, but it was a lag that occurred and they did correct it to the extent of one.

Representative PRICE. I think the Navy has not asked for more.

Senator JACKSON. More conventional submarines?

Representative PRICE. That is right.

Admiral RICKOVER. You do have Admiral Burke over the barrel to this extent. Prices are going up 7 percent a year on everything. The Navy gets a certain amount of money for ship construction in accordance with the President's proposals, and the Bureau of the Budget. The nuclear-powered ships cost a lot, but he still has to meet today's problems and commitments.

I would strongly urge that for any development type ship there should be a separate budget. This would encourage the Navy to go ahead and try out new ships. As long as you attach costly development type ships to the one shipbuilding budget it poses an almost intolerable situation for the people responsible for running the Navy. It would not do any good to set up another budget but reduce the regular naval construction budget. That would not help. If something like a separate additional budget for the first of a development-type ship could be set up it would be a tremendous help in improving the Navy.

New type ships are very expensive, so if the Navy could be encouraged by having a separate budget for the first development ship of a type it would be a very tremendous thing. Because now Admiral Burke is spending for the nuclear-powered cruiser and for the nuclear submarines—and I imagine that is why there are only four submarines in fiscal 1958—he is spending quite a considerable portion of the shipbuilding funds on just a few ships, but he has many world commitments to meet, so what else can he do?

Representative PRICE. On your shipbuilding reactor program, are you just working on 1 type of reactor for each ship or do you have 1 or 2 or maybe more reactors that you are exploring for the various ships?

Admiral RICKOVER. For the submarines we are exploring different types, but for the surface ships, because of the large amount of development money involved, we are only working on two different types. That is for the aircraft carrier and the cruiser. Considering the many billions of dollars——

Representative PRICE. You mean 2 types for each ship or 1 type for the aircraft carrier and 1 type for the cruiser?

Admiral RICKOVER. It is immaterial whether it is for either one. We have a reactor a certain size.

Representative PRICE. They would fit in each ship?

Admiral RICKOVER. They would fit interchangeably. It would be wise from a national standpoint to be working on more than two types, considering the fact that we are going to be spending many millions of dollars on these ships.

Representative PRICE. Have you recommended we work on more than two types?

Admiral RICKOVER. I would like to recommend it. I would like to be encouraged by you to recommend it.

Representative DURHAM. That is the prototype land base you are speaking of?

Representative PRICE. If you think there is some question about the feasibility of it there might be a possibility of getting a more efficient reactor from one of them. I do not know why we should not explore it.

Admiral RICKOVER. You see, Mr. Price, we cannot ask for just any sum of money. We have to ask for something within reason. And I say, the Commission certainly has treated me very decently. But if I thought I could get more money, I would ask for more, because I know I can use it wisely.

Representative DURHAM. AEC furnishes all the money for prototypes for shore bases?

Admiral RICKOVER. Yes, sir; the AEC does that.

Representative DURHAM. Have you any new ones planned in the program?

Admiral RICKOVER. There will be one new one, but we have not asked for it yet. It has not gone far enough. It will be the frigate prototype. We expect to install it in the sphere at West Milton. We expect to ask for it in fiscal 1959.

Representative PRICE. Do you have in mind any particular type of reactor you would like to experiment with?

Admiral RICKOVER. Yes, sir; I would like to work on another type of reactor that would fit into a cruiser or an aircraft carrier. We are doing two different types now. I think it is important enough that we should do still another one.

Senator JACKSON. Mr. Chairman, I think it would be helpful, without going into it now, if we could get from the Admiral some indication of what additional funds he feels he needs. We requested it of him. It is not in the budget for fiscal 1958, under the request relating to reactors, research and development, and that whole area. If you could submit that to us at our request?

Admiral RICKOVER. I can give you a rough round figure. I could use $20 million. I could use in this fiscal year about $7 million or $8 million more. I could use a total of $20 million and get good use out of it.

Senator JACKSON. Just AEC funds?

Admiral RICKOVER. A combination of AEC and Navy funds. It would be roughly $10 million AEC and $10 million Navy. It is immaterial where the money comes from. I think you know we use some of it interchangeably. Your committee knows it and has approved of it.

Representative PRICE. How much do you need for this shipbuilding reactor program for one additional approach?

Admiral RICKOVER. It would take about $10 million.

Representative PRICE. About $10 million.

Admiral RICKOVER. About $10 million.

Representative VAN ZANDT. Have you the personnel?

Admiral RICKOVER. I can train additional personnel. That is a problem that comes up with any new program. If you have got to do something and you have an organization, you just go ahead and get more people and train them. No one doing a real job ever has any extra people.

Representative COLE. What are the two types you have now?

Admiral RICKOVER. We have two types which are good either for the aircraft carrier or the cruiser.

I will sketch it. You have in the center a part that is enriched uranium, and surrounding it is natural uranium. This is what we use on the PWR. In the PWR there are about 70 kilograms of U-235. The rest is natural uranium. The reason we have designed the core this way is because we are trying to use a minimum of enriched uranium in the large cores. We have to have enriched cores in the little reactors such as those for submarines, but for the larger ones we try to use as much natural uranium as possible, because to use enriched uranium entirely simply means running the natural uranium through a diffusion plant, and so burning up coal. There is not much sense to burning up a lot of coal, and then put it in a ship and use it for power, unless there is no other way to do it.

We are using the same principle of using as much natural uranium as possible, which can be used in one of the reactor cores which we are designing now, either in the aircraft carrier or the cruiser. We have designed it this way in order to have a minimum dependence on diffusion plants. We do not like to use too much enriched uranium because it is the same as burning up coal indirectly.

Representative COLE. Was it new?

Admiral RICKOVER. I do not know what it would be.

Representative COLE. I thought you had a concept.

Admiral RICKOVER. No, I would like to turn it over to the scientists working on it and have them come up with another type. I know I would like to design another one, but I do not yet know what it will be, sir.

Representative PRICE. I wonder if you could explain the different models so we know what we are looking at.

Admiral RICKOVER. Yes, sir. Let me first show you these charts. This is the control cubicle, the complete control for the propulsion plant of the submarine. This is where the operator controls the tur-

bines, and this is where he controls the reactor, and this is where he controls the electrical plant. This cubicle is very small. I think you are familiar with the large control stations you normally see around the various production plants. We have to get the control station very small. This shows you how small we can get it.

This chart shows the reactor which can be used either in the cruiser or in the aircraft carrier. This model of a man shows its actual size.

We have ordered the reactor pressure vessels both for the cruiser and the aircraft carrier. They have been ordered by competitive bidding. We have ordered the heat exchangers for both of those ships on competitive bidding, too.

This is a pretty good picture of what the aircraft carrier will look like. It has a 1,040-foot water line length, and almost 1,100-foot length flight desk. Her displacement will be about 86,000 tons.

Representative DURHAM. Could it get through the Panama Canal?

Admiral RICKOVER. No, sir. Nor can the other large carriers.

Representative VAN ZANDT. Is it possible to build a reactor into those ships with sufficient fuel to provide propulsion for the life of the ship, based on, say, 15 to 20 years.

Admiral RICKOVER. I would say this: with the type of operations we are doing now we sure should get about 5 years of operation.

Representative VAN ZANDT. Five years?

Admiral RICKOVER. Five today. But progress in reactor design is going so fast, sir. I think you could figure that ultimately you might get a reactor which will last the life of the ship. The goal we have now is to design a reactor which will last for a war. I think we have come pretty close to that. That is a great thing in itself because nuclear-powered ships will not need oil bases and oil storage tanks and oil tankers. Furthermore, you could store all the fuel you need for a nuclear navy in a few buildings. The fuel is not radioactive, and if you do not use it for the Navy you can always convert it to other uses.

This is the guided-missile cruiser. It is 690 feet long with a beam of 70 feet. This cruiser will be built at Quincy, Mass. It will displace about 14,000 tons.

This picture shows a preliminary design of a nuclear powered destroyer. We already have the nuclear propulsion plant in an early stage of design. The Knolls Atomic Power Laboratory is designing the reactor.

To show some of the models I have brought, this is a cross section of the core in the 578 class submarine. This is one of the fuel elements, and these crosses are the control rods.

This model is the machinery space of the 578 class, and this is the reactor plant.

We have two different types of reactor compartments for these plants. We have one where we use a tunnel type shield, that is, we spread the machinery out, and we use the B–29 tunnel concept of just shielding the passageway where the people walk through. The other type uses a regular shield deck like in the *Nautilus* where everything is under the shield. We are trying out both of these. We are building two ships of each type, and are going to try them out in service and see which is the better.

This is the *Albacore* hull type submarine. This is a cutaway of the pressure vessel and the core. The core is down here, and this is the

control-drive mechanism. You can get an idea of what the pressure vessel head looks like.

This is a model of the 578 class submarine.

This is another model of it.

This is a very interesting reactor.

This is the reactor which will go into the large radar picket submarine. It is a different type than the others. The others all use plates for fuel elements.

I have here a sample of a control-drive mechanism for the aircraft-carrier core. This one will be used at the carrier plant prototype at Arco. As you know, that prototype has two reactor cores.

Here is a sample of the control drive mechanism for the radar picket submarine. It is being designed by the United Shoe Machinery Corp., of Beverly, Mass.

Representative DURHAM. Is the shoeman in this business too?

Admiral RICKOVER. The United Shoe Machinery Corp. is pretty good in developing mechanisms. You see in the shoe business they have to use a lot of very complicated machinery. They are experienced in developing mechanisms. This gives you some idea of how we are spreading our development out.

Senator JACKSON. The Russians will have a terrible time trying to figure out what the American corporations are doing if they just read the stock lists and try to make a decision.

Admiral RICKOVER. That is right. We have companies such as Borg-Warner, Bendix, and Aerojet making controls. We have all kinds of companies in our business.

This model shows the pressurized water reactor. You will have to come closer to see it.

The scale of this PWR model is, I think, one-eighth. This is the core. This is the core loading and unloading mechanism.

I do not know if we brought one of the 600-pound bolts here or not.

Representative DURHAM. No.

Admiral RICKOVER. These bolts are 6 feet high and weigh 600 pounds, and secure the head to the pressure vessel. There are 43 of them. You can see that they are large items.

I wanted to talk a little bit about declassification before it is too late, because Dr. Libby has been involved in this. There has been a lot of criticism of the Commission for not putting out more information. So recently we declassified all naval reactor technology, but not the designs.

I would like to make a distinction between technology and design. I must make this clear so you do not misunderstand me. The experimental information which has been developed in the naval program is now unclassified and is available to all of our industry. But the actual details of the designs are confidential. That is, we do not give away design details.

The PWR is completely declassified and all the design details as well as the technology is now available to the public. We just started putting out all of the PWR reports on an unclassified basis. In naval reactors program all of the technology, all of the scientific and engineering information that anyone in this country needs to know to build a water-cooled reactor can be obtained. But we do not give away actual dimensions and design details except to those who need to know them.

There were two good reasons for doing it. One, as I see it, the Government is directly and indirectly subsidizing everybody anyway, so we might as well give them the information.

The other thing is it is getting too darn hard to operate with so many people coming into the laboratories and getting the information personally. We either had to spend much of our time and the time of the people in the laboratories handing out the information on a personal basis, or else do this through declassification. We decided it was better to take a little bit of a chance and so be able to get ahead with our own work. What we have done is, I think, rather significant. It will take some of the curse off the Commission and they will not be accused so much of not handing out information.

We have done another thing in the naval reactors program to make information public. We have put out a series of manuals on our program over the past few years.

We have published five manuals so far: The Liquid Metals Handbook, Metallurgy of Zirconium, the Metal Beryllium, Reactor Computer Codes, and the Reactor Shielding Design Manual. I have copies of these books here on the table. Another manual, Corrosion Handbook for Water-Cooled Reactors, should be published in about 2 months. There are five more on various technical subjects being written. They will all be published within about a year and a half. All of these manuals are completely unclassified. They are all real working books for people who have to do reactor design work. A man can use the shielding handbook, for example, and design a shield.

Representative HOLIFIELD. What is your evaluation of the philosophy behind making, let us say, all the shielding information or all the metal information like zirconium available? That immediately, of course, becomes available throughout the world. Do you think that gives us a disadvantage with our enemies?

Admiral RICKOVER. Certainly it does, sir. But this is the problem we are facing all the time: You get more of an advantage than you get a disadvantage. You get much more of an advantage because you get the information out to your own people. There is no use in subsidizing a lot of people to do work on zirconium in the United States and hinder them by not giving them the information which is available. But another serious thing——

Representative HOLIFIELD. In other words, the know-how, you are training them for a wider basis of participation.

Admiral RICKOVER. The information in the zirconium book does not tell you how to design a core. You have to get much more information to be able to do that. But it does have the complete story on how to make and use zirconium. I am not so sure other people are not just as smart as we are, anyway.

I think sometimes when you lock yourself up in a room and put the restricted data tag on it, I think perhaps you are kidding yourself more than you are other people. You can very soon get yourself in the position of thinking that you know it all, when you really do not.

These handbooks are put out commercially, by the way, and the publishing companies want to print them. By having these books available you get the people in the universities and in other places starting to think about the problem and making improvements. I do not think we could have gotten lower prices on zirconium if we had not made this information widespread.

Mind you, these books contain technology. We are not putting out actual design information on the dimensions of the shield for any of our ships. We do this for the PWR. That shield is concrete anyway.

Representative DURHAM. Was that not the first book published entirely on zirconium?

Admiral RICKOVER. There have been many papers published on zirconium, but this book is now the standard for zirconium all over the world, as far as I know. It certainly has all of the latest information in it.

It has another advantage. It was written by the people who are actually working with zirconium. Normally, text books are about 10 years out of date by the time they are published. But when it is done by the people who are actually working on it you have something right up to date.

Representative HOLIFIELD. In other words, the diffusion of this knowledge, you figure, among a wider base of potential engineers and scientists and so forth will give us the capacity for rapid advancement which you could get no other way.

Admiral RICKOVER. I will call on Dr. Libby to remind him of that famous day quite a few years ago when I appeared before his committee—he was at that time the chairman of the senior reviewer's committee—urging him to back us in putting out a Liquid Metals Handbook. That was about 1948, and it was the first time there had been any declassification in reactors. As a result of the Liquid Metals Handbook we got people in the universities and in other places to start working in this field.

You will find that today these are the standard books in the United States on this subject. There are no others. There are not any others with detailed scientific and engineering information in this field. So you have to make up your mind 1 of 2 ways. You are either going to keep the information locked up in Government or give it to industry and to the schools and so make it available to many people.

Sure you are giving information away. Is that not one of the strengths of a democracy, that we assume we can live in a democratic way and still stay alive? It does not mean that we must give away all the details of everything, but it does mean we must give away enough so that our own people can be aware of our problems and help us. You cannot give without taking chances.

Representative HOLIFIELD. It is the only way you can really widen the base in this shortage we have?

Admiral RICKOVER. I am not talking now, believe we, from a theoretical standpoint. We declassified information because we found out that our work was being hampered.

Representative VAN ZANDT. Admiral, are these books becoming part of the atomic library that the AEC is planning throughout the Nation?

Mr. RODDIS. Yes, sir.

Admiral RICKOVER. There are quite a few more coming out.

As I have said, six more are coming out in the near future. There is one every half year or so.

Representative DURHAM. Have you declassified the separation of hafnium from zirconium?

Admiral RICKOVER. Yes, sir.

Representative DURHAM. When did you do that?

Admiral RICKOVER. I think in the last 2 or 3 months. We will include this information in the Hafnium Handbook which is now being prepared. We found out the Russians know about hafnium anyway.

One other thing I believe I should tell you is how we run our projects. We operate the Naval reactors program in somewhat of a different way than other programs are operated.

There are two theories for running reactor programs. One is to assign it to a contractor and give him the money and let him run the job all by himself. The other way is to retain control at a headquarters organization.

When you have to design and build a reactor for a ship you cannot turn it over to somebody else because it has to be integrated right into the ship. For instance, a submarine has to have neutral buoyancy. You cannot leave it up to a contractor to start designing the reactor, and a year after the ship is started you suddenly find out that the reactor weighs too much. The ship will sink.

So our philosophy has been from the beginning, and we have used it on the PWR also, that the Naval Reactors Branch approves the details of the design. We keep in constant touch with what the reactor designers, the machinery designers, the shipbuilders, and the construction contractors are doing.

We have in my organization at headquarters about 90 officers and civilians. They constitute the headquarters engineering and administrative staff.

These people work both for the Atomic Energy Commission and the Navy interchangeably. There are officers and there are civilians, and I think you all know that I assign people to positions on the basis of ability and not rank, or whether he is a civilian or an officer. The best qualified man gets the job.

We operated this way for a long time, and it is now accepted. In my opinion it is the only way you can run any kind of technical organization; put the best people in the most important jobs.

Senator JACKSON. Would you explain the two hats I think you wear at some point?

Admiral RICKOVER. Yes, sir. My primary duty is on assignment to the Atomic Energy Commission. I am Chief of the Naval Reactors Branch of the Atomic Energy Commission. So I act for them with the AEC laboratories and other activities. I have an additional duty as Assistant Chief of the Bureau of Ships for Nuclear Propulsion. That means that anything that has to do with the nuclear-populsion plant for the first vessel of a type comes under my cognizance. Of course, I am responsible to the Director of Reactor Development in the Atomic Energy Commission and to the Chief of the Bureau of Ships in the Navy Department.

Senator JACKSON. Is this two-hat system working?

Admiral RICKOVER. Yes, sir. I would advise anyone who has a large-scale development job to do to adopt 2 hats or 3, whatever number is necessary. It is a new experiment in development administration. We have never had anything quite like this. We have always had liaison among bureaus and between one Government agency and another. But here there is one group of people working for both a civilian agency and a military agency. It is sometimes a little diffi-

cult, but it works, and it works particularly because the AEC people have sure stood for a lot from us—I don't know why, but they do—and they deserve a great deal of credit for their forbearance and their help. In my opinion, this type of Government operation is absolutely essential in the future for any large-scale development operation. If you do not have it this way you get fouled up with lots of redtape.

Representative VAN ZANDT. Is there any effort afoot to dissolve this unique organization you have?

Admiral RICKOVER. I do not know. I would imagine there are people in some places that are not too happy with it, but I certainly hope, whether it is me who is involved, or someone else, that this organization not only is not dissolved, but I would strongly urge that people in Congress should eye this method as a new form of Government research and development administration. I think it is absolutely essential that the military no longer attempt to fight wars all by themselves, or do large-scale research and development all by themselves.

My organization is really more than a two-hat one, sir, because it includes more than the AEC and the Navy. Actually there is a third hat—industry. I think some of you know how closely we work with industry. We have gotten a real integration of industry, the Navy, and the AEC on this job. There is no other way we can do it.

Representative PRICE. We are getting close to that point at least. Could you explain your Branch? The Naval Branch of the AEC is set up to carry on the Shippingport project?

Admiral RICKOVER. Yes, sir. The PWR Shippingport project was assigned to me by the Commission because of the previous experience that my group had had with pressurized-water reactors. The Westinghouse Co.'s Bettis plant was assigned to do the necessary development work. The development work is now almost finished for the first core. The Duquesne Light Co. was selected from among nine contenders to be the operator of the entire plant because they made the best financial offer to the Commission. But my organization is responsible to the Commission for the design, the construction, and the operation, and we spend a great deal of time on it.

The Bettis plant submits the nuclear-plant designs to us and we analyze them and reach joint agreement. In many cases the Duquesne Light Co. is also involved in nuclear problems, because they are going to operate the plant. We are able in this way to reach quick decisions between the Government, Westinghouse, and Duquesne.

We are able to operate this way because there is a single, assigned responsibility. Otherwise, we would all be writing letters from now to doomsday, between the Commission and somebody else, to get permission to do anything. We are able to do our work with few letters and no fuss. There has never been a single letter written between the Commission and the Duquesne Light Co. since the contract was signed with them. It has never been necessary, because all of the dealings have been done orally between myself and their manager or Mr. Philip Fleger, their chairman of the board.

Representative PRICE. You mentioned a minute ago you had about 90 people in your headquarters organization. Is that strictly for the ship-reactor program?

Admiral RICKOVER. No, sir; that includes people who work on the Shippingport reactor. But, besides these people, we use somewhere

between 150 and 200 people in the Bureau of Ships to work with us not only on the naval program, but also to help on the Shippingport project. It makes no difference what Government people do the work, because the money all comes from the same source. The Navy is glad to help, because we are not only helping the Commission and the country, but we are also learning a great deal for the naval program by doing so.

We try as far as possible not to duplicate facilities between the Navy and the AEC. For example, when we buy nuclear cores on competitive bidding for the Navy, for which the Navy pays, we do it through the AEC. If we did not do it through the AEC, we would have to set up a separate organization to account for the fissionable material, and to prepare the specifications, to inspect the cores, and to take care of other items.

So far, we have been permitted to use the Atomic Energy Commission facilities for this purpose. I strongly urge that, wherever we can, regardless if the core is for a naval reactor, as long as the AEC has the people and facilities for doing so we should use them, and not set up additional, duplicate, Government organizations. It is not only wasteful of manpower and money, but every additional organization you set up saps your strength and does not give you time to do your technical work. You get completely bogged down with administration and redtape.

Representative PRICE. Have you moved this Shippingport organization over to some other project intact, or what will you do with that?

Admiral RICKOVER. We are working on a new core design for the PWR reactor. The present core is designed for 60,000 kilowatts of power. The Commission authorized us about a half year ago to work on a new core which will be of an improved design. The new core is being designed to generate 100,000 kilowatts of power and to produce this power more cheaply than the first core. We have already started the new core design for the PWR reactor.

Representative PRICE. How close do you come to keeping any sort of schedule on the Shippingport project?

Admiral RICKOVER. We have not come close to keeping on schedule.

(There was discussion off the record.)

Representative PRICE. I saw where you got considerable publicity on a speech you made recently about the engineering and technical problems encountered in the atomic-power program. In view of your experience with PWR and Shippingport and other facilities, do you think we have a realistic schedule now on our current atomic power development program?

Admiral RICKOVER. From my experience so far, I would say that all reactors are going to cost considerably more and take considerably more time to design and construct than was originally anticipated. I am, of course, talking from my own experience, but certainly we are pushing our programs hard and still running into many delays, so I would think other people are going to experience the same thing. As you know, the costs at Shippingport have gone up.

Representative HOLIFIELD. How much have they gone up?

Admiral RICKOVER. Congress originally authorized the Shippingport project for a total of $100 million. We estimated then it would cost $85 million. The latest cost estimate as of today is about $42 million for research and development and $55 million for construction

of the nuclear portion of the plant. Of this $55 million, $5 million was given to the Government by the Duquesne Light Co. as part of the contract we made with them when they were selected. So the Government cost is $50 million for construction plus $42 million for research and development, which makes a total of about $92 million. That will be the cost provided we can finish it in a reasonable time. If the time is stretched out, the cost will go up.

(Discussion off the record.)

Admiral RICKOVER. The costs of all reactors are going up, sir. I would estimate from what I have seen you can expect probably at least a 50 percent increase in all of the large reactor projects. I am talking from my own experience. This is what my experience indicates.

(Discussion off the record.)

Representative PRICE. Admiral, going back to your experience at Shippingport now, do you think your trained force, experienced people, with the experiences they obtain and develop at Shippingport and other jobs in your program, could take on the job of building a large-sized, gas-cooled reactor to supplement the present power program?

Admiral RICKOVER. Mr. Price, you know I am not responsible for civilian reactors.

Representative PRICE. I know you are not. I am just asking, do you think your people, at least from Shippingport experience, would be interested in such a project and kind of like to undertake such a project?

Admiral RICKOVER. If it were assigned to us we would take it. That is up to the Commission.

Representative PRICE. Would you like to do it?

Admiral RICKOVER. Sure, I would like to do it because most of the people in my program are not in it for the naval program alone. They are interested in helping to bring atomic power to our country, because they believe atomic power is necessary for our future welfare. The Navy is helping to bring atomic power to our country because whatever we develop or learn in the naval program is of use in civilian reactors.

Representative PRICE. Do you think your group is in pretty good shape to take on an assignment like that?

Admiral RICKOVER. No, sir, we are not, but we could put ourselves in shape.

Representative VAN ZANDT. Projecting our thinking into the future, Admiral, when your reactors are standardized as far as ship propulsion is concerned, would it then be necessary for this unique organization you head to continue?

Admiral RICKOVER. For quite some time, yes, sir; because the whole reactor game hangs on a much more slender thread than most people are aware. There are a lot of things that can go wrong and it requires eternal vigilance. All we have to have is one good accident in the United States and it might set the whole game back for a generation. We do not want that to happen.

It requires a high order of ability and intelligence among the engineers and a high order of watchfulness to insure that nothing goes wrong.

Representative PRICE. Let us get back for a minute to the point I raised in regard to your recent speech about a realistic schedule in our power program. Do you think the target dates for around 1960 and 1962 are realistic?

Admiral RICKOVER. For what, sir?

Representative PRICE. For a large-scale operation of power reactors.

Admiral RICKOVER. To have a considerable number of power reactors in the United States?

Representative PRICE. Yes.

Admiral RICKOVER. No, sir.

Representative PRICE. What do you think would be a realistic target date?

Admiral RICKOVER. You are really getting me out of my field and I think you have people here far more expert on that than I am.

Representative PRICE. I am asking your personal opinion. You have had quite a bit of experience in the reactor building program and are not entirely unfamiliar with the power program.

Admiral RICKOVER. I think some of the people working on the program are not entirely realistic, they are not facing up to the facts of life. I do not think they have had enough practical experience. I do not think they worry enough.

You see, when we design reactors we have to think of putting them into a ship, and I have one design criteria. I always ask myself, would I want my son to go in the ship? If I can answer that question affirmatively, I say it is all right.

Representative PRICE. When you say they are not considering all problems, what would you mean by that?

Admiral RICKOVER. For instance, some of these reactor designs, as I see them, do not take into account some of the things that might go wrong, so they do not put into the design enough features, in my opinion, which would take care of them.

Representative PRICE. In other words, you do not feel the reactor is a proven thing until you know what it would do in event of an accident.

Admiral RICKOVER. That is why we build prototypes and try them out.

Representative PRICE. What do you think—I am just trying to think of an example of what you are talking about there. Would you say EBWR at Argonne, for instance, is a proven thing until you have something go wrong there?

Admiral RICKOVER. No. As far as I am concerned, all EBWR has so far proved is that if you get enough uranium together you have a critical mass. I think Argonne has done a fine job in designing and getting the EBWR in operation so fast. They are learning from it a great deal about control and operation of boiling water reactors. But until they fully try it out and, deliberately perhaps, have a ruptured fuel element go into the system and see what happens, I do not think they have finally proven the reactor system.

Representative PRICE. They have never done it?

Admiral RICKOVER. I think probably they will do it.

Representative PRICE. They have not done it yet?

Admiral RICKOVER. As far as I know, not.

Representative PRICE. Of course not with the new reactor, but with any similar type reactor have they done it?

Admiral RICKOVER. In my opinion, I see no point in conducting experiments of that kind unless you finally go to the bitter end and see what happens if you do have a burst fuel element. Only then will you know whether you will be able to operate the plant or not.

Representative DURHAM. Did they not test the one prior to Argonne?

Admiral RICKOVER. As far as I know they are planning to do this, but as far as I know—probably Commander Roddis knows better than I do.

Representative DURHAM. Was not the original borax tested, Doctor, originally?

Mr. RODDIS. There have been three experiments of boiling water reactors conducted: Borax 1, 2, and 3. One of them, borax 1, carried to destruction. Borax 2, during its operation and later borax 3, during its operation, have operated with aluminum fuel elements, which have received pittings, according to previous information. The type fuel element being used in the EBWR is a different type of fuel element which is much more susceptible to corrosion than the borax reactors.

Admiral RICKOVER. What I was saying was this: Until there is a burst fuel element and it gets into the system and you see whether you can operate the plant the experiment is not finished. That, to me, is the most important part of the experimental program on a reactor of that type. I am sure they will do it.

Representative HOLIFIELD. Let me try to follow you on this. Have you done that in the prototype of the PWR reactor?

Admiral RICKOVER. You see the PWR reactor has heat exchangers so that the water which goes through the reactor is kept separate from the steam system. But in a boiling water reactor the steam goes directly from the reactor to the turbine. We deliberately burst a fuel element in our submarine prototype plant at Arco and because of the heat exchangers in the system we were able to keep on operating.

Representative PRICE. That is what we are trying to find out, what you meant by that.

Admiral RICKOVER. These are the things you have to take into account in a naval plant. We have to postulate that these things will happen. So that even with a burst fuel element at Arco we were able to live with it. We have also had more than 100 gallons leakage of water per hour but we found we can live with it.

A lot of complication in our designs is due to the fact that we go to great expense and trouble to visualize these sorts of situations and in designing for them. This is what makes our plants complicated and expensive.

Representative PRICE. What sort of management controls do you use?

Admiral RICKOVER. I receive once a week a series of critical items lists from each of our projects. These are the items that are in design trouble, or need help, or where there is delay or there is something wrong. We give immediate attention to these problems.

Representative VAN ZANDT. You write no letters?

Admiral RICKOVER. We do write letters especially on technical matters, but a great deal of our work, particularly on expediting development and manufacture, is done by telephone. We do not wait, We get after this kind of stuff right away.

We also receive technical progress reports regularly from our contractors, and we comment on them. If you will look at these [indicating] you will see the type of reports we get on each project. We try to get the reports in 10 days after the end of the month or whatever the reporting period is, and we comment on it and tell the contractors and laboratories what we do not agree with. We do a lot of this sort of work all the way through each project.

Representative PRICE. How many people are there in the naval program?

Admiral RICKOVER. We have at Bettis about 1,300 scientists and engineers. These take care of the PWR project also. At the Knolls Atomic Power Laboratory, about 500; at combustion engineering, about 200; which makes a total of 2,000. At the laboratories and factories of our subcontractors there are probably an additional 1,000 scientists and engineers. Therefore, there are about 3,000 scientists and engineers working in the naval program plus the PWR.

On a project like the PWR it takes about 250 scientists and engineers, plus another 750 people, 5 years, roughly, to do the development work. That is pretty much the number of people and the time required for the research and development of any of these large projects.

These are fairly well trained people, not people you hire right now or pick off the street. A good many of them have come from the parent organization and have not been newly hired from outside.

Representative HOLIFIELD. In other words, some group that starts to build one of these large-size reactors without this organization——

Admiral RICKOVER. They will take several years to really learn.

Representative HOLIFIELD. Several years to perfect their organization to the point where you have it now, is that right?

Admiral RICKOVER. Yes, sir. It took us quite a few years to do it. My organization has been in the process of being formed for quite a few years. We are constantly training people. Every engineer or scientist I hire goes through a special selection system, the officers as well as the civilians, and we give each one of them special courses of training in the reactor field. Every engineer in my office has been given specialized education.

Representative PRICE. You give them training on the projects?

Admiral RICKOVER. Training at schools too.

Representative PRICE. What type of schools?

Admiral RICKOVER. The Oak Ridge School of Reactor Technology, Massachusetts Institute of Technology. We have schools at Bettis also. The school there lasts about 6 months. Every one of our engineers, officer or civilian, must attend one of these schools.

The young officers we get must agree to stay for 4 years before they can come into my organization, and many of them stay on as civilian engineers after their naval time is up. They stay with us because the work is interesting and they are able to do as much as they are capable of—to fulfill themselves. You have got to train people if you want to keep them.

Representative DURHAM. You have regular Navy personnel working for you, do you not?

Admiral RICKOVER. Yes, sir; I have Naval Reserve, regular Navy, and civilians.

Representative DURHAM. How about the pay schedule? Ordinary civilians, say as a civilian engineers, do they complain about the difference in pay?

Admiral RICKOVER. No, they do not. The officers are really discriminated against in this situation. We have fairly young civilians working right alongside officers with a good many years of service, and the civilians get more pay.

I hope that the Cordiner report, or some similar measure is placed into effect to take care of the outstanding and motivated officers in my organization. Although our people are not working entirely for money, nevertheless nearly everyone in my office could get a job at a much higher salary. We have a very low level of departures.

Representative DURHAM. Have many left you?

Admiral RICKOVER. No.

Representative DURHAM. Naval personnel, I am talking about.

Admiral RICKOVER. We have had more naval personnel leave than civilians, but our rate of attrition is not high.

Representative VAN ZANDT. Is it a change of assignment or do they resign?

Admiral RICKOVER. The officers who have left, generally get out of the Navy. They say they are not making enough money to support their families properly, and there are many opportunities in industry at much higher pay for people who are as qualified in this field as they are.

But most of them, not all, who have left have not been the most outstanding. Commander Roddis, however, was one of our outstanding people.

Mr. RODDIS. I did not leave for money though.

Admiral RICKOVER. No. But some who have left—we have not cared very much whether they left or not. In general, our good officers stay around, and I think you can get people to become dedicated to a job where they feel they are doing something constructive and worthwhile—they learn that money is not everything in life. And, of course, it is not. There is also personal fulfillment when you are doing the kind of job you like to do, and that is really the important thing.

Representative PRICE. What kind of action do you get out of AEC area offices on contract expediting?

Admiral RICKOVER. The AEC area offices do a good job for us. The whole AEC does a good job for us. It is a very fine organization. As I said before, without the AEC we would not now be on the way to a nuclear-powered Navy.

Representative PRICE. In many of your cases, do the area offices handle the contracts?

Admiral RICKOVER. Yes, sir. I do not know what the figure is. Up to a certain amount they can be handled locally in the local AEC offices, and anything over about $2 million has to come into Washington.

Mr. RODDIS. It varies with different offices. Mr. Flaherty at Argonne has authority to make contracts for $5 million.

Admiral RICKOVER. I would like again to state Admiral Holloway—our Chief of Naval Personnel—has done an outstanding job in taking care of our needs. He has helped us tremendously.

Representative VAN ZANDT. Some years ago we were concerned about the possible thievery, we will say, of trained personnel from your organization when authority was granted to establish, we will say, the shipbuilding capabilities in this atomic field. Has that actually taken place?

Admiral RICKOVER. You mean, has there been any trouble with pirating of people trained in the nuclear field in the shipbuilding organizations?

Representative VAN ZANDT. That is right.

Admiral RICKOVER. There has not been much, sir. You see, whenever a new shipbuilder gets into this game we know it ahead of time and we train his people; we arrange for his people to go to school. We also send people from my organization to deliver lectures and to advise them on training. We have set up, not only for shipbuilders but for every industrial outfit that comes into our game, a system of training.

Let us take metals and controls, as an example. We sent people there to deliver lectures, and we arranged for their people to go to schools. This is part of the regular course of training.

It so happens that Captain Dunford is responsible for that phase of our work in my office. We make sure that all of the industrial companies that work for us are trained. We cannot just let them do this by themselves because they do not know how to go at it. And that applies to the shipbuilding companies, too.

Representative VAN ZANDT. Is Ingalls down in the gulf developing capabilities to build submarines—atomic submarines?

Admiral RICKOVER. Yes, sir.

Representative VAN ZANDT. How long is it going to take them to assemble trained personnel?

Admiral RICKOVER. They started 2 years ago. Two years ago, when it was decided they would enter the submarine field, we started training their people. Now they have a number of people who are qualified. But it will take much more training. It takes several years. The Newport News Shipbuilding & Dry Dock Co. and the Bethlehem Steel Co.'s shipbuilding division at Quincy, Mass., have been training their people for several years in the nuclear field. We have helped them. They are still giving special training to their people. It takes several years to train a shipbuilding organization to become reasonably competent to build nuclear-powered ships. They must change some of their previous methods. It takes higher skilled people than you usually have in the shipbuilding industry.

For a company to get ahead in this game at the present time they must assign their best people and the man in charge must be of top-management caliber. The companies have done this. They needed some urging, of course, but once their managements finally saw the nature of the problem, they did what was necessary. We have had trouble, but we have been able to obtain cooperation. Perhaps you were referring to the Committee on Education that you and Mr. Price were on last year.

I said at the time I thought there were too many people just crying and wringing their hands and waiting for somebody to come along

and help them. In many cases they themselves can do much. Anyone responsible for a reactor project must take on the problem of seeing that his contractors and subcontractors hire the right sort of people and then trains them. Unless he does this he is in for trouble.

Representative VAN ZANDT. Then all of this excitement kicked up here a year or so ago about raiding your organization actually did not materialize, did it?

Admiral RICKOVER. No. No one is raiding my organization. Any one in my organization who wants to leave, I would just as soon have him out of it.

Representative PRICE. Are there any questions?

Thank you very much, Admiral, for a very fine presentation.

Representative VAN ZANDT. It was well done.

Senator JACKSON. I think he is to be commended always. We get forthright answers and he has done a good job.

Representative PRICE. The committee has always had fine relations with Admiral Rickover.

Admiral RICKOVER. You know how I enjoy coming up here. I know that you are all my friends and I sincerely thank you. I know you will do anything you can to help me in this program.

Representative PRICE. Thank you very much for your testimony this afternoon. It was very interesting and helpful.

(Whereupon, at 4:05 p. m., the hearing was concluded.)

PROGRESS REPORT ON NAVAL REACTOR PROGRAM AND SHIPPINGPORT PROJECT

FRIDAY, APRIL 12, 1957

SUBCOMMITTEE ON RESEARCH AND DEVELOPMENT,
JOINT COMMITTEE ON ATOMIC ENERGY,
CONGRESS OF THE UNITED STATES,
Washington, D. C.

The Subcommittee on Research and Development met, pursuant to call, at 10:30 a. m., in the conference room No. 1, Westinghouse Bettis plant, Pittsburgh, Pa. Hon. Melvin Price (chairman of the subcommittee) presiding.

Present were: Representatives Carl T. Durham (chairman of the full committee), Chet Holifield, and Melvin Price.

Staff members present: James T. Ramey, executive director, and George E. Brown, Jr.

Representatives of the Atomic Energy Commission: Kenneth E. Fields, General Manager; W. Kenneth Davis, Director, Division of Reactor Development; Rear Adm. H. G. Rickover, Chief, Naval Reactors Branch, and Commander V. A. Lascara, Naval Reactors Branch, John M. Bodley, special assistant to Commissioners Libby and Vance; Bryan F. LaPlante, congressional liaison officer; L. D. Geiger, Manager, Pittsburgh Area Office and Comdr. R. V. Laney, assistant to Pittsburgh area Manager (reactors).

Representatives of Westinghouse Electric Corp.; Charles Weaver, vice president in charge of atomic energy; John Simpson, manager, Bettis plant; Dr. Sidney Krasik, manager, central physics department; Dr. Benjamin Lustman, manager of metallurgy department; Mr. Alexander Squire, manager, submarine fleet reactor; and Dr. Walter Esselman, manager, advanced development group.

Representative PRICE. The meeting will come to order.

The Research and Development Subcommittee of the Joint Committee on Atomic Energy is here at the Bettis plant today in connection with its continuing studies in the atomic energy program. The joint committee is empowered to hold hearings at such times and places as it chooses and this meeting today is in keeping with the committee's tradition of visiting key installations as frequently as possible. First of all, I would like to commend the Government and private employees of Bettis. Your achievements in developing the *Nautilus* is of worldwide fame and I am sure my colleagues on the committee share my confidence that your work ahead will bring forth equally successful projects. I understand that Admiral Rickover will introduce this morning's speakers.

Will you proceed, Admiral Rickover?

STATEMENT OF ADM. H. G. RICKOVER, CHIEF, NAVAL REACTORS BRANCH, ATOMIC ENERGY COMMISSION

Admiral RICKOVER. First I would like to express for the Commission and for Bettis our appreciation of the opportunity to have you people here. We appreciate it a great deal and I am sure that by getting a firsthand look at some of the things that are going on here you will have a better understanding of what it takes to build a reactor plant.

Representative PRICE. Before you proceed, I would like to call to the attention of the gentlemen who are present here that while this is a subcommittee group here this morning, we are honored with the presence of the chairman of the full committee, Mr. Durham.

Admiral RICKOVER. It is a coincidence, but yesterday the *Nautilus* went to sea with her new core. Her core was successfully installed and she is at sea again. We have both the *Nautilus* and the *Sea Wolf* at sea. The *Sea Wolf* is at sea for about a month. She left early this week. However, the *Nautilus* refueling job is completed and the new core will give her significantly greater cruising radius. As you know, this new core was also designed by the Bettis Laboratory.

Since this meeting is intended to show what the Westinghouse Co. has done with the Bettis Laboratory, I will stop and introduce Mr. Simpson who is the general manager here. I think I should also introduce Mr. Weaver who heads up the nuclear work for Westinghouse and who was the first manager here. Mr. Simpson will take over.

Representative PRICE. Mr. Simpson, before you start I would like the record to show also that the General Manager of the Atomic Energy Commission, Mr. Fields, and the Director of the Reactor Division, Dr. Davis, are also present at this meeting.

STATEMENT OF JOHN SIMPSON, MANAGER, BETTIS PLANT, WESTINGHOUSE ELECTRIC CORP.

Mr. SIMPSON. Mr. Chairman and gentlemen, my purpose today is to try to tell you a little of what we at Bettis do; how we go about the work of the research and development and design of reactor powerplants.

As you know, Bettis was started about 8 years ago in order to do the work necessary to build the prototype of the *Nautilus*. We are very proud of the fact that the Bettis plant was started by Westinghouse taking largely people from other Westinghouse locations in an attempt to start the laboratory without taking people from other existing atomic-energy installations; that is to create a new factor in the atomic-energy field and not merely transfer existing technical manpower. We were quite successful in that and even today the average service with Westinghouse of the 400 supervisors at Bettis is more than 8 years. In the early days approximately 80 percent of all the supervisors, both technical and administrative, came from other Westinghouse locations.

At Bettis we do the engineering job on reactor powerplants, but in addition to that we do the necessary research and development that is essential for the engineering of those reactors. The reactors that we

have been working on in conjunction with the Naval Reactors Branch and the Pittsburgh area office of the Atomic Energy Commission for the latest submarines and the surface vessels have all been pressurized water reactors, but they are far different from the reactor that powered the *Nautilus*. There has been and there still is much research and development needed within the scope of the pressurized water reactor. It is a big development job.

As an illustration of the development required in the building of a reactor I would like to use the reactor core itself. In the reactor core we bring to focus physics, metallurgy, and straight engineering in the mechanical design sense of mechanical and thermal design. There is a big interrelation between the physics and metallurgy in the reactor design which requires an unusually close cooperation—a step-by-step development process—either one of which being adverse sufficiently can eliminate the material from consideration. So it is a typical problem to keep the two in balance.

Along this line it is relatively simple to determine, for example, that a new material appears to have great promise in a reactor; uranium-niobium, for example, or uranium oxide. In a few days a few metallurgists and physicists together can come up with the feasibility of such a material, but then there is an order of magnitude of tens to hundreds of times more work before you get to the point where you can actually use that material in a reactor. Uranium oxide, because it is an oxide, does not—it cannot further oxidize and it would be noncorrosive and inert in water. You can know in general what its physics properties are, but in order to really use it, many months and even years of development are required in the physics to understand how it is used.

In physics our reactor program has been limited in the past and still is to some extent by the lack of knowledge of the basic physics concepts. For example, until recently, there was difficulty in knowing exactly what the yield from fission was in the way of xenon. While this is not very important if you are just designing reactors in general, when you are getting down to the brass tacks of a particular design that must work, that and other similar constants, to a high degree of accuracy, become quite important.

The next area in which we must do a lot of work is taking the various assemblies of uranium of various enrichments and various geometrical configurations in determining the basic reactor. Having gotten through that, which usually requires a critical experiment which is an expensive and time-consuming thing, and which takes a great deal of technical manpower, we design the reactor.

Then in the design we must know other things than the basic parameters of the reactor. We must know exactly what is the distribution of neutrons in the reactor because that determines how the heat is produced and you must cool it properly in relation to the way the heat is produced, or you produce a hotspot and this may cause the reactor to melt down. You must know to a relatively high degree of precision the distribution of the flux.

Today's technology does not permit calculation on analytical techniques. This must be determined by experiment. We must determine how many control rods to use. We must determine whether or not the reactor will, in fact, be critical even with some of the rods not

all the way in, to be sure that it is safe. These things can be determined from a critical experiment run in a laboratory. A certain amount of the dynamic action of a reactor can also be determined by laboratory experiments.

Then you get into the more difficult things in which we must have a good basic theory in order to extrapolate, and the most important of these is the lifetime behavior. As you know, a power reactor must last for many thousands of hours. Uranium is burned and plutonium is formed. These are the fissionable materials and, therefore, the net reaction varies with time. You must determine and predict in advance what the control situation will be and what the flux distribution will be even after many thousands of hours of operation. This we cannot do by experiment in a laboratory because, in fact, it depends upon running the experiment at power for long periods of time. This information is verified from data we get from such reactors as those at Idaho, from the naval reactor facility, from the *Nautilus*, and which we expect to get from the Shippingport reactor. But in order to calculate these to the degree of accuracy required, we must have a firm foundation in the experiments we can make, which are with the cold reactor in this nude condition.

Moving on to the materials aspect, with each new reactor there is usually a new requirement for a material that will best fill the needs of that reactor type. This means a large metallurgical program in the development of the materials. By the development of materials, I mean developing a material that is adequately corrosion resistant, that is irradiation resistant and, in determining its physical properties, whether it can be machined, its strength, its ductility, and its other physical properties that you must know how to fabricate. Now this is not an easy job. In times past, as you know, a new alloy has taken 5, 10, or more years from its inception to where it is really reduced to practice.

It is a relatively easy thing—and I repeat this—it is a relatively easy thing to believe that an alloy with a given number of constituents would really be a useful and good alloy in a reactor, but to actually prove this—to develop all the knowledge of what impurities cause it to corrode, just how it works best, its corrosion resistance in pile and its irradiation resistance—it is a big and laborious job.

Then, having gotten the material, one must learn how to fabricate it. Fabrication is a difficult thing because there you must learn to weld. You must learn what the shrinkage and distortion is. You must learn all the many problems of rolling and fabricating so that you can, in fact, make a reliable core to be used in the reactor. We must go into the material itself from the pilot stage, where you make a small ingot, on to the production stage where many suppliers make large ingots.

Recently in something that we know almost the most about, which is the zirconium alloy—zircaloy 2, we found that when we went to large-scale production that the difference brought about by large-scale supplier production as against laboratory production produced certain inharmonies; lack of uniformity of material and this, in turn, affected its corrosion resistance. Some variations in the process had been instituted and these interfered with its purpose as a corrosion resistant alloy.

Another area in which a lot of work is done and a lot of work is needed is in the area of heat transfer and fluid flow. I think many

people had believed that water being such a common substance surely almost everything was known about its heat transfer properties, about its flow resistance in pipes even with a little steam present, but the facts are that very little is known about this. More is being learned each year, but still the data is not as good as is needed for the design of a reactor.

In addition to the development work on the core, as represented by the heat transfer, the mechanical, the metallurgical, and the physics, the rest of the plant is a big development job because essentially none of the components that we can put into a reactor plant today can be taken off the shelf. They, themselves, have many developmental aspects.

Aside from the development aspects of the components, we have the big design engineering job of coordinating all of this equipment into one powerplant to produce power—electricity or a shaft turning. Why is that so complex a job? The jobs are not larger in physical size than many other jobs that are done in the United States. I think the biggest reasons are the newness and the uniqueness of the components and the close interrelation of all these components. A reactor powerplant is closely coupled in several unique ways. The core emanates radiation and, therefore, you must shield the entire plant. This is, of course, a larger job on a submarine or a naval vessel than it is in a land powerplant because you must tie everything together in interrelating the shielding, the hull, the weight and space, and you must know what effect the components have in filtering out some of the radiation. The job must be done as a whole. You can't do parts of it and then just casually fit them together. The job must be engineered from the ground up with the entire scope of the project in mind.

Then you have the radiation heating of all the material that you use to support the core. These materials absorb gamma rays and this introduces design complications. Then in a pressurized water plant large sections of pipe are under high pressure. The whole plant acts as one unit from a thermal expansion viewpoint and you must arrange to take up this expansion, otherwise you will cause a break in the pipe and release of the fluid. Then you have the problem of radioactive contamination. The corrosion products that go through the core have radioactivity introduced into them and they go through the pumps, the pipes and the valves and you must be prepared to handle the maintenance of the plant despite the fact that you have some radioactive contamination.

Then there is that close coupling of a reaction upon the power of the reactor due to the pressure and temperature of the water. Opening a valve some place—starting a pump—can change the temperature of the water going through the core and therefore change the power. So your control must be such that those things don't happen except in the way that you predict and your overall control must be better due to the close coupling and reflection back on the core itself of all of the parts of the plant.

A reactor plant must have all of its components working in order to run. You cannot run a reactor powerplant with parts of it not working. The control drive mechanism, the instrumentation, the pumps, the valves, the heat exchangers—they must all work. This

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means that in order to get the maximum amount of information out of a plant like Shippingport you must keep it running in order to find out what you need from the core.

In building a reactor for a naval vessel we are not building experimental reactors. We are building reactors that must power the fighting vessels of the Navy. These reactor powerplants then are built so that they are rugged, reliable, and easily maintainable. They must work in fact. A submarine powerplant cannot shut down when it is on a battle mission or even when it is cruising simply because something doesn't work. You must have it so that these things do not happen.

In order to accomplish this, it has been necessary to what might appear at a casual glance to overdesign, to overtest—in order to give what we think of as maximum assurance that all of the parts individually will work and that they will work as a unit when coupled together. We have had to do a great deal of testing of components, and then test groups of components, and then overall testing of the plant. This we feel is a very important part, made doubly so, as I said, because these components that go to make up the plant are not ones that you can buy—that have been used for years and that have a long history of satisfactory operation.

For example, canned motor pumps were developed in the sizes used for the *Nautilus* and, therefore, in order to make sure that these pumps were satisfactory, a great deal of development and testing was required. The same is true of our heat exchangers, valves, particularly pressure relief valves and the pressure vessel itself which seems like a relatively simple item—just a heavy-walled vessel with a head on it—but the head required a tremendous amount of development so that it could, in fact, be built with a large number of holes in it for the control drive mechanisms and other purposes.

I have gone through the broad picture of the development problems in physics, in metallurgy, and in powerplants. I would like to call on some of my colleagues to take these particular areas and expand upon them so that you will have a better detailed picture of just what it takes in the way of, for example, the physics effort to build one of these reactors.

Representative PRICE. I think Mr. Durham has a question.

Chairman DURHAM. Mr. Simpson, this is an excellent statement. Personally, I have enjoyed listening to it. We all realize the difficulties in a brandnew development. However, is there any way you can arrive at a percentage as to the amount of conventional development that you can apply to the development of a reactor? Would you say it is 90 percent that is new and 10 percent conventional of that which has been worked out prior to this development process?

Mr. SIMPSON. With respect to the *Nautilus*, I hesitate to say that a hundred percent was developmental so I will back off just a little bit. However, there was scarcely a single item of any type that could be taken off the shelf; that had been used and could be used, therefore, in the *Nautilus*. Almost every item was developmental well in excess of 90 percent.

Admiral RICKOVER. May I answer that question, sir, because there is something that goes beyond what they did here. I can state unhesitatingly that I don't think there was a single item in the entire propulsion plant that didn't have to be developed.

Chairman DURHAM. I asked the question because I don't think people at large realize that fully. They think a lot of this work has been accomplished before.

Mr. SIMPSON. For example, even the steam plant for the *Nautilus* was new. No steam plant had ever been put on a submarine before. No steam plant, therefore, had ever run at these submerged depths. So a simple item like a condenser, which on a surface vessel operates at just a few pounds pressure, must operate on a submarine at many hundreds of pounds pressure. Take a simple thing like the safety valve. The steam system, instead of just opening and blowing the steam into the air, must open and blow it into the ocean. You can go on from there. All of the valves that we used in the *Nautilus* in the reactor plant had never been built before. The same is true of the pressure vessel; in fact, all of the components. High-pressure water had not been used in this way to any degree in the United States. High temperature water is, of course, used in boiler plants, but it is a different temperature and different degrees of corrosion are required. Therefore, the components are different in order to be suitable for a reactor.

Chairman DURHAM. Would your statement apply generally to all reactors?

Mr. SIMPSON. Yes. Obviously, they are less developmental as each succeeding reactor is built so it is difficult to assign a percentage to a new submarine today. The pumps that went into the Shippingport plant are much larger than the *Nautilus* pumps and, therefore, the mere fact of size involved a considerable development. A reactor vessel from a small vessel moved into a large one would require a different type of plate. In fact, the plate for the PWR vessel was the thickest clad plate that had ever been rolled in the United States. Therefore, many of the plates had to be thrown away because they weren't suitable.

Chairman DURHAM. Since the *Nautilus* was the first one, could you estimate on a percentage basis what knowledge which you gained in developing the *Nautilus* could be used in developing the *Sea Wolf*?

Mr. SIMPSON. I am not familiar with the *Sea Wolf*.

Admiral RICKOVER. I think I could answer, Mr. Durham. Because in a naval vessel you have to get the smallest weight and space, I would say that we did learn something about the principles, but as far as the actual development of items it was entirely different. That is not only true of the *Sea Wolf*, which is a liquid metal plant, but it is almost equally true of all the other submarine and surface-ship developments. You will find with these different projects that there is hardly a single item in one that you can use exactly in another one because we don't have acres of space to put it in. Actually, when we undertake the development of a new propulsion plant for a naval vessel—and it is practically true of all nuclear-powered naval vessels—we have got to develop all of the items individually. It is not only true of the reactor plant. It is true of the entire plant including the steam plant, the turbines and gears and so on. For example, we had to develop special turbines on the *Nautilus*. We had to develop special gears. You might ask, "Why isn't that available?" It just isn't.

Chairman DURHAM. What I am getting at, Admiral, is this. The members of this committee are often faced with a question from an

individual who says, "They tell us the *Nautilus* is obsolete so what do we have after this investment of taxpayers' money?" That is the hue and cry. Can we make some estimate as to the knowledge we have gained through the development of the first reactor? It would help us if we could have some estimate or if some estimate could be worked out. I don't know whether it can be or not. I am just raising the question here as a matter of discussion because that money is invested in knowledge.

Admiral RICKOVER. Perhaps I could answer it this way. Actually we could talk two different ways: One is about the development principles and scientific and engineering knowledge; the other is for specific items.

From the first standpoint, unquestionably most of the technology which exists in the country today on pressurized water reactor plants has come out of the efforts of this laboratory and to some extent out of the Knolls Atomic Power Laboratory.

Representative HOLIFIELD. Where?

Admiral RICKOVER. The Knolls Atomic Power Laboratory, because they have been working for several years on the radar picket submarine which has a pressurized water reactor. When it comes to individual items, however, the people who are working on commercial pressurized water plants by and large copy what has been developed because they have room to do so. The General Electric Co., in their San Jose plant, have copied the design of a PWR pump, for example. They can afford to because they have the space in which to copy exact items, the principles of which have already been developed. However, when it comes to a shipboard plant we can't do that because we have the intolerable situation of weight and space so we have to take these items and redesign them. That makes a terrific load in getting one of these projects done. One of the members of your committee asked me the question from the money standpoint several years ago. I stated then and I will state unhesitatingly again that I think there has been more advantage to American industry from the work that we have done than the actual cost of these projects. There are the developments in metallurgy, not only in zirconium and hafnium but in stainless steel. We have taken the lead in the last 2 or 3 years in developing stainless-steel technology. This will be covered a little later.

I think that is what Mr. Durham is really getting at. I think there has been a net gain to the American economy from this effort which is above and beyond any material or national gain in having these plants.

Chairman DURHAM. I think we on the committee realize that fully. We, of course, have to have something that is a selling point when we get into these big sums of money.

Admiral RICKOVER. If you would permit me, I would like to add subsequently to the record a list of these things we have contributed to the American economy.

Representative PRICE. I think that is an excellent idea. Will some of the speakers who follow cover what you have done here at Bettis in the development of new alloys and new materials?

Mr. SIMPSON. Yes sir.

Admiral RICKOVER. If that is satisfactory, sir, I would like to submit that list.

Representative PRICE. We would like to have it very much.
(The information referred to follows:)

DEVELOPMENTS IN THE NAVAL REACTORS AND PWR PROGRAMS WHICH ARE APPLICABLE TO OTHER CIVILIAN PROGRAMS

This report summarizes some of the developments in the naval reactors and PWR programs which may prove useful to others. However, the major contributions are broader than can be indicated by a list of items. For example, working out with the AEC's Safeguards Committee how to analyze the safeguards problems associated with the first reactor in the world to produce large quantities of useful power (the Nautilus prototype at Arco, Idaho); the first power reactor to operate near heavily populated areas (the *Sea Wolf* prototype at West Milton, N. Y.; the first reactor to power a ship (the *Nautilus*)—many of the precedents worked out for these problems can now be followed by others.

A similar situation exists for the development of techniques for power reactor design . A methodolgy has been worked out involving large-scale digital computers, mockup and flexible nuclear assemblies, large apparatus for testing fuel elements under simulated plant conditions of radiation, heat generation, temperature, and flow; specifications and equipment for testing all of the specially developed so-called conventional components, such as pumps, valves, and instrumentation.

The information below is grouped into three categories: Techniques and basic information, materials, and equipment development.

CONTENTS

I. NEW TECHNIQUES AND BASIC INFORMATION

1. Naval reactor technology handbook series

Interim and internal technical documents in the naval reactors program may not always be helpful to a person in a civilian reactor program because—

(*a*) Such documents often contain information which is fragmentary, unevaluated, or even data which later proves to be incorrect.

(*b*) Such documents usually contain military design information; the person in question may not be entitled to receive military information.

To reconcile these facts against the real need of civilian reactor groups to obtain information developed in the naval reactors program, a series of reactor technology handbooks is being prepared. Virtually all of the information developed in the naval reactors program which is applicable to civilian reactors is being published in this manner. By editing and by recalculation of sample problems, military information can be avoided and nearly all of the technology can be released. An important feature of these books is that work in each field is reviewed by people actually working in that field, and evaluated information is published rather than possibly conflicting or confusing data. The meaning of the data is also discussed.

The following handbooks have been published to date:

Liquid Metals Handbook	1st edition, June 1950; 2d edition, June 1952; 3d edition, June 1955; 2d printing, November 1955.
Metallurgy of Zirconium	July 1955.
The Metal Beryllium	July 1955.
Bibliography of Reactor Computer Codes	Dec. 1955
Reactor Shielding Design Manual	Mar. 1956.
Corrosion and Wear Handbook for Water-Cooled Reactors	June 1957.

Currently in preparation:
Naval Reactor Physics Design Manual
Heat Transfer and Fluid Flow Manual
Reactor Core Design Manual
Reactor Plant Piping Handbook
Radiological Aspects of Naval Nuclear Propulsion Plants
Hafnium Handbook
Irradiation Testing and Hot Lab Techniques
Nuclear Fuel Elements

2. Basic heat transfer research

For both sodium and water it was found at the beginning of the naval reactors program that basic data on physical properties, heat transfer rates, and pressure drops were not known with sufficient accuracy over the necessary range of operating conditions. In particular, the problem of heat transfer under transient conditions required study. Therefore, for several years basic experimental and theoretical work on heat transfer has been underway in the naval reactors program. The sodium work has been reported in the Liquid Metals Handbooks, and several new analytical methods, for example for natural convection in irregular shapes and for thermal shock caused by rapid temperature changes in the liquid, have been developed and reported. For water, better measurements of the pertinent physical properties, heat transfer rates without boiling and with the various possible modes of boiling, pressure drops with and without boiling, and limiting conditions beyond which fuel element meltdown can occur, are being studied. This work is being done both with electrically heated test sections and with specially instrumented fuel assemblies in the Naval Reactor Facility in Idaho. The variables affecting heat transfer in the reactor are now being added to the reactor physics equations so that an entire reactor heat generation and heat transfer problem can be studied as a single entity on a large-scale digital computer. These techniques should be valuable to other reactor designers. A handbook on heat transfer and fluid flow for pressurized water reactors is now in preparation.

3. Reactor physics

The naval requirement for a compact reactor of high performance necessitates that heat generation rates be calculated at many locations within the reactor core, for various times throughout the reactor life, and with various assumed casualties such as stuck control elements. A procedure for analyzing reactors in this detail is being developed using digital computers for calculations, in combination with experiments with laboratory assemblies of uranium and other materials designed to simulate, from the nuclear standpoint, characteristics of the operating reactor. Many of these techniques are now being used by other reactor projects.

In the course of this development several new physics concepts and techniques have been worked out. Some of these are listed below:

(*a*) *Seed and blanket principles.*—This is a concept whereby a natural uranium reactor, which cannot operate by itself in ordinary water, is kept going by neutrons leaking from a U–235 reactor. The resultant combination permits power to be extracted from natural uranium and also decreases the amount of control required.

(*b*) *Power flattening methods.*—By use of different concentrations of fuel and of poisons in different parts of the reactor, the generation of heat throughout the core can be made more uniform, and thus the core volume and the uranium content can be utilized more effectively.

(*c*) *Nuclear constants.*—Many nuclear constants have been measured more accurately for the naval program and the data are now available for other reactor designers. An example is the poisoning effect of xenon, a particularly troublesome fission product.

(*d*) *Epithermal reactors.*—The physics of reactors operating with neutrons of intermediate energy was largely unexplored prior to the development of the *Sea Wolf* reactors. Many of the physics constants and analytical techniques developed for it are applicable to the civilian fast reactor program.

(*e*) *Slightly enriched reactors.*—In exploring methods to obtain maximum utilization of U–238 in a reactor, a program for exploring slightly enriched reactors was undertaken by Bettis plant with help from Brookhaven National Laboratory. This program developed information and methods of analysis which can be used by other civilian reactor programs.

(*f*) *Large-scale digital computers.*—Most of the computation methods developed for naval reactors and the computer codes for implementing them are sufficiently general to be used in civilian reactor programs. A book of these computer codes was published by the Naval Reactors Branch and is being kept current by a nuclear codes group set up for the purpose by members of the American Nuclear Society. Arrangements have been made with computer manufacturers to supply punched cards and instruction books for these codes, without charge, to anyone requesting them.

4. Reactor design

Instruments have been developed to measure temperature, waterflow, and neutron flux at many locations inside a reactor core. This will permit more accurate study of how a reactor operates. Cores for the pressurized water reactor and for naval prototypes will all have such instrumentation.

The following are some particular developments in the naval reactor design field which are of possible application to civilian reactors:

(*a*) Use of airflow techniques for establishing the flow distribution through the core and hydraulic forces on control rods.

(*b*) Establishment of pressure drop characteristics of waterflow when some parts of the reactor are locally boiling.

(*c*) Development of a method for identifying and locating failed fuel elements. This required development of sampling means, identification of the influence of piping characteristics on the system, and establishment and proof testing of means for measuring delayed neutrons.

(*d*) Computational techniques for defining the shutdown requirements arising from the simultaneous and sequential loss of pumps during an assumed casualty.

(*e*) Establishment of the feasibility of and defining the conditions under which labyrinth seals can be used for controlling water leakage in the internal parts of core structures. Previous applications of labyrinth seals were generally limited to steam and gases.

(*f*) Computational techniques for including nonnuclear variations in fuel element properties in defining operating limitations of a reactor. This includes such effects as fabrication tolerances, warpage characteristics, maldistribution of flow, and uranium density variations. Both local and average influences are accommodated.

(*g*) Contributions toward understanding the phenomena associated with exothermic zirconium-water reactions, and their implications to reactor safeguards.

(*h*) Efforts to identify, measure, and take into account local neutron flux peaking caused by water channels adjacent to fuel elements, which have proved to be a highly important design limitation.

(*i*) In the general fields of heat transfer and hydraulics, data have been published from many experiments that have been conducted on flow mixing, flow coastdown, decay heat, loss of coolant, boiling, and parallel channel flow.

(*j*) Development of a mechanism which provides partial shutdown of the reactor to permit rapid recovery from inadvertent shutdown signals or easily corrected casualties.

5. Pressurized water reactor technology

The problem here was to provide a hermatically sealed system into which highly purified water could be introduced, in which continuous purification of water could take place, and which could be kept at a pressure higher than the boiling point of the hottest spot in the reactor. Build-up of radioactivity in the system outside the reactor and deposition of corrosion products on heat transfer surfaces had to be held to a minimum. The motion necessary to circulate the water, to move valves and reactor control elements, to sense and to transmit temperature, pressure, flow and position information—all of this had to be transmitted from within the system to control and power equipment outside, without violating the all-welded high pressure boundary. The particular

components to accomplish these various jobs are discussed in section III. Below are listed some of the developments of this technology which may have application to civilian reactors. This information is reported in the Wear and Corrosion Handbook published by the Naval Reactors Branch, June 1957.

(*a*) The adaptation of a bypass purification system using ion exchange resin for control of water purity in a high temperature water reactor system.

(*b*) The application of controlled alkalinity in a reactor coolant system to minimize general corrosive attack and deposition on heat transfer surfaces in a radiation field.

(*c*) Use of lithium-form and ammonium-form ion exchange resins in the purification system to provide alkalinity control compatible with the characteristics of pressurized water reactors.

(*d*) Use of dissolved hydrogen in the water to suppress water dissociation in reatcors, a phenomenon which can lead to unsatisfactory water conditions.

(*e*) Analytical methods and equipment for pressurized water reactors, including a high pressure conductivity cell and techniques for determining dissolved hydrogen and dissolved oxygen in high temperature water.

(*f*) High temperature purification media are presently under development with the objective of eliminating the need in the purification system for heat exchangers and overtemperature protection.

6. *Sodium reactor technology*

A new technology was built up to permit handling of radioactive sodium on a large scale at high temperatures. Methods had to be developed for maintaining high purity, insuring leak-tightness, and providing control of flow and temperature. Basic data were obtained in some cases up to 1,500° F. on corrosion, transport of radioactivity, self-welding of contacting surfaces, bearings, seals, sampling, analysis and purification of sodium, and maintenance techniques. Some of the major components developed are discussed in section III. Heat transfer in sodium was found to require new theoretical treatment, since the standard equations used for lower conductivity fluids such as water and air were not applicable. This information is reported in the 3 editions of the Liquid Metals Handbook published in 1950, 1952, and 1955. Analytical techniques for determining minute quantities of impurities in sodium were developed. Methods for cold-trapping oxides from the sodium to decrease corrosion were also developed.

7. *Testing fuel elements*

The most developmental, most expensive, and most critical part of a reactor plant is generally the reactor core itself. The problem of proof-testing the nuclear fuel elements is extremely difficult, since the environment of heat generation and radiation cannot easily be simulated. A means for installing pressurized water test loops into reactors was developed whereby most of the appropriate conditions could be simulated. The equipment required to achieve and control this environment has now become reasonably standardized and the technique is being followed by others. In particular the concept of initiating deliberate failures of various kinds in the fuel element to study the growth of a flaw from a minor to a serious one has proved useful.

8. *Reactor control and safety*

The problems of controlling a nuclear propulsion plant were largely unexplored when the first naval reactor designs were begun. Fast-acting reliable automatic control equipment was developed.

The safety problems associated with operation of power reactors had to be defined and explored. Information has been obtained on such general problems as chemical reaction rates of sodium and of zirconium with water and with air, flammability and explosiveness of mixtures of hydrogen with steam and air, and rate of heatup of a reactor when the coolant is suddenly lost. Much of the information developed during these studies has been brought together and reported in a series of reports on the safeguards analysis of the PWR plant. These reports are unclassified and will be available to the general public after they have been reviewed by the AEC Advisory Committee on Reactor Safeguards.

II. MATERIALS

1. *Zirconium and its alloys*

Zirconium was selected as a reactor structural material for pressurized water reactors because of its favorable nuclear properties and corrosion resistance.

In 1948 only a few thousand pounds of crude zirconium were produced in the United States. In that year only 86 pounds of refined zirconium were produced, which sold for about $200 per pound. A production plant for refined zirconium was built and operated at the AEC's Bettis plant. Steps have since been taken to get several commercial producers to begin production. Today the production of sponge zirconium is about 100,000 pounds per month, and the cost has dropped to about $11 per pound.

To further improve the value of zirconium, an alloy called zircaloy-2 has been developed. Zircaloy-2 is cheaper than pure zirconium and has improved corrosion and mechanical properties. An additional benefit of this alloy development was the arc melting technique developed by the Bureau of Mines for making zirconium alloy ingots. This arc melting technique has since been applied to titanium melting by the titanium industry. In the nonnuclear field zirconium may prove useful to the chemical industry because of its excellent corrosion properties.

To disseminate information obtained from the zirconium program, a handbook of over 700 pages was published in 1955.

2. Hafnium

Hafnium is a byproduct of zirconium refinement and had few uses when it first was separated in quantity. It was known to have excellent mechanical properties and corrosion resistance, similar to zirconium, but unlike zirconium to be a strong absorber of neutrons. Hafnium has since proved to be an excellent control-rod material for pressurized water reactors. Hafnium may also find uses in nonnuclear fields because of the very high melting point of its compounds and because of its heavy weight (greater than lead). A handbook on hafnium, its properties, fabrication methods, and other technical data, is now in preparation.

3. Other neutron absorbers for reactor control

To stretch the supply of hafnium a program was begun to develop alternate control rod materials. The principal materials studied were boron in stainless steel, boron in titanium, and silver alloys. Although pure silver dissolves in hot water, the addition of cadmium and indium to silver prevents this corrosion. These elements are also beneficial from a nuclear standpoint. The resulting alloy is cheaper than hafnium and may find use in commercial nuclear reactors.

4. Nuclear fuel materials

The naval reactors and PWR programs have worked on the development of uranium zirconium, and uranium oxide fuel materials:

(*a*) *Uranium-zirconium.*—Uranium zirconium has been developed as a fuel alloy material. This alloy has been shown to be capable of producing heat at a high rate for a long time. A large fraction of the fuel can be used up while still maintaining the strict mechanical specifications for reactor fuel elements.

(*b*) *Uranium-oxide.*—Uranium oxide has been developed as a fuel material and it will be used in rod form in the PWR core. This material has several desirable properties. It is inert and does not dissolve in water. It is dimensionally stable under irradiation. Compared with uranium alloys, it is less expensive to make and to fabricate into fuel elements.

Methods of using uranium oxide in plate form are also being studied.

5. Unlubricated moving parts for pressurized water reactors

To develop satisfactory mechanisms which required moving parts to operate in highly purified reactor water systems necessitated development of a new techology. Bearings, valves, control mechanisms, and various locking and latching devices in and near the reactor were involved. Satisfactory materials and combinations of materials for these various applications have been developed, along with equations defining the tolerable clearances, bearing loads, temperatures, torque, and other limiting criteria which determine operating characteristics.

The data on this program are reported in the Wear and Corrosion Handbook published by the Naval Reactors Branch, AEC, June 1957.

6. Stainless steel for reactor systems

A program was carried out to develop conditions under which commercially available stainless steels could be utilized in pressurized water reactor systems. Corrosion data, static and dynamic, effects of radiation and of chemical composition of the water, and other pertinent data were developed. The use of

stainless steel for this purpose has now been demonstrated and the information is being used by other pressurized water reactor plant designers.

7. Materials for handling high-temperature liquid sodium

A program for developing materials for handling liquid sodium and sodium-potassium alloy at temperatures up to 1500° F. was carried through in both laboratory tests and large-scale plant equipment. Springs, bellows, bearings, and valve parts were studied from a materials standpoint and satisfactory materials for these purposes were developed. This information has been made available in the unclassified Liquid Metals Handbooks series.

III. EQUIPMENT DEVELOPMENT

1. New vendors for nuclear equipment

Approximately a year and a half ago the AEC's Bettis plant was the only organization equipped to build reactor cores for pressurized water naval reactor plants. Steps were taken to acquaint industry with the requirements for fabricating these reactor cores in order to build up other sources of supply. The criteria used in considering prospective core manufacturers were:

(*a*) The company must have the necessary privately owned facilities or be willing to provide these facilities at no expense to the Government.

(*b*) The company must be willing to train personnel.

(*c*) The company must be willing to compete on a fixed-price bidding basis with other companies.

Today, there are five companies which are acquiring the necessary facilities, trained personnel, and capability to manufacture complete naval reactor cores. Fixed-price competitive contracts have already been let for five submarine cores, and competitive fixed-price bids for several more submarine cores of another design are now being reviewed by the Government prior to contract award.

For so-called conventional equipment, a somewhat similar path was followed. Starting with the procurement of equipment for the *Nautilus* prototype, the naval reactors program has worked toward the development of more than one source of supply for components of nuclear propulsion plants, anticipating the time when every item could be purchased on a competitive fixed-price basis.

Reactor plants for 5 fleet-type nuclear submarines (2 in the fiscal year 1955 and 3 in the fiscal year 1956 shipbuilding programs) were the first ones to benefit from this effort.

The estimated cost of these five reactor plants is $24,779,524, exclusive of (*a*) minor components and installation work which are covered by appropriate construction contracts with the shipbuilders, and (*b*) steam machinery and reactor cores which are being purchased competitively under separate contracts.

With the exception of the first core for these submarines which is being fabricated at the AEC's Bettis plant, and some plant control and instrumentation equipments for which development work was still under way, the remaining 4 cores and all other reactor plant components for these 5 submarines are being furnished on a fixed-price competitive-bid basis.

Approximately 300 subcontractors and first-tier suppliers are engaged in furnishing these 5 reactor plants.

It is expected that other reactor programs will also benefit from these actions.

2. Nuclear reactor cores

The following list includes some of the items developed in the naval reactors program in the field of reactor-core fabrication which appear to have application to civilian reactors:

(*a*) Nondestructive testing techniques for the inspection of clad fuel plates, fuel rods, and fuel assemblies. These include ultrasonic, eddy current, X-ray, and mechanical methods.

(*b*) Production methods for forging, rolling, extrusion, and other metalworking processes necessary for the fabrication of complex zircaloy shapes.

(*c*) Fabricating techniques for seamless zircaloy tubing and manufacture of such tubing for high-pressure in-pile test loops, as well as for PWR fuel rods.

(*d*) Welding techniques for joining zircaloy to zircaloy. This includes the design and manufacture of remotely operated welding chambers.

(*e*) Techniques for arc melting homogeneous zirconium-uranium fuel alloy, including the design of new types of furnaces.

(*f*) Manufacturing techniques for zircaloy-clad fuel plates. This includes cladding by roll-bonding and edge flanging techniques.

(*g*) Methods for joining fuel plates into fuel assemblies maintaining accurate spacing of the plates. These include both fusion welding and resistance welding methods.

(*h*) Standardized high-pressure hot-water and steam corrosion-testing techniques for reactor fuels and cladding.

3. Components for pressurized water reactors

Even so-called conventional components such as pumps, valves, and heat exchangers, when used in a reactor system, were found to require complete redesign and development. For example, in order to operate pumps in a pressurized water system without seals, a canned rotor pump was developed in which the rotational power is transmitted magnetically through a welded can. A similar principle is used to operate solenoid valves in the system. The major valves are powered hydraulically by the reactor coolant itself, which in turn is actuated by a solenoid pilot valve. The control mechanisms which move the reactor control elements also operate without seals. In addition, all of the moving parts and bearings surfaces in these reactors are designed to permit replacement without major maintenance. These components have been widely reported in the unclassified literature and are already used in many reactor systems, test facilities, and other systems requiring hermetic sealing of a dynamic system. The pumps are now being used as recirculation pumps in high-pressure commercial boilers, and may also find use in the chemical industry.

The naval reactors laboratories are working with manufacturers of boilers and heat exchangers to stimulate development of designs and materials for heat exchangers which can provide the high integrity essential to a reactor plant. This program is expected to have some influence on the design of conventional heat exchangers. Similar programs with industry are being carried out for pressure vessels, heavy forgings, castings, and plates.

4. Components for sodium reactors

When the naval sodium reactor work began, a similar equipment problem existed for sodium as for pressurized water, but with the factor of high pressure being replaced by one of higher temperature and a less understood chemical environment. Seals using frozen sodium as a barrier were developed. Electromagnetic pumps with no moving parts were developed for high pumping capacity against high head. Valves of several sizes and types have been developed which operate satisfactorily. Equipment of this type and the design principles behind it have been discussed in many unclassified papers and are being used by civilian reactor projects.

5. Nuclear instrumentation

Nuclear instrumentation, when the naval reactor designs were begun, was delicate, complex, and handmade. Rugged, stable, wide range, interchangeable, and maintainable equipment had to be developed. Specific developments completed or underway are:

(*a*) Development of reactor control circuitry to operate in the power range without any electron tubes.

(*b*) Use of standardized parts to decrease size, cost, and simplify maintenance.

(*c*) Development of temperature detectors for use in high-radiation and high-temperature regions.

Representative HOLIFIELD. Mr. Chairman, I would like to ask Mr. Simpson or Admiral Rickover a question in an effort to narrow the general knowledge down to a specific problem. How much of these techniques which you have developed in your pressurized water project, for instance, can be carried over into a sodium reactor or do you face a completely different set of problems—a different set of designs in your sodium or metal-cooled reactor?

Admiral RICKOVER. Yes, sir; you do. You use different material by and large with every different type of coolant. For instance, in a sodium plant you have beryllium as a moderator. We use no beryllium in water plants. You use stainless steel in a sodium plant, but the qualities in stainless steel that you want in a sodium plant are different than the qualities you want in a pressurized water plant. Sodium, for example, in noncorrosive as far as stainless steel is con-

cerned. Water is corrosive, so that in this respect sodium has an advantage. Sodium, if it touches air, burns. Water, if it touches air, does not burn. The physics is entirely different so you might say that you have two completely unrelated problems.

Representative HOLIFIELD. Do you have to develop a completely different technology when you change from one of these reactors to another.?

Admiral RICKOVER. When you change your coolant you have an entirely different problem. Of course, there are some things that you know in general about metallurgy and some things about physics, but by and large it is as different as day and night between two different coolants.

Representative HOLIFIELD. In starting a new type of reactor then, you almost have to start from scratch and go through the same painful process that you went through on the *Nautilus* or is there a certain ability on the part of people who have done a reactor to know the types of problems involved so that they are starting with a more professional approach than just coming from building airplanes, for example? The coolants and materials may be different, but you do know the types of problems so you are more efficient in handling them· You know the fields of inquiry but you still have to do the development.

Mr. SIMPSON. There are certain areas in which you get some advantage. For example, thermal reactors have certain kindred—they are cousins. A sodium reactor has a different energy spectrum, therefore the physics is radically different. However, a gas-cooled reactor, like a thermal reactor has certain similarities to a pressurized water reactor in the physics. Thus some of the information you get, some of the codes for computers are useful.

Admiral RICKOVER. We have had to make a lot of studies in each reactor project which will help other projects, particularly on materials. For example, we recently completed a study: We tried to get away from the use of helium as a cover gas. We wanted to use nitrogen instead. We have been studying this thing for 2 years and we have found nitrogen may have bad effects on account of nitriting steels. If somebody comes up with the idea of using nitrogen as a coolant in a gas-cooled plant, we have information as to the deleterious effect that nitrogen may have. There are basic things you learn and certainly any organization that has become proficient in designing reactors may develop a sort of feel for what is required. It is as though you were starting a clerk somewhere. Obviously a person who had been through high school would be better than someone who has never been to school at all. You might say you have people who are through the high-school stage so that they can understand what you are talking about. That takes a long time in this game, Mr. Holifield, to really understand what you are doing. That is the sort of thing we have to have in a laboratory of this type.

When these people put out a reactor it doesn't have a lot of acres. They have to design it exactly. If we decide that it is going to have 2 fuel elements and so many control rods, it has to work with 2 fuel elements and that many control rods because there isn't any space for any more than that.

Interestingly enough we went critical—that is, in the *Nautilus*—the new core went critical exactly in the way we had calculated. We have

to because our ships, particularly our submarines, have a limited weight and space. If we don't meet it, she can't submerge—she will sink. We are getting into this game in a very, very thorough way. We must know what we are doing to the very last thing which is something you don't have to have in a lot of other plants. So our design must be extremely accurate and painstaking.

To get back to your original question, when you put a new coolant in a reactor you have a tremendously big problem.

Representative HOLIFIELD. Does that carry over into heat transference as well?

Admiral RICKOVER. Yes, sir. Let's take gas as compared with water. Gas is normally a poor heat transfer medium. That means you have got to have a much larger pumping power for gas than for water. You have to make many calculations and this is particularly true for a naval vessel.

For a land reactor of the Calder Hall type I would design the pressure vessel as large as it could be made. I would have extra fuel elements available in case it didn't work. I would keep on adding fuel elements until it did work.

This was a very penetrating question. Every time you start a new reactor—if you start a homogeneous reactor, you have different problems than you have gotten into before.

Chairman DURHAM. How does that apply to your core?

Admiral RICKOVER. The core is going to be completely different for the homogeneous reactor than it is for a gas-cooled reactor or for a pressurized water reactor.

Chairman DURHAM. In other words, that is a new job, too?

Admiral RICKOVER. Yes sir, that is a completely new job. I think you should understand it is completely new.

Representative HOLIFIELD. Actually in place of being able to do it experimentally like the British have been doing it, you have had to cover that point of accomplishment in your laboratory so that when you did install the rod type, you had already passed the experimental state. How did you scale up your computations to know that the *Nautilus* would go critical at a certain point?

Admiral RICKOVER. That was in our basic philosophy. We say we will not go to these intermediate stages in our program. We will pick a ship that we want to put the reactor in, and we will start designing those very items. At the stage of testing we will build a prototype. That is where the prototype comes in. We say this prototype will be exactly the same as the ship, and we have got to make the prototype work.

The other approach is to start in with little things, but you do not get dimensional similitude in the reactor game. For example, if you have an automobile storage battery, from that we can go and design a submarine storage battery which has about 50 times as much volume. There are many things in engineering which can be done this way. But not with reactors.

Representative HOLIFIELD. I don't understand. Why?

Admiral RICKOVER. There are two main reasons. One is that you are dealing with phenomena. There are these little phenomena that you have going on in atoms where you are concerned with a thousandth of an inch, and just a couple of thousandths of an inch may make a lot of difference in the control and the way the whole thing works. Gen-

erally in engineering you have gross effects. Take a valve. Let's say that it is good for 100 gallons of water a minute. I can take one that is designed for 100 gallons of water a minute and I can pretty much figure out a design for one that will take twice as much water with a very good chance of success, but this is not true with reactors. The only way I can do this in a reactor is by making it larger than necessary. If you make it very big and you have lots of room, then essentially your reactor is a critical experiment and not a real power reactor. Take the EBWR. They computed they would require 48 fuel elements. They actually required 78 to make it go critical. We can't do that in a naval vessel.

To get back to your original question we build something and we test it in every way we can. For example, we ran the first pumps as much as 10,000 hours before we actually installed them in the prototype. We rigged up equipment here at Bettis. We kept on testing. We rolled them 30° from side to side while they were running. There was pitch under all of these conditions too. We tested them in a live submarine under depth-charge conditions. Congress permitted us to use an old submarine, and we installed these pumps in the submarine. We exploded a 300-pound depth charge 30 yards away. The pumps kept on working. We tested a lot of stuff that way. That is how we learned.

Chairman DURHAM. Is there any way to determine the number of neutrons in a core?

Admiral RICKOVER. We know the neutron level in a core. In the prototype of Arco we have been running tests with what we call flux wires. We insert these wires in a core and we can measure neutrons. We actually know what goes on in the core. Incidentally, one of the reasons for the considerable expense is the vast amount of instrumentation that we are putting in so we can learn from future design. We have not looked at any of our plants as jobs that you just build to make it work. We have always borne in mind that we had to develop lessons for our own future and for the future of other people in the game. This is one thing that this laboratory has done a great deal toward. We have always been conscious that while we were trying to get a particular job done we had to learn lessons for the future. We had to find out why things were happening. Naturally you never do as much of that as you want to, but there has been a considerable amount of effort and money spent to develop basic information.

Chairman DURHAM. How about the speed of neutrons?

Admiral RICKOVER. The speed of neutrons depends on whether you have a thermal reactor, an intermediate reactor, or a fast reactor. All of the pressurized water reactors that we are designing here are thermal reactors; that is, they use slow neutrons. The *Sea Wolf* has an intermediate reactor. Thermal reactor neutrons have an energy level of about one-fortieth of an electron-volt. The *Sea Wolf* has about 5 electron-volts. Fast reactors have much more than that. They may be anywhere from 5 or so electron volts up to 100,000. All of our reactors except the *Sea Wolf* are thermal reactors.

Representative PRICE. Mr. Simpson, did you plan to tell us something about the size of the laboratory and the size of your organization?

Mr. SIMPSON. Yes, sir, after the various gentlemen have talked I will describe a typical reactor project. From that I will show you the number of projects and sizes.

Before we go on though, I would like to give one specific illustration of the problem of scale-up that the admiral mentioned. If you take a small reactor similar to the *Nautilus*, it has a negative temperature coefficient. If you keep making it larger and larger and keep everything relatively the same, somewhere you reach the point where it no longer has a negative temperature coefficient, but it has a positive coefficient. Instead of the reactor having inertia and tending to shut itself down and, therefore, being safe as the temperature goes up, it reverses and the power tends to rise. It then has a possible runaway condition. At that transition point you must change the design to maintain the negative coefficient, so what you have learned in the small one does not per se enable you to design the larger one, because a leakage of neutrons out of the reactor is strongly dependent on design geometry of the reactor.

Chairman DURHAM. Then there is no way to determine the efficiency of a reactor on the basis of size?

Mr. SIMPSON. This can be done analytically to the extent we are today competent. We are not yet, nor is anyone, competent to completely describe a reactor's performance with simply analytical techniques without experiments.

Chairman DURHAM. Actually you have to build it to get the full knowledge of its efficiency?

Mr. SIMPSON. Yes, sir.

Admiral RICKOVER. The next speaker is Commander Laney, who heads up the technical staff here for Mr. Geiger, the manager of the Pittsburgh area office of the AEC. As you may know, we have civilians here and officers from the Navy who are experienced in naval engineering, helping out in the field offices. Commander Laney works for Mr. Geiger and heads up the technical staff.

STATEMENT OF COMDR. R. V. LANEY, ASSISTANT TO PITTSBURGH AREA MANAGER, ATOMIC ENERGY COMMISSION

Commander LANEY. In my remarks, Mr. Chairman and gentlemen, I will attempt to point out some of the naval characteristics of a ship which influence the problem of the reactor, the reactor plant and cycle.

Each naval reactor project here has a specific end in view. It is intended to be installed in a definite ship at some definite time in the future. Because of the fact that the building time for a ship and the building time for a reactor and the reactor plant components are different, this means that the ship is partly built—it is almost entirely designed and it is on the building ways while the reactor and the reactor equipment are still being designed. Visualize the ship sitting there—its characteristics, its length, beam, its speed, its submergence depth—all are determined, frozen. The steel has been rolled and put together. The reactor designer meanwhile hasn't yet fixed his design and his components have not yet gone into production. This means that his task is very sharply defined for him and there is a very high premium on the success with which he does it.

Much is committed, in other words, before he reaches the point of committing his material to the manufacturing shop. The reactor designer then must conceive, develop, design, and produce a reactor which, when delivered to the ship, will fit into the reactor vessel which it has never seen before. That reactor vessel is resting in a ship which is a stranger, and the reactor, the vessel, the pumps, the heat exchangers, and the intricate control equipment must, the first time they operate in unison, operate correctly, so that the ship will have the necessary amount of power to produce the speed for which he has designed and which he has to come up with.

These remarks indicate perhaps how the reactor designer is given a very specific task, a very specific chunk of space and weight, performance, and criteria to meet. Long before he knows just how he is going to do this, other things contingent on that have been done. This accounts in a large measure for the very extensive development and engineering testing which accompanies and goes along with the reactor—just because the premium on this working and working right, as designed, the first time, is so high.

While you are here today you will see some of these large-scale engineering tests and with this in mind, you will understand why they are essential to carrying out the program such as I am describing. It is also true that we are by no means sufficiently able to predict the performance of a design as we would like to predict it. There is a good deal of the basic type of information still lacking. This means that it is all the more important that the engineering or performance tests are adequately and thoroughly done. Programs such as these, which span several years in time, in which the end result—the ship operating at sea—is closely and intimately linked with the design of even the components in the plant obviously call for a very closely knit system of program control in which even the seemingly more technical decisions are directly related to their influence on the ship. Will it adversely affect its weight? Will it adversely affect its speed, its safety or reliability? We, therefore, have developed a system of program control which has a rapid response time, which we hope detects quickly and corrects any of the work of design which gets out of line and which is not directed to the objective in view.

Basically the ship operating characteristics and the powerplant functional requirements are specifications prescribed by the Naval Reactors Branch in Washington. They are the working specifications to which work here is performed. Stemming from these original design objectives and working between NRB and the contractor there are developed successively refined plant designs beginning with the most tentative design in which what is feasible has to be distinguished from what is desirable but may not be feasible, so as to obtain the assurance necessary along the road ahead. These designs are then successively refined. The components, the systems of the plant are described analytically in words, and experimental programs are devised to test them at each stage of the way. The design decisions made have to be measured against the end product and its result upon the end product. All of the major design or technical decisions which are made in these programs are made essentially by agreement among the principal participants. That is the Atomic Energy Commission, the Navy, and Bettis. All major technical decisions are based on essential agreement. If there is a strong dissent

on the part of any one of these parties, the only way we feel it is possible to resolve it is by talking it through until essential agreement is reached. On the other hand, the lesser technical decisions which derive from the major ones are made in somewhat the same way in most cases—being referred back to our principal technical source of direction, the Naval Reactors Branch.

We have here at the site a joint AEC/Navy administrative and small technical group. This small group is responsible for administering the contracts under which the work here is performed and for assuring that the work is done within the prescribed procedures of the AEC and the Navy. Also this small technical group serves as a liaison between NRB and the contractor, attempting to interpret the objectives and clarify them. We monitor the work continuously so as to assure so far as we are able that it remain pointed toward the objectives we have and paced so as to achieve them in time.

Representative PRICE. Mr. Durham has a question.

Chairman DURHAM. Commander, I understood you to say that you have the hull of the ship before you begin designing or before you have knowledge as to how you can develop the reactor in a ship. Do you think that would apply to a plane as well as a ship?

Commander LANEY. I don't know the answer to that, Mr. Durham. I am not familiar with the manner in which planes and their engines are designed. In the case of the ship, the present building time for a submarine is perhaps 3 to 3½ years. That is good building time. In order to put off as late as possible the time when we freeze our reactor design, which is desirable to get the best reactor, we wait until the ship is a year or a year and a half along. Since we can build a submarine-type reactor in 12 to 14 months, we use that intervening time not to freeze the reactor, but to improve it. It is for this reason that the reactor goes into its manufacturing period after the ship has been completely committed and is in its principal characteristics fixed.

Representative HOLIFIELD. Mr. Chairman, I want to ask one question. Probably this question should go to Admiral Rickover. Could you explain very briefly what the composition of the Naval Reactors Branch in Washington is? I know you are the head of it, but how many people do you have? How many Navy people? How many civilians? What, in general, does it take in the way of qualifications from the Navy side to get in that Branch?

Admiral RICKOVER. As you know, the organization is such that my primary duty is in the Commission and I work for Mr. Davis, who is the Director of Reactor Development. I head up the Naval Reactors Branch. Our system of operation is quite different than any other branch there. We are not just building reactors. We have to build reactors, as Commander Laney has explained, that must fit in ships which have already been started. The Navy wants them to make so much speed which means so much horsepower so we are very limited. There are a lot of things we can't do. We have to work harder to make it fit.

The technical direction of our program comes from my organization in Washington. We make the more technical decisions as Commander Laney has said. Where there is an argument we have to fight it out. There has never been one time in this game that we have ordered anyone to do anything. Of course, ultimately you get to

questions where policy decisions are involved and then I make the final decision. That is where you do one thing or another. Ultimately, somebody in life has to make those decisions.

We have about 90 people in the Naval Reactors Branch. Most of them are very young. Several years ago when I started to hire people for our organization I found there were very few people who were qualified for our work. By "qualification," I do not mean, necessarily, their technical ability, but the desire to work long hours and to be dedicated to the job as well. At the state we were in we had to get people of that kind. So I came to the conclusion that first we should have a long training period. As a result of that the Commission finally set up the Oak Ridge School of Reactor Technology. We were instrumental in getting that started. There is no one in my organization who was not given special training. Everyone who comes into my organization must receive this special training.

We adopted the procedure of getting only young people. We get the best graduates from universities in the country. I have arrangements with deans of the various engineering and scientific schools whereby they recommend their best people. They come in for an interview. I am talking now of people coming from universities. They are interviewed by a board of four people. Each member of the board interviews them separately. Then I interview them. There is not a single person who comes into the Naval Reactors Branch, except secretaries, who do not have to go through this procedure. Our rate of acceptance even of these people who are so highly recommended is about 1 out of 4.

The same thing applies for naval officers. No civilian or naval personnel are ordered into this program. They are all selected. Admiral Holloway, Chief of Naval Personnel, realizing the importance of this program to the future of the Navy has made an exception to the normal procedures. He permits people to come in for an interview. We get engineering duty officers in once a year. Out of an application list of about 40 or so engineering duty officers, a board screens their records and recommends 15 or 16 to come in for an interview. They go through this process I have described. We will probably take 4 or 5.

Most of the officers in our program are young officers—Reserve officers—who have agreed to stay on for 4 years instead of the normal 2 or 3 years. We have been successful, in many cases, when their tour in the Navy is over in that they shift over into civil service. This is really one reason we take these young people in. We feel it is essential to get smart young people and train them in our way of doing things. I think it has worked out very well.

Representative Holifield. You are not reaching out then for topflight scientists. Actually you are training your own.

Admiral Rickover. A lot of topflight scientists are not topflight scientists. A lot of topflight scientists are that by reputation only. We can't afford to have people around who have reputations who don't work hard. We would rather have people who work hard and don't have the reputations. No, sir; we have been rather disillusioned.

Representative HOLIFIELD. This would indicate, then, it is on the capability to direct men into a certain field rather than to rely on the basis of what they had already accomplished in that field; would it not?

Admiral RICKOVER. If we get in people with more experience it takes too long to have them unlearn the bad things they know. We haven't got time for that. So we have come to the deliberate conclusion that it is best for us to take young people in who have not yet been out in industry and have learned the way industry or other organizations do things. We have our own way of doing things. A lot of people don't like the way we do business and we are very well aware of that, but there is no one in my organization who can't get out and get a job at a much higher salary. Very few of them leave though, so that is the proof of the pudding. I don't know whether I have answered your question.

Representative HOLIFIELD. It gives me an idea of the type of branch you have.

Admiral RICKOVER. I would like you to come up some time. I know you are all interested in education. I would like you to meet some of these young people, talk to them and hear what they say.

Representative PRICE. Thank you very much, Commander Laney.

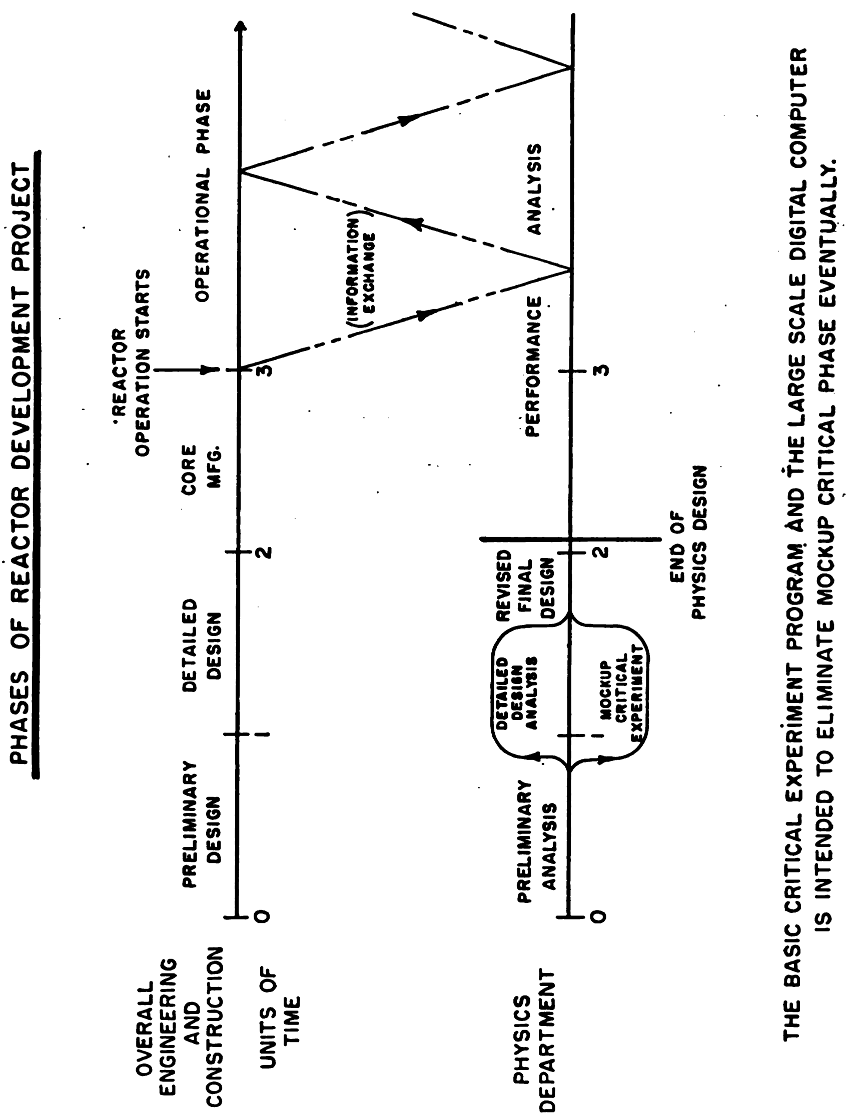
Mr. SIMPSON. I would like to ask Dr. Sidney Krasik, the manager of the Bettis Central Physics Department to discuss the physics aspects of reactor development.

STATEMENT OF DR. SIDNEY KRASIK, MANAGER, CENTRAL PHYSICS DEPARTMENT, WESTINGHOUSE ELECTRIC CORP.

Dr. KRASIK. Mr. Chairman and gentlemen, we plan today to show you some of the interesting things we have here at the laboratory and to tell you how we carry on our reactor design.

You will find a great many of the problems in the reactor design are centered and focused around the physics problems of a reactor. One of the things we want to show you is our critical experiment facilities. I thought perhaps I might take a few minutes to explain how the physics design of a reactor is accomplished, how it fits into the overall design of a reactor plant, the problems we face, the things we can do and the things we hope we will be able to do.

Actually it might be just as simple for me to make a few sketches on the board here and talk informally. Suppose we take the duration of a reactor project as being three units of time with a unit of time being more or less a year or so. This is the stage of completion of the project. The reactor core is now inserted in the ship and it is ready to go into operation so that is the operational phase of the reactor project. This is the engineering and construction phase. Something of the order of a year is associated with the core manufacture and installation in the plant. Another unit of time more or less of the order of a year is associated with the detailed design; that is the working out of the actual detailed mechanical problems. There are tremendous problems that have to be worked out in the preparation of the drawings—the release of these drawings for procurement of components.



There is left open approximately one unit of time on the order of a year for the preliminary studies which essentially fix the nature of the project. As Commander Laney told you it is about this stage of the game that the characteristics of the vessel have been fixed into which this reactor component will go. The freedom then which a reactor designer has; the freedom which a physicist has, is very limited. Indeed, all we have is this time here to decide how the reactor of which we do not yet have a good picture is going to work in that plant; how it is going to meet the final weight specifications; how it is going to fit into the space that is allocated and perform as predicted.

Let's take a look at this from the physics point of view. There have been several questions asked. Mr. Holifield asked a question about

scale up and similitude. I would like to go into that a little bit by indicating how the physics design is accomplished. The fact of the matter is, as Mr. Simpson has said, that we really don't know enough to sit down with pencil and paper and slide rule, a desk computer or even a larger automatic computer. We simply do not know how to sit down and calculate the properties of this reactor. We don't have that knowledge. We don't have that understanding of the way the reactor will operate.

The way that is accomplished then is sort of like this. This is the physics part of the overall reactor design. This is the overall reactor design. This is the physics part of it. You start out by making survey studies—trying to explore in a very rough fashion what you can do. You may spend some months doing that.

As you begin to see what it is that you are going to be able to do, what you are going to be able to meet, you can actually go into the preliminary design phase in which you begin to draw some pictures of what this reactor is going to look like. You can begin to make sketches of its size. You know what size reactor vessel it must fit into—dimensions and so on.

As soon as we have reached that stage we then proceed to go into a physics experiment which is an essential part of this design procedure. This physics experiment we call a critical experiment. It represents, as nearly as we can tell at this stage of the game, a nuclear mockup of the final reactor as we visualize it. Obviously at this stage of the game we don't know what the reactor is really going to be like so we begin this experiment with some degree of flexibility so it can be changed and modified as we learn more about the reactor.

At the same time, in order to proceed and gain as much time as possible, we actually go into a detailed analysis of the design on the assumption that we do know how to do it. That is rather than going into a detailed analysis and then into a critical experiment. Then correcting the detailed analysis, we carry them on in parallel in order to save something approximating a year in this overall procedure.

When the critical experiment—and we hope to show you several of these today—is in operation, we begin to get information which then gets put into the analytic part of the job. After, say, 8 or 10 months when the critical experiment has been constructed and it is ready to go, we find out we have made some pretty bad mistakes so we begin working back and forth between the detailed analysis and the critical experiment.

Then we go into the revised final design. This is the final design we make, and this has been released for manufacture. So you might say that as far as the physics design of the reactor is concerned, there was precious little flexibility back here even, because all of the characteristics—the size and so on—have been fixed and by the time you reached this point there is no further influence we can have on the design.

It would be a very serious mistake to stop that effort at this point. What really is needed is a followthrough on the effort and this stage which we might call the performance analysis. That is we continue the physics work. Now that we know what the reactor is going to look like, know its shape and size and some details of design, we actually begin to study in considerable detail what this reactor will

actually perform at. It may be at this particular stage of the game we can uncover a bug. If so, we are in a far better position to correct it than we would be if we waited until the reactor went into operation.

As soon as a reactor operation begins, the operational lifetime for performance, efficiency, and so on gets back into this. This performance analysis of the reactor, this followthrough on the project is an extremely important part. It is here you can make a great deal of progress for your technology. I might even cite an example. Admiral Rickover mentioned the *Nautilus* core. I think it is noteworthy that the principle that was incorporated which extended the lifetime of that core had, in effect, been suggested by a member of Admiral Rickover's staff. The suggestion was originally made and presented late in 1952, as I recall, after the first *Nautilus* core had been completed. There was nothing we could do about incorporating it in the first core then, but we proceeded to factor the information from the *Nautilus*—the Arco operation into that and during 1953 that principle was examined analytically. It then was incorporated into the prototype. That prototype was fully tested in the plant at Arco and now it is ready to be used in the *Nautilus* and do demonstrate its worth.

Chairman DURHAM. I have a question at that point. I suppose your analysis applies to all types of power reactors as well as to the *Nautilus*. Would it apply to all reactors?

Dr. KRASIK. If the procedure that is carried out in determining—in developing the reactor is the philosophy which we adopt here; namely, to make this time as short as we possibly can.

Chairman DURHAM. That is the point of course which disturbs us all. I suppose Westinghouse hopes to get into the position of selling reactors at some stage of this game. Looking at it as a hardheaded business concern which wants to purchase a reactor, how are we going to sell him that reactor on the basis of that kind of analysis? That is on the basis that all propulsion systems have to be determined more or less on the end product. You can't tell this concern whether or not this reactor will perform; can you?

Dr. KRASIK. No.

Chairman DURHAM. As I understand this analysis, you have got to go to a point where there are large expenditures of funds by somebody before you can guarantee to that purchaser that he is going to get performance out of the reactor?

Dr. KRASIK. That is for a developmental reactor.

Chairman DURHAM. That is right.

Dr. KRASIK. This analysis would certainly apply to the Shippingport plant where exactly the same type of development program is under way.

Chairman DURHAM. Here is my point. At the present time we have many companies throughout the United States looking at these things, coming in and making applications to the Atomic Energy Commission and starting in a small way. I don't see how we can ever sell a man a reactor on the basis of your analysis. If I was sitting on the board of directors of some company, I would consider this as a hardheaded businessman.

Representative PRICE. Doesn't this apply only to the first of any type of reactor?

Dr. KRASIK. What I wanted to emphasize, Mr. Durham, is that this is the way to carry out the development of the first plant of its class. This is the way the first Shippingport plant would be carried out. This is the way a new and advanced core for Shippingport might be carried out. It would not, however, apply to a duplicate plant in which all of the technology, all of the information developed here would be made available. Once you have reached this point and you know what this reactor will do you can copy it without doing any of this.

Chairman DURHAM. I realize that. Of course we are at a point of development in this country of having nothing but Shippingport at that point, you might say.

Representative HOLIFIELD. And where every type of reactor that is proposed must go through this uncertain and unpredictable stage and then operate it after you have completed the work. It has got to be operated over a period of time before you get a history of performance.

Dr. KRASIK. Exactly.

Admiral RICKOVER. We are still learning from the plant out at Arco. It is our most valuable tool. Again, let's go back to the dimensional similitude problem. The think is completely different than any conventional apparatus. You can build a turbine. After the first turbine worked around 1880, for practical purposes it was relatively easy to build a larger one. You didn't have to go through this. There were details you had to work out such as better manufacturing of buckets and fastening of buckets and the wheels and so on, but you didn't have to go through this. I don't know of any other scientific and engineering game where you have to go through this process.

Representative HOLIFIELD. You have stated that you started within the limitation of the whole—of the ship. Commander Laney explained that. Is there some advantage and if so, how much, to starting a new type of reactor without that limitation? I am thinking now of a new type of land-based power reactor. What is the advantage of not having to conform to a specific space?

Admiral RICKOVER. If you want to get a job done and have it done on time and have it work, then you had better tie the guy down within boundaries. You have to say, "You have got to produce a certain thing in a certain time." Otherwise he is going to take easy, dilatory methods. I think you will ultimately get more progress—much more progress out of this hard regimen than you get where somebody just builds something.

Representative HOLIFIELD. A hard regimen in performance and in time?

Admiral RICKOVER. Over everything, sir. That is the only way you are forced to find out what really goes on in this reactor.

Representative PRICE. It is better for the morale of your organization if they are working on a schedule.

Admiral RICKOVER. Better for the morale of the people who are working, but not for their wives and families, as I think some of these people will attest. However, that is a basic principle in performing any work of this kind. If you tell people to just build a reactor and, to use a figure of speech that they have got acres of space, they can go along and do anything. They are not pushed. When they try it out,

and if it doesn't work, they say, "We will do this and we will do that." But when you force scientists and engineers to stick to something that has got to work in exactly that way, then there is something happening every day and night. They have got to find out why as Dr. Krasik has been telling you. He has got to find out why these things are. We give him such tough limitations in space and in weight that he can't possibly do this unless he really learns why. That is the reason we are developing a science of reactors and reactor technology because we have had to find out why it works.

Chairman DURHAM. I was impressed by the statement of Commander Laney when he said that every reactor here was designed for a useful purpose.

Admiral RICKOVER. For a specific purpose.

Chairman DURHAM. In other words, something you can put in the storehouse.

Admiral RICKOVER. It either works or it is junk as far as we are concerned.

Representative HOLIFIELD. If you were designing it for a land-based powerplant, can you not tolerate more latitude in size?

Admiral RICKOVER. Let me answer that. No, sir. The thing that dictated the power output of the PWR was the maximum size of the pressure vessel that you could make. That was the largest one that American industry was capable of making. Incidentally that determined the basic design in the Calder Hall plant, also. They built what at that time was the largest pressure vessel for that pressure that could be built in England. Once you do that, the next thing you want to design and get is the maximum amount of power output you can. Your volume is fixed.

Representative HOLIFIELD. In this case your limitation was the technology of building the pressure vessel, just like in your submarine it is the space in the hull?

Admiral RICKOVER. That is right. I could have taken a different approach and said, "Fine. I have got the largest pressure vessel. I will just build a reactor. It will work and we will get out of it what we can." We didn't do that because we not only knew when we started out the maximum size of the pressure vessel, we also set the power for it. We didn't have to tie ourselves down, but we did tie ourselves down. Is that point clear?

Representative HOLIFIELD. It is clear.

Admiral RICKOVER. We tie everybody down because the natural human tendency is for everybody to create some kind of monopoly—that is to be let alone. We think that is bad—to let people alone.

Representative PRICE. Does that mean the maximum capacity of a pressurized-water powerplant in the future will be limited by the maximum size of the pressure vessel?

Admiral RICKOVER. No, sir; it does not mean that at all. When we said 60,000 kilowatts of electrical power, we wanted to give a figure that we thought we could meet. We are designing a new core based on a great deal of information that we have learned from the first one. We are designing a new one here which we think will give about a hundred thousand out on the same pressure vessel. There are many other things we have learned that we can do. Perhaps we can stand some amount of boiling in the reactor. Perhaps we can increase the size of the pressure vessel by learning how to get down to lower

pressures. There are a lot of other things. But basically the thing Mr. Holifield was asking about is the real major principle and how you go about doing the job.

Representative HOLIFIELD. I was trying to find out if the power-station people are going to be bound by limitations of one type or another the same as you are.

Admiral RICKOVER. Yes, sir. The General Electric Co. when they are building a reactor with only 1,500 pounds pressure—whatever it is—they are also bound. They are making the largest pressure vessel they can with 1,500 pounds pressure so they have already got that fence right there.

Mr. SIMPSON. There is another facet to that, sir, that I would like to bring out. Not only is there the limitation of the size of the vessel, but strange as it may seem, the actual design, manufacture, and installation of the pressure vessel takes more time than getting the core so that we have ordered and started cutting metal on the pressure vessel and even if we could have built a larger one, we were already building the one of that size.

Representative HOLIFIELD. You were committed.

Mr. SIMPSON. We were committed. Before the reactor design had reached the point where we knew as much as we wanted to about the power you could get out, the metal was rolled for the pressure vessel and at that point you can't change.

Admiral RICKOVER. It took 2½ years to make that pressure vessel.

Mr. SIMPSON. The only limitation of the larger vessel you could build is the limitation of the vessel you did build.

Admiral RICKOVER. That is the only way you can do it—if you set yourself a goal that you will have something working by a certain time. The other way you do it is to set a bunch of people designing cores and people designing a pressure vessel and sometime, maybe 2 or 3 years after they started, they get together and find they are away off. They would have to compromise at that time and you have to start all over again. It might take you 10 or 15 years before you get a reactor. You have got to set limits at some time and work to them or you will never get there. If you are working with small units, you can overbuild them. You are not concerned with the exact power you get out, particularly if it is a small order of power. When you start to work with large blocks of power or in cases where the reactor has to perform exactly, you sure have to be tied down. That is what really makes the trouble in this game. Outside of that it would be a very fine game to be in.

Chairman DURHAM. Suppose somebody comes to Westinghouse now and says, "We want a reactor that will produce 200,000 kilowatts of power." At what point could you tell that person that his plant would operate?

Dr. KRASIK. If this were the first plant of a type?

Chairman DURHAM. You have built a plant here at 60,000 kilowatts. Then someone comes in and says to Westinghouse, "We want 200,000 kilowatts."

Dr. KRASIK. Having done Shippingport and having operated the Shippingport plant we could probably now give a fairly good estimate back in here of what the feasibility would be of a 200,000-kilowatt plant.

Admiral RICKOVER. He said "feasibility" though. Be careful.

Chairman DURHAM. I am trying to get to the point where we can sell some of these.

Representative HOLIFIELD. You would be selling "feasibility" then if you actually went into the market to sell it. You wouldn't be selling "actuality."

Mr. WEAVER. This would be true, Mr. Holifield, until we have operating experience with any one of these plants.

Dr. KRASIK. I would like to add just one word to what has been said about setting the goals and setting the limits. Actually the important thing in reactor development is to explore the technology to find out what you are really up against. If you go down through a valley like a wandering river and take the easy place at each turn, you would not know what your limits are. You have to run up against the real limitations of this problem in order to find out what is going to stop you from doing something. I think from that point, quite apart from the time scale which I think you appreciate, the setting of goals is an important part of this job.

Chairman DURHAM. I think we are all trying to find out exactly where we are at in the development of these reactors. It seems from your statement that we are still in the development stage.

Dr. KRASIK. We are in a very preliminary development stage.

Chairman DURHAM. Preliminary?

Dr. KRASIK. Preliminary and it is quite unsatisfactory to all of us to go through as cumbersome and time consuming a process as this to get to a reactor state. All of us are straining to do something to eliminate this prolonged elaborate procedure.

For example, this critical experiment—quite apart from the money that it costs to build, and it is a very formidable sum of money indeed—takes somewhere on the order of 10 to 15 months in the schedule to construct. As you will see from your visit later today, it must be a rather good duplicate of the plant of the reactor that you intend to build.

Chairman DURHAM. When the Shippingport reactor is completed and operational, will it still be in the preliminary development stage?

Dr. KRASIK. We are trying to do something about this, sir. We are trying to develop the reactor physics to a state where it isn't an art where somebody guesses and puts in a lot of costly time-consuming experiments. We are trying to do a different type of critical experiments, which we will show you, and which are basic critical experiments. From these we hope to get not what is the overall properties of the reactor, but what are the individual things that contribute to these properties. Furthermore with our large scale digital computation ability we are hoping to advance the theoretical so that the combination of these two will in the course of, perhaps, 3, 4, or 5 years enable us to eliminate this completely from the reactor development schedule and to start in at a survey study. This would give you the flexibility at that stage of the game to predict what the reactor is going to do at 10,000 hours of operation. This we must be able to do. This is one of the important things which we feel is our obligation.

Represenative HOLIFIELD. Can this sort of thing be done now, say, in an automotive engine—the airplane engine?

Dr. KRASIK. I am not familiar, sir, with those engineering problems.

Admiral RICKOVER. You don't have the many variables in the automobile engine or the aircraft engine that you have here. It is not

inherent in the thing. You never had it even back during the days of the Wright Bros.

This is the most difficult thing to explain. Perhaps I can't explain it well, but really we are dealing with entirely different phenomena. We have to set up science and technology which doesn't exist, and we are attempting to do that. That is why we have these large computation machines. By using these machines, perhaps in 3 years from now they may develop the beginning of a science of reactor technology.

I think, perhaps, with all the talk that has been going on about reactors, people have the idea that reactors are much further advanced than they are. They are not as far advanced as most of the public would think they are.

Representative HOLIFIELD. In other words, you are lacking in a lot of the basic——

Admiral RICKOVER. Basic technology.

Representative HOLIFIELD. On which to base a really sound production.

Admiral RICKOVER. That is right. We cannot do that yet. However, even though this laboratory is one of the best reactor laboratories in the United States from the standpoints of experience in designing and building reactors, and number of trained people, they cannot yet predict anything. The man who is in charge of this is telling you right now that probably he won't be able to even 3 or 4 years from now.

Representative HOLIFIELD. Then when these people come before us and predict they are going to get 6-mill or 11-mill power out of a completely undeveloped reactor, they are just guessing?

Admiral RICKOVER. You can dignify it by calling it guessing.

Mr. SIMPSON. There are two ways to design anything. One is by really knowing basically what you are doing in designing and the other is by experence and rule of thumb. With the gasoline engines they built so many of them that they built up a backlog of experience. You don't quite know why it works. In designing a circuit breaker, you don't quite know why it interrupts, but you have done so many of them that you can modify it a little bit. If you start counting the number of power reactors that have operated for long periods of time, it doesn't take very long because there are so few. So the rules of thumb, the experimental data to just give you a broad background to build on is lacking. Lacking that, you must know more of the basic science of building reactors and do more of this preliminary design work in order to do it. So the day when we have built hundreds of power reactors, a lot of people will be able to design them by rule of thumb too, but that is a long way off.

Admiral RICKOVER. We know more about what goes into a reactor design today, I think, than most automobile engine manufacturers know about the design of their engine. We have to. They have built, maybe, 100 million automobiles in this country and they have minor changes from year to year in the engines. They are not confronted with a problem of mass safety either. That is another thing. We can't just say that we will take a chance and do something. We have to be absolutely certain that this thing will work and that it will be safe.

The costs are tremendous. The man who designs a new automobile engine is not investing a hundred million dollars and it doesn't take many, many years to do it. So you have all of these imponderable

things that weigh on you here. We do not yet know why we are doing certain things in design. We were quite surprised when the prototype for the *Nautilus* worked as well as it did. We didn't know how it would work. We were very much surprised.

Representative PRICE. I was interested in your comment a moment ago on the cost per kilowatt. Representation has been made to us that at least one reactor in the power demonstration program will produce electrical energy from 12 to 14 mills or a maximum of 16 mills per kilowatt immediately upon the start of operation. Can you explain the difference in that optimism and the pessimism that you have just indicated on that point?

Admiral RICKOVER. I could only answer it this way. About 2 years ago we had a seminar on the PWR. One of the issues which came up was whether we should tell what the costs were at that time. It worked out to be 52 mills. I insisted we should say this publicly. Today it is more than 52 mills. I don't know the exact figure. It might be 65 mills.

Of course, one gets engrossed in his own work and he pretty soon begins to think he is pretty good and no one else knows anything. In order for somebody to turn out a reactor which will produce at 14 to 20 mills, he is going to have to build one a lot better than we now have or know how to build.

Representative PRICE. You know all of the reactors in this power demonstration program. Can you think of one right now which in your opinion could accomplish this?

Admiral RICKOVER. I wouldn't buy any stock in any company which guarantees to produce power at that rate. If you have any extra money, I would advise you to invest it somewhere else.

Representative PRICE. It would be a conflict of interest if we were to invest in it.

Admiral RICKOVER. I am just trying to tip you off. I would not invest money in any reactor group that claims it can do this. I think Mr. Davis, himself, has said he doesn't expect atomic power to be competitive until about 1980——

Mr. DAVIS. No! No!

Representative PRICE. It would be pretty close to being competitive. Do you even expect to do that for a while?

Admiral RICKOVER. There are a lot of other people working on this and maybe they have got some secrets or tricks. I wouldn't know how to do it. I don't know how. Maybe other people do. I obviously can't talk for them. My personal opinion is that I would be inclined to be doubtful.

Representative HOLIFIELD. Of course I have been doubtful all along on this. I haven't been carried away by any of these predictions myself.

Admiral RICKOVER. I think you could ask people, "Why are you going to do it so much cheaper than the PWR?" Maybe we could learn how to design it better, how to operate it better, and have better people doing it. We don't know how.

Representative PRICE. It could be a different system of bookkeeping.

Admiral RICKOVER. That helps too. You have another thing also. What credits do you get for plutonium? What do you charge for fuel? This came up when I was in England. They said they were

putting out very cheap power with the Calder Hall reactor. I said, "Your plant cost so much to build. It costs so much to build in the United States and it is not much off from the power of the PWR."

I said, "I am not a mathematician. I am not an economist, but it seems to me if the two plants cost about the same to put up, why do you get this vast difference in cost?" It is all Government operated. They don't have depreciation charges and so on.

Representative PRICE. What did they say it cost?

Mr. DAVIS. Calder Hall, about $45 million.

Admiral RICHOVER. $45 million, but if you took it on American costs to build, it would mean may be another 50 percent more.

Mr. DAVIS. We had some estimates made of how much we think it would cost to build a similar reactor in the United States. The best guess we have is that it would cost perhaps 50 percent more to build a Calder Hall type of reactor in the United States just because of the difference in labor rates. This would make it of the order of close to $70 million—somewhere between $60 and $70 million.

Admiral RICKOVER. You run around the same and yet they say that their power costs are about a third or fourth of ours.

Mr. DAVIS. I think you have to point out that the costs they are talking of are for Central Electricity Authority and not for the Calder Hall reactor. When they talk about 7 mills they are talking about Central Electricity Authority reactors and still on this different accounting system. They have never really said, except by indirection or just letting people believe it, that there might be 7-mill power from the Calder Hall reactor.

Admiral RICKOVER. I would say, in general, that you cannot expect anywhere in the foreseeable future, say 10 years—I don't like to go beyond 10 years—that you will have an atomic powerplant that would be competitive.

Representative PRICE. Do you think it would get close to being competitive within the next 10 years?

Admiral RICKOVER. It should, because we are learning a lot and, also, the cost of fossil fuel is going up. If there is to be a real future in atomic power, it is bound to be that way sometime. I think to sell atomic power on the basis that you are going to save money is just like the argument about putting atomic power in the Sahara Desert. I say that is a good way to build railroads. If you want to build a railroad, decide to put the plant there. Then you have to build a railroad in order to haul in the materials for the atomic powerplant. It is sort of like going around in a circle. So, one way of developing the railroad industry is to put reactors in places like the Sahara Desert.

Representative PRICE. What do you think would be the cost per kilowatt-hour of the Shippingport project.

Admiral RICKOVER. It should be considerably less, because we expect to get more power out of it, if for no other reason. We expect to get about 100,000 kilowatts out of the second core instead of 60,000 for which the first core is designed.

Representative HOLIFIELD. You could do it more quickly without this research work?

Admiral RICKOVER. We still have to do the research work, because it is a different reactor, but we have got the plant there. We just put a new core in.

Representative PRICE. What do you think the difference would be?

Admiral RICKOVER. At the time, we estimated 52 mills for the first core, I believe. We estimated 39 mills for the second one.

Mr. SIMPSON. The second core.

Admiral RICKOVER. Then we estimated for the third core that we might get down somewhere around 14 mills. That is for a new plant—building a new plant. We would use all of the lessons we have learned. Today I wouldn't be that optimistic.

Representative PRICE. That is the third core?

Admiral RICKOVER. The third core would be the first core in the second plant.

Representative PRICE. What was the figure?

Admiral RICKOVER. We gave that figure 2 years ago as 14 mills. Today I would be inclined roughly to double it.

Representative PRICE. Would that indicate this plant at Shippingport would not be as efficient as some of the claims on some other plants?

Admiral RICKOVER. Any plant you haven't built yet is always more efficient than the one you have built. That is obvious. They are all efficient when you haven't done anything on them. They are in the talking stage. Then they are all efficient. They are all cheap. They are all easy to build, and none have any problems. That is quite correct. They do not have any problems at that stage.

Representative PRICE. Have you finished, Dr. Krasik?

Dr. KRASIK. I think I have essentially concluded my remarks.

Representative HOLIFIELD. I hope you will pardon our interruptions. I think we can learn more by asking for information as we go along, and that might be the best way for us to learn.

Representative PRICE. I would say your fine presentation led to the questioning.

Representative HOLIFIELD. Mr. Chairman, I would like to have this presentation given before our full committee sometime, because this has given me a better concept of the breakdown of the time schedule than I have had before.

Representative PRICE. That is a good idea.

Mr. SIMPSON. Mr. Chairman, I would like to have Dr. Lustman, who is the manager of the metallurgy department, say a few words on the metallurgical aspects of reactor design.

STATEMENT OF DR. BENJAMIN LUSTMAN, MANAGER, METALLURGY DEPARTMENT, WESTINGHOUSE ELECTRIC CORP.

Dr. LUSTMAN. Mr. Chairman and gentlemen, as Mr. Simpson and Dr. Krasik have mentioned, once the engineering design and fabrication requirements of a specific reactor are fixed, the fuel and cladding material are then fairly well chosen. However, once this choice is made, an entire spectrum of materials development problems arise which require solution before the reactor core could be considered to be feasible. I will only give you a few of the most important type of development problems.

For example, the zirconium and zirconium alloy cladding developments—the technology which was developed in the course of the construction of the *Nautilus* core can be applied quite directly to the

construction of new submarine cores or to the A1W core of the large ship reactor.

Representative HOLIFIELD. What is the A1W?

Admiral RICKOVER. That is the aircraft-carrier project.

Representative PRICE. Let me ask you this question before we get too far along. Do you have any work here at all on the merchant-marine reactor. Do you cooperate in any way with that program?

Admiral RICKOVER. We make available the information we have learned.

Dr. LUSTMAN. In connection with his cladding problem for the PWR, pressurized-water reactor, simply a change in the design from the shape which is used for the *Nautilus* and for these other reactors required an extensive materials-development program and close cooperation an liaison between Bettis personnel, who are familiar with zirconium fabrication technology, and fabrication companies who are familiar with the fabrication of metal tubing.

On fuel materials, Bettis has taken the lead in the development of two essentially new types of fuel material. One of these is uranium oxide, which is used in the pressurized-water reactor for Shippingport. At the time uranium oxide was chosen as the preference fuel for the pressurized-water reactor, only a slight amount of laboratory-scale information was available on this material. Many of the properties which were necessary to assess the feasibility of this material as a fuel were not available. Furthermore, the technology had not been developed to permit the fabrication in shapes from uranium oxide power into production quantities.

Such basic questions as the reaction of the uranium oxide with zirconium alloy cladding, the conditions of water chemistry necessary to maintain uranium oxide in contact with water, thermal conductivity of uranium oxide—all of these questions had to be investigated and determined before the feasibility of such a material could really be supported.

In connection with a question asked by Mr. Holifield, during the course of the PWR development we did work on a similar type of alloy in the same class—uranium molybdenum alloy. The technology which was developed for this alloy is being used by the Detroit Edison group in their reactor.

Chairman DURHAM. At this point, which emits the most gamma rays—oxide or alloy?

Dr. LUSTMAN. I think it is the quantity of uranium and the amount of uranium burnt up which controls the radioactivity of the material. Large development was required for the alloys because of the lack of knowledge which existed prior to their investigation here. There were all sorts of factors—hot water corrosion resistance, thermodynamics stability of the material, specifications from purity contents, method of melting, casting, rolling, bonding with zirconium cladding—even the development of sources of metal to use as an alloy and, in addition, of suitable purity—all of these questions had to be answered. Furthermore, they had to be answered within the time period, which Dr. Krasik outlined, which is available for the development of the reactor. Certainly, many development questions have had to remain unanswered because this time period is much too short. Decisions have to be made and concurred with between Bettis per-

sonnel and Naval Reactors Branch technical personnel to permit the job to go on within the required scheduled period.

Another important field is the field of evaluation of fuel elements and fuel materials under reactor operating conditions. The greatest uncertainty in the materials field, I believe, resides in this area. This is also the widest area of research and development in reactor core materials which remains today. This uncertainty is because to a large extent there is no theoretical background which permits prediction of the effects of radiation on these fissionable materials, particularly at high levels and furthermore no background exists to guide us as to which type of experiments ought to be carried on to determine the feasibility of these materials.

Representative HOLIFIELD. You mean you don't have the knowledge and at this time you see no way of getting it?

Dr. LUSTMAN. We are getting the knowledge in the course of this development, but there is no background of metal physics or solid-state physics such as exists in other fields which gives you a lead or which tells you some things can be ignored, but other things have to be investigated more closely.

Representative HOLIFIELD. It is not a matter of making experiments along the lines that you already know. You have got to find out how to make the experiments.

Dr. LUSTMAN. How to make the experiments and what are the right experiments to make. Certainly we have made a lot of wrong experiments. We know that.

Another factor here is the very restricted amount of space which is available in reactors for doing this sort of testing, the difficulty of performing power tests in these very high radiation fields which exist in test reactors and the limitations which the very high radioactivity of the test space means after they are discharged from the reactor, the limitations of the type of examination which you can perform on these materials. All of these factors limit the rate of progress in this field.

Representative HOLIFIELD. You spoke of trouble in making the tests afterward. Is that because of the fact you have to handle them remotely?

Dr. LUSTMAN. We have to handle them remotely behind 6-foot concrete walls. You are restricted to just the simplest types of examination—things which we wouldn't even think of doing if we could phyically handle the samples ourselves.

Chairman DURHAM. Where do you do most of your metallurgical testing?

Dr. LUSTMAN. The in-power testing is done in the materials-testing reactor. A small amount is done at Hanford. Some is also done in the Canadian pile at Chalk Rover.

Chairman DURHAM. That has to be done off the base here?

Dr. LUSTMAN. That is all done off the base. Examination after they are returned from a reactor is done here at Bettis in general.

Another factor is that as the design life of advanced reactor cores is continually increased and the amounts that burn up of the fuel are increased, you get into problems at these high burn-up levels which are not in the low burn-up levels, or relatively negligible at least at the low burn-up level.

Chairman DURHAM. Which would you classify as your greater problem at the present time: control of radiation or the heat problem?

Dr. LUSTMAN. From the point of view of materials, I think the most serious problem is the effect of irradiation on the stability of the material.

Chairman DURHAM. Created by heat—would that be true?

Dr. LUSTMAN. Partly by heat, but primarily by specific effects of radiation which we do not understand at the present time.

In the course of the evaluation of the PWR fuel element, for example, almost 100 sample fuel elements were exposed in a reactor and examined after their discharge from the reactor. It would be anticipated that at least as many in-power experiments would be required for the large ship reactor and probably more to fully evaluate or to get the same stage of evaluation as we did in the PWR. Even in the PWR we simply do not have all the problems evaluated which we are sure we are going to meet in reactor operation. It is only by the operation of a reactor core that you really can evaluate the use of a new type.

Decisions have to be continually made to bypass or shortcut some sort of experiment and these decisions have to be made between Bettis and NRB technical personnel again. So we don't expect that we have made all the correct decisions.

Representative HOLIFIELD. In other words, like true scientists you would just keep on making experiments to find out more about it if you weren't stopped by the one that said we have got to get on with the job and use what we have got.

Dr. LUSTMAN. We have to say at some point, "This is far enough."

Admiral RICKOVER. We have never fixed on a reactor design yet where we had all the information we really needed. Every reactor we have built in our program has taken a calculated risk. We are doing it every day. We just don't know enough and if we waited until we got enough information we would probably never build a reactor.

Dr. LUSTMAN. As I said, we certainly don't think we have made all of the right decisions.

I think the final important area of development here at Bettis which I will just briefly mention are the development problems which arise during the course of building and assembling a reactor core.

A large number of important, unspectacular developments are necessary to make sure that the fuel elements and assemblies are fabricated under actual production conditions to tolerance levels and qualitative levels to assure efficient core performance.

Representative PRICE. Thank you very much, Dr. Lustman. Are there any questions?

Representative HOLIFIELD. I am in somewhat the position of Dr. Lustman. I don't know just exactly what question to ask.

Chairman DURHAM. The problem is no different from all the basic research in past history.

Dr. LUSTMAN. That is right. There is no physical way whereby we can reproduce these effects in fuel materials. There is no way we can simulate these effects. We have to put these things into reactors.

Representative HOLIFIELD. Are you using the Hanford reactor?

Dr. LUSTMAN. Primarily the MTR and NRX reactor at Chalf River.

Representative PRICE. What part has Bettis played in the development of zirconium?

Dr. LUSTMAN. Bettis developed the alloys which are presently being used and developed the fabrication technology. Reduction technology was developed by the Bureau of Mines primarily.

Mr. SIMPSON. Zircaloy 2, which is commonly used, was developed at Bettis. The reduction of that to being able to melt and fabricate and weld and use it in a reactor was developed here at Bettis. The creation of the materials from which to start was done by the Bureau of Mines basically. Of course, Bettis, Argonne, Battelle, and many others participated.

Chairman DURHAM. In the fabrication is there much difference in the work involved between the two?

Dr. LUSTMAN. Surprisingly enough, not very much. I think the fact that when you fabricate a metal you are able to fabricate in terms of large ingots whereas oxide is fabricated in terms of small pellets, causes a higher price for the latter type of procedure. The differences in cost are not that striking. As development continues there will be relatively small differences in price between these materials.

Representative PRICE. Thank you very much, Dr. Lustman.

Mr. SIMPSON. Mr. Chairman, in my discussion I referred to the problems of components design, components development and the materials development other than the core materials and the fuel materials.

I would like to ask the manager of the SFR, that is the submarine fleet reactor, Mr. Squire, if he would say a few words on that.

STATEMENT OF ALEXANDER SQUIRE, MANAGER, SUBMARINE FLEET REACTOR, WESTINGHOUSE ELECTRIC CORP.

Mr. SQUIRE. Mr. Chairman and gentlemen, I would like to combine two things in my discussion. One is the development of the design of the reactor plant. The second one is the specific problems on what one might call nonnuclear components. By nonnuclear components, I mean such things as the steam generator, a model of which I have over here and to which I will refer later. Mr. Simpson mentioned the basic concept of maximum reliability of components. For a reactor plant, particularly for a naval reactor plant such as the SFR, we are continually mindful of the consequences of failure of even the smallest component in a submarine plant. Since a submarine must operate submerged or maneuver in time of war we do everything in our power to develop components, to design and to manufacture components that have the maximum degree of reliability. At the same time it is necessary, as Admiral Rickover has emphasized, to utilize the available weight and space at our disposal to the maximum degree.

There is a third item of which we are cognizant as we develop new plants such as the submarine fleet reactor plants. That is, we are conscious of the fact that the Idaho plant and the *Nautilus* plant are the first of a type. That has been discussed to a certain extent here and I think it should be reemphasized that we recognize that is the model of the nuclear powerplant industry.

It would be relatively simple for us to duplicate the *Nautilus* plant for these later submarine powerplants. We think that would be wrong because we feel we can only develop knowledge by taking a fresh look at these plants and doing everything we can to develop new

concepts. When a desire for additional submarine nuclear powerplants was expressed, we got together with the naval reactors people and the Pittsburgh area office people and we attempted to set ourselves objectives to be gained. Objectives that we had not met in the *Nautilus* plant or in the Idaho plant, but which we believe to be desirable.

Some of these objectives were: better use of weight and space at our disposal; improved access to components in case they should fail or in case they should require maintenance in service; improved ability to service the components once we had got to them.

Representative HOLIFIELD. You are not talking about shifting to sodium?

Mr. SQUIRE. No sir, we are talking strictly about water-cooled plants and attempting to improve on that foundation. Our activities have been restricted to the water-cooled plants so far.

So we got together with the technical people from the Naval Reactors Branch and with their knowledge of naval requirements set ourselves a number of objectives for submarine plants—the submarine fleet reactor plants which I am using only for illustration purposes.

As we attempted to develop our ideas we constructed models of possible plants in addition to the paper studies of these plants. One of the reactor plants incorporated a steam generator of the type I am showing you—this model standing here in this room. The steam generator is, of course, the main vehicle for transferring heat from the reactor plant to what we call the secondary plant, the turbine plant. This new steam generator offered real promise for improved space utilization. However, a steam generator of this type to perform in accordance with our operational requirements had not been built before.

The Naval Reactors Branch people and we, jointly, went to the manufacturers of components of this type and discussed with them the feasibility of the manufacture of the steam generator.

Representative HOLIFIELD. You are talking about changing from the *Nautilus* type to an improved type?

Mr. SQUIRE. Yes, using in general the same principle. The manufacturers of equipment of this sort saw no reason why such a unit could not be built. We decided it was worth a trial. It had not been done before, but it was worth trying out for the possible gains to be made in advancing the design of reactor plants.

Representative HOLIFIELD. How does the model compare in size?

Mr. SQUIRE. This, sir, is one-quarter of the size. That is for this particular plant. A plant of larger capacity, obviously, would require a larger steam generator.

As I said, we decided to go ahead and make this. To make a long story short, we had to make dozens of forgings before we could get forgings of adequate quality to put in these units.

There is an obvious question as to why we had so much difficulty getting these forgings when people have been making large forgings for many years. This brings us to another basic point in component design and construction. In naval powerplants or in any reactor plant, for that matter, one of our requirements is that we be able to heat up and cool down these plants rapidly. The obvious reason for this is that it may be necessary for these plants to get up a head of steam and get out of a given port or a given location on very short notice.

In order to minimize the problems associated with rapid heatup and in order to minimize the thermal stresses that are set up in components like this, it is necessary to use as thin sections as is consistent with reliability of the components. In order to use the thinnest sections possible, we have got to get the soundest material possible. We did not set ourselves goals that people believed were unattainable, but we set ourselves goals that we believed we could meet, but not very easily.

We finally obtained enough forgings to take care of this program, but in so doing actually the forging technology had to be advanced. Melting practices had to be improved by the forging suppliers and so one might say that there has been a broad gain by standards set up at Bethlehem and Midvale Co., just in building these units.

Chairman DURHAM. What did they do to it?

Mr. SQUIRE. It was more a question of controlling the melting practice more closely, limiting the time that the material was kept at temperature, controlling the additions that they used for the refinement of the grains in the material, and controlling the forging processes more closely than they normally do. There has been no basic change in composition.

Chairman DURHAM. No change in composition of the alloy?

Mr. SQUIRE. No basic change in composition of the alloy although at one stage in the game, the companies threw up their hands.

Admiral RICKOVER. Mr. Durham, on surface ships with which you are familiar, like the one you were on during World War I, we get one-tenth of 1 percent of sea water through our condensers in what we consider a normally tight condenser. With our boiler water compounds we can blow down and get rid of it. With stainless steel you can't do that. With stainless steel, if you get a few parts per million—it is just fantastically small amounts that will cause cracking in tubes.

As Mr. Squire said, since the whole source of your water—all your fresh water on naval vessels comes originally from salt water, it is very difficult to keep minute traces out and yet it is the minute traces that concern us and cause failure. We do get around it by having suitable boiler water treatment and renewing water frequently.

Mr. SQUIRE. There are several ways of preventing failure of boiler tubes. One is complete removal of the chlorides and oxygen or any other chemical that might influence failure.

There has been a major effort put on this by all of the manufacturers of steam generators in this business. The Naval Reactors Branch have been working with the boiler manufacturers.

I just want to leave this thought with you before I terminate. All the problems are not associated with cores. There are many, many problems associated with the more common materials and actually these problems seem to hold us up just as much as such things as uranium.

Mr. SIMPSON. We have now covered the physics, the metallurgy as applied to cores, the materials as applied to the other components and a typical design problem of the nonnuclear components.

I would like now to ask Dr. Esselman if he would take a few minutes to describe the advanced development work which we are doing in reactor plant design concepts and in looking at some of the other possibilities than the plants which are actually built. Dr. Esselman spent 2 years out in Idaho in actual "rubbing shoulders" work with radioactivity. He was in charge of the technical operation of the naval reactor facility there. He has known radioactive contamination firsthand and also the maintenance problems. He heads up our development here with a real down-to-earth background of the practical aspects of reactor design and operation.

Representative PRICE. Dr. Esselman.

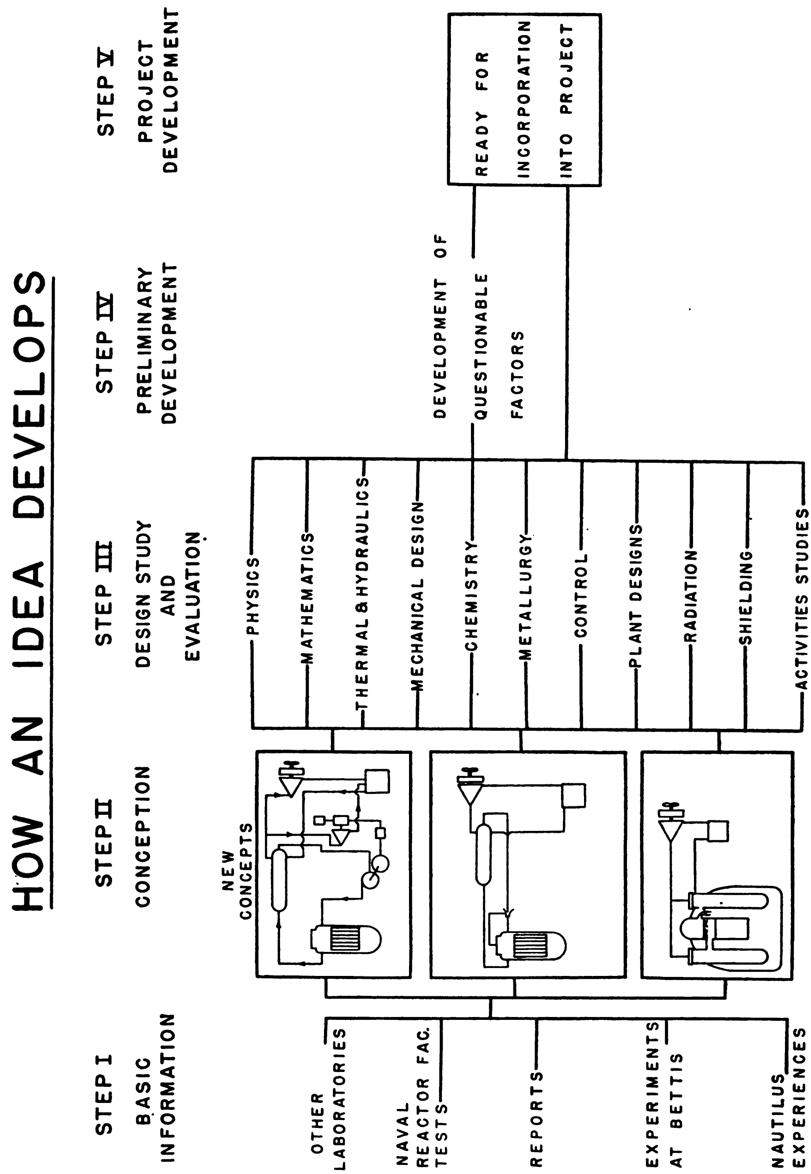
STATEMENT OF DR. WALTER ESSELMAN, MANAGER, ADVANCED DEVELOPMENT GROUP, WESTINGHOUSE ELECTRIC CORP.

Dr. ESSELMAN. Mr. Chairman, the topic which I wish to cover is the procedure for developing an idea before it is incorporated into a project. Many ideas are always being suggested and it is necessary for someone to determine whether it is feasible, desirable, and worthy of being incorporated in a project at Bettis. This work is done by the advanced development group.

The work of the advanced development group is primarily concerned with applications for naval reactors. However, the concepts which are considered worthy of consideration would apply to all types of reactors. This point is illustrated by the objectives of this group in that their studies are all based upon trying to achieve reduced cost of plants and cores, reduced weight per shaft horsepower and, third, maintenance and improvement of the reliability of the nuclear reactor plant.

The first of these—at reduced cost—naturally is of value to all types of nuclear reactors. Secondly, to reduce the weight per shaft horsepower also reflects in the plant cost and usually if a plant is lighter, it will cost less. The third one which is the reliability objective, I have stated as "maintaining reliability" because I do not wish to imply that we are not pleased with the reliability of the powerplants which we have built, but rather any new idea which we would conceive or evaluate should not jeopardize the record which has been made to date.

In order to illustrate the steps which are taken prior to the use of any idea, your attention is invited to this chart. This chart indicates how an idea is developed.



Step 1 is the need for basic information. Basic information is not much stimulus for coming up with new concepts and if you come up with a new concept, you have no information or data with which to evaluate. This information is obtained from other laboratories in the case of plants which are different types than we are building here.

The second one of our great sources for information is the naval reactor facility at Idaho. The third is various reports. The fourth that I have indicated are experiments at Bettis and the fifth is experience on the *Nautilus*.

In order to illustrate a typical example of where information is of great value, I would like to take the field of thermal hydraulics. During design stages on core No. 1, very little was known about the amount of heat that could be passed through a surface—metal surface without causing melt down. A laboratory was set up at Bettis to get experimental points under various values of waterflow, various values of temperature and today we believe we know quite accurately what the burnout points or melt-down points are on heat transfer surfaces. There are many other facets of hydraulic problems of which very little is known. Today we feel we have solved some of these problems. This field is very important in that the power output for any particular core is governed by the amount of heat we can pass through this surface.

We are continuing experiments in this thermal hydraulic laboratory in this field of transient heat flow. At the present time output on several of our cores is limited because of the transient that occurs if the pump would stop. To date no laboratory has done any experiment on transient heat transfer which we can apply to designs which we are making on water-cooled reactors.

The data that we obtain from the naval reactor facility is also important. In fact, it is the only power reactor that we have available to obtain information on reactor operation. At that facility we are continually checking on the activity buildup in loops and performing other chemistry tests. Experiments are being performed on core lifetimes. Unless you have a power facility you cannot really determine the practicality of these principles and you cannot really determine what the life of a core would be.

We are also doing experiments in heat transfer.

When we get this information the next step is the conception of new ideas. Actually the conception of new ideas is not a difficult part of this process. The difficult part is to evaluate the ideas that you have to determine which are good ones and which are to be followed. Many ideas are available for evaluation. The thing that is difficult is to limit the number of studies we want to make by this group to a number which can be accommodated with the few people that we have. At some time we combine several ideas or a number of ideas and discuss the possibility of starting a design study on a typical type of ship. We discuss with the AEC Naval Reactors Branch. We decide, for example, that the study should be conducted for a submarine or some other type of surface vessel.

We are making at this point a very practical study of a design concept. We are making a desirability study of a plant for a particular ship. The third step then is the design study and evaluation. The number of technologies that are involved are indicated. Actually there is probably only one of these various technologists performing this study so this is a relatively small organization. The more scientific aspects require the work of physicists, mathematicians, and metallurgists. The engineering aspects are covered by thermodynamicists and thermal hydraulic mechanical design, control engineering and you see here things such as plant designs. Actually in the study we will make an actual plant layout.

These studies are sometimes affected by things in the engine room which are quite remote from the actual reactor which we are design-

ing. Radiation studies are made in various plants and are compared based upon irradiation to the crew in this ship. If we were making a general study and were not confining this to a particular type vessel, the radiation problem would change, and this changes the concept of what we might build considerably.

At the end of this study we now have established that a particular concept is desirable. I hesitate to use the word "feasible" because at this point what we have come up with is a knowledge that a certain type plant would have a certain weight per shaft horsepower. It will have certain operational characteristics, but there are usually a number of problems that require development. The development of these is then undertaken on a small scale.

Step 4 can be undertaken on a small scale to determine whether some of these features that we consider problems really exist. Finally after we have done this it is ready for incorporation into a project. If we have enough knowledge of the plant which we are designing, for example, if it was a water-cooled plant, we would not have to do too much development in this step 4. It could go directly into a project.

I do not wish to indicate that the development is anywhere nearly completed at this point. It is really only beginning. What we have established to the point of incorporation in a project is that an idea will probably be feasible when the development in the project is completed. We have merely sifted the various ideas which were presented to those which are probably capable of solution and should be incorporated in a reactor plant.

Representative PRICE. May the committee have a copy of those charts for its record?

Dr. ESSELMAN. Certainly.

In conclusion I wish to state that we are looking at various types of reactors. The advanced development group's thoughts are quite broad in scope. We are looking at improvement to water-cooled reactors as well as other types of reactors. I think the setup which we have whereby the various people in the study group are so closely related to the people in the projects results in a very practical approach. The fact that the people in the study group realize that the ideas which they are trying to sell will very soon be in a project forces the practical approach. Also the people in the group are the ones who have had experience with the design of core 1 or the mark 1 plants or with the operation of a plant so that the personnel we have will certainly take the practical approach in any design study.

Representative PRICE. Thank you very much, Dr. Esselman, I see it is close to 1 o'clock. I think the committee should recess and if there is any further presentation we can return here this afternoon.

(Whereupon at 1 p. m. the subcommittee recessed.)

AFTERNOON SESSION

The Subcommittee on Research and Development resumed its meeting at 4 p. m. in conference room No. 2, Westinghouse Bettis plant, Pittsburgh, Pa., Hon. Melvin Price (chairman of the subcommittee) presiding.

Present were Representatives Carl T. Durham (chairman of the full committee), Chet Holifield, and Melvin Price (presiding).

Staff members present: James T. Ramey, executive director and George E. Brown, Jr.

Representatives of the Atomic Energy Commission: Kenneth E. Fields, General Manager; W. Kenneth Davis, Director, Division of Reactor Development; Rear Adm. H. G. Rickover, Chief, Naval Reactors Branch, and Comdr. V. A. Lascara, Naval Reactors Branch; John M. Bodley, Special Assistant to Commissioners Libby and Vance; Bryan F. LaPlante, Congressional Liaison Officer; L. D. Geiger, Manager, Pittsburgh Area Office, and Comdr. R. V. Laney, Assistant to Pittsburgh Area Manager (Reactors).

Representative PRICE. The subcommittee will be in order.

I think the committee may want to discuss some of the things that we saw here this morning and get the reactions to the presentations that were made this morning. We would also like to know from the local people here, representing the AEC, what they find their problems to be—matters of supply—matter of funds.

I think the members of the committee all agree with me that this is probably one of the most important projects to our national defense of any that we have in the AEC program. I think they would also agree with me that the AEC and Navy both should give it the fullest possible importance and do everything they possibly can do to promote this project and to expedite the various programs.

If someone would like to make a presenation on behalf of the Atomic Energy Commission, this committee would certainly be happy to have your presentation.

STATEMENT OF KENNETH E. FIELDS, GENERAL MANAGER; W. KENNETH DAVIS, DIRECTOR, DIVISION OF REACTOR DEVELOPMENT, AND ADM. H. G. RICKOVER, CHIEF, NAVAL REACTORS BRANCH, ATOMIC ENERGY COMMISSION

Mr. FIELDS. I didn't have any presentation to make from our side on this endeavor. I would definitely second what you have said so very strongly that this is one of the most important installations we have. I have been impressed with it ever since my first association with it in the sense that it is a clear illustration of what is necessary, I believe, in a number of areas in our reactor program—not just here, but elsewhere too. You have got to go all the way through a reactor program to come out with effective reactors. You can't just build the end product. There is a great deal of research and development in material, physics, and metallurgy that must be done. From what I have been able to make out you can do everything with respect to reactors here that needs to be done with one exception. That is the chemical reprocessing of fuels. You don't attack that problem here. Otherwise it is a complete system, as far as I can see, in propulsion plant development.

Chairman DURHAM. Is this the only place that we have where we can build a reactor from the ground up?

Mr. FIELDS. Knolls can do this and I would think——

Mr. DAVIS. Argonne.

Mr. FIELDS. Argonne and Oak Ridge not quite to that extent, but they are building reactors or getting to it. They haven't built them as big as the PWR.

Chairman DURHAM. I am talking about something of the size of the PWR.

Mr. FIELDS. I think it is a very complete research and development installation in that it has the scientific resources and research sort of efforts that go on here and are necessary to develop a particular concept of reactor. Here the work you are doing in the pressurized water reactors will be used very widely in the naval field, but it will also be extremely useful in a civilian power field. You have the general key coming from this. I believe parts of the Consolidated Edison central station reactor very definitely flow from this.

As far as I know we have given this every help that it needs as well as encouragement. We are very strongly of the belief that there is a great future for this kind of a reactor both for naval use and for civilian power use. Although it is useful in naval propulsion it is the same information you have to have for civilian power reactors. This material you have out here would behave the same whether it is a military reactor or a civilian reactor. So there are great benefits to a civilian program that will flow from the military research that is being done. There has been a great deal of declassification that has opened up much of this information to the civilian program.

Representative PRICE. How extensively is this project declassified?

Mr. FIELDS. You had better answer that, Admiral Rickover. I am under the impression that practically everything on the PWR is now declassified.

Admiral RICKOVER. The PWR is completely declassified. We have actually gone much further than that. We have taken practically all the technical information from the naval programs and have declassified it also. We have set up a distinction between the technology as such, as opposed to specific design and dimensional characteristics. We have decided and the Commission has backed us that pure technology shall be declassified. We have made that unclassified information so that anybody in this country who wants to build a pressurized water reactor can get all of the technology. As far as giving our specific dimensions for a submarine reactor, we are keeping that classified.

Representative HOLIFIELD. What about specific cores for the PWR and all of these—your techniques for making these fuel rods?

Admiral RICKOVER. Let me put it this way. There is not a single thing in the PWR which is now classified. I don't know of a single aspect of the PWR, whether it is technology or whether it is design, or construction or dimensions that is any longer classified.

There are sometimes arguments as to whether new reactor ideas should be made public. You know the theory in the reactor game that everything that is born is always born with a restricted data stamp on it. We are pretty much disregarding that with the new PWR cores that we are developing. I am sure your committee would want it that way. We feel that for civilian reactors there is no reason why we should go back to the old system where scientists and engineers could not talk with each other. We think it is of much greater advantage to the United States to get this information out into the people's hands. I am talking now as a military man. I think it would hurt this country not to do it.

Chairman DURHAM. Does that include all metallurgy also?

Admiral RICKOVER. It is all-inclusive for the PWR, Mr. Durham. It includes the technology, the physics, the fabrication techniques—anything at all. There is nothing in the PWR we classify. It is just the same as if you were going to buy a toaster, a phonograph, or something like that.

Mr. FIELDS. I would like to footnote a couple of things that occurred to me while we were listening to these presentations and in going through here today.

At some point in the briefing there was a discussion of the second submarine core. I am not sure it was the second core actually. It was a subsequent core where we developed a longer life and the fact that the idea for this concept came in 1952. I believe that was the date the man gave. Here it is 5 years later. It was first thought of 5 years ago. Let me just put that one fact down. In the second one they brought out these different materials.

The reason I wanted to sort of footnote these is because these are the successes in development and achievements that let this go forward, but they are really only the first steps. There is a long way down the road to go yet even in this particular area. I believe we make the mistake too often when we have gone through and developed something like the PWR of thinking that is the end of the road when it is really only the first step for the pressurized water reactor. There are any number of years when this same group here should continue this development of new types of cores and modifications of the PWR itself to improve the power levels and the performance of that particular reactor. That is the way you are really going to go forward in this business.

Representative HOLIFIELD. I am glad you brought that up. First let me ask you when the PWR will be finished? That is in the fall, isn't it?

Mr. FIELDS. The last I heard is late in the year.

Admiral RICKOVER. We have hopes that it will go critical late this year. It may not be this year and it may not be until some time early next year. We don't know. We are trying to get a reading on it.

There are two principles we follow. One is that under no circumstances should we go ahead and make an announcement about something here, advertise it and not have it come through. Second, I don't think we should do anything—take any shortcuts which would jeopardize the successful operation. We have a large amount of instrumentation in this core which you don't need in an ordinary reactor. We are doing it for one reason, because we conceive it was the intent of the Atomic Energy Commission and the Congress for us to learn design lessons for future reactors from this core. That is one of the most important things. We don't want to take any shortcuts which will stop us from learning. The job is so much vaster than any of us originally thought. We all fell down on the time estimate. So it may take a litle longer than we originally thought.

I think you are finding this is true of all reactor programs. I think you have to realize that we are just today beginning to get a glimmer of what a vast and difficult thing a power reactor is to design and build.

Representative PRICE. How much of the physical part of the reactor is manufactured outside of your own installation?

Admiral RICKOVER. About the only thing we have made here is the core. That is our policy. Make the first core of its type in the laboratory because it is developmental. Practically everything else is made outside and nearly all on competitive bidding.

Representative PRICE. Have any cores been made outside? I am not talking about power reactors.

Admiral RICKOVER. May I answer that question in a different way? Let me rephrase your question if you will permit me to do so. What we have done is to establish an industry to make parts both for the naval reactor program and the PWR. Can I answer it that way because I think it will bring it out better?

When we strted out, of course, we had only one core for the Mark 1, but we realized if we were going to have a nuclear Navy, we would have to build up a nuclear industry. An industry for nuclear power consists of two parts. First you have to get certain raw materials. The basic raw material is uranium. That is supplied to us by the AEC so we dont' have that to worry about. The next thing is material such as zirconium. When we started out we made it at Government laboratories such as the Bureau of Mines Laboratory at Albany, Oreg., and by Government subsidization of Foote Mineral Co. and the Carborundum Co.

Then about a year and a half ago we went out and made contracts with three large companies for zirconium. This was all on a competitive basis, lump sum. One of the contracts was with the Carborundum Co. which was already in the business. Another company, National Distillers, is setting up a plant. The National Research Corp. is also building a zirconium plant.

We made contracts with these companies with funds supplied from Navy shipbuilding appropriations to get about 2 million pounds of zirconium a year for 5 years. At the end of that time it will be a completely open competitive market. These companies will put in enough additional capacity to take care of civilian reactor requirements also.

I would like to say that with the steps we have taken zirconium will be in open supply by a competitive industry.

Mr. FIELDS. Do you have any figures you could quote with respect to price before and after?

Admiral RICKOVER. I will give you some figures. When we started several years ago we were paying $300 a pound for zirconium sponge. That was the price when we first started. Next we made a contract with the Foote Mineral Co. What did that run to?

Commander LASCARA. It cost us about $250 a pound and we furnished the feed material.

Admiral RICKOVER. Mind you, that was the first contract.

Representative PRICE. With what company?

Admiral RICKOVER. The Foote Mineral Co. near Philadelphia. I think the total contract was about $2.5 million.

The next contract we made was with the Carborundum Co. for quantities which totaled up to about 30,000 pounds a month and we got that for about $13.20 a pound for sponge, roughly. So we have gone down from $300 to $250 to $13.20 a pound. The contract which we made this past year with Carborundum was for $7.72 a pound instead of $13.20 and the other contracts were even lower.

Mr. FIELDS. How long a term contract?

Admiral RICKOVER. These are 5-year contracts. These contracts are to the great advantage of the Government.

Representative PRICE. How much zirconium is being produced a year?

Admiral RICKOVER. We will get roughly out of all of these contracts about 2 million pounds a year, which we will need for the naval program and for PWR cores.

Representative PRICE. What is the potential demand for zirconium in the future?

Admiral RICKOVER. We figure with the expanded capacity that these people are putting in they will be able to take care of General Electric Co. and Babcock & Wilcox and others who are building commercial reactors. They are putting in additional capacity. This is a new industry, but it should follow the path of other industry. We feel the Government can get out of the zirconium business at the end of the 5-year period and buy it on a normal, competitive basis.

However, the General Accounting Office has just come up with the thought the Commission should not contract for raw materials in this manner.

Representative HOLIFIELD. I was going to ask if the other contract was a Navy or a commission contract.

Admiral RICKOVER. The Navy transferred funds to the AEC. The Commission handled it because all of the know-how for handling this material is in the Commission so we were faced with the proposition of setting up another Government agency——

Mr. FIELDS. There was another reason for procuring zirconium and beryllium in this way. We didn't see why we should have separate agencies trying to develop this industry.

Representative HOLIFIELD. I heartily agree with you.

Mr. FIELDS. We all agreed one agency should do it and I think it came out we felt we should do it and they agreed with us.

Admiral RICKOVER. Under no circumstances should any other Government agency get into this field as long as it is being handled properly and efficiently by the AEC. It so happens that the Commission has designated Mr. Geiger, the manager of the Pittsburgh area office, to handle all zirconium procurement. We deal through this one source and in this way we are able to get an advantage in price.

There is another major advantage. Most of the technical know-how on zirconium is located right in this Bettis plant. We have the people here who are qualified to instruct all of the new people how to do it and to do it efficiently and properly.

The Commission may need the help of Congress in connection with our present method of zirconium procurement.

Mr. FIELDS. What kind of an obligation do we have to make against that contract?

Mr. DAVIS. The terms of our contract?

Mr. FIELDS. In terms of contract you obligate money—do you obligate it so that if we cancel a contract, we give them some kind of cancellation determination?

Admiral RICKOVER. Commander Lascara, you are familiar with the details of that.

Commander LASCARA. The Navy has put up $7.5 million for cancellation charges for its share of the zirconium production over a 5-year period. I think the AEC has about $1.6 million in cancella-

tion charges for its share. During the 5-year period, if the contracts are canceled, say, at the end of the first year of the contract we are obligated to pay these companies a fixed amount for cancellation. I don't know what the exact dollar amount of the cancellation charge is.

Mr. GEIGER. The contracts are written so that if the Government were to cancel them before they start production or during the year or year and a half required to build the plant and get into production, we have certain obligations to pay up to a portion of their capital investment.

Chairman DURHAM. Up to 5 years?

Mr. GEIGER. No, sir; this is until the time they begin production. From the time they actually begin production, there are smaller cancellation charges. They are relatively small.

Mr. FIELDS. Going down to zero.

Mr. GEIGER. By the end of the 5-year period, of course, it is zero.

Representative PRICE. How many companies are in this business?

Commander LASCARA. There are three long-term contracts. We have a fourth company which has a short term contract.

Mr. GEIGER. Carborundum Co., National Distillers Corp., National Research Metals.

Admiral RICKOVER. National Research Corp.

Mr. GEIGER. NRC Metals is a wholly owned subsidiary of National Research Corp.

Mr. DAVIS. Only half.

Mr. GEIGER. Fifty percent of it was recently purchased by Columbia Southern, a subsidiary of Pittsburgh Plate Glass.

Representative PRICE. What is the fourth?

Admiral RICKOVER. Wah Chang. They are running the Bureau of Mines zirconium plant at Albany, Oreg. They have a lot to do with titanium. They have leased the Bureau of Mines—the Government-owned plant at Albany, Oreg. and are making zirconium for us now.

At any rate I would like to assure you that we have gotten a much better price, this way of doing it, for the Government than we could have done any other way. I hope the joint congressional committee backs this idea of doing it in this manner.

Chairman DURHAM. Is Wah Chang operating offshore of the continental United States?

Mr. GEIGER. No; Wah Chang is a domestic company.

Chairman DURHAM. He is all over the world.

Mr. GEIGER. It is an American company.

Chairman DURHAM. An American-Chinese company. He is the man who got a lot of tungsten out of China for us during the war.

Mr. GEIGER. He has brought in many minerals from many countries.

Chairman DURHAM. He is down in Brazil at the present time. Can you give us the contract price for each company?

Admiral RICKOVER. We can supply it for the record.

(The information referred to follows:)

National Distillers, $4.53 per pound; NRC Metals, $6.50 per pound; Carborundum Metals (Akron, Ohio), $11.42 per pound; Carborundum Metals (Parkersburg, W. Va.), $7.72 per pound; Wah Chang, $9.07 per pound (Government-owned facilities).

Chairman DURHAM. You mentioned a price of $13.

Mr. GEIGER. I can give you the price.

Chairman DURHAM. How long does that contract run at $13?

Mr. GEIGER. That original Carborundum contract runs until December 1958. Since they started production we have negotiated 3 annual price redeterminations bringing the price down to where, today, we are paying $11.42 a pound.

Chairman DURHAM. There is a renegotiation clause?

Mr. GEIGER. We renegotiate a new price once a year prospectively under that contract.

Representative PRICE. Do all of these contracts have renegotiation clauses?

Mr. GEIGER. The new contracts were all competitively bid contracts.

Representative PRICE. What was the lowest bid that you had?

Mr. GEIGER. The lowest bid is the one submitted by National Distillers of $4.53 a pound.

Representative PRICE. How many pounds do they supply?

Mr. GEIGER. A million pounds a year.

Mr. RAMEY. That is your largest contract.

Mr. GEIGER. That is the largest contract.

Chairman DURHAM. Are they producing?

Mr. GEIGER. No; they are not yet in production. They are building a plant at Ashtabula, Ohio. They were scheduled to be in production July of this year, but they are going to be late.

Chairman DURHAM. Where did they get their experience?

Mr. GEIGER. Several years ago National Distillers decided to diversify into the chemical field, and they set up a chemistry division. They are in the petrochemical field and they are also large producers of sodium metal. It was their interest in sodium metal which led them into the titanium and zirconium fields.

Chairman DURHAM. What was Wah Chang's bid? Do you remember?

Mr. GEIGER. Wah Chang is producing zirconium in a Government-owned plant. They are operating a Government-owned plant and selling zirconium to us for $9.13 a pound.

Representative PRICE. What makes the big difference in the prices? Was there no experience to begin with? Is it because the earlier process was not quite as efficient?

Mr. GEIGER. The biggest single factor is the rate of production. Today we are paying $9.13 a pound for material produced in a Government-owned plant where there is no amortization. We are paying $11.42 a pound in a plant that can produce at a rate of 300,000 pounds a year. The lower prices are based on production rates in excess of a million pounds a year.

Mr. RAMEY. Is there no difference in processing?

Mr. GEIGER. There are improved processes, but I think the production rate has a lot to do with the prices. If the production rate goes up, the price goes down.

Representative HOLIFIELD. I think we have got off the track a bit from our first line of questioning, although I am very much interested in this. I had asked a question as to when we would be through with the PWR. The reason I asked that question was because I wanted to see what kind of a breakdown your organization, Admiral Rickover, has in naval work that they are doing on the submarines and the work that they are doing on the PWR. Are you doing work on both of those projects?

Admiral RICKOVER. Yes.

Representative HOLIFIELD. You gave us some figures once before on the number of people you have in your organization. How much of that organization is devoted to the PWR and how much is devoted to the naval portion? Can you differentiate?

Admiral RICKOVER. I will try to give you some rough figures. Of course most of the developmental work in the PWR is finished. It has been for some time in the construction stage. Today the great majority of the work is in the followup of components and seeing that the installation gets done. I would say that about 20 to 25 percent of the effort of my organization is now devoted to the PWR. However, you must bear in mind that the technology in the PWR and the things that we do—what we learn on one reactor, we are learning for the others. That, of course, is the great advantage for having done the PWR here at Bettis and one of the reasons why the Commission assigned it here because it uses the same technology at the naval plant. For instance, a great deal we have learned on the PWR we are incorporating in the large ship reactor project. A great deal that we learned in naval reactors we have incorporated in the PWR. So even though we may spend 25 percent of our effort on it, on the other hand if we weren't doing it on the PWR, we would probably have to use about 12½ percent of the effort on naval cores anyway.

Representative HOLIFIELD. In the main you have put your best developmental people into the naval portion of the program?

Admiral RICKOVER. We use the same quality people on the naval program and on the PWR. We are starting a second core for the PWR. We have taken some of the developmental people, both here and in the Naval Reactors Branch, off the PWR and we are putting them on the other work because we no longer need as many on the new PWR core as we did on the original one.

Representative HOLIFIELD. As far as your organization is concerned, what will you be working in when you finish the PWR? You have a pretty good organization put together. Are you going to be able to absorb them in this big naval aircraft carrier and the submarine projects? Would you be able to take an additional project?

Admiral RICKOVER. You are saying, "When a man is already carrying a heavy load, could you add another straw before you break his back?" Sure, we could take on another project.

Representative HOLIFIELD. No, that isn't what I mean. If you have 25 percent of your people in the PWR, are you going to move them over into the naval program? Are you going to retain them or are they going to be dispersed throughout the country?

Admiral RICKOVER. No, they are not going to be dispersed. There is so much to be done that you can always find use for them. You were talking about another project. You asked me the last time I testified before your committee in Washington whether we could take another program. I said, "Yes," because I would just make out. I would train more people. You just keep on. That is all.

Representative HOLIFIELD. Do most of your finances now on the naval part of your program come from the AEC or does it come from the Navy?

Admiral RICKOVER. The finances are split two ways according to an agreement the Department of Defense made with the AEC. The research and development part is paid for by the AEC. The production part is paid for by the Navy. Any research and development on the steam part of the plant is paid for by the Navy, and construction of nuclear ships is paid for by the Navy. I think I can illustrate that better by giving you an example of how a big project works.

We will take the large ship reactor project. The AEC finances all of the research and development for the first plant. They also finance the construction of the prototype at Arco, and they finance the operation of the prototype. The Navy finances the entire cost of the ship plus the cost of the reactors in the ship. That is the general line of division between the AEC and the Navy. The AEC finances research and development plus prototype. The Navy pays for the ship and the cores that go into the ship.

Representative HOLIFIELD. Mr. Fields, how much are you using in the way of AEC funds to sustain this operation of Admiral Rickover's here?

Mr. FIELDS. I think you had better answer that, Rick.

Admiral RICKOVER. I will talk about fiscal year 1958. The AEC is putting up about $86 million for research and development.

Mr. FIELDS. $72 in fiscal 1957. That is operating funds.

Admiral RICKOVER. $86 million in fiscal year 1958. That does not include construction funds from the AEC.

Commander LASCARA. That is right.

Admiral RICKOVER. $25 million for construction funds—something like that. I don't know the exact amount.

Mr. RAMEY. How many people do you have?

Admiral RICKOVER. At Bettis we have about 5,300 people of whom about 1,300 are scientists and engineers.

Mr. FIELDS. This entire budget isn't spent here.

Admiral RICKOVER. It is spent here, at General Electric, Combustion Engineering, and many other places.

Chairman DURHAM. Does that $86 million mean completion of all the submarines that we have authorized so far?

Admiral RICKOVER. No, sir; that is not for actual construction of submarines. That is for research and development on naval nuclear propulsion—for the entire naval program.

Chairman DURHAM. I know that.

Admiral RICKOVER. You mean when we finish up with the $86 million?

Chairman DURHAM. Yes.

Admiral RICKOVER. No, sir. There will be continuous research and development in the coming years too.

Chairman DURHAM. In other words, you must expect research and development funds if you are not to continue building copies of the *Nautilus* type submarine reactor?

Admiral RICKOVER. We are not building any more *Nautilus* type.

Chairman DURHAM. I know. I am talking about whether you expect research and development funds from the AEC to continue the program on a research basis regardless of how many submarine reactors you build.

Admiral RICKOVER. Yes. If the Congress wants us to have improved reactors and wants us to learn, we have got to do this. This is the only way we know how to do it.

Representative PRICE. I want to get back to what Mr. Holifield started in his questions. Since the beginning of the Shippingport project which is when you are going through the peak years, what percentage of your work went into the project?

Admiral RICKOVER. You are talking about manpower?

Representative PRICE. Manpower and the efforts of your organization.

Admiral RICKOVER. At the peak it was at least 30 or 40 percent or perhaps more of this laboratory's efforts. I think it took about 300 scientists and engineers together with the necessary supporting personnel a total of about 5 years to do that job. That was a considerable effort. That same percentage applied in my headquarters organization in Washington. We actually gave the PWR the highest priority—even over the Navy program.

Representative PRICE. About 300 scientists and engineers during that period working exclusively on the PWR?

Admiral RICKOVER. That is right here at Bettis. This doesn't include people in the other organizations that contributed.

Representative PRICE. You are talking about the Bettis organization.

Admiral RICKOVER. That is about correct, isn't it?

Mr. GEIGER. I think that is right.

Representative HOLIFIELD. What are your general thoughts, Mr. Fields, for keeping this organization busy?

Mr. FIELDS. I don't think we have a problem in keeping them busy, Mr. Holifield. In a way this is the point I was trying to make. We have a tendency too often to think once one of these reactors go critical, that you have come to the end of the job, but you haven't at all. You have a tremendous payoff. Another 5 or 10 years in development at least—and probably more—in this field in extending the life of these machines, improving the cores, possibly modifications will come along in components, will simplify it, make it better.

Representative HOLIFIELD. How about starting from scratch and building on the basis of that knowledge—building another reactor? Is there a payoff there?

Mr. FIELDS. There could be, but——

Representative HOLIFIELD. In the meantime you keep on getting operating experience and you are improving.

Mr. FIELDS. You don't get operating experience by just operating a core longer than its life and replacing that same core in there. You need an improved type of core if you are going to progress down the line.

Admiral RICKOVER. Let me try to answer this question. I will give you an example.

Mr. FIELDS. O. K., but let me just say again that I think there is a tremendous amount of improvement possible in all of these things we have started. The PWR I would predict would be here for 15 years.

Admiral RICKOVER. Take the PWR. We will install the first core in it, and it will start operating toward the end of this year or the early part of next year. That core may last 3 or 4 years. In the

meantime we are designing another core. You might say, "Why don't we start and build another PWR reactor?" I wouldn't do that. I don't think there is enough gained.

Representative HOLIFIELD. That is what I am trying to get at.

Admiral RICKOVER. I wouldn't do that. I know this is contrary to what a lot of other people are saying. To go ahead and build a brand new pressurized water reactor and start from scratch will take 5 years at least, and the people who are going to design and build it are going to come around here and copy what they have here. So what have you got at the end of 5 years? You have got something that is already obsolete. As General Fields has said, we think it is much cheaper to design improved cores and use them in the present PWR plant.

Representative HOLIFIELD. Pull the old core out and put that in?

Admiral RICKOVER. When the first core is used up, install the second one; meanwhile start the design of a third new and improved type core. That would be the cheapest and best and quickest way of learning.

Mr. DAVIS. I think there is one more thing. It seems to me you have two problems in trying to develop something like the pressurized water reactor to the point where it is going to be pretty useful. One is to get the cost of the plant itself, which is very high in terms of dollars per kilowatt, down. The other is to get the cost of the fuel down. These are the two main components of the cost that are very high today.

You will do this in somewhat different ways. I think we have talked quite a bit about how we can cut the cost of a particular core and also extend its life. If you can do both of those rather successfully, you can cut the fuel costs into a fraction of what it is for the first core. You do this by putting in one core, then building an improved core and another improved core. You can put these all into the same reactor.

The cost of the reactor itself, I think, will come down largely after you have run the plant for a while and find out where it was that you were overly conservative when you designed it in the first place. I think you will discover, after you have run the PWR for a while and have perhaps put in 1 or 2 additional cores, that the water circulating system, the heat exchangers and the turbogenerators are, in fact, only perhaps a fraction as big as they should be to take the actual output of what you have out of the plant now that you know how to remove the bottlenecks. At that point somebody can come along and build a plant that, perhaps, wouldn't cost a great deal more than the PWR, but which will put out perhaps 2 or 3 times the output.

Admiral RICKOVER. It is too early now.

Mr. DAVIS. It is too early now and too early until you have run it and found out how to do these things. At that point you can build a plant which in capital cost—in terms of dollars per kilowatt—have become quite reasonable. At the same time you develop fuel cost where it is down to a point which is quite reasonable. At that point which, perhaps, is 3 or 4 years after you complete and operate your plant, you have a real sound basis for going ahead with a very much improved pressurized water reactor.

I think you have to take these things together and keep in mind that the things that bring down the cost in reactors are these two different

aspects. You have got to be able to do both of them. To do one of them alone is never going to make the grade.

Representative PRICE. How much is invested in the PWR right now?

Admiral RICKOVER. Investment in cost at the present time is $55 million for construction and about $40 million for research and development. This is exclusive of the cost of the steam plant, which is being borne by the Duquesne Light Co. I would say, roughly, the total expense of the PWR today is about $110 million.

Representative PRICE. Today.

Admiral RICKOVER. That is what we see will be the cost when it is finished about the end of the year. I am giving you overall cost.

Representative PRICE. By the time it is in operation?

Admiral RICKOVER. Yes sir. It looks today like roughly $110 million.

Representative PRICE. How much in excess of the original figure is that?

Admiral RICKOVER. I think Congress authorized originally $100 million for research and devolopment plus construction, which was $85 million for the reactor portion, so it is running over.

Mr. DAVIS. It was $85 million. You took out $15 million for the turbogenerator.

Admiral RICKOVER. About $100 million was the original estimate back in 1955. It has turned out to be about $110 million.

Representative PRICE. You have had these men working on this pressurized water reactor for about 5 years. I think they are probably among the best men in this field of reactors. Has any one of them ever expressed a desire to get into the business of building and testing gas-cooled reactors?

Admiral RICKOVER. Some of them have.

Representative HOLIFIELD. Do you need something in the way of a real challenge to keep these people now? Is there enough of a continuing challenge in improving the cores and other improvements on the PWR to keep your top people together or do they need something that would provide a real new challenge to them?

Admiral RICKOVER. I will put it this way. The people who are in this game, both in my organization in Washington and here, are not in it because of naval reactors alone. They are in this game in order to develop atomic energy in the United States. To them and to some extent this is true of me also, the naval program leads to this. So they would welcome a challenge of another type of reactor to work on. We have had discussions about this here at Bettis and in my organization and if the Commission decided to do this, we would be happy to take it on.

Certainly it would add more work for us, but we would make out. We would hire more people.

Representative HOLIFIELD. In other words, you could phase these people right into the program without damaging your large ship reactor.

Admiral RICKOVER. It always damages a little. We started out with one reactor, the mark 1, at Arco. Then as we undertook more reactor projects, we hired and trained more people. But all of the technology you learn on one type of reactor will add to another.

As I mentioned this morning, we are not starting off with novices. We are starting off with people who have, at least, a pretty good concept of what the design of a large scale reactor is. We would hope that we would have learned many lessons from what we have done in the past and not make all of the same mistakes.

Representative PRICE. Do any of them see any greater promise in a gas-cooled reactor than a pressurized water reactor?

Admiral RICKOVER. I think until you try out different types you can't say. I don't think you can hypothesize reactors. I think you have to try one of them out in order to find out.

Representative PRICE. Do they talk about the advantages of one over the other or what they think might be the advantage of one over the other?

Admiral RICKOVER. The major advantage of a gas-cooled reactor is that you can use natural uranium. If the policy of the country is to sell reactors abroad; if this is our national policy, you pretty much have to learn to design natural uranium reactors. Otherwise you force the countries to buy their fuel from this country if it is enriched uranium, or you force them to set up a uranium separation plant.

Representative HOLIFIELD. If you had the privilege of naming the type of reactor that you would like to go into, which one would you select?

Admiral RICKOVER. Gas cooled.

Representative HOLIFIELD. Gas rather than the homogeneous?

Admiral RICKOVER. Yes, sir.

Representative HOLIFIELD. Was the decision to go ahead with additional submarines in the pressurized-water field rather than in the sodium field based on the factor of sodium leakage?

Admiral RICKOVER. When we started out in the nuclear-submarine program we didn't know whether sodium was better than water, or vice versa. They both offered promise. As we went along—and before the *Sea Wolf* was finished—we recognized that water was better. By this time we had a water plant operating, so we then made the decision that for additional submarines we would go to water. We did that long before the *Sea Wolf* was operating, because we saw already that water was better than sodium for naval plants. However, we figured that we should go on and finish the job and try it out. We had to find out, and the only way you really find out is to operate the plant under service conditions.

The *Sea Wolf* is operating right now, and will keep on operating. If she develops another leak, we will have to take a look and see how expensive it is to repair. If it is too expensive, we may not continue operation. We are making plans now to replace the sodium reactor of the *Sea Wolf* with a pressurized-water type similar to the one that we have in the *Nautilus*.

Chairman DURHAM. Would there have to be any redesigning of the reactor to fit the *Sea Wolf?*

Admiral RICKOVER. We will use things we already have. There is plenty of room in the *Sea Wolf* to take a pressurized-water plant.

Representative PRICE. You are still running the *Sea Wolf* on shakedown?

Admiral RICKOVER. She is out operating right this minute. She will be at sea for about a month.

Mr. RAMEY. On the gas-cooled reactors, why can't we rely on the British for this information?

Admiral RICKOVER. It all depends on what philosophy you take on reactors. Normally, you can't learn the whole story from somebody else. You just cannot do it. You can get technical data, but if you really want to learn how to do something, you have got to do it yourself. Sure, you can get a lot of help from them, but you can't really learn unless you develop much of the technology in this country.

Chairman DURHAM. Why, then, are the British so anxious to get our information on these things?

Admiral RICKOVER. They are interested in water-cooled reactors, also. They want to get all the information they can so that, if it is necessary, they can try it out themselves. I did want to say a few more words about getting industry to do this job—which touched this off. I would like to talk for a couple of minutes on that.

Representative PRICE. Before you do that, could we get a list of the information on that project which has been disseminated to industry?

Admiral RICKOVER. Yes, sir; not only on the PWR, but the whole Navy program.

Representative PRICE. And also tell us how you get this out to industry?

Admiral RICKOVER. I will get something through the Commission on that to your committee.

(The information referred to follows:)

DECLASSIFICATION AND DISSEMINATION OF INFORMATION DEVELOPED IN THE NAVAL REACTORS AND PWR PROGRAMS

As of February 23, 1957, the following policy was established in the naval reactors and PWR programs:

(*a*) All information on the PWR plant at Shippingport is unclassified.

(*b*) All reactor technology developed on naval reactors is unclassified.

(*c*) All naval reactor design information is classified "confidential," "restricted data," or "defense information," as appropriate.

As a result of a document review conducted during March and April of this year at the Bettis plant, the Knolls Atomic Power Laboratory, and the Combustion Engineering Laboratory, over 5,000 classified naval reactor documents have been released for review and possible application to the AEC civilian application program. The information contained in these documents covers areas such as: reactor physics, reactor design, metallurgy, reactor plant components, and reactor control. To supplement this continuing document review, naval reactor laboratories will issue quarterly unclassified reports on the latest developments in reactor technology in these laboratories. These reports will be available to the public through the AEC's Technical Information Service at Oak Ridge, Tenn.

Representative HOLIFIELD. Is that released through the Commission?

Admiral RICKOVER. The Commission has set up a regular method of declassifying. What we are doing now is getting our laboratories to go over all of the information they have and put it in a form so it can be published.

Representative PRICE. Specifically, I meant the Shippingport project, but we would also like to have the information on other projects.

Admiral RICKOVER. I will give it to you for everything that we do.

Mr. FIELDS. May I say one thing at this point? On this question of adding another reactor project here, the Commission—I shouldn't speak for the Commission, but let me speak for myself at this point, which would be a recommendation, I suppose, to the Commission if we got to that point. I haven't seen anything the last year that I have

been around here to indicate that the people here are without work and don't have a lot to do and that there wouldn't be a serious problem if we were to confront this group with undertaking such a reactor as to just how much impact it would have on the naval program. I think there are two sides to this question, and I haven't seen any evidence that there isn't a lot to do here yet.

Chairman DURHAM. I would certainly have to be thoroughly convinced that it would in no way affect the Navy program before I would ever be for it. I think we face a serious challenge in submarines. After reviewing this program, there is no doubt in my mind that we are face to face with quite a vicious type of threat—not just a few weapons, but numbers and numbers of them.

Mr. FIELDS. Quite apart from the question of whether this country goes ahead with a gas-cooled reactor or not, there is the question of where it should go, and then another question is what you want to do.

Mr. DAVIS. I think it should be said that what we will get from the British on gas-cooled reactors is that we will find out what their economics are. We won't necessarily get the know-how with which to build one. I think it is important to recognize the difference between these two things, too.

Representative PRICE. What will we get, then, from the British?

Mr. DAVIS. We will certainly get enough information to evaluate the reactors with respect to what they can do. I don't think we will get enough so that we could, without doing quite a bit of development ourselves, actually build one similar to theirs or improve on it.

Representative PRICE. I thought there was some question about relying on British figures for the economics of their reactor.

Mr. DAVIS. I think what one has to do is to make his own figures, but base them on their costs. That is like trying to develop figures, for example, of what it would cost to build another PWR. You don't assume everything costs the same. You go through and make your own figures as to where it is going to be built and the conditions under which it is going to be built.

There is more to extrapolating the British reactor to United States figures than there is to extrapolating the PWR from here up to Rowe, Mass., for example. Nevertheless, I think it can be done and that there will be sufficient information to do this. However, if we gave somebody the job of manufacturing the fuel elements similar to the British fuel elements to put them into a gas-cooled reactor, there would be a lot of know-how which they would either have to go and get or they would have to learn by themselves. These, as I say, I think are related but still somewhat different considerations.

Chairman DURHAM. Do you have a proposal at the present time to build a gas-cooled reactor?

Mr. FIELDS. Aerojet.

Mr. DAVIS. We are going ahead with Aerojet. However, that is a very small, quite high temperature, closed-cycle, gas-turbine reactor.

Chairman DURHAM. Don't you have a regular power-producing reactor proposal before you of the gas-cooled type?

Mr. FIELDS. I believe Mr. Durham is referring to one under consideration by the Florida Power——

Mr. DAVIS. There is a Florida nuclear-power group.

Chairman DURHAM. At least you announced it several weeks ago.

Mr. DAVIS. We have had discussions with them. They have not

made a formal proposal to us yet. We have had a number of discussions. They are thinking about a heavy-water moderated reactor using natural uranium, either gas or heavy-water cooled. I think their proposal, when it finally comes in, will probably be, at least for the first year or so, to consider both gas cooled and heavy-water cooled and to make a selection later depending upon which looks the best. These would be quite different than the British reactors, which are graphite moderated. It would be more similar to what the Russians have said they are working on—gas-cooled, heavy-water moderated.

Representative HOLIFIELD. In computing the British costs, do you have any difficulty with respect to what allowances they are making for plutonium?

Mr. DAVIS. We have some ideas, but it depends so much on who has been doing the talking. I think in this case they haven't really decided yet.

Mr. FIELDS. We don't have any report that says, "This is the allowance we are making for it," if that is what you mean.

Mr. DAVIS. Actually with the Calder Hall reactor, you see, they are getting what they can for power, which is about 5 mills, which is what Central Electricity Authority will pay the Atomic Energy Authority for power. The plutonium will cost whatever it would have to cost to balance off the rest of the cost. With the Central Authority reactors, presumably they are going to do it the other way around. The Central Authority will sell the plutonium to the Atomic Energy Authority and they will try in their power cost to make up the difference. They are still trying to negotiate, as I understand it, to make things come out right but they haven't arrived at a final figure as far as I am able to find out. I don't know what the figure is.

Mr. FIELDS. I wonder if I could finish what I started to say.

Representative PRICE. Go ahead.

Mr. FIELDS. We started from the point that you have a rather complete research and development effort here in the field of water reactors. What concerns me, frankly, about the situation and the arguments that we have had between various groups on how to proceed with reactors is this: You look at this effort. It has been very expensive and it is going to be expensive for a good many years ahead if this is to be a strong effort here at this installation just in this area of water-cooled reactors. Similarly we have the effort at Argonne which is also in a water field, although this has been boiling-water reactors, and they are in breeding reactors and other aspects of the reactor field. We have a strong group at Oak Ridge in the homogeneous reactor field and in some other activities there. There are certain other groups like Los Alamos that are beginning to get into the plutonium type of reactor. These groups in themselves are going to be very expensive and have a yearly operating expense of anywhere from $20 million to $50 million per installation just to keep those going. They are going to be able to come up with fruitful developments for a long time to come. It is far more important, I think, to this country for those groups to stay strong and active and thoroughly engaged in their fields of endeavor which are in slightly different reactor-development areas, than it is to start building a certain number of new complete production plants or powerplants at any particular time.

Once in a while they will reach a certain stage where there should be some powerplants built. We are building powerplants—not necessarily big ones, but like the EBWR. However, I hope in the considerations ahead that we continue to support these main-line development efforts that are so important to the progress in the field. I think this concerns all of us who are sitting here as much as anything in the picture.

Admiral RICKOVER. I agree.

Mr. FIELDS. We admittedly should build some once in awhile.

Representative PRICE. If you don't build some once in awhile, you are not going to get any place. You are always going to have experimental projects.

Mr. FIELDS. It is a matter of judgment as to what kind you build. I agree there should be targets along the way. Maybe you set a target like the EBWR at Argonne at one stage and maybe next time it has to be as big as the Dresden reactor or something of that type.

Mr. DAVIS. Almost all reactor projects have reactor experiments.

Representative PRICE. You can't keep them in the experimental stage forever. You have got to advance.

Mr. DAVIS. What has happened here is that, for example, the boiling water reactor is being followed by the G. E. small boiling water reactor and about a year later will be followed by the full scale boiling-water reactor which is being built for Commonwealth. If this kind of development is picked up at the proper stage after reactor experiment, then I think you have got a very logical sequence of development of these reactor types. If you get one that is developed and looks like it is a really good idea and somebody ought to do it and nobody does it, eventually you have got a problem. I think this is the way, if it works, that it will probably work the best—at least to my mind.

Mr. RAMEY. However, Argonne is building your water technology—your boiling water—and they are doing the breeder type which are dissimilar types of reactors.

Mr. DAVIS. Very dissimilar.

Mr. RAMEY. North American has a sodium graphite program and is doing an organic moderated job, so you do have, shall we say, a precedent for other installations doing two different types of reactors. I think one question then is why couldn't Bettis do two different types of reactors? Why couldn't Bettis do the pressurized water and the gas-cooled? Granted that they would start out on a small one probably——

Mr. DAVIS. These other installations are not doing military projects at the same time.

Mr. RAMEY. Argonne started off on the naval reactor program as well as civilian reactors.

Mr. DAVIS. Oak Ridge is an installation where we have got one main civilian power reactor effort and one main military reactor effort; that is true.

Mr. FIELDS. It is possible certainly. It hasn't happened.

Representative PRICE. I think Admiral Rickover was about to make a statement. We yield the floor.

Admiral RICKOVER. I wanted to add a few words about what we are doing in getting industry into this picture. We are in the process of establishing heat-exchanger manufacturers, valve manufacturers,

pressure-vessel manufacturers—all on a competitive basis. Although they started with a naval project, these same organizations are capable of doing this work for civilian projects.

The last time I testified I mentioned that even for our naval program 86 percent of our money being spent for procurement, is being spent competitively and on competitive bids. This includes reactor cores, valves, instrumentation, and so forth. That is the thing I wanted to touch on for a couple of minutes. We now have Olin-Matheson, Combustion Engineering, Westinghouse Electric, Babcock & Wilcox, and Metals & Control Corp., at Attleboro, Mass., all engaged in building naval reactor cores. As these people get experience they will be available to make cores for anybody. We will soon have more than one company capable of bidding competitively for most of the items that go to make up a nuclear plant. We are also doing this with respect to zirconium on the same basis that the AEC handles zirconium procurement. The AEC is purchasing reactor cores for the Navy, but the Navy pays for them by transfer of funds.

Unless we do it this way we are going to be faced with the problem of duplicate facilities being set up by the Navy. Since cores are still in the developmental stage, I think that would be wrong. I think it ought to be under one Government agency so that we do not duplicate purchasing organizations, inspection groups, and so on. In this way it will be most economical for the Government.

I think it is very important that we not divert our efforts and have, for instance, the Air Force start getting people to make cores and the Navy start getting people to make cores. This early in the development stage, I think it is extremely important that one agency, the AEC, have charge of it.

Representative HOLIFIELD. I certainly agree with that.

Chairman DURHAM. I felt like that on reactors all the way through.

Admiral RICKOVER. There is one more think I must say which I have said many times before, but I would like to say it again. Had it not been for the Atomic Energy Commission and the Joint Congressional Committee we would not have any nuclear-powered naval vessel today. I think these two organizations and their way of operating deserve most of the credit.

Representative PRICE. Are there any further questions?

Chairman DURHAM. Do you have enough funds to carry out your work this year?

Admiral RICKOVER. No sir, no one ever does. You ought to know that, Mr. Durham, as many years as you have been in Congress. You ask a question like that. It is surprising.

I would like to answer your question though, Mr. Durham. The AEC has given me all the money that I have asked for in the naval program. The program is advancing rapidly, however. We can use more money.

Chairman DURHAM. Has the Navy given you any?

Admiral RICKOVER. The Navy hasn't given me as much as I have asked for. But Admiral Burke has stated that he would make more money available next year to carry out the program.

Representative PRICE. If there are no further questions, I want to close this meeting and to express our deep appreciation to Admiral Rickover and his organization, Mr. Geiger and his organization, and the Westinghouse people for this fine presentation. I don't see any

of the Westinghouse people here so I hope someone will convey our appreciation to them for this very kind reception to the committee. I think this has been most helpful and informative. I hope in the future our committee can get out to some of these installations and hold official meetings right on the scene while the problems are still fresh in our minds and cause us to raise questions, the answers to which will be helpful to the whole committee.

Chairman DURHAM. Mr. Chairman, I concur in your words. I was at Bettis with a former chairman in 1947, I believe—certainly in 1948—when we first broke ground. When you see such a plant as this today and the accomplishments which have developed since that time, you feel like your money has been spent for something worthwhile.

Mr. FIELDS. Mr. Chairman, may we say we appreciate your coming here and doing this. It is a boon to our morale and that of the people here to see such an interest. I am sure it will be useful to all of us. I hope you can do it other places too. It is most effective.

Representative PRICE. Our thanks to all of the people who testified here today. The committee will stand adjourned.

(Whereupon at 5:05 p. m. the meeting was adjourned.)

INDEX

PARTICIPATION ACT FOR THE INTERNATIONAL ATOMIC ENERGY AGENCY

HEARING

BEFORE THE

SUBCOMMITTEE ON AGREEMENTS FOR COOPERATION

OF THE

JOINT COMMITTEE ON ATOMIC ENERGY
CONGRESS OF THE UNITED STATES

EIGHTY-FIFTH CONGRESS

FIRST SESSION

ON

S. 2341

PARTICIPATION ACT FOR THE INTERNATIONAL ATOMIC ENERGY AGENCY

JULY 2, 1957

Printed for the use of the Joint Committee on Atomic Energy

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CONTENTS

PARTICIPATION ACT FOR THE INTERNATIONAL ATOMIC ENERGY AGENCY

TUESDAY, JULY 2, 1957

CONGRESS OF THE UNITED STATES,
SUBCOMMITTEE ON AGREEMENTS FOR COOPERATION
OF THE JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The subcommittee met, pursuant to notice, at 10 a. m., in room P–38, the Capitol, Hon. John O. Pastore, chairman of the subcommittee, presiding.

Present: Senators Pastore (chairman of the subcommittee), Hickenlooper, and Bricker.

Representatives Durham (chairman of the Joint Committee), Price, Cole, and Jenkins.

Senator PASTORE. This is a meeting of the Subcommittee on Agreements for Cooperation of the Joint Committee on Atomic Energy. Notices of this meeting have been sent to all members of the committee, even though they are not members of the subcommittee, so that they could attend if they so desire.

This meeting is to consider the Participation Act for the International Atomic Energy Agency.

This bill was introduced in the House by Mr. Price, H. R. 8251, and by Mr. Cole, H. R. 8254. I introduced the bill in the Senate, S. 2341.

All three of these bills are identical and are the legislation which has been suggested by the Department of State and forwarded by the Secretary of State to the Speaker of the House and the President of the Senate.

While the treaty itself was passed by the Senate on June 18, the treaty has not yet been signed. The legislation we are considering today is to establish the rules within the United States for participating in the Agency. This act is similar to the Participation Act we have for the Unted Nations and many other international organizations.

The first section would give the title of the act as the "International Atomic Energy Agency Participation Act of 1957."

The second section permits the President to appoint a representative and a deputy representative to the Board of Governors, the General Conference, and to other organs of the Agency.

It also permits the President to appoint delegates to the General Conference. The President can also appoint representatives to other organs of the Agency.

The bill provides for the rates of pay for those designated to represent the United States at the Agency.

The Participation Act would require the participation of the United States to be in conformity with both the statute of the Agency and the Atomic Energy Act. The President is to report to the Congress on the Agency's activities at least once a year.

Section 4 requires the representatives of the United States to vote at the Agency in accordance with the instructions from the President.

Section 5 provides the authorization for the appropriations to carry into effect the actual participation of the United States through its representatives.

I now ask that S. 2341 be inserted in full in the record at this point.

(The bill, S. 2341, is as follows:)

[S. 2341, 85th Cong., 1st sess.]

A BILL To provide for the appointment of representatives of the United States in the organs of the International Atomic Energy Agency, and to make other provisions with respect to the participation of the United States in that Agency, and for other purposes

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That this Act may be cited as the "International Atomic Energy Participation Act of 1957."

SEC. 2. (a) The President, by and with the advice and consent of the Senate, shall appoint a representative and a deputy representative of the United States to the International Atomic Energy Agency (hereinafter referred to as the "Agency"), who shall hold office at the pleasure of the President. Such representative and deputy representative shall represent the United States on the Board of Governors of the Agency, may represent the United States at the General Conference, and may serve ex officio as United States representative on any organ of that Agency, and shall perform such other functions in connection with the participation of the United States in the Agency as the President may from time to time direct.

(b) The President, by and with the advice and consent of the Senate, may appoint or designate from time to time to attend a specified session or specified sessions of the General Conference of the Agency a representative of the United States and such number of alternates as he may determine consistent with the rules of procedure of the General Conference.

(c) The President may also appoint or designate from time to time such other persons as he may deem necessary to represent the United States in the organs of the Agency. The President may designate any officer of the United States Government, whose appointment is subject to confirmation by the Senate, to act, without additional compensation, for temporary periods as the representative of the United States on the Board of Governors of the Agency in the absence or disability of the representative and deputy representative appointed under section 2 (a) or in lieu of such representatives in connection with a specified subject matter.

(d) All persons appointed or designated in pursuance of authority contained in this section shall receive compensation at rates determined by the President upon the basis of duties to be performed but not in excess of rates authorized by sections 411 and 412 of the Foreign Service Act of 1946, as amended (22 U. S. C. 866, 867), for Chiefs of Mission and Foreign Service officers occupying positions of equivalent importance, except that no Member of the Senate or House of Representatives or officer of the United States who is designated under subsection (b) or subsection (c) of this section as a delegate or representative of the United States or as an alternate to attend any specified session or specified sessions of the General Conference shall be entitled to receive such compensation. Any person who receives compensation pursuant to the provisions of this subsection may be granted allowances and benefits not to exceed those received by Chiefs of Mission and Foreign Service officers occupying positions of equivalent importance.

SEC. 3. The participation of the United States in the International Atomic Energy Agency shall be consistent with and in furtherance of the purposes of the Agency set forth in its Statute and the policy concerning the development, use, and control of atomic energy set forth in the Atomic Energy Act of 1954, as amended. The President shall, from time to time as occasion may require, but not less than once each year, make reports to the Congress on the activities

of the International Atomic Energy Agency and on the participation of the United States therein.

SEC. 4. The representatives provided for in Section 2 hereof, when representing the United States in the organs of the International Atomic Energy Agency, shall, at all times, act in accordance with the instructions of the President, and such representatives shall, in accordance with such instructions, cast any and all votes under the Statute of the International Atomic Energy Agency.

SEC. 5. There is hereby authorized to be appropriated annually to the Department of State, out of any money in the Treasury not otherwise appropriated, such sums as may be necessary for the payment by the United States of its share of the expenses of the International Atomic Energy Agency as apportioned by the Agency in accordance with paragraph (D) of article XIV of the Statute of the Agency, and for all necessary salaries and expenses of the representatives provided for in section 2 hereof and of their appropriate staffs, including personal services without regard to the civil service laws and the Classification Act of 1949, as amended; travel expenses without regard to the Standardized Government Travel Regulations, as amended, the Travel Expense Act of 1949, as amended, and section 10 of the Act of March 3, 1933, as amended; salaries, expenses, and allowances of personnel and dependents as authorized by the Foreign Service Act of 1946, as amended; services as authorized by section 15 of the Act of August 2, 1946 (5 U. S. C. 55a); translating and other services, by contract; hire of passenger motor vehicles and other local transportation; printing and binding without regard to section II of the Act of March 1, 1919 (44 U. S. C. 111); official functions and courtesies; such sums as may be necessary to defray the expenses of United States participation in the Preparatory Commission for the International Atomic Energy Agency, established pursuant to annex I of the Statute of the International Atomic Energy Agency; and such other expenses as may be authorized by the Secretary of State.

Senator PASTORE. On June 19, because of the interest of the Senators in the problems discussed on the floor of the Senate during the ratification, I announced that public hearings would be held on the Participation Act to receive statements from interested Senators. I reextended that invitation on June 26 when I announced on the floor of the Senate that this hearing date had to be postponed until today, July 2. No Senators have requested permission thus far to appear or to file a statement. Therefore, I will turn to the witnesses from the executive branch.

Today we have representatives from the Department of State and the Atomic Energy Commission ready to appear before us.

I would like to suggest that first we hear from Mr. Francis Wilcox, Assistant Secretary of State for International Organization Affairs, who will deal with the technicalities of the Participation Act and with the general Department of State position.

Second, we will hear from Admiral Strauss of the Atomic Energy Commission, with respect to the Commission's views on this statute.

Third, I would like to round out this hearing with a statement from Ambassador Wadsworth as to the intent and scope of the statute in order to get the record complete for any questions which might be raised about the Agency in both Houses.

I believe that this is especially important, since this will be the first record of the Agency which will be presented to the House of Representatives for its consideration.

I have been told by the Department of State that the presentation by the three witnesses will take about a half hour. They have requested that there be a minimum of questioning until the complete presentation is over, at which time the three witnesses will be available for any questions the committee members might desire to ask.

Mr. Wilcox, will you please present your statement.

STATEMENT OF FRANCIS WILCOX, ASSISTANT SECRETARY OF STATE, ACCOMPANIED BY GERARD SMITH, SPECIAL ASSISTANT TO THE SECRETARY OF STATE FOR ATOMIC ENERGY

Mr. WILCOX. Mr. Chairman, your very succinct summary of the participation act has made my task much easier.

I appreciate this opportunity to appear before the committee to assist in presenting the views of the executive branch with respect to our participation in the International Atomic Energy Agency. As the committee knows, the President and the executive branch attach great importance to this Agency.

The passage of a participation act will give substance to our membership in the International Atomic Energy Agency. The President's original proposal for the establishment of such an agency, the subsequent 3½ years of negotiations, and United States membership in the Agency would be meaningless without the passage of this law to provide for United States representation in its organs and to authorize contributions to its expenses.

President Eisenhower's concept of an international organization designed to bring the most newly discovered force of nature, atomic energy, within the reach of every country, was greeted with great enthusiasm throughout the world. By its active participation, the United States will continue in its role of leadership in applying atomic energy to peaceful purposes.

The statute of the International Atomic Energy Agency sets out two major objectives for the Agency.

First:

> The agency shall seek to accelerate and enlarge the contribution of atomic energy to peace, health, and prosperity throughout the world.

And, second:

> It shall ensure, so far as it is able, that assistance provided by it or at its request under its supervision or control is not used in such a way as to further any military purpose.

More specifically, it is to assist international research on and practical applications of atomic energy; to make provision for materials, services, equipment, and facilities to meet the needs of such research and practical application; to foster the exchange of scientific and technical information and to encourage the exchange and training of scientists and experts in the atomic energy field; and to establish international standards of safety for the protection of health, and to minimize danger to life and property involved in peaceful uses of atomic energy.

In carrying out all these functions, the agency is to establish and administer safeguards designed to ensure that any assistance it renders to member countries is not used in such a way as to further any military purpose.

On June 18, the Senate, by a vote of 67 to 19, gave its advice and consent to United States ratification of the IAEA statute. This action was taken only after thorough examination of all aspects of the proposed Agency and the advantages of United States membership.

ADVANTAGES TO THE UNITED STATES

In his opening statement before the Senate Foreign Relations Committee in support of the ratification of the IAEA statute, Secretary Dulles emphasized these advantages:

First, the Agency will accelerate the peaceful development of the atom;

Second, the Agency will provide an effective system of safeguards to ensure the development of atomic energy with adequate security. The spread of nuclear technology and facilities throughout the world is inevitable.

If this expansion is unsupervised, the threat of nuclear weapons will increase proportionately. The International Atomic Energy Agency will be in a position to provide an international safeguard system, established and operated by all member states, which can guard against diversion to military purposes of materials and services made available to member countries under its auspices.

Third, the Agency will be the appropriate body to develop and promulgate international codes to protect the health and safety of the populations which are otherwise benefiting from atomic energy.

Fourth, by providing a channel for cooperation and coordination, the Agency can assure the most efficient use of the limited supply of skilled manpower in this highly technical field.

Fifth, the Agency may have a constructive impact on the solution of one of the most difficult problems in the disarmament field, the development of a satisfactory system for international inspection and control.

It can do this by providing concrete evidence that such a system can be agreed to and can be successful in operation. If progress of this kind were made, it would constitute a breakthrough of considerable significance.

Sixth, the openness which this system will promote may help to prevent the spread of nuclear weapons.

The availability of nuclear weapons to countries not now possessing them could seriously impede arms control. With the development of the peaceful atom taking place under international security provided by the Agency, nations will feel less impelled to develop nuclear weapons out of fear that their neighbors may be doing so.

Finally, the unusual degree of cooperation which has taken place thus far between West and East in this new and important field may show the way to cooperation and understanding in other areas of difference.

PROVISIONS OF PARTICIPATION ACT

It is within this framework that we now consider legislation to enable the United States to participate effectively as a member of the agency. This legislation follows the practice established in the United Nations and the other major international organizations in which we participate. In those cases, the administration requested and the Congress enacted similar legislation authorizing United States participation.

I would now like to comment briefly on the five substantive sections of the participation act that is before the committee.

94441—57——2

Section 2 (a) provides for the appointment by the President of a United States representative and deputy to the IAEA. The representative and his deputy are to represent the United States on the Board of Governors and may also serve on the United States delegation to the General Conference and also any other organ of the Agency.

Paragraph (b) provides that the President may in addition appoint a representative and alternates to attend specified sessions of the General Conference. This is much the same practice that is followed in making up the United States delegations to the United Nations and other international organizations.

As the chairman knows from his participation in the delegation for a specific session of the General Assembly, for example, representatives are appointed to serve in addition to Ambassador Lodge and his deputy, Ambassador Wadsworth.

Paragraph (c) provides for additional appointments as necessary.

Paragraph (d) of section 2 provides for the compensation of persons designated under this section.

In the initial period of the Agency's existence its program will of necessity be relatively small and the major activity will be in planning rather than implementation.

Consequently, in our judgment, the staff of the United States representative need not be large. Indeed, it ought to be quite small.

Our present thinking is that, in addition to the representative and his deputy, 1 political and 1 scientific adviser should provide adequate support for this initial period. There would also be an executive officer, and the necessary clerical assistance.

Section 3 provides that United States participation in the Agency as set forth in the statute shall be consistent with and in furtherance of the purposes of the Agency and the Atomic Energy Act of 1954. This section provides that the President is to report to Congress as occasion may require and at least once a year on the activities of the Agency and on United States participation therein. This we believe desirable in view of the deep interest of the Congress in the development of the peaceful uses of atomic energy.

The Department of State and the AEC will work closely together to prepare these reports in order to present to Congress information which is as complete and current as possible.

Of course, it is the intention and desire of the Department of State and the Atomic Energy Commission to go beyond this requirement and to keep Congress, and this committee, especially, informed on a more informal basis of major developments in the Agency. Appropriate officers of both agencies will be available on request to furnish information and answer questions.

Section 4 simply provides that the United States representatives will act and vote in accordance with instructions of the President. This language will permit the same close cooperation between the Department of State and the Atomic Energy Commission in backstopping the United States representative at the Agency that has characterized our negotiation of the statute and the subsequent work in the Preparatory Commission.

Section 5 authorizes the appropriation of funds annually to the Department of State for the payment of the United States share

in the expenses of the Agency, for the support of the permanent mission, for expenses in connection with conferences and meetings, and to defray the expenses of United States participation in the Preparatory Commission. Our present estimate indicates that these costs for the first year would be approximately as follows:

United States share, not to exceed one-third, of combined expenses of Preparatory Commission and IAEA in the first year, $2,300,000;

Support of permanent United States mission when at full strength during initial period, $200,000;

Expenses of United States delegations to conferences, $100,000.

Total, $2,600,000.

The first year's budget for the Agency has been estimated by the Preparatory Commission to be about $7 million, including 2 nonrecurring items, $500,000 to repay the United Nations loan on which the Preparatory Commission is now operating, and a $2 million working capital fund to cover contingencies.

In my opinion, the cost of the United States participation in the Agency is indeed modest when compared to the potential advantages to the United States.

I should say also it is quite modest when compared to the other international organizations in which the United States now participates.

Mr. Chairman, the International Atomic Energy Agency is about to become a reality. It is expected that in the near future the necessary number of ratifications to bring the Agency into existence will be deposited.

Since the conference to draft the Agency's statute concluded its deliberations last October, the Preparatory Commission, established by the conference, has been making rapid progress in arranging for the first meetings of the General Conference and the Board of Governors.

It has set October 1 of this year as the date for the meeting of the first General Conference, and the Board of Governors will meet soon thereafter. Both meetings will take place in Vienna, the city which is to be the headquarters of the Agency.

Mr. Chairman, this concludes my statement. Again, I want to thank the committee for giving me this opportunity to present the administration's views on this act.

Admiral Strauss and Ambassador Wadsworth, both of whom have devoted a great deal of attention to the establishment of this Agency, and both of whom know more about the statute certainly than I do, will doubtless wish to provide further information of interest to the committee. After that I shall of course, be glad to answer questions.

Mr. Gerard Smith of the State Department, who has had a great deal to do with the negotiating of statute, is also here, together with other officials from the State Department, Atomic Energy Agency, and I hope we will be able to answer satisfactorily any questions which the committee might have.

Senator PASTORE. Thank you, Mr. Wilcox.

Now we will hear from the Chairman of the Atomic Energy Commission, Mr. Strauss.

STATEMENT OF HON. LEWIS L. STRAUSS, CHAIRMAN, ACCOMPANIED BY A. A. WELLS, OFFICE OF GENERAL COUNSEL, ATOMIC ENERGY COMMISSION

Mr. STRAUSS. Mr. Chairman, my purpose in making this very short statement is simply to be on record as endorsing the participation.

I appreciate the opportunity to make a brief statement in support of the proposed International Atomic Energy Agency Participation Act.

My connection with the inception of the idea which has resulted in the atoms-for-peace program and with the Agency is, I believe, well known to you. I am convinced that the institution will be a substantial contribution to the peace of the world, and will be a necessity for world health and safety.

It is my understanding that legislation similar to the bill which is under consideration, is the customary means of providing for the appointment of United States representatives to international bodies and of making the necessary administrative arrangements to support United States participation in such organizations.

The provisions of the bill seem to me to provide an adequate basis for United States participation in the Agency and for furthering our national objectives designed to promote the peaceful uses of atomic energy as widely and as soon as practicable, with due regard to our own needs and to prudent consideration of security.

Section 3 of the bill provides that participation by the United States in the agency shall be consistent with the policy concerning the development, use, and control of atomic energy as set forth in the Atomic Energy Act of 1954, as amended.

This section recognizes the fundamental principles enunciated by the Congress to govern the activity of the United States agencies both at home and abroad in the peaceful uses of atomic energy.

It insures, therefore, that the disposition and use abroad of special nuclear material and atomic technology of the United States will be in keeping with the requirements of the Atomic Energy Act.

The Senate has now given its consent to United States ratification of the statute of the agency.

Prior to that action by the Senate, which I delivered before the Foreign Relations Committee and in which I outlined my understanding of what the agency was intended to accomplish, I also said that if the President had not proposed the agency initially in his speech to the United Nations on December 8, 1953, there would be a profound need now to invent such a body.

My reason for this statement is the pressing requirement for uniform, meaningful health and safety standards to govern the use, transportation, storage, and disposal of radioactive materials the world over.

This is one of the immediate functions of the agency. The prerequisite constitutional basis for United States participation in such an agency having now been accomplished, the passage of this participation act would make it possible to concentrate on the next task of establishing an effective organization and of initiating a sound program in which a system of prudent international safeguards will have first priority.

The agency's program, besides promoting the beneficial uses of the atom throughout the world, will provide the basis for necessary international agreements on security controls and on the standards of health and safety to which I have referred.

The particular importance of these is emphasized by the growing use of the peaceful atom in research, in industry, agriculture, and medicine and, of course, power.

Unregulated by a common standard, these activities can conceivably create widespread hazards and risks for populations well beyond the boundaries of the countries in which the materials are used.

I know of no way except through international cooperation to promote the use of the atom under standards of control and safety which will protect everyone, ourselves included, from these hazards.

It is for this reason that I am happy that we have reached this stage in atomic energy development and that you are today considering this participation act whose passage I must sincerely advocate.

Thank you, Mr. Chairman.

Senator PASTORE. Thank you, Mr. Strauss.

Now, Mr. Wadsworth.

STATEMENT OF HON. JAMES J. WADSWORTH, DEPUTY REPRESENTATIVE OF THE UNITED STATES TO THE UNITED NATIONS

Mr. WADSWORTH. Thank you very much, Mr. Chairman, also for the opportunity of appearing before you again.

I recall your most valuable services to the United States delegation to the conference on the statute last fall in New York.

I also recall with great pleasure your consistent and helpful interest in this matter from the very outset.

Mr. Chairman, it has been suggested that in my statement this morning I give my impressions of the negotiation of the statute, cite some of its salient features and discuss briefly the prospects for future development of the agency.

Since our primary purpose today is to concentrate on the participation act, my comments will be geared to foreshadowing the problems which the future United States representatives to the agency are likely to face once it is established.

To begin with, Mr. Chairman, I should like to reiterate my belief expressed in earlier hearings, that the negotiation of the statute was a remarkable and unusual experience. Here is a document signed by 80 nations after 3½ years of painstaking negotiations which nonetheless reflects all the basic elements of the United States proposal included in President Eisenhower's speech at the U. N. in December 1953.

The negotiation was certainly not one broad, smooth road to success and understanding free of detours or pitfalls.

But the power of an aroused public opinion, inspired by the President's noble concept of this agency, was so great that it was reflected time and again at the conference table in a determination to overcome all obstacles.

You will remember that for more than a year, in fact, almost 2 years, the Russians totally rejected our proposals, but when in late 1955 it became evident that others would join with the United States in pur-

suing the goal of atoms for peace, with or without the Soviets, they finally participated in the negotiation group.

At several points difficult and new problems arose, such as how are we to reconcile sovereign rights of nations with the needs for adequate safeguards against the diversion of atomic recourses to military uses, or how to distribute voting participation and responsibility in the key bodies of the agency.

These troublesome matters would never have been resolved were it not for the businesslike persistent determination which characterized the approach of all the key delegations during these negotiations.

If, as I hope, this spirit is maintained the establishment of this agency will become a symbol and a precedent for earnest worldwide cooperation in the quest for peace.

In turning to the statute, Mr. Chairman, it is not my intention to comment on all of its provisions. Unless members of the committee have questions on certain other articles, I shall confine my brief comments to articles III, V, IX, XI, XII, and XIV, which I think cast some light on the task which will face our representatives to the agency and thus afford us a basis for determining how to set up our arrangements for participating in the agency's activities.

Article III sets forth the functions of the Agency. These are broad in scope, but the activities in the early years are likely to be modest.

Thus, the United States representatives provided for in this participation act must sooner or later be equipped to form well-considered recommendations on a wide range of technical and political questions.

However, as Assistant Secretary Wilcox has said, the size and character of the staff they need and the nature of the backstopping and reporting arrangements can only be worked out as the Agency's plans and activities are further developed, and it is for this reason that present plans call for a very small group.

Article V deals with the role of the General Conference. The point of key significance for us today in considering the General Conference is that the United States was successful in placing in the Board of Governors the principal responsibility for policymaking for the Agency. This means that our representative on the board will have a key role in formulating policy recommendations and specific proposals relating to the Agency's program.

The General Conference, however, has important responsibilities. Among others, it reviews and approves the budget; it confirms the appointment of the director general of the Agency, and it elects 10 members of the Board of Governors.

Now, if the United States representative to the General Conference is to achieve our objectives in political negotiations of this character without substantial difficulty, we know from long experience that extensive and careful preparatory negotiations will be necessary on the part of our representative on the Board of Governors.

Article IX relates to the supply of materials. It should be noted that no nation is compelled to make materials available to the Agency. The decision to sell such materials and the terms on which they will be offered to the agency is up to the supplying government.

Working out recommendations on these terms, methods of delivery and similar problems will require the United States representatives to cope with many technical questions.

Before leaving this article, I would like to cite a key provision which is basic to the concept of the Agency. This provision is paragraph (j) of article IX, which reads as follows:

J. The materials made available pursuant to this article shall be used as determined by the Board of Governors in accordance with the provisions of this statute. No member shall have the right to require that the materials it makes available to the Agency be kept separately by the Agency, or to designate the specific project in which they must be used.

I mention this point, because it highlights the importance of the leadership which we expect to exert in the Board of Governors in examining and approving Agency projects which might call for materials being sent by the United States to some other country.

Article XI tells how the Agency's projects will be developed.

Application for Agency assistance to a project can be made by a member or groups of members. The Agency can assist members in obtaining financing, but cannot be required to undertake any guaranties or assume any financial responsibility itself.

The criteria which the Board must consider in deciding upon projects are set forth in paragraph (e). You can see from reading this paragraph that the Agency is going to give careful and exhaustive consideration to Agency projects before approving them and that the work of the United States representative on the Board of this Agency is going to be quite voluminous.

Article XII deals with safeguards against diversion of Agency assistance to military uses. This key article was carefully drawn to insure that the Agency would have the authority to do its safeguards job.

As Secretary Dulles has pointed out, and Assistant Secretary Wilcox just this morning, article XII may do more than make possible the acceleration of peaceful atomic development throughout the world. It may make, and I believe it will make, a positive contribution to solving the problem of nuclear disarmament.

In this article the Agency is given authority to send inspectors into the Territory of recipient governments, these inspectors—

who shall have access to all places and data and to any person who deals with materials, equipment, and facilities which are required by this statute to be safeguarded.

It can thus be seen that the United States Board member will be faced with many new technical and political problems as the Agency's safeguards policies evolve.

Article XIV is the financial one. It was drafted in a spirit of care and conservatism to avert the danger that the "have not" nations could ever launch the Agency on extravagant and unsound programs.

The Board's task of establishing appropriate charges for Agency materials, services, equipment, and facilities will require careful and resourceful thought.

Mr. Chairman, I have sought in these brief comments to point up the fact that the scope of the questions with which the Agency must deal will be broad. They will require that our representatives find solutions for novel political and technical problems of some complexity.

I do not believe that the United States delegation need be large in the early years. However, from my experience with the negotiations of the statute and I worked with the Preparatory Commission with

this Agency, I well know that the effective United States representation in this field requires active participation of staff drawn both from the Department and from the Atomic Energy Commission, and that the arrangements must be such that help will be at hand when the United States representative needs it.

The provision of this participation act will make possible the continuance of the cooperative arrangements which have well served the needs of the United States delegations to the negotiating group, to the conference on the statute, and to the Preparatory Commission.

I have no hesitation in supporting this proposed legislation which is to govern future participation in this great new venture.

Thank you, Mr. Chairman.

Senator PASTORE. Thank you, sir.

Mr. Smith is here from the State Department; do you desire to make a formal statement, too?

Mr. SMITH. No, thank you, Senator. I will be happy to try to answer any questions.

Senator PASTORE. Now, for the purpose of the record, I think at this point we ought to insert the statute.

If there is no objection, that will be done.

Secondly, I think we ought to insert here an analysis of the statute which was prepared by our staff, which is not too long; and

Thirdly, I think we ought to insert here in the record the letter addressed by Secretary of State John Foster Dulles to Senator Fulbright, with reference to an agreement of understanding.

(The material referred to follows:)

STATUTE OF THE INTERNATIONAL ATOMIC ENERGY AGENCY

Article I. Establishment of the Agency

The Parties hereto establish an International Atomic Energy Agency (hereinafter referred to as "the Agency") upon the terms and conditions hereinafter set forth.

Article II. Objectives

The Agency shall seek to accelerate and enlarge the contribution of atomic energy to peace, health, and prosperity throughout the world. It shall ensure, so far as it is able, that assistance provided by it or at its request or under its supervision or control is not used in such a way as to further any military purpose.

Article III. Functions

A. The Agency is authorized:

1. To encourage and assist research on, and development and practical application of, atomic energy for peaceful uses throughout the world; and, if requested to do so, to act as an intermediary for the purposes of securing the performance of services or the supplying of materials, equipment, or facilities by one member of the Agency for another; and to perform any operation or service useful in research on, or development or practical application of, atomic energy for peaceful purposes;

2. To make provision, in accordance with this Statute, for materials, services, equipment, and facilities to meet the needs of research on, and development and practical application of, atomic energy for peaceful purposes, including the production of electric power, with due consideration for the needs of the underdeveloped areas of the world;

3. To foster the exchange of scientific and technical information on peaceful use of atomic energy;

4. To encourage the exchange and training of scientists and experts in the field of peaceful uses of atomic energy;

5. To establish and administer safeguards designed to ensure that special fissionable and other materials, services, equipment, facilities, and information

made available by the Agency or at its request or under its supervision or control are not used in such a way as to further any military purpose; and to apply safeguards, at the request of the parties, to any bilateral or multilateral arrangement, or, at the request of a State, to any of that State's activities in the field of atomic energy;

6. To establish or adopt, in consultation and, where appropriate, in collaboration with the competent organs of the United Nations and with the specialized agencies concerned, standards of safety for protection of health and minimization of danger to life and property (including such standards for labour conditions), and to provide for the application of these standards to its own operations as well as to the operations making use of materials, services, equipment, facilities, and information made available by the Agency or at its request or under its control or supervision; and to provide for the application of these standards, at the request of the parties, to operations under any bilateral or multilateral arrangement, or, at the request of a State, to any of that State's activities in the field of atomic energy;

7. To acquire or establish any facilities, plant and equipment useful in carrying out its authorized functions, whenever the facilities, plant, and equipment otherwise available to it in the area concerned are inadequate or available only on terms it deems unsatisfactory.

B. In carrying out its functions, the Agency shall:

1. Conduct its activities in accordance with the purposes and principles of the United Nations to promote peace and international cooperation, and in conformity with policies of the United Nations furthering the establishment of safeguarded worldwide disarmament and in conformity with any international agreements entered into pursuant to such policies;

2. Establish control over the use of special fissionable materials received by the Agency, in order to ensure that these materials are used only for peaceful purposes;

3. Allocate its resources in such a manner as to secure efficient utilization and the greatest possible general benefit in all areas of the world, bearing in mind the special needs of the underdeveloped areas of the world;

4. Submit reports on its activities annually to the General Assembly of the United Nations and, when appropriate, to the Security Council: if in connection with the activities of the Agency there should arise questions that are within the competence of the Security Council, the Agency shall notify the Security Council, as the organ bearing the main responsibility for the maintenance of international peace and security, and may also take the measures open to it under this Statute, including those provided in paragraph C of article XII;

5. Submit reports to the Economic and Social Council and other organs of the United Nations on matters within the competence of these organs.

C. In carrying out its functions, the Agency shall not make assistance to members subject to any political, economic, military, or other conditions incompatible with the provisions of this Statute.

D. Subject to the provisions of this Statute and to the terms of agreement concluded between a State or a group of States and the Agency which shall be in accordance with the provisions of the Statute, the activities of the Agency shall be carried out with due observance of the sovereign rights of States.

Article IV, Membership

A. The initial members of the Agency shall be those States Members of the United Nations or of any of the specialized agencies which shall have signed this Statute within ninety days after it is opened for signature and shall have deposited an instrument of ratification.

B. Other members of the Agency shall be those States, whether or not Members of the United Nations or of any of the specialized agencies, which deposit an instrument of acceptance of this Statute after their membership has been approved by the General Conference upon the recommendation of the Board of Governors. In recommending and approving a State for membership, the Board of Governors and the General Conference shall determine that the State is able and willing to carry out the obligations of membership in the Agency, giving due consideration to its ability and willingness to act in accordance with the purposes and principles of the Charter of the United Nations.

C. The Agency is based on the principle of the sovereign equality of all its members, and all members, in order to ensure to all of them the rights and

94441—57——3

benefits resulting from membership, shall fulfill in good faith the obligations assumed by them in accordance with this Statute.

Article V. General Conference

A. A General Conference consisting of representatives of all members shall meet in regular annual session and in such special sessions as shall be convened by the Director General at the request of the Board of Governors or of a majority of members. The sessions shall take place at the headquarters of the Agency unless otherwise determined by the General Conference.

B. At such sessions, each member shall be represented by one delegate who may be accompanied by alternates and by advisers. The cost of attendance of any delegation shall be borne by the member concerned.

C. The General Conference shall elect a President and such other officers as may be required at the beginning of each session. They shall hold office for the duration of the session. The General Conference, subject to the provisions of this Statute, shall adopt its own rules of procedure. Each member shall have one vote. Decisions pursuant to paragraph H of article XIV, paragraph C of article XVIII, and paragraph B of article XIX shall be made by a two-thirds majority of the members present and voting. Decisions on other questions, including the determination of additional questions or categories of questions to be decided by a two-thirds majority, shall be made by a majority of the members present and voting. A majority of members shall constitute a quorum.

D. The General Conference may discuss any questions or any matters within the scope of this Statute or relating to the powers and functions of any organs provided for in this Statute, and may make recommendations to the membership of the Agency or to the Board of Governors or to both on any such questions or matters.

E. The General Conference shall:

1. Elect members of the Board of Governors in accordance with article VI;
2. Approve States for membership in accordance with article IV;
3. Suspend a member from the privileges and rights of membership in accordance with article XIX;
4. Consider the annual report of the Board;
5. In accordance with article XIV, approve the budget of the Agency recommended by the Board or return it with recommendations as to its entirety or parts to the Board, for resubmission to the General Conference;
6. Approve reports to be submitted to the United Nations as required by the relationship agreement between the Agency and the United Nations, except reports referred to in paragraph C of article XII, or return them to the Board with its recommendations;
7. Approve any agreement or agreements between the Agency and the United Nations and other organizations as provided in article XVI or return such agreements with its recommendations to the Board, for resubmission to the General Conference;
8. Approve rules and limitations regarding the exercise of borrowing powers by the Board, in accordance with paragraph G of article XIV; approve rules regarding the acceptance of voluntary contributions to the Agency; and approve, in accordance with paragraph F of article XIV, the manner in which the general fund referred to in that paragraph may be used;
9. Approve amendments to this Statute in accordance with paragraph C of article XVIII;
10. Approve the appointment of the Director General in accordance with paragraph A of article VII.

F. The General Conference shall have the authority:

1. To take decisions on any matter specifically referred to the General Conference for this purpose by the Board;
2. To propose matters for consideration by the Board and request from the Board reports on any matter relating to the functions of the Agency.

Article VI. Board of Governors

A. The Board of Governors shall be composed as follows:

1. The outgoing Board of Governors (or in the case of the first Board, the Preparatory Commission referred to in Annex I) shall designate for membership on the Board the five members most advanced in the technology of atomic energy including the production of source materials and the member most advanced in

the technology of atomic energy including the production of source materials in each of the following areas not represented by the aforesaid five:

(1) North America
(2) Latin America
(3) Western Europe
(4) Eastern Europe
(5) Africa and the Middle East
(6) South Asia
(7) South East Asia and the Pacific
(8) Far East

2. The outgoing Board of Governors (or in the case of the first Board, the Preparatory Commission referred to in Annex I) shall designate for membership on the Board two members from among the following other producers of source materials: Belgium, Czechoslovakia, Poland, and Portugal; and shall also designate for membership on the Board one other member as a supplier of technical assistance. No member in this category in any one year will be eligible for redesignation in the same category for the following year.

3. The General Conference shall elect ten members to membership on the Board of Governors, with due regard to equitable representation on the Board as a whole of the members in the areas listed in subparagraph A-1 of this article, so that the Board shall at all times include in this category a representative of each of those areas except North America. Except for the five members chosen for a term of one year in accordance with paragraph D of this article, no member in this category in any one term of office will be eligible for reelection in the same category for the following term of office.

B. The designations provided for in subparagraphs A-1 and A-2 of this article shall take place not less than sixty days before each regular annual session of the General Conference. The elections provided for in subparagraph A-3 of this article shall take place at regular annual sessions of the General Conference.

C. Members represented on the Board of Governors in accordance with subparagraph A-1 and A-2 of this article shall hold office from the end of the next regular annual session of the General Conference after their designation until the end of the following regular annual session of the General Conference.

D. Members represented on the Board of Governors in accordance with subparagraph A-3 of this article shall hold office from the end of the regular annual session of the General Conference at which they are elected until the end of the second regular annual session of the General Conference thereafter. In the election of these members for the first Board, however, five shall be chosen for a term of one year.

E. Each member of the Board of Governors shall have one vote. Decisions on the amount of the Agency's budget shall be made by a two-thirds majority of those present and voting, as provided in paragraph H of article XIV. Decisions on other questions, including the determination of additional questions or categories of questions to be decided by a two-thirds majority, shall be made by a majority of those present and voting. Two-thirds of all members of the Board shall constitute a quorum.

F. The Board of Governors shall have authority to carry out the functions of the Agency in accordance with this Statute, subject to its responsibilities to the General Conference as provided in this Statute.

G. The Board of Governors shall meet at such times as it may determine. The meetings shall take place at the headquarters of the Agency unless otherwise determined by the Board.

H. The Board of Governors shall elect a Chairman and other officers from among its members and, subject to the provisions of this Statute, shall adopt its own rules of procedure.

I. The Board of Governors may establish such committees as it deems advisable. The Board may appoint persons to represent it in its relations with other organizations.

J. The Board of Governors shall prepare an annual report to the General Conference concerning the affairs of the Agency and any projects approved by the Agency. The Board shall also prepare for submission to the General Conference such reports as the Agency is or may be required to make to the United Nations or to any other organization the work of which is related to that of the Agency. These reports, along with the annual reports, shall be submitted to members of the Agency at least one month before the regular annual session of the General Conference.

Article VII. Staff

A. The staff of the Agency shall be headed by a Director General. The Director General shall be appointed by the Board of Governors with the approval of the General Conference for a term of four years. He shall be the chief administrative officer of the Agency.

B. The Director General shall be responsible for the appointment, organization, and functioning of the staff and shall be under the authority of and subject to the control of the Board of Governors. He shall perform his duties in accordance with regulations adopted by the Board.

C. The staff shall include such qualified scientific and technical and other personnel as may be required to fulfil the objectives and functions of the Agency. The Agency shall be guided by the principle that its permanent staff shall be kept to a minimum.

D. The paramount consideration in the recruitment and employment of the staff and in the determination of the conditions of service shall be to secure employees of the highest standards of efficiency, technical competence, and integrity. Subject to this consideration, due regard shall be paid to the contributions of members of the Agency and to the importance of recruiting the staff on as wide a geographical basis as possible.

E. The terms and conditions on which the staff shall be appointed, remunerated, and dismissed shall be in accordance with regulations made by the Board of Governors, subject to the provisions of this Statute and to general rules approved by the General Conference on the recommendation of the Board.

F. In the performance of their duties, the Director General and the staff shall not seek or receive instructions from any source external to the Agency. They shall refrain from any action which might reflect on their position as officials of the Agency; subject to their responsibilities to the Agency, they shall not disclose any industrial secret or other confidential information coming to their knowledge by reason of their official duties for the Agency. Each member undertakes to respect the international character of the responsibilities of the Director General and the staff and shall not seek to influence them in the discharge of their duties.

G. In this article the term "staff" includes guards.

Article VIII. Exchange of information

A. Each members should make available such information as would, in the judgement of the member, be helpful to the Agency.

B. Each member shall make available to the Agency all scientific information developed as a result of assistance extended by the Agency pursuant to article XI.

C. The Agency shall assemble and make available in an accessible form the information made available to it under paragraphs A and B of this article. It shall take positive steps to encourage the exchange among its members of information relating to the nature and peaceful uses of atomic energy and shall serve as an intermediary among its members for this purpose.

Article IX. Supplying of materials

A. Members may make available to the Agency such quantities of special fissionable materials as they deem advisable and on such terms as shall be agreed with the Agency. The materials made available to the Agency may, at the discretion of the member making them available, be stored either by the member concerned or, with the agreement of the Agency, in the Agency's depots.

B. Members may also make available to the Agency source materials as defined in article XX and other materials. The Board of Governors shall determine the quantities of such materials which the Agency will accept under agreements provided for in article XIII.

C. Each member shall notify the Agency of the quantities, form, and composition of special fissionable materials, source materials, and other materials which that member is prepared, in conformity with its laws, to make available immediately or during a period specified by the Board of Governors.

D. On request of the Agency a member shall, from the materials which it has made available, without delay deliver to another member or group of members such quantities of such materials as the Agency may specify, and shall without delay deliver to the Agency itself such quantities of such materials as are really necessary for operations and scientific research in the facilities of the Agency.

E. The quantities, form and composition of materials made available by any member may be changed at any time by the member with the approval of the Board of Governors.

F. An initial notification in accordance with paragraph C of this article shall be made within three months of the entry into force of this Statute with respect to the member concerned. In the absence of a contrary decision of the Board of Governors, the materials initially made available shall be for the period of the calendar year succeeding the year when this Statute takes effect with respect to the member concerned. Subsequent notifications shall likewise, in the absence of a contrary action by the Board, relate to the period of the calendar year following the notification and shall be made no later than the first day of November of each year.

G. The Agency shall specify the place and method of delivery and, where appropriate, the form and composition, of materials which it has requested a member to deliver from the amounts which that member has notified the Agency it is prepared to make available. The Agency shall also verify the quantities of materials delivered and shall report those quantities periodically to the members.

H. The Agency shall be responsible for storing and protecting materials in its possession. The Agency shall ensure that these materials shall be safeguarded against (1) hazards of the weather, (2) unauthorized removal or diversion, (3) damage or destruction, including sabotage, and (4) forcible seizure. In storing special fissionable materials in its possession, the Agency shall ensure the geographical distribution of these materials in such a way as not to allow concentration of large amounts of such materials in any one country or region of the world.

I. The Agency shall as soon as practicable establish or acquire such of the following as may be necessary:

1. Plant, equipment, and facilities for the receipt, storage, and issue of materials;
2. Physical safeguards;
3. Adequate health and safety measures;
4. Control laboratories for the analysis and verification of materials received;
5. Housing and administrative facilities for any staff required for the foregoing.

J. The materials made available pursuant to this article shall be used as determined by the Board of Governors in accordance with the provisions of this Statute. No member shall have the right to require that the materials it makes available to the Agency be kept separately by the Agency or to designate the specific project in which they must be used.

Article X. Services, equipment, and facilities

Members may make available to the Agency services, equipment, and facilities which may be of assistance in fulfilling the Agency's objectives and functions.

Article XI. Agency projects

A. Any member or group of members of the Agency desiring to set up any project for research on, or development or practical application of atomic energy for peaceful purposes may request the assistance of the Agency in securing special fissionable and other materials, services, equipment, and facilities necessary for this purpose. Any such request shall be accompanied by an explanation of the purpose and extent of the project and shall be considered by the Board of Governors.

B. Upon request, the Agency may also assist any member or group of members to make arrangements to secure necessary financing from outside sources to carry out such projects. In extending this assistance, the Agency will not be required to provide any guarantees or to assume any financial responsibility for the project.

C. The Agency may arrange for the supplying of any materials, services, equipment, and facilities necessary for the project by one or more members or may itself undertake to provide any or all of these directly, taking into consideration the wishes of the member or members making the request.

D. For the purpose of considering the request, the Agency may send into the territory of the member or group of members making the request a person or persons qualified to examine the project. For this purpose the Agency may,

with the approval of the member or group of members making the request, use members of its own staff or employ suitably qualified nationals of any member.

E. Before approving a project under this article, the Board of Governors shall give due consideration to:

1. The usefulness of the project, including its scientific and technical feasibility;

2. The adequacy of plans, funds, and technical personnel to assure the effective execution of the project;

3. The adequacy of proposed health and safety standards for handling and storing materials and for operating facilities;

4. The inability of the member or group of members making the request to secure the necessary finances, materials, facilities, equipment, and services;

5. The equitable distribution of materials and other resources available to the Agency;

6. The special needs of the underdeveloped areas of the world; and

7. Such other matters as may be relevant.

F. Upon approving a project, the Agency shall enter into an agreement with the member or group of members submitting the project, which agreement shall:

1. Provide for allocation to the project of any required special fissionable or other materials;

2. Provide for transfer of special fissionable materials from their then place of custody, whether the materials be in the custody of the Agency or of the member making them available for use in Agency projects, to the member or group of members submitting the project, under conditions which ensure the safety of any shipment required and meet applicable health and safety standards;

3. Set forth the terms and conditions, including charges, on which any materials, services, equipment, and facilities are to be provided by the Agency itself, and, if any such materials, services, equipment, and facilities are to be provided by a member, the terms and conditions as arranged for by the member or group of members submitting the project and the supplying member;

4. Include undertakings by the member or group of members submitting the project: (*a*) that the assistance provided shall not be used in such a way as to further any military purpose; and (*b*) that the project shall be subject to the safeguards provided for in article XII, the relevant safeguards being specified in the agreement;

5. Make appropriate provision regarding the rights and interests of the Agency and the member or members concerned in any inventions or discoveries, or any patents therein, arising from the project;

6. Make appropriate provision regarding settlement of disputes;

7. Include such other provisions as may be appropriate.

G. The provisions of this article shall also apply where appropriate to a request for materials, services, facilities, or equipment in connexion with an existing project.

Article XII. Agency Safeguards

A. With respect to any Agency project, or other arrangement where the Agency is requested by the parties concerned to apply safeguards, the Agency shall have the following rights and responsibilities to the extent relevant to the project or arrangement:

1. To examine the design of specialized equipment and facilities, including nuclear reactors, and to approve it only from the viewpoint of assuring that it will not further any military purpose, that it complies with applicable health and safety standards, and that it will permit effective application of the safeguards provided for in this article;

2. To require the observance of any health and safety measures prescribed by the Agency;

3. To require the maintenance and production of operating records to assist in ensuring accountability for source and special fissionable materials used or produced in the project or arrangement;

4. To call for and receive progress reports;

5. To approve the means to be used for the chemical processing of irradiated materials solely to ensure that this chemical processing will not lend itself to diversion of materials for military purposes and will comply with applicable health and safety standards; to require that special fissionable materials recovered or produced as a by-product be used for peaceful purposes under continuing Agency safeguards for research or in reactors, existing or under construction, specified by the member or members concerned; and to require deposit with the

Agency of any excess of any special fissionable materials recovered or produced as a by-product over what is needed for the above-stated uses in order to prevent stockpiling of these materials, provided that thereafter at the request of the member or members concerned special fissionable materials so deposited with the Agency shall be returned promptly to the member or members concerned for use under the same provisions as stated above;

6. To send into the territory of the recipient State or States inspectors, designated by the Agency after consultation with the State or States concerned, who shall have access at all times to all places and data and to any person who by reason of his occupation deals with materials, equipment, or facilities which are required by this Statute to be safeguarded, as necessary to account for source and special fissionable materials supplied and fissionable products and to determine whether there is compliance with the undertaking against use in furtherance of any military purpose referred to in subparagraph F-4 of article XI, with the health and safety measures referred to in subparagraph A-2 of this article, and with any other conditions prescribed in the agreement between the Agency and the State or States concerned. Inspectors designated by the Agency shall be accompanied by representatives of the authorities of the State concerned, if that State so requests, provided that the inspectors shall not thereby be delayed or otherwise impeded in the exercise of their functions;

7. In the event of noncompliance and failure by the recipient State or States to take requested corrective steps within a reasonable time, to suspend or terminate assistance and withdraw any materials and equipment made available by the Agency or a member in furtherance of the project.

B. The Agency shall, as necessary, establish a staff of inspectors. The staff of inspectors shall have the responsibility of examining all operations conducted by the Agency itself to determine whether the Agency is complying with the health and safety measures prescribed by it for application to projects subject to its approval, supervision or control, and whether the Agency is taking adequate measures to prevent the source and special fissionable materials in its custody or used or produced in its own operations from being used in furtherance of any military purpose. The Agency shall take remedial action forthwith to correct any noncompliance or failure to take adequate measures.

C. The staff of inspectors shall also have the responsibility of obtaining and verifying the accounting referred to in sub-paragraph A-6 of this article and of determining whether there is compliance with the undertaking referred to in subparagraph A-2 of this article, and with all other conditions of the project prescribed in the agreement between the Agency and the State or States concerned. The inspectors shall report any noncompliance to the Director General who shall thereupon transmit the report to the Board of Governors. The Board shall call upon the recipient State or States to remedy forthwith any noncompliance which it finds to have occurred. The Board shall report the noncompliance to all members and to the Security Council and General Assembly of the United Nations. In the event of failure of the recipient State or States to take fully corrective action within a reasonable time, the Board may take one or both of the following measures: direct curtailment or suspension of assistance being provided by the Agency or by a member, and call for the return of materials and equipment made available to the recipient member or group of members. The Agency may also, in accordance with article XIX, suspend any noncomplying member form the exercise of the privileges and rights of membership.

Article XIII. Reimbursement of Members

Unless otherwise agreed upon between the Board of Governors and the members furnishing to the Agency materials, services, equipment, or facilities, the Board shall enter into an agreement with such member providing for reimbursement for the items furnished.

Article XIV. Finance

A. The Board of Governors shall submit to the General Conference the annual budget estimates for the expenses of the Agency. To facilitate the work of the Board in this regard, the Director General shall initially prepare the budget estimates. If the General Conference does not approve the estimates, it shall return them together with its recommendations to the Board. The Board shall then submit further estimates to the General Conference for its approval.

B. Expenditures of the Agency shall be classified under the following categories:

1. Administrative expenses: these shall include:

(a) Costs of the staff of the Agency other than the staff employed in connexion with materials, services, equipment, and facilities referred to in subparagraph B-2 below; costs of meetings; and expenditures required for the preparation of Agency projects and for the distribution of information;

(b) Costs of implementing the safeguards referred to in article XII in relation to Agency projects or, under subparagraph A-5 of article III, in relation to any bilateral or multilateral arrangement, together with the costs of handling and storage of special fissionable material by the Agency other than the storage and handling charges referred to in paragraph E below;

2. Expenses, other than those included in sub-paragraph 1 of this paragraph in connection with any materials, facilities, plant, and equipment acquired or established by the Agency in carrying out its authorized functions, and the costs of materials, services, equipment, and facilities provided by it under agreements with one or more members.

C. In fixing the expenditures under subparagraph B-1 (b) above, the Board of Governors shall deduct such amounts as are recoverable under agreements regarding the application of safeguards between the Agency and parties to bilateral or multilateral arrangements.

D. The Board of Governors shall apportion the expenses referred to in subparagraph B-1 above, among members in accordance with a scale to be fixed by the General Conference. In fixing the scale the General Conference shall be guided by the principles adopted by the United Nations in assessing contributions of Member States to the regular budget of the United Nations.

E. The Board of Governors shall establish periodically a scale of charges, including reasonable uniform storage and handling charges, for materials, services, equipment, and facilities furnished to members by the Agency. The scale shall be designed to produce revenues for the Agency adequate to meet the expenses and costs referred to in subparagraph B-2 above, less any voluntary contributions which the Board of Governors may, in accordance with paragraph F, apply for this purpose. The proceeds of such charges shall be placed in a separate fund which shall be used to pay members for any materials, services, equipment, or facilities furnished by them and to meet other expenses referred to in subparagraph B-2 above which may be incurred by the Agency itself.

F. Any excess of revenues referred to in paragraph E over the expenses and costs there referred to, and any voluntary contributions to the Agency, shall be placed in a general fund which may be used as the Board of Governors, with the approval of the General Conference, may determine.

G. Subject to rules and limitations approved by the General Conference, the Board of Governors shall have the authority to exercise borrowing powers on behalf of the Agency without, however, imposing on members of the Agency any liability in respect of loans entered into pursuant to this authority, and to accept voluntary contributions made to the Agency.

H. Decisions of the General Conference on financial questions and of the Board of Governors on the amount of the Agency's budget shall require a two-thirds majority of those present and voting.

Article XV. Privileges and Immunities

A. The Agency shall enjoy in the territory of each member such legal capacity and such privileges and immunities as are necessary for the exercise of its functions.

B. Delegates of members together with their alternates and advisers, Governors appointed to the Board together with their alternates and advisers, and the Director General and the staff of the Agency, shall enjoy such privileges and immunities as are necessary in the independent exercise of their functions in connection with the Agency.

C. The legal capacity, privileges, and immunities referred to in this article shall be defined in a separate agreement or agreements between the Agency, represented for this purpose by the Director General acting under instructions of the Board of Governors, and the members.

Article XVI. Relationship With Other Organizations

A. The Board of Governors, with the approval of the General Conference, is authorized to enter into an agreement or agreements establishing an ap-

propriate relationship between the Agency and the United Nations and any other organizations the work of which is related to that of the Agency.

B. The agreement or agreements establishing the relationship of the Agency and the United Nations shall provide for:

1. Submission by the Agency of reports as provided for in subparagraphs B-4 and B-5 of article III;

2. Consideration by the Agency of resolutions relating to it adopted by the General Assembly or any of the Councils of the United Nations and the submission of reports, when requested, to the appropriate organ of the United Nations on the action taken by the Agency or by its members in accordance with this Statute as a result of such consideration.

Article XVII. Settlement of Disputes

A. Any question or dispute concerning the interpretation or application of this Statute which is not settled by negotiation shall be referred to the International Court of Justice in conformity with the Statute of the Court unless the parties concerned agree on another mode of settlement.

B. The General Conference and the Board of Governors are separately empowered, subject to authorization from the General Assembly of the United Nations, to request the International Court of Justice to give an advisory opinion on any legal question arising within the scope of the Agency's activities.

Article XVIII. Amendments and Withdrawals

A. Amendments to this Statute may be proposed by any member. Certified copies of the text of any amendment proposed shall be prepared by the Director General and communicated by him to all members at least ninety days in advance of its consideration by the General Conference.

B. At the fifth annual session of the General Conference following the coming into force of this Statute, the question of a general review of the provisions of this Statute shall be placed on the agenda of that session. On approval by a majority of the members present and voting, the review will take place at the following General Conference. Thereafter, proposals on the question of a general review of this Statute may be submitted for decision by the General Conference under the same procedure.

C. Amendments shall come into force for all members when:

(i) Approved by the General Conference by a two-thirds majority of those present and voting after consideration of observations submitted by the Board of Governors on each proposed amendment, and

(ii) Accepted by two-thirds of all the members in accordance with their respective constitutional processes. Acceptance by a member shall be effected by the deposit of an instrument of acceptance with the depositary Government referred to in paragraph C of article XXI.

D. At any time after five years from the date when this Statute shall take effect in accordance with paragraph E of article XXI or whenever a member is unwilling to accept an amendment to this Statute, it may withdraw from the Agency by notice in writing to that effect given to the depositary Government referred to in paragraph C of article XXI, which shall promptly inform the Board of Governors and all members.

E. Withdrawal by a member from the Agency shall not affect its contractual obligations entered into pursuant to article XI or its budgetary obligations for the year in which it withdraws.

Article XIX. Suspension of Privileges

A. A members of the Agency which is in arrears in the payment of its financial contributions to the Agency shall have no vote in the Agency if the amount of its arrears equals or exceeds the amount of the contributions due from it for the preceding two years. The General Conference may, nevertheless, permit such a member to vote if it is satisfied that the failure to pay is due to conditions beyond the control of the member.

B. A member which has persistently violated the provisions of this Statute or of any agreement entered into by it pursuant to this Statute may be suspended from the exercise of the privileges and rights of membership by the General Conference acting by a two-thirds majority of the members present and voting upon recommendation by the Board of Governors.

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Article XX. Definitions

As used in this Statute:

1. The term "special fissionable material" means plutonium–239; uranium–233; uranium enriched in the isotopes 235 or 233; any material containing one or more of the foregoing; and such other fissionable material as the Board of Governors shall from time to time determine; but the term "special fissionable material" does not include source material.

2. The term "uranium enriched in the isotopes 235 or 233" means uranium containing the isotopes 235 or 233 or both in an amount such that the abundance ratio of the sum of these isotopes to the isotope 238 is greater than the ratio of the isotope 235 to the isotope 238 occurring in nature.

3. The term "source material" means uranium containing the mixture of isotopes occurring in nature; uranium depleted in the isotope 235; thorium; any of the foregoing in the form of metal, alloy, chemical compound, or concentrate; any other material containing one or more of the foregoing in such concentration as the Board of Governors shall from time to time determine; and such other material as the Board of Governors shall from time to time determine.

Article XXI. Signature, Acceptance, and Entry Into Force

A. This Statute shall be open for signature on 26 October 1956 by all States Members of the United Nations or of any of the specialized agencies and shall remain open for signature by those States for a period of ninety days.

B. The signatory States shall become parties to this Statute by deposit of an instrument of ratification.

C. Instruments of ratification by signatory States and instruments of acceptance by States whose membership has been approved under paragraph B of article IV of this Statute shall be deposited with the Government of the United States of America, hereby designated as depositary Government.

D. Ratification or acceptance of this Statute shall be effected by States in accordance with their respective constitutional processes.

E. This Statute, apart from the Annex, shall come into force when eighteen States have deposited instruments of ratification in accordance with paragraph B of this article, provided that such eighteen States shall include at least three of the following States: Canada, France, the Union of Soviet Socialist Republics, the United Kingdom of Great Britain and Northern Ireland, and the United States of America. Instruments of ratification and instruments of acceptance deposited thereafter shall take effect on the date of their receipt.

F. The depositary Government shall promptly inform all States signatory to this Statute of the date of each deposit of ratification and the date of entry into force of the Statute. The depositary Government shall promptly inform all signatories and members of the dates on which States subsequently become parties thereto.

G. The Annex to this Statute shall come into force on the first day this Statute is open for signature.

Article XXII. Registration With the United Nations

A. This Statute shall be registered by the depositary Government pursuant to Article 102 of the Charter of the United Nations.

B. Agreements between the Agency and any member or members, agreements between the Agency and any other organization or organizations, and agreements between members subject to approval of the Agency, shall be registered with the Agency. Such agreements shall be registered by the Agency with the United Nations if registration is required under Article 102 of the Charter of the United Nations.

Article XXIII. Authentic Texts and Certified Copies

This Statute, done in the Chinese, English, French, Russian and Spanish languages, each being equally authentic, shall be deposited in the archives of the depositary Government. Duly certified copies of this Statute shall be transmitted by the depositary Government to the Governments of the other signatory States and to the Governments of States admitted to membership under paragraph B of article IV.

In witness whereof the undersigned, duly authorized, have signed this Statute.

Done at the Headquarters of the United Nations, this twenty-sixth day of October, one thousand nine hundred and fifty-six.

ANNEX I. PREPARATORY COMMISSION

A. A Preparatory Commission shall come into existence on the first day this Statute is open for signature. It shall be composed of one representative each of Australia, Belgium, Brazil, Canada, Czechoslovakia, France, India, Portugal, Union of South Africa, Union of Soviet Socialist Republics, United Kingdom of Great Britain and Northern Ireland, and United States of America, and one representative each of six other States to be chosen by the International Conference on the Statute of the International Atomic Energy Agency. The Preparatory Commission shall remain in existence until this Statute comes into force and thereafter until the General Conference has convened and a Board of Governors has been selected in accordance with article VI.

B. The expenses of the Preparatory Commission may be met by a loan provided by the United Nations and for this purpose the Preparatory Commission shall make the necessary arrangements with the appropriate authorities of the United Nations, including arrangements for repayment of the loan by the Agency. Should these funds be insufficient, the Preparatory Commission may accept advances from Governments. Such advances may be set off against the contributions of the Governments concerned to the Agency.

C. The Preparatory Commission shall—

1. Elect its own officers, adopt its own rules of procedure, meet as often as necessary, determine its own place of meeting and establish such committees as it deems necessary;

2. Appoint an executive secretary and staff as shall be necessary, who shall exercise such powers and perform such duties as the Commission may determine;

3. Make arrangements for the first session of the General Conference, including the preparation of a provisional agenda and draft rules of procedure, such session to be held as soon as possible after the entry into force of this Statute;

4. Make designations for membership on the first Board of Governors in accordance with subparagraphs A-1 and A-2 and paragraph B of article VI;

5. Make studies, reports, and recommendations for the first session of the General Conference and for the first meeting of the Board of Governors on subects of concern to the Agency requiring immediate attention, including (*a*) the financing of the Agency; (*b*) the programmes and budget for the first year of the Agency; (*c*) technical problems relevant to advance planning of Agency operations; (*d*) the establishment of a permanent Agency staff; and (*e*) the location of the permanent headquarters of the Agency;

6. Make recommendations for the first meeting of the Board of Governors concerning the provisions of a headquarters agreement defining the status of the Agency and the rights and obligations which will exist in the relationship between the Agency and the host Government;

7. (*a*) Enter into negotiations with the United Nations with a view to the preparation of a draft agreement in accordance with article XVI of this Statute, such draft agreement to be submitted to the first session of the General Conference and to the first meeting of the Board of Governors; and (*b*) make recommendations to the first session of the General Conference and to the first meeting of the Board of Governors concerning the relationship of the Agency to other international organizations as contemplated in article XVI of this Statute.

ANALYSIS OF STATUTE OF THE INTERNATIONAL ATOMIC ENERGY AGENCY

1. The primary point of interest in the new statute is the composition of the Board of Governors of the agency.

The statute requires there to be a Preparatory Commission (annex I), composed of the following named countries:

Australia
Belgium
Brazil
Canada
Czechoslovakia
France
India
Portugal
Union of South Africa
Union of Soviet Socialist Republics
United Kingdom of Great Britain and Northern Ireland
United States of America

The following additional nations were chosen by the International Conference on the Statute of the International Atomic Energy Agency to be members of the Preparatory Commission:

Argentina
Japan
Egypt
Peru
Indonesia
Pakistan

This would make at least 13 out of 18 who certainly would be expected to be friendly to us.

The Board of Governors is selected from the members of the agency (art. VI). The initial membership of the Agency is composed of those states which are members of the United Nations, or of any of the specialized agencies which shall have signed the statute within 90 days after it is open for signature—or through January 24, 1957 (art. IV). Other members shall be those which accept the statute and are approved by the General Conference on the recommendation of the Board of Governors (art. IV).

The Board of Governors of the Agency is to have five members from those most advanced in the technology of atomic energy as designated by the Preparatory Commission or by the prior Board of Governors (art. VI). In addition, there are to be members representing those of the following areas which are not repesented by the first five members:

1. North America
2. Latin America
3. Western Europe
4. Eastern Europe
5. Africa and the Middle East
6. Southern Asia
7. Southeastern Asia and the Pacific
8. Far East

Next, two members are designated by the Preparatory Commission, or by the Board of Governors, from the following countries:

Belgium
Czechoslovakia
Poland
Portugal

One other member is to be designated as a supplier of technical assistance.

Lastly, 10 members are elected by the General Conference with an eye to equitable representation from the areas designated in the first list of areas, except North America. The members elected by the General Conference and designated by the Board of Governors under the latter two categories are not eligible for reelection in the same category in the following year.

The formula for establishing the membership for the Board of Governors is not as favorable to us as was the formula originally set forth in the draft statute of August 1955. It was recognized in the hearing last year that there would have to be further negotiations on the formula for the Board of Governors if, and when Soviet Russia decided to join in the International Agency.

The old proposal called for 5 members from the most important contributors of technical assistance and materials, 5 members from the most important suppliers of source materials, and 6 members to be elected by the General Conference. The provisions in the original draft practically provided for self-perpetuation of the original Board of Governors. This ability of self-perpetuation has been materially cut down in the statute.

It is to be noted that the description of the very first class of governors has been changed from "important contributors" of materials and technical assistance to those "most advanced in the technology of atomic energy."

At the Conference at the United Nations there was considerable discussion about possible changes in the composition of the Board of Governors. However, none of these changes was accepted. The Conference decided not to disturb the delecate balance which had been obtained on the structure of the Board of Governors.

2. The second important point which has been raised is the relationship of the Agency with the United Nations. The Board of Governors of the Agency is authorized to enter into an agreement with the United Nations and with any other organization whose work is related to that of the Agency (art. XVI). The agreement is to provide for reporting the activities of the Agency to the General

Assembly of the United Nations and also to the Security Council of the United Nations where there are questions which arise which are within the competence of the Security Council (also art. III). The Agency is also to submit reports to the Economic and Social Council and other organs of the United Nations on matters within the competence of those organs. In addition, the Agency is to operate in consultation with, and where appropirate in collaboration with, the competent organs of the United Nations.

3. The third major problem in the Agency is the problem of inspection and controls (art. XII). The Agency has the authority to approve the design of facilities constructed under arrangements with it, but such approval is solely to see the facilities will not further any military purpose and that it will comply with applicable health and safety standards and to insure the effective application of safeguards; to require adherence to health and safety measures prescribed by the Agency; to require the maintenance and production of operating records to assist in insuring accountability for source and special fissionable materials; to receive progress reports; to approve the means used in chemical processing and require the fissionable materials recovered to be deposited with the Agency until the member has peaceful uses for the material; to send inspectors into the states who have been designated by the Agency after consultation with the states; to have access to all information required for accounting for materials and to determine whether the materials are being used for peaceful purposes and in accordance with the health and safety measures; take corrective steps within a reasonable time, to suspend or terminate assistance and withdraw materials.

There was considerable pressure at the Conference to minimize the inspection and control clauses and to exempt source materials completely from the controls. Strangely enough, France appeared to join with India in this attempt. However, the attempt was stopped. The inspection and control clauses, as rewritten slightly, were accepted almost unanimously.

The staff of inspectors is specifically established in the statute. The reports of noncompliance are to go to the Director General who transmits the report to the Board of Governors. The Board calls on the states to remedy any noncompliance. Further noncompliance is reported to all members, to the Security Council, and to the General Assembly of the United Nations. The Board may curtail or suspend assistance, call for return of materials, and initiate proceedings to suspend membership.

The inspection and control procedures are to be made applicable not only to Agency projects but also to those projects sponsored under bilateral agreements between members of the Agency where both members request the Agency to take over the inspection and control. The inspection and control functions for the internal atomic energy program of any nation may be taken over by the Agency upon the request of that nation.

At the Conference many of the small nations requested that the inspection and control functions be limited by the sovereign rights of the nations inspected. The United States' answer, which effectively stopped this complaint, was that the very fact that a nation entered into an agreement with the Agency for assistance which required inspection and control was an exercise by the nation of its sovereign rights. Such an agreement did not abrogate the sovereign rights of the nation but constituted an exercise of them.

4. The fourth major point under the statute is the contribution of materials. Three months after the statute becomes effective with respect to any member the member is required to designate the amount of materials it may make available. This designation is to be made annually thereafter. Materials are to be contributed in such amounts as the member deems advisable and on terms as may be agreeable with the Agency (art. IX–A).

There is the requirement that—

> Each Member shall notify the Agency of the quantities, form, and composition of special fissionable materials, source materials, and other materials which that Member is prepared, *in conformity with its laws,* to make available immediately or during a period specified by the Board of Governors (art. IX–C).

Special fissionable materials made available may be stored by the member or, on agreement with the Agency, in the Agency depots.

5. The fifth major point is a question of financing. The budget of the Agency is divided into two parts—administrative and capital (art. XIV). With respect to the administrative budget, assessments can be levied against the members in accordance with the scale of the assessments, to be fixed by the General Confer-

ence, which is to be guided by the principles used by the United Nations in establishing its scale. The capital budget of the Agency is not to be assessable against any member but is to be made up either from gifts or from revenues other than assessments.

The Agency was specifically given authority to assist recipient nations on problems involved in obtaining financing from outside sources but the Agency cannot guarantee the financing or assume financial responsibility for any project (art. XI–B).

SUMMARY OF STATUTE

I. Establishes the Agency.

II. The objective is to accelerate the contribution of atomic energy to the peace, health, and prosperity throughout the world and to insure that assistance is not used for military purposes.

III. The Agency is authorized to encourage research on peaceful uses and to act as intermediary for having one member of the Agency supply services and materials to another; encourage the exchange of scientific information; to foster the exchange and training of scientists; establish safeguards for materials and services supplied by the Agency on request of those parties to a bilateral arrangement and to apply the safeguards to the bilateral; or, on the request of any nation, to the atomic energy program of that nation; establish health and safety standards; to acquire facilities, plants, and equipment. The Agency is required to conduct its policies in line with the policies of the United Nations to promote peace; establish controls over special fissionable materials received by the Agency; allocate its resources to insure efficient utilization and greatest benefit to all parts of the world: to submit reports on its activities to the General Assembly and to the Security Council where there are questions within the competence of the Council for the maintenance of international peace and security, and submit reports to the Economic and Social Council on matters within the competence of that Council.

Amendments to the statute to have the Agency publish a periodical, establish an international university, grant scholarships and convene international scientific conferences were not accepted by the Conference as all being included in the present powers of the Agency.

The Agency is forbidden from making assistance to members subject to conditions which are incompatible to the statute. The activities of the Agency are to be carried on with due observance of sovereign rights of states, subject to the statute and the agreements with the states which shall be in accordance with the provisions of the statute.

IV. Initial membership is limited to members of the United Nations and specialized agencies who sign within 90 days. Other members are to have approval of the General Conference on the recommendation of the Board of Governors but applicants are to be judged on their ability to abide by the principles of the United Nations Charter. The Agency is based on the sovereign equality of all members.

V. The General Conference of all members is to meet annually or in special sessions as called by the Director General at the request of the Board of Governors or the majority of members. The Conference is to elect a President and other officers. The majority is a quorum.

Decisions on the budget and financial questions, on amendments to the statute, and on suspension of membership for violation of the statute are to be taken by two-thirds of those present and voting. Other questions to be decided by a majority of members present and voting.

The General Conference may consider any matters within the statute. The functions of the General Conference are to elect governors; approve new members; suspend members from the privileges and rights of membership; consider the annual report; approve the budget or return it to the Board with its recommendations; approve reports to be submitted to the United Nations; approve any agreement between the Agency and the United Nations or other organizations; approve rules regarding borrowing powers of the Board; approve amendments to the statute; approve the appointment of the Director General.

The General Conference may make recommendations to the Board of Governors either relating to functions of the Agency or on matters brought to the attention of the General Conference by the Board. They may propose matters for consideration by the Board.

VI. Governors. (See initial analysis.)

In addition, the Governors designated by the prior Board of Governors or by the Preparatory Commission shall sit for 1 year but those elected by the General Conference shall sit for 2 years with 5 to be elected at each General Conference. Two-thirds of the Board of Governors constitute a quorum and the Board is to governed by majority rule. The Board is to elect a chairman, officers, and committees and is to prepare an annual report.

VII. The Director General is to be appointed by the Board of Governors, with the approval of the General Conference, for 4 years and is to be the chief administrative officer for the Agency. He is to be responsible for staffing and perform his duties in accordance with regulations adopted by the Board. The staff is to be kept to a minimum and shall be composed of qualified scientific and technical and other personnel as may be required. The paramount consideration is to secure employees with the highest standards of efficiency, technical competence, and integrity, otherwise to have as wide a geographical distribution as possible.

The Director General and staff are not to get instructions outside the Agency. Subject to the staff's responsibilities to the Agency, the staff shall not disclose industrial secrets or confidential information learned by reason of their official duties.

VIII. Members are to make information available which would be helpful to the Agency and to make all information available which is developed as a result of assistance extended by the Agency. The Agency is to assemble and make the information available among its members.

IX. Materials. (See initial analysis.)

In addition, the Agency is to be responsible for storing and protecting materials in its possession from weather, diversion, sabotage, and forcible seizure. In storing special fissionable materials the Agency is to insure geographical distribution so a large concentration will not go to any one part of the world.

X. Members may make services and other materials available.

XI. The member may request assistance of the Agency on any atomic energy project for peaceful purposes, the requests to be considered by the Board of Governors. The Agency may assist a member in obtaining financial assistance but may not make any guaranty or assume financial responsibility. The Agency may arrange for supplying materials and services by itself or one or more members. It may send qualified persons into the country to examine the request.

The Agency shall consider—

1. Usefulness of project.
2. Adequacy of plans.
3. Adequacy of health and safety standards.
4. Ability of member to secure necessary assistance.
5. Equitable distribution of sources available to the Agency.
6. Other relevant matters.

On approval of a project the Agency has to agree with the member on:

1. Allocation of materials.
2. Transfer of materials under conditions to meet health and safety standards.
3. The terms for the Agency supplying of materials and services.
4. Safeguards that the assistance will not further any military purpose and the project will be subject to further safeguards provided in the statute and in the agreement.
5. Appropriate provision for patent rights.
6. Appropriate provision regarding settlement of disputes.
7. Other appropriate provisions.

XII. Agency safeguards. (See initial analysis.)

XIII. Unless otherwise agreed, members supplying services and materials are to be reimbursed.

XIV. Finance. (See initial analysis.)

The Board of Governors is to submit an annual budget to the Conference. The expenditures are to be classified under administrative expenses, which include cost of staff and cost of implementing safeguards, and other expenses, including the costs of acquiring and operating plants used by the Agency in its authorized functions.

The Board of Governors is to establish a scale of charges including uniform storage and handling charges. The Board of Governors may borrow in accordance with the rules and regulations of the General Conference. Two-thirds of the General Conference and the Board of Governors must approve finance questions and the budget. The Agency may accept voluntary contributions.

XV. The Agency is to enjoy such privileges and immunities in the territory of each member which are necessary for its functions. Delegates and their staffs also enjoy such privileges and immunities as are necessary in the independent exercise of their functions. The legal privileges and immunities and capacity shall be defined in a separate agreement between the Agency, represented by the Director General, and the members.

XVI. Board, with approval of General Conference, may enter in agreements with the United Nations and other international organizations.

XVII. Disputes on interpretation or application of the statute may be referred to the International Court of Justice. The International Court may give an advisory opinion on request of the Board of Governors or of the General Conference.

XVIII. Amendments may be proposed by any member and are to be communicated to all members by the Director General at least 90 days in advance of consideration by the General Conference.

The agenda for the Fifth Annual General Conference after the statute becomes effective shall include the question of revision of the statute. The majority shall approve the review to take place at next General Conference.

Amendments shall come into effect when approved by two-thirds of the General Conference after consideration of the observations made by the Board of Governors and when considered by two-thirds of all members with respect to their constitutional processes.

At any time after 5 years from the date the statute goes into effect, or whenever an amendment unsatisfactory to the members is accepted, the member may withdraw. The member may be suspended if it is in arrears in its dues in an amount equal to more than the dues of the last 2 years.

XIX. Two-thirds of the members may vote to suspend a membership if the member is persistently violating the statute, or is 2 years behind in financial contributions.

XX. Defines "special fissionable material," "Uranium enriched in isotopes 235 and 233" and "source material."

XXI. Sets the date when the statute is open for signature and has the period of signature run for 90 days. The signatory parties become parties to the statute by the deposit of an instrument of ratification. The Government of the United States is designated as the depository government. Ratification or acceptance is to be effected by the States under their usual constitutional processes and becomes effective when 18 States have deposited instruments of ratification (including 3 of "Big Five" nations named). The depository government is to notify all states of the date of deposit of ratification.

XXII. The statute and the agreements entered into by the Agency are to be registered with the United Nations in accordance with the terms of the article 102 of the charter.

XXIII. Sets forth that the authentic texts are in Chinese, English, French, Russian, and Spanish.

ANNEX I. PREPARATORY COMMISSION

The membership of the Preparatory Commission has been set forth in section I. The expenses are to be met by a loan from the United Nations which is to be paid out of assessments of members of the Agency. The Preparatory Commission is empowered to elect its officers; appoint an Executive Secretary and staff; arrange for the first session of the General Conference; designate members of the first Board of Governors in accordance with the statute; and report to the first General Conference on—

1. The financing of the Agency.
2. The Agency programs and budgets for the first year of the Agency.
3. The technical problems of the Agency.
4. Establishment of a permanent staff.
5. Location of permanent headquarters.

It is also empowered to recommend provisions for a headquarters agreement, to negotiate a draft agreement with the United Nations, and to recommend to the General Conference concerning the relationship of the Agency with other international organizations.

DEPARTMENT OF STATE,
Washington, June 4, 1957.

The Honorable J. W. FULBRIGHT,
Committee on Foreign Relations,
United States Senate.

DEAR SENATOR FULBRIGHT: Your subcommittee has asked for clarification of certain testimony given by representatives of the Department of State and the Atomic Energy Commission during the course of the Senate Foreign Relations Committee hearings on the statute of the International Atomic Energy Agency and on the policy to be pursued in connection with United States participation in the Agency. I am glad to provide this clarification.

It has been asked whether the statute of the International Atomic Energy Agency, if ratified by the United States, would supersede the procedures for the transfer of special nuclear material required by the Atomic Energy Act of 1954, as amended. It would not. Section 124 of the Atomic Energy Act and related sections 123 and 54 would govern the transfer of special nuclear materials to the Agency, including the 5,000 kilograms of U–235 which the President offered to make available to the Agency last October. The United States cannot, under the terms of the Atomic Energy Act, transfer materials to the Agency until the conditions of the Atomic Energy Act are met, including the following: (1) The Atomic Energy Commission has submitted to the President a proposed agreement for cooperation with the Agency, including among other provisions the terms of the transfer of materials to the Agency; (2) the President has approved and authorized the execution of the agreement after making a determination in writing that the performance of the agreement will promote, and will not constitute an unreasonable risk to, the common defense and security; and (3) the proposed agreement has been submitted to the Joint Congressional Committee on Atomic Energy and the period of time prescribed in the act has elapsed. It should be noted that article IX (C) of the statute of the International Agency provides that any member making materials available will do so in conformity with the laws of that member state.

It has also been asked whether amendments to the statute would be submitted to the Senate for advice and consent to ratification. I can assure you that this would be the case. Amendments to the statute come into force for all members only after they have been accepted by two-thirds of them in accordance with their respective constitutional processes. However, the interests of the United States are protected since it would have the right provided in article XVIII (D): "Whenever a member is unwilling to accept an amendment to the statute it may withdraw from the Agency by a notice in writing * * *" There have been questions as to whether the President would act to effectuate withdrawal of the United States from the Agency if the Senate did not consent to the ratification of an amendment that had been accepted by two-thirds of the members of the Agency. We believe that he would and should do so if a majority of the Congress were of the view that the amendment so altered the nature of our treaty obligation as to render our continued participation in the Agency inconsistent with the interests of the United States. However, in my opinion, and as I said in my testimony before the Senate Foreign Relations Committee, the participation of the United States is so vital to the existence of the Agency that I cannot visualize a situation in which an amendment would be ratified by two-thirds of the members of the Agency that would impair or endanger the continued wholehearted support of the United States.

The subcommittee has asked whether the statute is self-executing and whether implementing legislation is needed to enable the United States to participate in the Agency. Effective United States participation will require three types of legislation: (1) The existing Atomic Energy Act of 1954; (2) a participation act; and (3) annual appropriations.

The internationally binding character of the limited obligations contained in the statute is, of course, not conditioned on the enactment of legislation by the governments of Agency members. Within the United States, the payment of this country's share of the administrative expenses of the Agency will require both authorizing legislation and annual appropriations by the Congress. However, the other obligations contained in the statute of the Agency (to act as depositary, to afford the Agency certain limited privileges and immunities) do not require implementing legislation.

To cover the administrative details of United States membership in the Agency, a draft participation act has been submitted to the Congress; a copy of this legislation is attached. It will provide for the appointment of United States representatives to the Agency and authorize the appropriation of funds needed for United States participation.

It should be reemphasized that nothing in the statute of the Agency obligates the United States to provide materials, equipment, or facilities to the Agency. As article IX states, the furnishing of special nuclear and other materials is to be in conformity with the laws of each member. For the United States, this means the Atomic Energy Act of 1954, as amended, which already provides procedures for cooperation with the International Atomic Energy Agency. There is no conflict between the statute of the Agency and the United States Atomic Energy Act.

During the course of my testimony there were inquiries as to whether the United States would donate the special nuclear material we make available to the Agency and its projects. Article IX provides that material shall be made available on terms to be agreed with the Agency. Article XIII provides that supplying governments will be reimbursed by the Agency for such materials unless otherwise agreed. The intention and policy of the United States Government is to apply the principles now prevailing for the bilateral program which require reimbursement for the cost of special nuclear materials except in special instances when limited quantities may be transferred for research or medical purposes. If future developments should make a basic change in policy with respect to donation desirable and in the interests of the United States, appropriate congressional approval will be sought.

A question has also been raised as to what steps can be taken to encourage other governments to make material available to the Agency. In our judgment, the initial United States offer of a substantial quantity of special nuclear materials constitutes an earnest of our desire for an effective Agency and, as such is the best method of encouraging substantial offers from other countries. The scope and degree of our further support for the Agency will be decided upon, of course, in the light of the cooperation given by other governments. While the furnishing of material by any member of the Agency is entirely voluntary, the United States hopes and expects that cooperation will be forthcoming and will use its best efforts to encourage wholehearted support of the Agency by its members to the extent of their capabilities.

While only 2 or 3 countries are now in a position to sell, lease, or otherwise make available special nuclear materials, other countries could make available equipment and other useful materials, such as natural uranium which is also a reactor fuel. When the Agency, in accordance with the provisions of article IX (B), has determined its needs for such materials, facilities, and equipment, we are confident that these countries will make offers to supply them. In fact, once the Agency has been established and its safeguards system is in force, its members will have an incentive to use the Agency as a channel for marketing nuclear materials.

Questions have been raised with regard to the extent and nature of the United States commitment to deliver special nuclear material to Agency projects or to the Agency itself. Can the United States be required to deliver materials before there is need for their use in approved specific projects? Will ratification of the statute commit the United States to deliver special nuclear materials to the Agency without prior knowledge of the projects to which these materials will be allocated?

The United States presently has no legal commitment to deliver any material to the Agency. We plan to carry out the President's offer of 5,000 kilograms of U-235 plus a sum equal to all quantities of such materials made available by other nations prior to July 1, 1960, subject to an agreement being concluded with the Agency on the terms and conditions under which it will be made available in accordance with the Atomic Energy Act of 1954, as amended. After such an agreement has been concluded, the Agency may request delivery of specific quantities of this material as it is needed for specific projects which have been approved by the Board of Governors or for use by the Agency itself. Until that time, the United States will retain special nuclear material made available to the Agency within its own boundaries.

No member has the right to designate the specific projects in which the material made available to the Agency will be used. However, the United States, through its membership on the Board of Governors, will have full knowledge of

the Agency's projects and programs when we reach decisions in the future on amounts of material which we may subsequently make available to the Agency. Our decisions will be guided by our judgment of the needs and policies of the Agency and the security interests of the United States. Even with respect to the 5,000 kilograms, the United States will have ample opportunity beginning with the initial application for an Agency supported project, to be informed of the circumstances surrounding a proposed project and to be assured that the principles and objectives of the Agency are respected.

We were asked to clarify the interpretation of the word "terms" in the light of the negotiating history of the statute. The word "terms" as used in article IX (A) of the statute is not limited by the negotiating history. In our interpretation the word "terms" means "terms and conditions." It will be our policy to include in any agreement with the Agency under article IX (A) of the statute a provision that no materials will be shipped from the United States until they are needed for specific projects approved by the Agency. The Agency could not, of course, properly agree to the inclusion of any conditions in such agreements that were in conflict with any provision of the statute. For example, it could not agree to a term in such an agreement that was inconsistent with the provision in article IX (J) that "No member shall have the right to require that the materials it makes available to the Agency be kept separately by the Agency or to designate the specific project in which they must be used." In the case of the United States, special nuclear materials cannot be made available to the Agency except in conformity with an agreement for cooperation negotiated under the provisions of the Atomic Energy Act of 1954, as amended.

For purposes of evaluating the President's offer, it may be noted that 5,000 kilograms is approximately the amount of U–235 required to fuel 2 or 3 medium-sized reactors throughout their useful lives. Moreover, the degree of enrichment of this uranium will be far below the level required for weapons-grade materials. As Chairman Strauss of the Atomic Energy Commission has testified, the United States will not supply the Agency with material enriched in excess of 20 percent.

Some questions were also raised concerning the dividing line between administrative and other expenses. Article XIV (B) (1) of the statute, entitled "Finance," identifies those expenses which may properly be included in the administrative budget. Furthermore, it is the accepted practice in international organizations in which the United States participates to confine administrative expenses to those which are for general support, such as salaries of the Secretariat staff, conference or meeting costs, travel, etc. Also included in administrative expenses are costs of implementing safeguards for Agency projects and certain costs related to the handling of Agency materials. In the light of the provisions of article XIV (B) (1), and of the practice followed generally in international organizations, the Agency's administrative budget will not include or provide for physical facilities such as reactors or "universities." Such expenses would be financed by charges voluntarily agreed to by members benefiting from the projects concerned, and should the United States decide to participate therein the United States dollar share of these amounts would have to be authorized and appropriated.

Finally, a question often asked was whether we are contemplating furnishing the Agency classified information. No secret or classified information will be provided to the Agency.

Sincerely yours,

JOHN FOSTER DULLES.

Senator PASTORE. Now, I take it from the experience we have had on the ratification of the statute itself that the part that needs to be crystallized in the record at this point and comprehensively and intelligently explained, is this feature of control of the material that is furnished by the United States to the agency itself, with particular reference to section 124, section 123, and section 54 of the Atomic Act of 1954.

All this was pointed up emphatically and vividly by the proposed reservation that was suggested by the distinguished Senator from Ohio, Mr. Bricker.

I think we ought to develop the record on that particular point very extensively and very clearly for the understanding of Members of the Senate and Members of the House.

If there are any questions, Senator Bricker, you may proceed.

Representative COLE. Before you proceed, Mr. Chairman, may I interrupt for a moment to call to your attention the presence of a member of the Japanese Diet, who is also interested in atomic energy and who is visiting this country for a short while, Mr. Saito. I wonder if he will stand so that the members of the committee might see him.

Senator PASTORE. We are very happy to have you here today.

Senator BRICKER. I have only a question or two.

First, of Secretary Wilcox: You mentioned on the first page of your testimony that the proposal of the President and this organization was greeted with great enthusiasm throughout the world. Will you furnish for the record any statements or communications which underlie that statement?

Mr. WILCOX. I shall be glad to do that, Senator.

(The material referred to follows:)

STATEMENT ON WORLDWIDE REACTION TO PRESIDENT EISENHOWER'S PROPOSALS FOR AN INTERNATIONAL ATOMIC ENERGY AGENCY

At the close of the 8th session of the U. N. General Assembly, on December 3, 1953, President Eisenhower announced his "Atoms for Peace" proposal, including the suggestion that an International Atomic Energy Agency be established.

This proposal was not formally discussed by the General Assembly until its 9th session in the fall of 1954. At that time many delegations warmly commended the President's initiative. General Assembly Resolution 810 (IX), which was adopted unanimously, included the following: "Recalling the initiative of the President of the United States of America, embodied in his address of December 8, 1953 * * * Expresses the hope that the IAFA will be established without delay * * *"

Less than 2 years later, the Conference to draft the International Atomic Energy Agency Statute was convened at the United Nations. It was attended by representatives of 80 states and 7 specialized agencies. At the conclusion of the Conference, the statute was unanimously approved, and 80 states subsequently signed the statute.

This record of unanimity in voting is probably the best indication of the great and universal appeal of the President's proposal. As further evidence of the great acclaim accorded the President's proposals, I am submitting the official records of the opening debate in Ninth Session of the General Assembly. If the committee so desires, I will be glad to supply the complete and voluminous verbatim records of the Ninth General Assembly, both of the plenary and first committee meetings, and the summary records of the October 1956 IAEA Conference. The many speeches in praise of the President's proposal contained in these records are further proof of the enthusiasm with which the world received the President's proposals.

Senator BRICKER. Then on page 5 you say appropriate officers of both agencies will be available on request to furnish information and answer questions.

There will not be any requirement in this statute, as I understand it, which would require the agency representatives of the United States to keep this committee currently informed of all the developments.

Mr. WILCOX. No, sir; there is not any specific language except on page 4, where it provides that the President shall from time to time as the occasion may require make reports to the Congress, and so on.

But there is no specific language that requires informal consultation. Certainly it is the desire of the executive branch to keep the members of this committee and other interested committees informed of major

developments and we shall be glad to do that in any way that the committee would deem appropriate and desirable.

Senator BRICKER. Would there be any objection to putting into this statute a requirement such as in the Atomic Energy Act that this committee be kept currently advised of all developments?

Mr. STRAUSS. Might I respond to that?

Senator BRICKER. Yes.

Mr. STRAUSS. I think, Senator Bricker, that the obligation of the Atomic Energy Commission to keep the joint committee fully and currently informed would certainly apply to any information that reached the Atomic Energy Commission.

Senator BRICKER. There is no doubt about that.

Mr. STRAUSS. And since no material could be provided or anything involving the Atomic Energy Agency be agreed upon without our knowledge, I think the joint committee could under those circumstances expect to be fully and currently informed.

Now, an obligation placed by an amendment to this act on the Secretary of State, I think would involve a duplication of information, that is all.

Senator BRICKER. There would be no objection, if there were certain areas in which there would be information of value to this committee to be furnished by the Secretary of State, that that be put in the statute.

Mr. STRAUSS. I see none except the fact of duplication.

Chairman DURHAM. Do you not think section III covers that?

Senator BRICKER. That was my question if it did. I am so advised it did not except the President's annual report.

Chairman DURHAM. Reading the language I believe it will cover that.

Senator BRICKER. I wanted to get the attitude of the State Department in regard to that.

Chairman DURHAM. That provision is set forth in the original statute in 1954.

Senator BRICKER. Mr. Wilcox said they felt there was no obligation except the annual report to the President.

Chairman Strauss said there was no obligation except the Commission must reveal to us fully and currently all information which it receives.

And it would receive, as he says, the information in regard to the supply of material which would be, of course, presented to us immediately.

Mr. STRAUSS. I have this further observation, that just occurs to me. If there is a provision in this participation act that places the responsibility on the State Department to keep the Joint Committee informed of these negotiations and the Commission is relieved, let us say, to that extent, there is the possibility that in both agencies the presumption might arise in some matter that the other agency is informing the Joint Committee.

I would assume, were I otherwise situated, that you will get a more consistent line of information if the responsibility is in one direction, to 1 agency, rather than to 2.

Senator BRICKER. I would feel it would be just as consistent and achieve my objective as well if we placed that obligation on the Atomic

Energy Commission and that they be currently advised by the State Department of all developments.

What I am thinking about is a complete story of the development of atomic energy and the peaceful uses particularly.

Now, we are a great repertory in this committee of both classified and unclassified material. Certainly here is a field of great development, the magnitude of which we do not yet fully understand.

I think there ought to be a current history and story of the whole development some place that future generations might go to as an account of just what has been accomplished.

Mr. STRAUSS. The Commission appointed a historian, I think just a few days ago, with that as one of its primary responsibilities. I don't claim any clairvoyance, Senator Bricker, in reading what was in your mind, but we thought it was a desirable thing to do.

Senator PASTORE. If I may say a word or two on the same subject, I should think that possibly some desirable language could be agreed upon between our staff and the representatives here to carry out that very thought which is quite an important one, namely to keep the Congress and the people of the United States apprised and informed currently, within reason, as to how this thing develops, which is quite important.

But the one thing we must bear in mind, and that we must be careful about, is the language, because, first of all, our representatives there will be under the instruction of the President, himself. They will be able to do nothing unless they are so instructed by the President of the United States, as they do at the United Nations.

Therefore, whatever is done there will have to be done, of course, under the direction of the President.

The question arises, we must not encumber the President to the extent he does not have the flexibility and fluency he needs to carry out his executive responsibility in this premise.

On the other hand, it strikes me that once thees things have been done, something could be worked out whereby the Joint Committee would be kept constantly and currently informed as to the developments as we do with the liaison between the Commission and the Joint Committee.

I see no hardship here and I think that that can be worked out without tying the hands of the President so that he can be a free agent without having to report on every little instruction that is given out to the Agency in the conferences as to what we should do or should not do.

Senator BRICKER. I had no thought of covering that.

Senator PASTORE. I realize that.

Senator BRICKER. What I had in mind is that we be currently advised of our representatives' activities. He may be instructed by the President, but his instructions by the President may not be carried out by the Board of Governors.

I want all the official actions insofar as we can ascertain them presented to this committee so that there will be a continuity of the story fully developed.

Mr. STRAUSS. Mr. Chairman, may I read something that perhaps will be very reassuring along this line? This is a letter dated the 27th of May from the Secretary of State to the Speaker of the House

of Representatives. I assume that there was an identical letter written to the President of the Senate. I don't have a copy of it.

In this letter in the third paragraph the Secretary of State says:

> To direct United States participation in the work of the Agency regular procedures are being established within the executive branch for coordination and collaboration between the Department of State and the Atomic Energy Commission in the determination of positions to be taken by the United States in the organs of the agency and for obtaining recommendations regarding the policies to be followed by the United States in the course of its participation.

These procedures will make the Atomic Energy Commission aware of and a participant with the Department of State in these instructions and in this information and will provide the Commission with that information which it is duty bound to convey to the Joint Committee fully and currently.

Senator PASTORE. In order to bring this to a head, is there any objection in working out language that will carry out the thought expressed by Mr. Bricker. Does anyone see any disadvantage to that suggestion?

Mr. SMITH. It seems to me under section 202 of the Atomic Energy Act of 1954, you have the machinery that Senator Bricker is looking for. That requires any Government agency to furnish any information requested by the Joint Committee on the responsibilities of that agency in the field of atomic energy.

If you, under that section, will just lay a charge on us in the State Department to keep you fully and currently informed of all activities in the International Atomic Energy Agency, we will respond willingly.

Senator PASTORE. Do you see any harm in codifying it in this?

Mr. SMITH. No, sir.

Senator BRICKER. Of course, that will put the responsibility then where it belongs and it will not necessarily be on the Joint Committee.

Mr. WILCOX. Mr. Chairman, while the Department would recognize the deep interest of this committee in this subject matter, I don't think we would want to include such language that might imply that we were not interested in keeping the Foreign Relations Committee of the Senate or the House Foreign Affairs Committee informed of important political aspects of the atomic-energy field.

Senator PASTORE. You can say the appropriate committee. We can work out that language. That is merely mechanical.

Naturally, of course, we mean every pertinent committee interested in the functioning of this International Agency.

Senator BRICKER. I have one further question, Mr. Wadsworth, has there been a schedule of payments to the Agency yet made up? You note here there will be $2,300,000 of the United States share, not to exceed one-third. Has the schedule been made up for the contributions of the other members of the Agency?

Mr. WADSWORTH. No, the schedule is not. The reason why that one-third was in the statement is the idea it will follow more or less the United Nations practice and the United States share of the United Nations administrative expense is one-third.

Now, if the Congress and the Senate did adopt such an amendment, as they did 2 years ago, limiting the United States contribution to 25 percent on all the International Agencies, do you think that would be

a violation of the treaty and our obligations under it if such should become the law of this country?

Mr. WADSWORTH. That is a legal question, sir, that I am afraid I am not competent to answer.

Mr. WILCOX. If I can address myself to that rather obliquely, Senator Bricker, as you know, we are now contributing 33⅓ percent to the regular United Nations budget. The contributions which we make to many of the specialized agencies of the United Nations are determined by the pattern set by the contributions committee of the United Nations proper.

In the statute there is a provision that the principles which apply to the contributions scale in the regular United Nations budget should apply here.

In the light of the new membership in the United Nations, we are attempting to bring our contributions down below the 33⅓-percent ceiling.

The Congress could, of course, insert here a proviso to the effect that we ought not to contribute more than 25 percent to this Agency, but we would run into difficulty, because my assumption is that this Agency would determine its scale in accordance with the scale of the United Nations and they would suggest our contribution at the 33⅓-percent level until that contribution is reduced in the negotiations that will take place in the next year or so.

We would get into serious complications in our attempt to reduce all of our contributions to the various United Nations agencies. I did not mean to answer the legal problem.

Senator BRICKER. You do not mean to infer that the control of the contribution is out of the hands of Congress at all?

Mr. WILCOX. Well, of course, the executive branch is constrained by any action that the Congress would take with respect to our contributions. I think that we might find that it would constitute something of a violation of our obligation under the statute. I think the more effective way to go about it would be to press for a reduction of our contribution to the regular United Nations budget and then get a corresponding reduction in this Agency and in other specialized agencies of which we are a member.

Senator BRICKER. My question does not go to the matter of policy. It goes to the question of power and whether or not we are getting the appropriation authority out of the hands of the Congress of the United States, which is clearly ours under the Constitution.

My question is whether or not in your judgment the ratification of this treaty takes that appropiration power away from the Congress.

Mr. WILCOX. No, sir; I don't think it takes it away from the Congress. I think the statute lays down a reasonable basis for contributions. I think you will agree, Senator Bricker, that if each state were to determine for itself the amount of money that it could contribute to the regular budget of this Agency, you would have nothing but financial chaos because each state would attempt to reduce its contribution to the very minimum it could possibly get by with.

Therefore, you do have to have some kind of systematic approach to a determination of the fiscal, financial arrangements of an agency of this character.

Senator BRICKER. I grant you it might be very unwise for us in any way to vary what was allocated to us as a charge within reason

and yet I am trying to get to the question of whether or not the power here is taken away from the Congress to fix the amount of our contribution to this Agency.

Mr. WILCOX. No, sir; I don't think it is. I think we enter voluntarily into an arrangement which is a reasonable arrangement. I don't think that is a restriction of the power of the Congress.

I think that Congress can, if it wishes, proceed to appropriate less than 33⅓ percent. It can appropriate, as it did in the case of the International Labor Organization, appropriate less than that amount which was fixed by the conference of the Internatioal Labor Organization for our contribution. But it does ot result in a very happy arrangement, nor does it create good will or good relations as between the Untied States and the other members of the organization.

Senator BRICKER. I see now they are having trouble further with the Hungarian representatives.

These nonrecurring items mean that the United States will have paid all of the cost of the preparatory program, the cost of the agency which drafted this statute.

Mr. WADSWORTH. No, as I understand it, the costs will be apportioned among the 18 members of the Preparatory Commission in the first instance, but that the money which has been advanced by the Secretary General of the United Nations must be repaid to the United Nations by the Agency itself, not by the individual countries. So that that will be a nonrecurring cost in the Agency's first year budget.

Then we will, thereafter, have one-third of that.

Senator BRICKER. Our proportionate share?

Mr. WADSWORTH. That is correct.

Senator PASTORE. Now, on this point that was raised by Senator Bricker, do I understand it correctly when I say that regardless of what the agreement may be as to the Agency, as to the amount of money we will contribute as our share, that will have to be submitted as an estimate on the part of the Bureau of the Budget every year, as we appropriate all money and the final decision as to the dollars and cents to be contributed will be the responsibility of Congress predicated upon whatever agreements have been made by the executive branch.

Mr. WILCOX. That is correct.

Senator PASTORE. It is not a question of abrogation at all. It is a question that the final decision will always remain in the Congress as we do with foreign aid and as we do to all appropriations to Government agencies.

Mr. WILCOX. That is correct. Each year the executive branch comes before the Congress with the request which it would like to put before it in connection with our participation in these various agencies and it is up to the Congress to determine.

Senator PASTORE. To decide the dollars and cents to be contributed?

Mr. WILCOX. That is correct.

Senator BRICKER. I am the last one to ever think of the United States violating its obligations under an international agreement which has been entered into. The same thing goes for this.

I just wanted to lay the foundation of what we were obligated to do in the future in order to carry out the provisions of this treaty, whether a reduction of our contribution would be a violation of our obligations, which is a very serious matter to me in international law.

Senator PASTORE. Has not our obligation been spelled out in the treaty that we have already ratified?

Mr. WILCOX. That is correct.

Senator PASTORE. Congress has already ratified the statute which sets out the principle by which we have made our contribution. How that principle is worked out is still a matter to be decided between the executive branch and the Appropriations Committee.

Mr. WILCOX. Mr. Chairman, article XIV does set forth the method by which the Board of Governors shall apportion the expenses and the method by which the General Conference shall approve the estimates submitted by the Board of Governors.

It provides specifically that in fixing the scale the General Conference shall be guided by the principles adopted by the United Nations in assessing contributions of member states to the regular budget of the United Nations.

Now, our purpose in seeking this kind of language was to make certain that our contributions should not exceed the 33⅓ percent ceiling which we have been working toward in all these agencies.

We have been getting it down to that level. We wanted to make certain there would be no attempt on the part of the member states to get us above that ceiling of 33⅓.

If, in the meanwhile, our contributions to the regular budget can be reduced below the 33⅓, then automatically it would be reflected because of the application of article XIV, paragraph D of the statute.

Mr. STRAUSS. Mr. Chairman, may I offer an observation here.

Unlike many other international bodies this Agency will be in a position, it is hoped, to recoup a large part of its expenditures, if not all, by the price at which it will resell the material which it purchases from the nations that supply it.

There is no reason that this should continue to be a drain on the participating countries except insofar as the pay of the representatives of the countries to the board and other administrative expenses are involved.

I would hope that that would be a situation that could be reached after a few years of operation.

Senator PASTORE. On October 26, 1956, the President of the United States, through Admiral Strauss, made the concluding speech at the negotiating conference for the establishment of this International Agency. This was read by Mr. Hickenlooper on the floor of the Senate when we discussed and debated the treaty.

I want to read it at this time because I think there are certain pertinent questions that should be cleared:

> Here is what I, on behalf of the United States, propose

says the President:

> First, It shall be my care, when our Congress reassembles, to present the statute for official ratification by the Senate in accordance with our Constitution, and to request appropriate congressional authority to transfer special nuclear materials to the International Atomic Energy Agency. I wish my country to be among the first to recognize by official action what you at this Conference have accomplished.
>
> Second, to enable the International Atomic Energy Agency—upon its establishment by appropriate governmental actions—to start atomic research and power programs without delay, the United States will make available to the International Agency, on terms to be agreed with the Agency, 5,000 kilograms of the nuclear fuel uranium 235 from the 20,000 kilograms of such material

allocated last February by the United States for peaceful uses by friendly nations.

Third, in addition to the above-mentioned initial 5,000 kilograms of uranium 235, the United States will continue to make available to the International Atomic Energy Agency nuclear materials that will match in amount the sum of all quantities of such materials made similarly available by all other members of the International Agency, and on comparable terms, for the period between the establishment of the Agency and July 1, 1960. The United States will deliver these nuclear materials to the International Agency as they are required for Agency-approved projects.

Now, there are several questions that I should like to ask and I know that there are certain questions that Mr. Bricker, Mr. Hickenlooper, and other members, would like to ask also.

What was meant when the President said: "By appropriate congressional action," with reference to the 5,000 kilograms?

Mr. STRAUSS. That, Senator, referred to the language in the act in section 124, which is short and which I will read for the record, with your permission:

The President is authorized to enter into an international arrangement with a group of nations providing for international cooperation in the nonmilitary application of atomic energy and he may thereafter cooperate with that group of nations pursuant to sections 54, 57, 64, 82, 103, 104, or 144a: *Provided, however,* That the cooperation is undertaken pursuant to an agreement for cooperation entered into in accordance with section 123.

That section 123, the preceding section, specifies all the terms which the Commission has adhered to in the negotiation of bilateral agreements, a number of which have already been laid before your committee.

Senator PASTORE. In other words, in order to crystallize the record at this point, what was meant by the President on October 26 was precisely this: That insofar as the availability of the 5,000 kilograms is concerned we will have an agreement that will come here which will rest with the joint committee for a period of 30 days, upon which it will become consummated as a binding agreement.

Mr. STRAUSS. Precisely.

Senator PASTORE. In other words, the President of the United States will have exclusive authority under sections 124 and 123 to make available this 5,000 kilograms of U–235 which will become a consummated agreement after that arrangement has come before the Congress for a period of 30 days and if the Congress desired to do anything about it it would have to initiate legislation for the repudiation of that agreement.

Mr. STRAUSS. That is my understanding.

Representative COLE. On that point, I want to make certain that I understand Mr. Strauss' response.

By that do I understand you to indicate that if the President desires to give 5,000 kilograms to the International Agency, he may do so simply by way of the bilateral agreement under 123?

Mr. STRAUSS. Mr. Cole, in the course of testimony which I gave before the Senate Foreign Relations Committee on May 14, in reference to some representations that have been made in the press to the effect that the 5,000 kilograms or some portion thereof was intended as a gift, I said that—

The United States has not offered to make a gift of the materials to the Agency. The President's statement explicity referred to terms to be agreed

upon and that articles IX, XI, and XIII of the Agency statute likewise provide specifically for reimbursement.

and I added that—

In any event, the advice and authorization of the Congress would be specifically sought before any gift was made to the Agency or to any nation or group of nations should such a gift appear advisable at some future date.

Representative COLE. That hardly answered my question.

Mr. STRAUSS. It was intended to; I am sorry.

Representative COLE. Do you construe the present status of the law and the treaty to authorize the President to make a gift to the International Agency of this nuclear material solely by way of the bilateral agreement?

Mr. STRAUSS. Mr. Cole, I believe if there was a bilateral agreement which envisaged a gift and it was submitted to the Congress and that no action was taken by the Congress to——

Senator HICKENLOOPER. To the Congress or the joint committee?

Mr. STRAUSS. The joint committee. I speak of the joint committee, Senator Hickenlooper, and the Congress, perhaps erroneously, as one in this case—that if the joint committee took no action to change that course of events it would be a gift.

Representative COLE. Is it not a fact that the Commission has previously interpreted that section of the act authorizing the distribution of nuclear fuel abroad to limit them in such fashion that that the Commission is limited to sale?

Mr. STRAUSS. I beg your pardon, sir?

Representative COLE. Has not the Commission previously indicated to the joint committee that its interpretation of that section of the statute which authorizes the Commission to distribute nuclear materials abroad does not include authorization by way of a gift and that the Commission is limited to sale?

Mr. STRAUSS. Or transfer. Here is a letter writen by the Commission, Mr. Cole, on February 15, 1957, on this point.

DEAR SENATOR PASTORE: Please refer to the letter of January 5, 1957, from you and Senator Anderson requesting the views of the Commission as to its authority under the Atomic Energy Act of 1954, as amended, to make a gift of special nuclear materials without direct congressional action. Section 54 of the act authorizes the Congress to cooperate with any nation by distributing special nuclear material and to distribute such material subject to the terms of an agreement for cooperation to which such nation is a party. The General Counsel of the Commission advises that examination of other sections of the act and the legislative history does not reveal that the precise method of transfer—that is to say, by sale, lease, or gift—was specified.

Congress wisely permitted a degree of flexibility as to the means of transfer. Thus the Commission in fulfilling its statutory responsibilities to foster research and development on the peaceful application of atomic energy could, for example, make modest gifts of special nuclear materials for use in research under an agreement for cooperation made in accordance with section 123 of the act, as amended. It is the General Counsel's view, however, that the act does not clearly provide authority for a gift of special nuclear material involving substantial quantities and large sums of money.

Accordingly, the General Counsel has advised the Commission that should it wish to provide for a gift of a substantial quantity of special nuclear material in an agreement for cooperation entered into under the Atomic Energy Act of 1954, as amended, explicit congressional authority should be obtained.

The Commission does not presently have any such proposal before it.

Senator PASTORE. That is a statement of policy. That is not a statement of law. But insofar as the law itself is concerned under

section 54 of the amendment of 1954, the President of the United States, through a bilateral with this International Agency, if he so chose, could give it away.

I would like to get an answer "yes" or "no" on that and point up the part of the law that answers that question, because we have skirted all around that issue and we come down to a question of policy as against law.

Now, we are not talking about what the policy is. We are talking about what the law is.

Mr. STRAUSS. At that time, Mr. Chairman, the General Counsel's view was that the act and the legislative history were not clear. It was his view, however, that the act does not provide authority for a gift without specific congressional authority.

Senator PASTORE. He said for a large amount. But for a small amount the act does. It cannot be one law if you give a little and another law if you give a lot unless you say so.

I mean the quantity does not measure the authority of the law. That is the opinion we have gotten and we are a little confused with it.

Now, let me ask this question, if I may:

What would be the disadvantage to us and to the agency and to the negotiators if we wrote into this implementation act that any amount that is given to the Agency or any material cannot be given on terms less favorable at the time to domestic recipients?

What is wrong with that? Where is your disadvantage?

I would like to have that thrown out on the table and the record made clear.

Why can we not write in this act that you cannot give to a foreign nation under terms less favorable than to domestic recipients any amount of special nuclear material? What is wrong with that?

This is for power purposes. We are not talking about medical research. I am not getting into medical research.

Mr. STRAUSS. That small amount is significant in terms of dollars.

Senator PASTORE. What would be the objection in writing into this implementation legislation that whatever is given must be given on terms that are not more favorable than you are willing to give to American recipients?

Mr. STRAUSS. Would it not be preferable to write into the legislation at some appropriate point that there is no gift of special nuclear materials without specific congressional authorization, which is what the President has indicated he would request in any event?

Senator PASTORE. You are willing to compromise to this extent. Now, we have to think this through deeply. You are willing to have written in here that before the President can give it away he must come to Congress to get that authorization?

Mr. STRAUSS. I would be, yes. I would like to give my associates an opportunity to join or differ with me in that position, but speaking personally, I would think that is a proper thing, since it only gives effect to something that the President has already said he intends to do.

Representative COLE. To me it is important to know what you understand when you say specific congressional authority. Are you referring to authority by way of the bilateral agreement method, or are you referring to specific language in the statute?

Mr. STRAUSS. Well, at the present time, Mr. Cole, it does refer to the bilateral agreement method in sections 123 and 124.

Representative COLE. But the interpretation of your counsel says that if the Commission feels that the material should be given by way of an international agreement, bilateral agreement, explicit congressional authority should be obtained.

Of course, he is not here; we cannot explore his mind to determine what he had intended when he said: "Explicit congressional authority."

I had understood him to interpret that to mean that the basic law would have to be revised to authorize the President to make gifts, supplemented later on by way of bilateral agreement.

Senator BRICKER. Mr. Chairman, a further question arises at what point it becomes a gift. What would be the consideration? If it is only nominal would that be consiedred a gift, or would it be considered a sale or a transfer of compensation?

The act of 1954, as interpreted by the General Counsel, which is an interpretation based on policy thinking rather than upon the law, I am confident, has been amended by this statute which the Senate ratified, which does authorize a gift if the President desires to.

Now, you testified that they will submit that to the Joint Committee and that is all that is required.

Senator PASTORE. That is right.

Mr. STRAUSS. There is a representative of the General Counsel's office here, who participated in the preparation of this opinion. Would you permit me to call on him?

Senator PASTORE. All right, Mr. Wells.

Mr. WELLS. Mr. Chairman, I think I can state the thinking that was behind the letter which has been referred to in response to Congressman Cole's question.

As to the explicit authorization that we referred to there we were thinking in terms of the specific act of Congress. In response to Senator Bricker's question, if I may, our opinion—that is the opinion of the General Counsel in formulating this letter—and my opinion today, is that the ratification of the statute itself does not increase the Commission's authority to donate material.

Now, it is clear that in an interpretation of this kind surely minds of reasonable men will differ.

Senator Pastore's interpretation differs from ours in that respect.

I can only say what we concluded using our best judgment in analyzing the statute as a whole.

Senator BRICKER. Are you talking of the statute of 1954?

Mr. WELLS. The statute of 1954.

To repeat it and summarize it, our view is that neither through the International Agency nor under bilateral agreement does the Commission have clear authority to make donation of substantial gifts of special nuclear materials, although we believe that it may legally make gifts of small research amounts to carry out the mandate of Congress to disseminate information.

Senator HICKENLOOPER. On what theory do you base that, that they can give small amounts?

For instance, I might envisage, if it were clearly set out, that there might be some mutual advantage flowing, such as the results of re-

search which would flow from that donation which might be considered adequate compensation if it were authorized, but I do not know if it is really authorized.

I mean there may be some considerations other than cash on the barrelhead that might be considered compensation and take it out of the gift category. But I do not know where that is found in the law.

Mr. WELLS. Mr. Hickenlooper, we came to this conclusion by reading all the provisions of the act and relying heavily on the general mandates of the Commission to encourage research on the peaceful use of atomic energy.

It is a construction, as I said earlier, before the Joint Committee, which we reached trying to put into effect what appeared to be the intent of Congress. I would certainly agree that there would be cases where you can get a quid pro quo by exchange of information with somebody and you might not consider the transfer a gift.

Senator HICKENLOOPER. By the same token, if there was a quid pro quo, it would seem to me it would have to be one which was identified and recognized as such. That is, I agree with some of the other statements that the reason why you can give fuel away, but you cannot give a suit away, is a little bit fuzzy in my mind.

Property is property whether it is ounces or tons.

As I said a moment ago, I can envisage the possibility of some quid pro quo other than cash in connection with research. But it would seem to me that would have to be pretty clearly defined and identified in connection with it to take it out of a gift category.

Now, let me ask you this: The authority to dispose of special material under a bilateral comes exclusively from the statute, does it not?

Mr. STRAUSS. That is right.

Senator HICKENLOOPER. Does the authority to dispose of fissionable materials under this statute come from the statute that is the international statute or does it come from the Atomic Energy Act?

Mr. WELLS. The only authority to dispose of special nuclear materials comes from the Atomic Energy Act of 1954.

Senator BRICKER. One minute at that point, if you will excuse me. The last word on this is article XIII:

"Reinbursement of members," and this is the supreme law of the land today under article VI of the Constitution:

> Unless otherwise agreed upon between the Board of Governors and the member furnishing the agency materials, services, equipment, or facilities, the board shall enter into an agreement with such member providing for reimbursement for the items furnished.

Now, that is the last law in regard to the furnishing of materials and it clearly implies there and says, "Unless otherwise agreed upon."

Now, that means that they can be contributed without compensation. If we are going to amend that we will have to do it by congressional action.

If we are going to require payment it will have to be written into this authorization statute, otherwise you can enter into an agreement for a gift.

Maybe Congress wants to, I do not know.

Mr. STRAUSS. Senator, I construe that quite differently. I construe that as a permissive arrangement that permits the agency to receive gifts, but is not an obligation on the part of members to contribute gifts.

Senator BRICKER. Not an obligation, but it is an authorization.

Mr. SMITH. May I draw your attention to article IX which controls article XIII, which limits our ability to make material available to the authority granted by the law of the United States and if the United States does not permit gifts, nothing in article XIII gives us any right to make gifts.

Senator BRICKER. But this is the law of the United States. The whole Agency statute, the International Agency statute, is the law of the United States. It becomes the supreme law under the Constitution.

Mr. SMITH. Article IXc adopts the Atomic Energy Act.

Senator BRICKER. It does not say that. You testified to that, it is true, I think that is the general understanding in the Senate on the ratification, but it is not clear by any means.

Mr. SMITH. May I read article IXc?

Senator BRICKER. Yes.

Mr. SMITH (reading)

ARTICLE IX. SUPPLYING OF MATERIALS

C. Each member shall notify the Agency of the quantities, form and composition of special fissionable materials, source materials and other materials which that member is prepared, in conformity with its laws, to make available immediately or during the period specified by our Board of Governors.

Senator BRICKER. What are our laws? That qualifies, amends, or restricts the law of 1954. This is the last word and the Agency statute becomes the supreme law of the land. It says "Unless otherwise agreed upon."

I do not think there is anything in the act of 1954 that prohibits a gift if the President sends the agreement to the Joint Committee. I think that is the only requirement that there is.

Senator PASTORE. Let me see if I can crystalize this argument.

Do you take the position that the section Senator Bricker just read is merely a section that means that if the United States Government desired to give this special nuclear material to the agency this is the section of the treaty that enables the agency to receive it, whereas subsection (c) of article IX is the one that empowers the United States to do thus and so?

Am I right in that interpretation?

Mr. STRAUSS. That is my interpretation.

Mr. WELLS. That is my interpretation.

Senator PASTORE. In other words, Mr. Bricker, the argument that is being made here, this article XIII that you are reading, is the one that gives the agency power to do a certain thing, whereas article IX is the section that affects the power of the United States to do certain things.

Senator BRICKER. I think that is rather involved legal reasoning, myself.

Have you any objection to writing it into the statute and clarifying what our rights are?

Mr. STRAUSS. Senator, I have a suggestion that might meet your point and go even beyond it as covering one which has been raised by you and Senator Hickenlooper on the subject of the small gifts of material or gifts of small amount of material for research purposes.

Senator BRICKER. I do not think any of us are concerned about that.

Mr. STRAUSS. I think it might be well to button that up. Here is the suggestion:

Suppose that the Atomic Energy Act itself were amended to provide that no gift in excess of some fixed dollar amount in value, small, $10,000 for instance, could be made in any 1 year by the commission to any 1 nation or cooperating group without the specific approval of the Congress. That would not only plug the loophole of gifts of gram amounts, for instrumentation and research purposes, but would also make it clear that in any subsequent year or at any other time where a large gift of fissionable material were contemplated for any reason whatever, political or otherwise, to the agency or to a nation or to a group of nations, the specific authorization of Congress would have to be obtained.

Senator BRICKER. That is what I was getting at in my reservation which now the court of appeals declared unconstitutional.

I would be willing to say that any contribution for medical research, for agricultural research, could be a gift as far as that is concerned. I have no hesitancy in going the full way on that.

Let the President contribute any amount that he wants to as long as it is for that purpose.

But when you come to the power program I think there ought to be some control in the hands of Congress over the amount of material that is disposed of.

Mr. STRAUSS. My suggestion was that there be an amendment to the Atomic Energy Act rather than to this participation statute and that it would then cover not only the Agency, but other activities of the Commission in connection with the bilateral agreement.

Senator BRICKER. Then you put the Congress in the position of having to seek two-thirds agreement on this bill which would become part of the fundamental law.

Senator HICKENLOOPER. You can amend the Atomic Energy Act. That is not inviolate. It is subject to amendment.

Senator BRICKER. You can amend the Participation Act.

Senator HICKENLOOPER. What I am trying to nail down here is the question which has been touched on as to whether or not this international statute from a legal standpoint gives any authority, irrespective of the laws of the United States, to dispose of any of the fissionable materials which we have.

Mr. STRAUSS. In contravention of the Atomic Energy Act? I am informed that it does not.

Senator HICKENLOOPER. In any event?

Mr. STRAUSS. I am informed that it does not.

Senator HICKENLOOPER. Then the next question is: Does any possible authority which the administration may have or acquire to donate, sell, or transfer fissionable materials to the International Agency, stem from statutory laws passed by the Congress, either past, present, or future, from a legal standpoint.

Mr. WELLS. If you would eliminate the word "give," the answer would be "Yes," and that it stems from the Atomic Energy Act.

Senator HICKENLOOPER. What do you mean "give"?

Mr. WELLS. Donate. I am referring to the previous discussion that we had that the authority to transfer materials stems from the Atomic Energy Act of 1954.

Senator HICKENLOOPER. Transfer can either be a gift, sale, or anything else.

Does that authority stem from statutory law passed by the Congress of the United States?

Mr. WELLS. Yes.

Senator HICKENLOOPER. Does this treaty in any way interfere with or change or alter that situation?

Mr. WELLS. It does not.

Senator HICKENLOOPER. So that this treaty does not create any authority on the part of the administration to divest the United States of ownership or property rights either for free or for compensation in special nuclear materials; is that correct?

Mr. WELLS. Except in accordance with section 124——

Senator HICKENLOOPER. Wait just a minute. I am talking about the statute in and of itself, standing alone. Let us forget about 124.

Mr. WELLS. In and of itself standing alone, it does not.

Senator HICKENLOOPER. So that under this treaty, any material of which the United States divests itself, that is in the performance of the terms of this treaty, that divestment must come as a result of authority under 124, 123, or any other provision of law which the Congress has passed which may be applicable.

Mr. WELLS. That is right.

Senator HICKENLOOPER. Then Congress, in spite of this treaty or under the treaty, or anything else, that is regardless of the existence of the treaty, in the implementation of this treay Congress has he power to regulate to the point of prohibition the disposal of fissionable material to the International Agency; is that correct?

Mr. STRAUSS. By gift?

Senator HICKENLOOPER. Any way, by gift, sale, Congress can say you have to sell them. Or Congress can say you can give 10 percent away, or Congress can say you can give a fraction of 1 percent away, or Congress can say you can only sell it for research purposes, or you can give it for research purposes, and so on, or that you cannot make any available for power purposes.

In other words, Congress retains the sovereign control over the disposition of this material, that is, the divesting of the United States of its property rights in this material.

Mr. STRAUSS. Through the provision, Senator, whereby those agreements that contemplate the sale or lease or divestment of this material are made and submitted to the Joint Committee for 30 days while Congress is in session before they became binding.

Senator HICKENLOOPER. I understand that, but I do not want us to get confused with the fact that there is anything sacred about section 123 or 124.

The same power that enacted 123 and 124 can alter it, repeal it, change it, amend it, or make different provisions.

Mr. STRAUSS. There is no contest on that.

Senator HICKENLOOPER. I agree that any contract we have already entered into validity we will keep. Those are contractual relationships entered into under authority of existing law. I am just leading up to the point that Congress has the power either in this act of participation, which is law, or will become law if it is passed and signed, or by amendment to the Atomic Energy Act, or any other law, has the power to write the terms and conditions if it sees fit.

I would like the legal opinion of counsel on this.

Mr. WELLS. Yes, sir; I agree with you.

Senator PASTORE. As Mr. Smith has brought out under article IX, subsection C, it says specifically:

"In conformity with its laws," and the basic law that we operate under is the atomic-energy law of 1954 and that is controlling.

Senator HICKENLOOPER. That does not limit it to the law in existence at the time of the adoption of the statute.

Senator PASTORE. As the law may be amended, or changed, later on in 1978.

Senator BRICKER. The law of the country has been amended if this statute is inconsistent with the act of 1954 or if it qualifies it in that way.

Now, I want to ask further if there is any objection to this amendment which I am going to submit:

> *Provided further*, That the United States shall not make special nuclear materials available to the International Atomic Energy Agency except to such an extent and in accordance with such provisions and conditions as may hereafter be authorized or prescribed by act or concurrent resolution of the Congress.

I would be willing to add further:

> except in the case of research material for medical purposes or agricultural purposes.

Mr. STRAUSS. Senator, may I ask for clarification. This is an amendment you would propose to offer to the atomic-energy law or to this Participation Act?

Senator BRICKER. To this act here.

Mr. STRAUSS. Would you mind reading it again?

Senator BRICKER. Will you read it. I have to vote.

Mr. NORRIS (reading):

> *Provided further*, That the United States shall not make special nuclear materials available to the International Atomic Energy Agency except to such extent and in accordance with such provisions and conditions as may hereafter be authorized or prescribed by act or concurrent resolution of the Congress.

Mr. STRAUSS. Of course, Mr. Chairman, I would like to think about that, and I suppose all of us would at some length, before commenting.

But this condition could arise that when the Agency meets, our British friends and our Russian associates in the Agency would be there with their contributions, we would not have anything. We would have to then wait for some specific authorization from the Congress.

It would not be a very enviable position to be in.

Representative COLE. Would you entertain the same objection if the suggestion of Mr. Bricker were to be modified in such a way as to permit the Commission to make transfer by way of sale without explicit authority from Congress which is in conformity, as I understand to be the interpretation by the Commission, of its authority under the present law?

Mr. STRAUSS. I think that would be acceptable, Mr. Cole.

Chairman DURHAM. Under this suggestion you would have to go to Congress for every sale you made, as I interpret it; is that correct?

Mr. NORRIS. Yes.

Chairman DURHAM. I believe that is what you said.

Mr. STRAUSS. I don't know what the force of it would be, for example, during the period when the Congress was not in session if it were necessary to make a sale. I would like to examine it and to give you a considered opinion.

Chairman DURHAM. We were discussing the suggestion made by Senator Bricker, Senator Hickenlooper.

Mr. STRAUSS. When Senator Bricker returns I would propose that the language include the right to sell and specifically exclude the necessity for coming to the Congress for authorization for the first 5,000 kilograms to which the President made mention last October, for the reason that otherwise we would be almost undoubtedly faced with the situation to which I referred a moment ago, that is to say, to find that the Agency would begin with the offers of amounts smaller than this perhaps on the part of two nations, members of the Agency, and nothing on the part of the United States which originated it.

Chairman DURHAM. You feel that that commitment has already been made?

Mr. STRAUSS. Yes, to the extent that the President was empowered to make it. He said that he would seek congressional authority upon the terms.

This was read into the record a short while ago by the chairman, being an excerpt from his message delivered to the delegates to the conference on the statute on October 23. He said:

Here is what I, on behalf of the United States, propose.

Then he said his first proposal would be when Congress reassembled he would submit the statute for ratification.

His second proposal was that the United States would make available to the agency on terms to be agreed the 5,000 kilograms. This he had in mind doing through the negotiation of the bilateral agreement with the Agency when it came into existence and the submission of that bilateral in the regular course to the joint committee.

Representative COLE. Did he have in mind that that would contemplate a gift of the material?

Mr. STRAUSS. He did not, and I so stated.

As a matter of fact, Mr. Cole, you are probably aware of the fact that there was strong advocacy at the time for a gift, and that a gift should be announced, and that consent of Congress should be sought to make a gift. This was resisted and the statement was made in this form indicating that there would be terms connected with this transfer and these terms would be specified on a later date.

Senator PASTORE. Would you gentlemen answer this question, any one of you:

If the Congress did decide to write into this implementation agreement or amend the law to the effect that Congress would have to initiate any legislation to transfer his material, what position would that put us in?

Mr. STRAUSS. Congress would have to initiate it.

Senator PASTORE. In other words, you could not give this without the consent of Congress and not by way of the bilaterals that we have been talking about, which is a consummated agreement after it lies here for 30 days.

What position would we be placed in?

I think the record ought to show the position of the President and the State Department quite clearly on this subject.

Mr. STRAUSS. May I ask you for a clarification? Now, you are speaking of gift?

Senator PASTORE. I am speaking of anything. Even the transfer of material.

In other words, you would have to come here like you come here for foreign aid grants or mutual assistance grants, or any appropriation to your department before you could have any money.

Before this agency could have any material from the United States under any terms, under any conditions, either by gift or by payment, Congress would have to authorize it and grant it.

Now, what position would that put us in? Do you think that is a fair question, Mr. Bricker?

Senator BRICKER. Certainly.

Senator PASTORE. I think the record ought to show that because I think there are some people who feel that way and I think we ought to clear the record.

Mr. STRAUSS. I think, Mr. Chairman, that this would effectively take over an administrative function of the Commission which is presently safeguarded by the device originating with the Congress of having no transfer of substantial amounts of fissionable material occur except by specific provision of an instrument that has to be approved by the President of the United States as contributing to the common defense and security and the best interest of the Government of the United States. When that assurance from the President reaches the Joint Committee as the representative of the Congress during the session of Congress and remains before the Joint Committee for a period of 30 days before it becomes binding.

It seems to me that that is not only an adequate safeguard, but has been successfully operating now for better than 2 years without any incident having arisen to bring it into question so far as I am aware.

Representative COLE. Mr. Chairman, it seems to me that the problem that is disturbing to us is the authority of the Commission on behalf of the Government to give the material away.

If the Commission will commit itself to distributing material abroad by way of bilateral agreement only in those cases where the Commission makes the full charge for the material, I can see no criticism of that process.

However, if the Commission decides that it is justified in making a gift of the material abroad, then I think very definitely the authority should be obtained from Congress before it does it.

Mr. STRAUSS. There is no disagreement on that, Mr. Cole.

Senator PASTORE. I want to clear the record on another point, Mr. Cole. It is my function to clear this record up because questions will come up and I think, myself, these representatives, representing these very important departments who have very intimate contact with this legislation and with these negotiations ought to state their views on the record.

I think we ought to have it on the record. Mr. Wilcox and Mr. Smith, would you care to comment on the question I raised?

What would it do to us, how would it embarrass us, how would it tie our hands, what would be the advantages or disadvantages in carrying out the suggestion I just made?

Mr. SMITH. In the first instance, Mr. Chairman, I would like to go back to Mr. Strauss' point about what happens 3 months after the entry into force of this treaty.

Under article IX–F the countries that are proposing to supply this agency have to make an initial notification as to how much they are willing to sell.

The Soviet Union under present conditions could say "We will offer to sell 50 times more than the United States." If they put up 50 kilograms or infinitesimal times more because we would come up with a goose egg.

We would say, "I am sorry; Congress will assemble in January and perhaps we will get some authorization, we don't know."

That will have a tremendous impact on our ability to get our way in this agency.

That is one important practical consideration. If we divorce ourselves from any ability to transfer material from this agency until an uncertain contingency next year——

Senator BRICKER. That is true now under section 124.

Mr. SMITH. Of which act?

Senator BRICKER. Of the act of 1954. You have to file it with the Joint Committee. It cannot become effective until Congress comes back anyway.

Mr. SMITH. We hope that when the machinery is established we will not have to try to operate under conditions of uncertainty.

Senator BRICKER. Is there any more uncertaintly in this than there would be in the present law?

Mr. SMITH. We have authority now, we believe. If during this 30-day period the Congress amended the Atomic Energy Act or repealed section 124, granted that authority would be taken away, but I think one is in a better condition if one has authority and a condition subsequent on it than if one has no authority at all and has to seek it next year.

Representative PRICE. Under the practical application of section 124, you can make an actual proposal and you submit it to Congress and it lies there. Even if Congress did object to it, you could enforce that proposal. You do have some authority under section 124 to make an actual proposal which you would not have if you were not operating under 124.

Senator PASTORE. In other words, the difference here is that in one instance under the present law you would have the authority to enter into an agreement. The other way you would not have authority to enter into an agreement.

You would have to wait for Congress to make the agreement.

Senator BRICKER. Congress could either authorize the agreement in advance or if submitted it would act just exactly as the committee acts now, it could pass upon it when it comes down here, the same way as we make appropriations of money.

The Congress is the authority of the Government which is entitled under the Constitution to dispossess this country of any of its material or property.

Now, we are transferring it in effect to the President. That is the only thought that I have in mind.

Article 124 of the Atomic Energy Act is in fact a transfer of authority to the President to dispossess the property of this country. If

Congress passes an act, if they were dissatisfied with the amount of money that was paid for the material or the amount of material that was contributed, the President could veto it, and it takes two-thirds to overcome it which you cannot get. You know that as well as I do.

The practical effect is that we are transferring the constitutional authority of Congress to dispossess this country of material or property to the President of the United States.

Senatore Pastore. Mr. Strauss, could the Commission today enter into an agreement with the Yankee Atomic Energy Co. to sell it 5,000 kilograms of U-235 without coming to Congress?

Mr. Strauss. Yes. You could lease it.

Senator Bricker. You have to lease it to them?

Senator Pastore. Could you enter in an agreement to lease it?

Mr. Strauss. Without coming to Congress?

Sentor Bracker. Yes.

Mr. Strauss. We could; yes.

Senator Pastore. In so far as these two situations are concerned, would you say they are analogous?

Mr. Strauss. No, I don't think they are quite analogous, Senator, because we do not have to have a bilateral agreement with an American purchaser.

There are certain things that have to be done in the case of the American purchaser. He has to be licensed to use and to operate, but in the case of the foreign purchase, there has to be a bilateral agreement and that bilateral agreement has to specify the use that is to be put to the material and what is a happen to the material during the period that it cannot be used for other than peaceful purposes.

Senator Pastore. But if the argument were made that maybe we do not have enough U-235 to dispose of it even at a price—let us assume that argument was made—is it not a fact you have that authority without coming to the Congress to dispose of U-235 and transfer it to domestic users without having to initiate it in the Congress first?

Mr. Strauss. You asked the question, and I was, therefore, answering your question from the position of nuclear plenty. You changed it. You said assuming we didn't have this amount available.

Senator Pastore. I said if the argument was made that the reason why they want you to come to Congress is because they want Congress to determine what stockpile we have and whether or not we have any to make available either domesticwise or foreignwise, even at a price, and we were talking about the authority of the Commission to act without direct authorization from Congress.

The fact of the matter is that under our present law today the Atomic Energy Commission has the authority to lease to domestic users, U-235 without direct authority from Congress as to the amount which will go to X company.

Mr. Strauss. That is true.

But, Senator, you have overlooked the fact that because of the close relationship in the Commission and the Joint Committee, such negotiations would be known to the Joint Committee over a period of time and it would be impossible for such a thing to come to you as a surprise.

With you permission, I would like to return to a suggestion I have made which I think meets all the points that have been raised here;

namely, that material should not be contributed or given away without the express prior approval or authorization of the Congress and that small amounts should not be indiscriminately given.

I would suggest, Mr. Chairman and gentlemen, that if your staff and the staff of the Commission were delegated the responsibility of preparing or drafting an amendment to the Atomic Energy Act rather than to this participation act, which would then cover not only the Agency but all other parties, nations, or groups of nations, with which the Commission might deal, we could provide that there would be no gift in any year to any nation or any group of nations or any agency in excess of some small fixed dollar amount by the Commission without congressional authorization in advance.

Now, that would clearly cover any large contribution. It would make any large contribution impossible without prior congressional authorization.

Senator PASTORE. You mean gratis contribution?

Mr. STRAUSS. I used the word "contribution" as a transfer without compensation.

Senator BRICKER. Why would you rather have that in the Atomic Energy Act?

Mr. STRAUSS. For the reason, Senator Bricker, under those circumstances it would cover Euratom; it would cover the various other countries with which we have bilateral agreements and which will be amended, let us say, in the future.

Whereas if it is in the statute, the participation act, it would refer, I presume, only to the relationship between the Atomic Energy Commission and the International Agency.

Senator BRICKER. We could amend the Atomic Energy Act in this participation act if we wanted to. It is not a question of whether or not we want to or whether it is desirable to do it as you suggest, or in this way.

What is your thinking on that?

Mr. STRAUSS. I have no preference as to that. I thought it was a cleaner method to keep amendments to the Atomic Energy Act within that pice of organic legislation rather than spread out.

Representative COLE. That brings up a thought which I hope to explore with the representatives of the executive department: That is, that the participation act be written in such fashion that it is an amendment to the basic atomic-energy law, because the participation act is contemplated to be a permanent law in the atomic-energy field.

It would be my hope that any legislation of a permanent nature affecting atomic energy will be by way of amendment to the basic atomic-energy law so that in the future anybody who seeks a question pertaining to atomic energy can get the one law and all phases of atomic energy will be written into that.

Senator HICKENLOOPER. I wonder what effect it would have if some amendments were adopted to the statute which we did not approve and we retired from the Agency.

Representative COLE. Instead of repealing the Participation Act as an entity, you would simply adopt an amendment repealing certain sections of the Atomic Energy Act.

Senator HICKENLOOPER. I was thinking about the cleanness of the legislation.

Representative COLE. Do you gentlemen have any comment to make on that?

Mr. STRAUSS. Secretary Wilcox does have comment on that point.

Mr. WILCOX. While I have not had a chance to consult with my colleagues in the State Department, I would prefer that the Participation Act be a separate piece of legislation. It seems to me if it were attached to the Atomic Energy Act it would in itself confer primary responsibility for the agency upon the Atomic Energy Commission.

We have very carefully worked in harmony with the Atomic Energy Commission. We hope to keep our relations very close, but I think that any consolidation of the Participation Act and the Atomic Energy Act which would in itself constitute a conferring of primary responsibility for the Agency upon the Commission might do damage to this close working relationship that we have developed over the last 6 or 8 or 10 months.

Representative COLE. That conferring of responsibility would be only by way of inference; it would not need to be explicit in the amendment to the basic law.

Mr. WILCOX. Perhaps so.

Senator BRICKER. Under the Participation Act, who would file the articles of cooperation or the cooperation agreements with the Joint Committee? Would the State Department do that, or would the Atomic Energy Commission do it?

Mr. WILCOX. It is my understanding it is the Atomic Energy Commission. We would not want to disturb that relationship.

Senator BRICKER. The suggestion of Congressman Cole would not disturb that relationship. It would leave it right where it is.

Mr. WILCOX. That is correct.

Senator PASTORE. Will you give us some thought and let us know how you feel about that?

But I would like to have Mr. Smith conclude the statement he was making with reference to the political aspect of the question that I raised because I think it is quite important to have it in the record. I would like to have Mr. Smith conclude the statement he started to make.

Mr. SMITH. There is one other point I think we will have to look out for. That is that the Soviets in the last year or two have made quite a point about an apparent American willingness to make offers specifically in the disarmament field. Mind you, this was a propaganda line.

As soon as other countries have a disposition to accept their offers, the United States, according to the Soviets, pulls back—welshes, in effect.

Now, I think they would be given a marvelous propaganda opportunity here if, after the President made this proposal in 1953, and the Congress of the United States in 1954 specifically offered an avenue for us to proceed under in sections 124 and 123, and we proceeded under it in reliance on that authority to negotiate the statute, and we got a lot of countries to agree to a very unpleasant system of safeguards—some 80 countries have signed this statute; we think a great many will ratify it—after having gone down that avenue, if the United States then withdraws the authority which the executive branch has to support this Agency, I think we will be wide open to

that claim that we have made a proposition and now we are pulling back from it.

I think that will have intense political repercussions.

Senator PASTORE. I am happy to have that in the record.

Mr. WADSWORTH. Could I add one more thing on this basis of the original suggestion by Senator Bricker, without any of the possible amendments? In the event that this were attached to the Participation Act and in the event that the act were passed and signed by the President, this could have only two effects, as I see it, on the International Agency.

The one has already been referred to by Mr. Smith, which is the question of our going emptyhanded to the General Conference and the loss of influence and prestige as far as the Agency is concerned.

The second is a far more practical and not as political a consideration. Let us assume that if we go emptyhanded to the Agency, that the only other countries capable of giving any amount of fissionable material do not put up a great deal; this will mean that the Agency in reality will not operate.

Any country that wishes to get up a project to receive Agency assistance must go through a tremendous expense and a great deal of trouble working out the project with the Agency; then the Board of Governors has to examine it in accordance with all those criteria that we mentioned, and there is a tremendous amount of difficulty which will be experienced, particularly by the applicant country.

It stands to reason that if there is no certainty that an applicant country will receive any material, will receive any of this assistance, it will not even bother to make application.

For if they look many months ahead and see at the end of this particular road on which we are traveling that there must be a specific authorization by the Congress of the United States which cannot be guaranteed at all, which cannot even be held forth, they won't apply. Then there won't be any use for an Agency.

That is an oversimplification, of course, but I feel it is something that should be thought about.

Senator BRICKER. In other words, you do not trust that the Congress will pass any acts that will be reasonable?

Mr. WADSWORTH. That is not the point. The point is timewise for one thing.

Senator BRICKER. How soon do you expect to put this in operation? How soon do you expect us to deliver this material?

Mr. WADSWORTH. The Congress may not be in session at the time the project is completed.

Senator BRICKER. Will you please answer my question: When do you anticipate you will be delivering this material?

Mr. STRAUSS. May I answer that?

Senator BRICKER. Yes.

Mr. STRAUSS. It may be some years before material is delivered.

Senator BRICKER. Some years?

Mr. STRAUSS. Yes, but the project will have to be approved much earlier.

Senator BRICKER. So the argument of the Ambassador will be completely false. Congress will have plenty of time to act before you will be able to deliver any material. We can look at it and we can determine what terms it will be sold on, and how much.

Mr. WADSWORTH. I construed your amendment as requiring that when the United States receives a request for materials for a project, that then the Executive comes to the Congress.

Senator BRICKER. I anticipate there will be a general statute on it. I do not think there is any question about it.

Mr. WADSWORTH. It would be in advance then.

Senator BRICKER. Certainly. It could be just as well as afterward.

Mr. WADSWORTH. That is not the way I read your amendment.

Senator BRICKER. It would not change the Constitution or the power of the Congress to act.

Mr. WADSWORTH. I still believe if it should go to the point I described, and I am glad to be corrected on that, then indeed it would be catastrophic to the Agency. If that is not the case, then my argument does lose a good deal of validity.

But that still does not mean that we believe that this thing should go on this particular piece of legislation. I would like to just deal with that for a moment.

Mr. Chairman, the entire text of the Participation Act is as you see a simple formula for setting up representation to the Agency. In my opinion an amendment to this, having to do with transfer of materials, would be slightly incongruous.

I don't think it would be illegal. It would be something that would stick out like a sore thumb.

Senator PASTORE. And raise suspicion.

Mr. WADSWORTH. And raise suspicion, also. So I believe that an amendment of the type suggested by Chairman Strauss, if the Congress wished to make more limitation on the way in which materials can be transferred abroad, would be far more proper.

I believe, in common with Congressman Cole, if we got to a position where we felt we could no longer remain in the Agency we would repeal the Participation Act and go through the other things that have already been set forth in the understanding of Senators Knowland and Hickenlooper and the Executive pulls you out of the organization.

Chairman DURHAM. We have handled all the military application of weapons and everything through the AEC. All that legislation comes through AEC amendments.

Mr. WADSWORTH. Transfer of materials of course always will come through the AEC. This Participation Act is merely to allow the United States to send representatives to the IAEA, and it talks about their pay and travel allowances and things of that sort.

I don't believe this amendment would be particularly proper for that kind of legislation.

Senator PASTORE. On this subject of incongruity that was raised by Ambassador Wadsworth, with reference to putting some limitation as to a gift or contribution, would you want to see that in the basic law of 1954, or would you write it in the implementation legislation?

Mr. STRAUSS. My preference would be to see it in the basic law, but it is not a strong feeling. I don't believe that counsel see any difference in it.

I am impressed by Secretary Wilcox's point that the Participation Act should stand alone, since it seems to involve in his mind, and may very well in the minds of others, some unspecified shift of authority or responsibility from State to Atomic Energy Commission for a foreign relations aspect.

Certainly that was not intended. We do have some parental feeling of responsibility for this Agency which is understandable, but we would not expect, and are not staffed or equipped, to operate another small State Department in the Atomic Energy Commission.

And we have very close and satisfactory working arrangements with the Department of State. I do not suppose that any two organizations in the Government have ever been in such continuous daily contact as we are.

If the amendment which I have suggested should meet with the favor of the Joint Committee, I think the proper place for it would be in the Atomic Energy Act since it in effect qualifies sections 123 and 124 and could refer to them specifically.

Chairman Durham. At the present time you are already dealing with the State Department on agreements. This is nothing more than an added number of agreements. That is what it amounts to.

Representative Cole. I fail to see where there is any greater responsibility on the State Department with respect to the International Agency than presently exists with respect to bilateral agreements or is anticipated will exist with respect to the Euratom agreement.

Mr. Smith. If I may speak to that, Mr. Cole, the relationship between the United States and this Agency will not only be a matter of making agreements; it will be a matter of continuous day-to-day and week-to-week and month-to-month policymaking, and I think we should not think of this as just an extension of agreements for cooperation.

We are going to be working to set up health and safety codes, security systems. Now, in all of these things there are large political implications.

Representative Cole. Could your objection not be readily resolved by providing in the language in the participation act, even though it is by way of amendment to the basic atomic-energy law, that this shall be under the direction of the Secretary of State?

Mr. Smith. I think that basically it is a matter of legislative artistry on which I would be happy to bow to you, but my feeling is that it would be as inartistic to provide for United States participation in this Agency by amending the Atomic Energy Act as it would be to provide for United States participation in the United States International Labor Organization by amendment to the Wagner Act.

Representative Cole. Those are not comparable at all.

Mr. Smith. I think this is a question of setting up the relationship between the United States and International Agency which deserves separate treatment.

Representative Cole. It is not comparable at all. The Wagner law is domestic law. The Atomic Energy Act is both international and domestic.

Representative Price. It started as domestic law.

Representative Cole. As of now it covers the field of atomic energy both at home and abroad.

Mr. Strauss. I think the place where this should be enacted, if indeed it is enacted, is of relatively less concern to the executive branch than it is to you gentlemen.

Senator Pastore. Mr. Strauss, may I suggest, then, that you have your legal department forward an amendment to carry out the suggestion that you have made?

Mr. STRAUSS. I would like to cooperate on that with counsel for the Department of State and submit it in very short order.

Senator BRICKER. You see the situation here we are in now. I proposed this as a reservation—whether binding or not is to be determined by what the Supreme Court says on the Niagara Power Treaty—to the treaty, feeling that it was simply preserving the constitutional relationship of the President and the Congress which I thought ought to be done.

I was told then by the leadership and by those on the other side that it ought to be a part of the participation act.

Now we come to the participation act and then we are told it ought to be a part of the Atomic Energy Act. I am finally going to run this thing down in a hole somewhere because I think it is sound law.

I think it is in conformity with the constitutional principles.

Mr. STRAUSS. I will buy it either way.

Representative COLE. I wonder if I may be permitted to go into another subject.

Senator PASTORE. Yes.

Representative COLE. I would like to have the observation of the witnesses with respect to a thought that has occurred to me. This International Agency, as I see it, is not of the general type of a special agency of the United Nations. It is for a specific purpose which is highly technical, specialized in its field.

Since most of the program has been under Government sponsorship and, therefore, most of these technicians are Government employees, it occurs to me that a great many of them, if at all, would be very loathe to leave their Federal connection and thereby sacrifice their retirement rights, whatever Federal emoluments they may have as a Federal employee, in order to go over to this Federal Agency.

I would like to inquire of you, any of you who wishes to respond, whether that objective has any merit, trying to revise our laws in such fashion as may be necessary to make it attractive to Federal employees in the atomic program to move over into the international program.

Mr. WILCOX. Would you want to limit that, Congressman Cole, to employees in the Atomic Energy program? It is conceivable that Foreign Service officers or officers from some other department and under civil service might want to transfer for one reason or another to the Atomic Energy Agency for a period of service.

We think this is a very laudable objective. In fact, the Department has been trying to do its best for some time to encourage qualified Americans to take responsible positions in other international organizations in which we participate. We think it is highly important that this objective be worked upon in connection with the Atomic Energy Agency.

Representative COLE. Have you found that these employees who would like to move into the international field are hesitant or refuse to make the transfer for that reason?

Mr. WILCOX. They are hesitant for 2 or 3 reasons. One is the objectionable features perhaps of uprooting themselves and their homes and moving abroad for a period of time. That is something we can all appreciate.

The second one is the salary factor. The wages and salaries of the United States are quite high and it is very difficult to attract able

people from responsible position in the United States to go abroad for a period of years and to take perhaps a reduction in salary.

The third factor is that those individuals who are working for the Federal Government find that the way is not entirely clear for them to take leave of absence for a period of 2 or 3 years because they do not retain their full retirement rights and retain other benefits which normally accrue to them under civil service.

I, therefore, feel that your suggestion in the direction of paving the way to attract competent people to accept positions in this Agency is a very laudable one and that we ought to do what we can to facilitate it.

Representative COLE. I did not have in mind limiting it to just atomic-energy scientists, but any Federal employee, of the Justice Department, or any Federal activity.

Chairman DURHAM. Do they not retain their rights now when they go to the United Nations?

Mr. WILCOX. Not all of their rights, sir. The provisions in our Federal employment setup to protect an employee's benefits are not sufficiently broad to encourage Federal personnel to take leave of absence to go into the United Nations to make a contribution there.

Now we are working on that and hope that the way will soon be paved for any employee of the civil service to make such contributions.

Chairman DURHAM. You mean if you take somebody out of the State Department at the present time and transfer them up to the United Nations to do a job, they lose all their civil-service retirement?

Mr. WILCOX. Well, sir, we have not done that——

Senator PASTORE. You do not mean to the United States mission to the United Nations. You mean to work under the direction of the Secretary General of the United Nations?

Chairman DURHAM. Yes.

Mr. WILCOX. There are two things involved. Our mission in New York under Ambassador Lodge and Ambassador Wadsworth does, of course, employ a good many people that are under civil service and there are some Foreign Service officers there.

But with reference to the secretariat of an international organization, we do not have a completely satisfactory method of placing on leave for a limited period of time Federal personnel who can make contributions to such an organization.

So we are working on that.

Senator HICKENLOOPER. Does the United Nations have a system of retirement, and so on, of its own?

Mr. WILCOX. It does; yes.

If a person wants to resign from the Federal service to go to the United Nations, of course, that is his option. He may do so, but I think what Congressman Cole is suggesting goes somewhat beyond that. It opens the way to a loan for a period of time, 2 or 3 years perhaps, of qualified people for special services.

I think, myself, it would be very helpful to us in our relations with this Agency and in our relations with other international organizations, in which we might participate.

Representative COLE. This is a subject which perhaps can be explored more fully in the executive session so I do not care to prolong the discussion today, but I would urge that Mr. Wilcox and the others

give some further thought and submit language which will accomplish what we have in mind.

Of course, that will not apply to the persons covered by the participation act.

Mr. WILCOX. No, that is correct.

Senator PASTORE. In other words, this subject you have raised, you have no intention of having it included in this?

Representative COLE. Yes.

Senator PASTORE. Then will you submit a statement with reference to that?

Mr. WILCOX. We will be glad to do it.

Senator PASTORE. So that we can study it in executive session.

Is that agreeable to you, Mr. Cole?

Representative COLE. Yes.

Senator PASTORE. Are there any further questions?

Is there any further statement desired to be made by any of the witnesses here?

Are there any other witnesses who desire to speak on this subject?

This is a public hearing. The Chair hearing none, the hearings will come to a close under the conditions we have made about the suggested amendment and the statement with reference to the civil service and retirement status.

Mr. STRAUSS. I would like to express our appreciation for the opportunity of appearing.

Senator PASTORE. It is a pleasure and joy to have you gentlemen.

(Thereupon, at 12:25 the Joint Committee hearing was concluded.)

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REVIEW OF PROPOSALS UNDER POWER DEMONSTRATION PROGRAM

HEARING

BEFORE THE

SUBCOMMITTEE ON LEGISLATION

OF THE

JOINT COMMITTEE ON ATOMIC ENERGY

CONGRESS OF THE UNITED STATES

EIGHTY-FIFTH CONGRESS

FIRST SESSION

ON

REVIEW OF PROPOSALS UNDER POWER DEMONSTRATION PROGRAM

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HENRY DWORSHAK, Idaho

JAMES T. RAMEY, *Executive Director*

SUBCOMMITTEE ON LEGISLATION

CHET HOLIFIELD, California, *Chairman*

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BOURKE B. HICKENLOOPER, Iowa
HENRY DWORSHAK, Idaho

DAVID R. TOLL, *Staff Member*

REVIEW OF PROPOSALS UNDER POWER DEMONSTRATION PROGRAM

TUESDAY, SEPTEMBER 17, 1957

CONGRESS OF THE UNITED STATES,
SUBCOMMITTEE ON LEGISLATION OF THE
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The subcommittee met at 1:30 p. m., pursuant to notice, in the Old Supreme Court Chamber of the Capitol, Representative Chet Holifield (chairman of the subcommittee) presiding.

Present: Representatives Durham (chairman of the full committee) and Holifield (presiding).

Present also: James T. Ramey, executive director of the Joint Committee, George Norris, Jr., committee counsel, John T. Conway and David R. Toll, professional staff members.

Representative HOLIFIELD. The hearing will be in order.

This is a hearing before the Subcommittee on Authorization Legislation, and the purpose of this hearing today is to hear an explanation of the basis of proposals under the power demonstration program negotiated by AEC with three utilities which have been submitted by the Commission for review by the Joint Committee by letters dated September 13 and September 16. The three proposed arrangements are with the following organizations: Consumers Public Power of Columbus, Nebr., Northern States Power Co., of Minnesota, and Rural Cooperative Association of Elk River, Minn.

It is the committee's intent as set forth in the legislative history of section 111 b. of the Authorization Act to consider the bases of these proposed arrangements with a view to determining whether the committee will waive the requirements that such arrangements be submitted while Congress is in session and to also waive the 45-day waiting period.

(The text of a resolution approved by the Joint Committee on September 19, 1957, waiving the 45-day requirement for the Consumers and Northern States proposals will be found on p. 63.)

Representative HOLIFIELD. We would like to have a status report today on the other outstanding co-op arrangements if the Commission is ready to give it.

The second purpose of the hearing is to obtain a status report from the Commission on other items in the Authorization Act in which the Joint Committee is particularly interested. This would include the gas-cooled reactor development and design study and the plutonium design study as well as the plutonium recycle reactor.

At this point I would like to put into the record a letter from Chairman Durham dated August 24, 1957, which explains the background

of this hearing. Also I would like to put into the record the Commission's letters of September 13 and 16, together with the backup information on the three cooperative agreements. Further, without objection, I would also put in the committee's letter to the AEC announcing today's hearing together with the press release on it, dated September 14 and 16, respectively.

(The documents referred to follow:)

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
August 24, 1957.

Hon. LEWIS L. STRAUSS,
Chairman, Atomic Energy Commission,
Washington, D. C.

DEAR MR. CHAIRMAN: As you know, the AEC authorization bill has become law and the Congress has completed the processes of appropriating funds for the AEC thereunder.

I am sure you recognize that the members of the Joint Committee took an active part on the floor and in the House and Senate Appropriations Committees in obtaining and restoring necessary AEC funds.

In the Joint Committee's report on the authorization bill and during the floor debate on the authorization bill, as well as the hearings and debate on the appropriation measure, it was made clear that the Joint Committee would do everything practicable to expedite consideration of the basis of arrangements under section 111b, when they are submitted for committee review by the AEC. In particular I believe it is the committee's intent to expedite review of those arrangements which were in the final stage of consideration by the AEC at the time of the authorization hearing. It was our understanding that the Elk River project and possibly the Consumers Public Power project of Nebraska were in this category. We also understand that the Northern States arrangement may also be ready.

I am making arrangements with Congressman Melvin Price, chairman of the Research and Development Subcommittee, for members of his subcommittee to review the basis of any arrangement submitted to the Joint Committee during the recess.

As you know, a number of the committee members are going abroad on official business to the Vienna Conference, departing September 20. In order therefore for the subcommittee to review the basis of any arrangements and for the staff to obtain the necessary waiver, it would appear advisable for the Commission to submit any such basis on or before September 13, at the committee offices. After a brief staff review, Mr. Price may decide to hold a short hearing on the project in which AEC, the utility organization, and the equipment manufacturers would be requested to appear. Following the hearing, if the project appeared to represent no substantial problems, I would plan to poll all members to waive the remainder of the 45-day period.

The Joint Committee after the Vienna trip would also be in a position to review the basis of additional arrangements beginning around October 20.

With respect to the projects in which the Joint Committee was especially interested in the authorization bill it is of course expected that the Commission will proceed with these projects in good faith in accordance with the intent of the committee and the Congress. This would include project 58-b-8, plutonium production reactor design; project 58-e-14, natural uranium, graphite moderated, gas cooled power reactor prototype design; project 58-e-15, plutonium recycle experimental reactor; section 107b (2), Brookhaven cosmotron project; and section 111a (1), (2), and (3).

Sincerely yours,

CARL T. DURHAM, *Chairman.*

ATOMIC ENERGY COMMISSION,
Washington, D. C., September 13, 1957.

Hon. CARL T. DURHAM,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR MR. DURHAM: In accordance with the provisions of section 111 (b) of Public Law 85–162, I am transmitting herewith for your approval the basis of the arrangements which the Commission proposes to enter into covering power reactor demonstration projects with the following organizations:

1. Consumers Public Power District of Columbus, Nebr.
2. Northern States Power Co., Minneapolis, Minn.

In connection with these projects, we have requested the views of the Comptroller General as to the authority of the Commission to enter into present contractual undertakings to make payments during the period of operation. We anticipate a reply in the very near future and will advise you as soon as it has been received.

Your letter to me of August 24, 1957, indicated that, if submitted to you by September 13, 1957, these arrangements would receive your early consideration. We appreciate the cooperation you are extending in this matter.

Proposed arrangements with Rural Cooperative Power Association, Elk River, Minn., are not being submitted, since agreement has not been reached, but negotiations are still in progress and we hope to be able to make a separate submission to you before the hearings take place.

Your letter of August 24 also expressed interest in our plans for other projects which were included in the fiscal year 1958 Authorization Act. I am, therefore, including in this letter a brief report on these projects:

(*a*) Project 58–e–14, natural uranium graphite moderated, gas cooled, power reactor prototype; development, design, and engineering only; report on design required by April 1, 1958: An invitation was issued on August 29, 1957, for architect-engineers to submit proposals to develop design criteria and prepare an engineering design and cost estimate of a gas-cooled civilian power reactor of 40,000 kilowatts (electrical). It is expected that a selection of the contractor will be made by mid-October.

(*b*) Project 58–e–15, plutonium recycle experimental reactor designed for the production of 15,000 electrical kilowatt equivalent: Design work previously initiated at Hanford is continuing on an accelerated basis. Current planning includes the placing of procurement contracts for long lead delivery time items in October–November 1957 with ground breaking and construction estimated to begin about January 1958. Construction time is tentatively estimated at 18 to 24 months.

(*c*) Project 58–b–8, production reactor for special nuclear materials; development, design, and engineering only; report on design required by April 1, 1958: Our plans regarding this project were reported to you in our letter dated September 4, 1957, from Mr. R. W. Cook, Acting General Manager.

(*d*) Project 57–h–5, cosmotron target area, Brookhaven National Laboratory: Major procurement action and the award of a contract for architect-engineering work are scheduled for October 1957, the project to include the second phase of modifications contemplated by the authorization of $3,550,000. Award of a contract for the construction of the building and installation of necessary equipment is scheduled for May 1958.

Sincerely yours,

H. S. VANCE, *Chairman.*

ATOMIC ENERGY COMMISSION PROGRAM JUSTIFICATION DATA FOR COOPERATIVE ARRANGEMENTS

Submitted to the Joint Committee on Atomic Energy pursuant to section 111 (b) of Public Law 162, 85th Congress, September 13, 1957, 58–111–1 Consumers Public Power District of Nebraska; 58–111–2 Northern States Power Co., Minneapolis, Minn.

ARRANGEMENT NO. 58–111–1

A. Name of contractors

1. Atomics International Division, North American Aviation Co., for research, development, and nuclear design.
2. Architect engineer and construction contractors to be selected.
3. Consumers Public Power District of Nebraska for operation of reactor.

B. Description of proposed project

1. Objective: The objective of the proposed contract with the Consumers Public Power District of Nebraska is to encourage and accelerate effective development of nuclear power technology and the achievement of commercially feasible nuclear power. A sodium-cooled, graphite-moderated reactor designed to produce 710,000 pounds of steam per hour and 75,000 net electric kilowatts of energy, will be constructed by the Commission. Consumers will provide the site and turbogenerating facilities and will operate the entire plant for 5 years. It is the intent of the parties that the plant will be operated for the primary purpose of producing the maximum amount of electric energy therefrom.

2. Location: Near Hallam, Lancaster County, Nebr.

3. Technical aspects (data are approximate):

Heat output: 245 megawatts.
Electrical output (net): 75 megawatts.
Amount of fuel loading: 23,800 kilograms.
Enrichment percent U-235: 2.5 percent.
Moderator: graphite.
Coolant: sodium.
Reactor outlet temperature: 925° F.
Reactor pressure: 50 pounds per square inch.
Steam conditions: 800 pounds per square inch gage at 825° F.
Byproduct utilization to be investigated by consumers.

4. Estimated date of completion of reactor: Mid-1961.

C. Estimates of cost

[In thousands]

Element of cost	Allocation of estimated costs		
	Total	AEC	Consumers
1. Preconstruction research and development	$18,165	$18,165	
2. Plant construction:			
(a) Reactor	29,233	24,013	[1] $5,220
(b) Turbo generating facilities	11,350		11,350
Subtotal construction	40,583	24,013	16,570
3. Postconstruction assistance		[2] 8,000	
4. Waiver of fuel use charges		1,325	
5. Total value of AEC assistance		51,503	

[1] Fixed amount, subject to escalation within 25 percent.
[2] Fuel and extraordinary maintenance costs.

D. General features of proposed arrangements

1. Architect-engineer and construction contractors (to be selected). It is expected that the Commission will enter into prime contracts with the architect-engineer and construction contractors under arrangements to be established.

2. Consumers Public Power District—

(*a*) *Site.*—Under the contract, Consumers will furnish the site for the plant, comprising approximately 600 acres and located near Hallam, Nebr. Title and control of the site will rest with Consumers. The area upon which the reactor portion of the plant will be located will be made available to the Commission by a 40-year rent-free lease.

(*b*) *Construction.*—Consumers will furnish the turbine generator facilities without cost to the Commission. Consumers will also furnish materials, equipment, facilities, and services for construction of the nuclear portion of the plant at a cost of $5,220,000. Insofar as is feasible, title to the facilities so furnished by Consumers will be retained by Consumers. The dollar amount of this assistance will be subject to escalation with a limit of 25 percent above or below the figure indicated.

The Commission will furnish the reactor facilities, except as noted above.

(*c*) *Operations.*—Consumers will utilize all the steam produced by the reactor for the generation of electricity and will operate and maintain both the turbogenerator and reactor facilities. Except for the first fuel loading Consumers will pay for fuel cycle costs up to an amount per kilogram of uranium metal

which has been tentatively fixed at $36.40, such amount being subject to upward and downward escalation based on the average cost of fossil fuels.

The Commission will pay for extraordinary repairs and maintenance required in operation of the reactor facilities. AEC will furnish the initial fuel loading and pay for subsequent fuel cycle costs in excess of the amount contemplated per kilogram of uranium metal incorporated into the fuel. There will be no credits to Consumers for plutonium recovered.

Consumers is assuming certain basic risks in the project, including the possibility that the fuel life will be less than the 3,000-megawatt days per ton objective, and the risk that operations, including ordinary maintenance and repairs, will be in excess of the amount estimated. Consumers therefore has every incentive to keep nuclear power costs to the minimum. However, in the event that Consumers is able to pay more for fuel than the amount presently contemplated because the cost of power generated in the nuclear plant falls below that of power conventionally generated, negotiations will be held to reduce the Commission's assistance on fuel costs.

(*d*) *Term.*—The contract will be effective upon execution and will continue for a period of 5 years after the nuclear facilities become operable as a power producer, with a maximum period of 6 years after sufficient steam is produced to bring the turbine up to speed and to synchronize the generator.

(*e*) *Indemnity.*—The Commission will indemnify Consumers against losses or damages arising out of the radioactive, toxic, explosive, or other hazardous properties of special nuclear materials.

(*f*) *Options to purchase.*—After completion of construction of the nuclear facilities, if Consumers fails to proceed with the project, the Commission has the right to purchase all of Consumers' interests in the overall facilities at a price to be agreed upon, but in no event to exceed the original acquisition cost less appropriate depreciation and less other pertinent considerations. If the Commission exercises its option to purchase, Consumers has agreed to purchase the power generated by the facilities if requested to do so by the Commission.

After 3 years from completion of construction of the nuclear facilities, Consumers have the right to purchase the Commission's interest in such facilities at a price representing the value of such property in the future production of power and other products.

(*g*) *Termination.*—The Commission may terminate for convenience of the Government at any time upon 30 days' notice, in which case payment would be made to Consumers in an amount equal to its capital investment in the reactor facilities less depreciation plus reimbursable costs incurred in conformance with the contract. Title to the facilities in question would then vest in AEC.

Consumers may terminate if it is unable to obtain satisfactory financing within 12 months from the date of execution of the contract. No payments will be made in the event of such termination.

(*h*) *Patents.*—In case an invention or discovery is made by Consumers in connection with the work under the contract, the Commission will receive a nonexclusive, irrevocable, royalty-free license for governmental purposes.

(*i*) *Licenses.*—The contract is considered one with and for the account of the Commission, and no licenses will be required, except for any byproduct or irradiation activities in which Consumers plans to engage.

(*j*) *Information and records.*—Consumers will provide the Commission with full technical and economic information which can be used by the Commission as it sees fit. The Commission and its representatives will have the right to inspect and audit the work, records, reports, information, data, and activities of Consumers under the project. The contract also includes the statutory GAO audit provision.

ARRANGEMENT NO. 58-111-2

A. Names of contractors

1. Prime contractor: Northern States Power Co., Minneapolis, Minn.
2. Principal subcontractor: Allis-Chalmers Manufacturing Co., West Allis, Wis.—for development, design, and construction of the reactor.

B. Description of proposed project

1. Objective: The objective of the proposed contract is to encourage private participation in the development of nuclear power technology and to accelerate the achievement of commercially feasible nuclear power. Under the contract, a nuclear reactor of an advanced boiling water type, designated as the controlled recirculation boiling water reactor (CRBR) designed to produce 66,000 net electric

kilowatts of energy will be constructed and operated by the Northern States Power Co. as part of its system.

2. Location: Site to be selected within the NSP service area (parts of Minnesota, North Dakota, and South Dakota).

3. Technical aspects (data are approximate):

Heat output: Reactor: 164 megawatts; superheater: 39 megawatts.
Electrical output: 66 megawatts.
Amount of fuel loading: 4,100 kilograms.
Enrichment percent, U-235: 1.6 percent.
Moderator: Water.
Coolant: Water.
Reactor outlet temperature: 489° F.
Reactor pressure: 600 psig.
Steam conditions: 500 psig, 825° F., exhausting at 1½ inches mercury absolute.
Byproduct utilization: None presently contemplated.

4. Estimated date of completion: June 30, 1962.

C. Estimates of cost

[In thousands]

Element of cost	Allocation of estimated costs		
	Total	AEC	NSP
1. Preconstruction research and development:			
(a) Performed by contractor	$5,000	[1] $5,000	
(b) Performed in AEC facilities	500	[1][2] 500	
2. Development, fabrication, and construction:			
(a) Reactor system:			
Research and development	4,000		[3] $4,000
Fabrication and construction	5,600		5,600
(b) Balance of plant:			
Research and development	200		[3] 200
Fabrication and construction	11,800		11,800
Subtotal, development, fabrication, and construction	21,600		21,600
3. Postconstruction, research and development	1,700	[1][4] 500	[3] 1,200
4. Waiver of fuel use charges		[1] 1,000	
5. Total value of AEC assistance		[1] 7,000	

[1] These are maximum amounts.
[2] Irradiation work and consulting charges by AEC.
[3] Research and development contributed in part by CUAPA members.
[4] Operating tests and inspection of fuel elements, etc.

D. General features of proposed arrangements

1. *Site.*—The site of the nuclear powerplant will be provided and selected by NSP and approved by the Commission.

2. *Construction.*—The nuclear powerplant will be constructed by NSP. Preconstruction research and development, design, fabrication, and construction will be subcontracted to Allis-Chalmers Manufacturing Co.

3. *Operation.*—NSP will operate the nuclear powerplant for 5 years as part of its interconnected system furnishing electric power service to parts of Minnesota, North Dakota, and South Dakota.

4. *Financial assistance.*—The Commission undertakes to furnish the following assistance:

Preconstruction research and development:	
Costs actually incurred by contractor	$5,000,000
Work and services furnished by AEC	500,000
Postconstruction research and development	500,000
Waiver of fuel-use charge	1,000,000
Total AEC assistance	7,000,000

NOTE.—The above figures are maximum.

NSP and 10 other electric utilities operating in the Midwest have formed a nonprofit corporation known as Central Utilities Atomic Power Associates (CUAPA). The 10 companies are to furnish a total of $3,650,000 to aid in the research and development program on this project and will receive training and technical information from the project. There will be no contractual relationship between CUAPA and the Commission.

Allis-Chalmers is to perform under subcontract, the preconstruction research and development, design, and construction of the nuclear powerplant. A substantial portion of its work will be on a fixed-price basis with NSP. To the extent, if any, that Allis-Chalmers costs on the work covered by the fixed price exceed the price Allis-Chalmers will be contributing financial assistance.

NSP will pay all costs associated with the project except as indicated above.

5. *Title.*—NSP will be the sole owner of the nuclear powerplant.

6. *Term.*—The term of the contract will extend until the expiration of 5 years after the issuance by AEC of a license to operate the facility, or 5 years after completion of construction of the nuclear powerplant, whichever is later.

7. *Indemnity.*—As a licensee of the Commission, NSP will be subject to the provisions of Public Law 256, 85th Congress.

8. *Option to purchase.*—In general, the Commission will have the right to purchase the nuclear powerplant if NSP fails to proceed with the project.

9. *Termination.*—The contract may be terminated by mutual agreement. NSP may terminate if it cannot obtain necessary licenses, regulatory agency approvals, or satisfactory liability insurance coverage.

10. *Patents.*—Title and all rights to inventions originated in connection with Commission-financed work will become the property of the Government with a nonexclusive, irrevocable, royalty-free license for NSP. On inventions originated under NSP-financed work, title will be in NSP, with a comparable license in the Government for Government purposes. All subcontract patent provisions will be subject to Commission approval.

11. *Licenses.*—NSP undertakes to obtain all necessary licenses from the Commission, and such other necessary approvals from Federal, State, and local regulatory agencies as are required to permit the construction and operation of the plant and the sale or use of the energy produced therein.

12. *Information and records.*—NSP will keep, and require its subcontractors to keep, records of technical, economic, and financial data developed in connection with the project and supply that information to the Commission for its use and dissemination. The Commission and its representatives will have the right to inspect and audit the work, records, reports, information, data, and activities of NSP and its subcontractors under the project. The contract will also include the statutory GAO audit provision.

ATOMIC ENERGY COMMISSION,
Washington, D. C., September 16, 1957.

Hon. CARL T. DURHAM,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR MR. DURHAM: In accordance with section 111 (b) of Public Law 162, 85th Congress, I am transmitting herewith proposed cooperative arrangements with the Rural Cooperative Power Association, of Elk River, Minn., and the American Machine & Foundry Co. It is requested that these arrangements be considered by your committee along with those of the Consumers Public Power District of Nebraska and Northern States Power Co., Minneapolis, Minn., which were transmitted to you on September 13, 1957.

With respect to all three arrangements, the Commission requests that the committee waive, pursuant to its authority under section 111 (b), the 45-day period for which the bases of such cooperative arrangements are required to lie before it. This is essential in the present instance, since the American Machine & Foundry Co. agreement is contingent upon execution of a definitive contract no later than October 18, 1957.

Sincerely yours,

K. E. FIELDS, *General Manager.*

97670—57——2

Program Justification Data

ARRANGEMENT NO. 58-111-3

A. Name of contractors

1. Rural Cooperative Power Association, Elk River, Minn., for operation of reactor.
2. American Machine & Foundry Co., for research, development, fabrication, and construction.

B. Description of proposed project

1. Objective: The objective of the proposed contract is to bring new resources into the development of engineering information on the performance of nuclear power reactors and to advance the time when nuclear power becomes economically feasible. A closed-cycle boiling water power reactor (CCBR) designed to produce 225,000 pounds of steam per hour and 22,000 electric kilowatts of energy will be constructed by A. M. & F. under contract with the Commission. RCPA, under separate contract with the Commission, will provide the site and turbo-generating facilities and will operate the entire plant for 5 years as part of its system.
2. Location: Elk River, Minn.
3. Technical aspects (data are approximate):
   - Heat output: 58 megawatts (excludes superheater, 17 megawatts).
   - Electrical output: 22 megawatts (nominal).
   - Amount of fuel: 1,320 kilograms UO_2; 3,050 kilograms ThO_2.
   - Enrichment percent U-235: 9.8 percent.
   - Moderator: H_2O.
   - Coolant: H_2O.
   - Reactor outlet temperature: 533° F.
   - Reactor pressure: 900 pounds per square inch.
   - Steam conditions at throttle; 600 pounds per square inch at 825° F.
   - Byproduct production: None.
4. Estimated date of completion: August 1960.

C. Estimates of cost

[In thousands]

Element of cost	Allocation of estimated costs			
	Total	AEC	AMF	RCPA
1. Preconstruction research and development:				
(a) Performed in AEC facilities [1]	[2] 500	[2] 500		
(b) Fuel element development	[3] 915	[3] 915		
Subtotal	1,415	1,415		
2. Development, fabrication, and construction:				
(a) Reactor:				
Reactor plant (including superheater)	6,115	[3] 5,115	[4] 1,000	
Fuel for 1st core (including 15 percent spares)	[3] 1,400	[3] 1,400		
Conversion UF_6 to UO_2	300	300		
(b) Turbo-generating facilities	1,750			1,750
Subtotal	9,565	6,815	1,000	1,750
3. Operation:[5] Estimated cost to AEC	[6] 1,640	[6] 1,640		
4. Waiver of fuel use charge	125	125		
5. Total value of AEC assistance		9,995		

[1] May be performed in private facilities to extent feasible.
[2] This is a maximum.
[3] Subject to escalation not to exceed 5 percent.
[4] AMF retains as fee up to $300,000 of underruns on $5,115,000 total. AMF pays all overruns.
[5] Includes costs of training, RCPA consultations with A. M. & F., and other directly reimbursable work.
[6] After sale of steam to RCPA (in the event of excellent reactor performance the cost would be reduced or might even become a credit).

D. General features of proposed arrangements

1. *Direct contract with RCPA.*—

(*a*) *Site.*—Under the contract, RCPA will furnish the reactor site, which will be adjacent to RCPA's existing steam plant at Elk River, Minn. The Government's right to use the site for the purposes of this project will be assured by appropriate registration of the Government's interest, and by provision for rent-free lease following expiration of the contract term.

(*b*) *Construction.*—RCPA will furnish the turbine generator facilities without cost to AEC. AEC will provide the reactor plant.

(*c*) *Fuel elements.*—AEC will develop and furnish the fuel elements required for the initial core loading. The fabrication of subsequent fuel elements will be the responsibility of RCPA.

(*d*) *Operations; sale of steam.*—The basic principle of the arrangement is one of assuring that RCPA as a consequence of the undertaking, will incur no loss, and that it will make no profit while operating the reactor for AEC. The contract will provide for operation of the entire plant by RCPA and for the sale to RCPA of all steam produced by the reactor plant. The steam will be consideration for operating costs and vice versa; however, because the value of the two will not necessarily be equal, provision for adjustment to fulfill the "no loss-no profit" concept will be made as described below.

Instead of reimbursing RCPA directly for the costs of operating the reactor plant and receiving from RCPA specified prices for the steam it receives, the contract will provide for AEC to pay RCPA for those costs actually incurred in operating its system with the reactor plant in excess of the costs it would have incurred in operating its system with a conventional boiler of the same capacity at the same location. If RCPA's system costs with the reactor are less than its system costs would have been with a conventional boiler, RCPA will pay the difference to the AEC. The formula outlined will come into play once the reactor has achieved criticality. In arriving at RCPA's costs of operating the reactor as part of its system, costs incurred by it arising out of the nuclear character of the entire project will be taken into consideration. Thus the arrangement contemplates that RCPA will be made whole—that is, placed in the financial position it would have occupied if it had constructed the conventional plant it otherwise would have built. This arrangement permits the AEC to eliminate RCPA's risk of loss, while avoiding any operating subsidy.

In addition, AEC will directly reimburse RCPA for all capital costs it incurs for replacements and additions to the reactor plant, for the costs it incurs in the preoperation training program, for the cost of the A. M. & F. subcontract it had negotiated and prepared, and for services desired by AEC and performed prior to achievement of criticality.

For these purposes, AEC will obligate $1,640,000 at this time. Any further payment by AEC will be subject to the availability of funds.

(*e*) *Training.*—AEC will conduct, at the Government's expense, a program of training for up to 12 qualified employees of RCPA.

(*f*) *Term.*—The term of the contract will commence on the date of execution and will end 5 years after the acceptance of the reactor plant by AEC as being completed, tested, and operable.

(*g*) *Indemnity.*—AEC will indemnify RCPA against losses or damages arising out of nuclear incidents in a manner consistent with Public Law 256, 85th Congress.

(*h*) *Sale of the reactor plant.*—At the expiration or termination of the contract, the Government will, in accordance with law, offer to sell the reactor plant to RCPA pursuant to the provisions of Public Law 162, 85th Congress. The contract will provide that, if such offer is not accepted, AEC will have an option to lease the site of the reactor plant.

(*i*) *Termination.*—The contract will be subject to termination at any time by written agreement of the parties. Any such termination will be effective in the manner, upon the terms, and on the date specified in said agreement.

The Commission will have the right to terminate the contract (1) if it determines that the construction or operation of the reactor cannot be undertaken or continued without undue risk to the health or safety of the public (and the parties have not agreed to mutual termination), or (2) if, after the Commission has paid to the contractor the sum of $3,040,000 less costs incurred by the Commission for fabrication of the initial core loading (including spares), termination is desired for the convenience of the Government. In the event of termination by the Commission, the Commission will pay to the contractor, as full

termination costs, the costs for which the contractor is then entitled to direct reimbursement plus the net incremental expenses incurred in the operation of its system for such time subsequent to February 1960 as may reasonably be required by the contractor for the installation of a boiler plant using conventional fuels. The Commission has no present intention to exercise the right to terminate for convenience unless it finds that the economic and technical promise of the reactor for commercial utilization by RCPA or others is unfavorable.

(*j*) *Patents.*—In case an invention or discovery is made by RCPA in connection with the work under the contract, AEC will receive all rights.

(*k*) *Licenses.*—The contract will be considered one with and for the account of the Commission and no licenses will be required.

(*l*) *Information and records.*—RCPA will maintain accounts showing all costs of operating its system. RCPA will also provide the Commission with full technical, economic and financial information and reports and the Commission and the Comptroller General will have the right to inspect the work, records, reports, information, data and activities of RCPA under the project. AEC may use the information as it sees fit.

(*m*) *Approval by Rural Electrification Administration.*—RCPA has contacted the Administrator of Rural Electrification Administration and outlined the provisions of this agreement. RCPA was advised by the Administrator that these agreements, as he understands them, would not impair the feasibility of the REA loan and would be satisfactory subject to review of the final draft.

2. *Direct contract With A. M. & F.*—(*a*) *Construction and test operation.*—A. M. & F. will develop, design, fabricate, and construct the reactor plant (reactor and superheater) and perform 6 months of test operation by not later than August 1, 1960.

(*b*) *Training.*—A. M. & F. will conduct a program of training for RCPA's operators.

(*c*) *Title.*—Title to the entire reactor plant and the fuel elements, and superheater will be in the Government.

(*d*) *Indemnification against nuclear hazards.*—A. M. & F. will be indemnified by the Commission in the same manner as other similarly situated Commission contractors in accordance with the provisions of Public Law 256, 85th Congress.

(*e*) *Termination.*—The contract will be subject to termination at any time by written agreement of the parties, any such termination to be effective in the manner, upon the terms, and on the date specified in such agreement.

If construction of the reactor plant is made impossible by inability of A. M. & F. to obtain necessary licenses or regulatory agency approvals, A. M. & F. will have the right to terminate at any time, by written notice to AEC.

Neither the Government nor A. M. & F. will be required to reimburse the other or pay directly for any costs or expenses by reason of termination of the contract, other than costs, expenses, and commitments previously incurred by A. M. & F. for which it is entitled to reimbursement under the provisions of the contract.

The Commission will have the right to terminate the contract at any time for convenience of the Government. Upon such termination, A. M. & F. would be entitled to reimbursement for those costs, expenses, and commitments previously incurred by it for which it is entitled to reimbursement under the provisions of the contract, plus reasonable costs of termination, plus A. M. & F.'s million-dollar investment in the program, to the extent such investment has actually been made to the effective date of termination.

(*f*) *Patents.*—Title to all inventions made or conceived in connection with A. M. & F.'s portion of the project will become the property of the Government, with a nonexclusive, irrevocable, royalty free license for A. M. & F.

(*g*) *Licenses.*—This contract will be with and for the account of the Commission and no licenses will be required.

(*h*) *Information and records.*—A. M. & F. will keep, and require its subcontractors to keep, records of technical, economic, and financial data developed in connection with the project and supply that information to the Commission for its use and dissemination. The Commission and its representatives will have the right to inspect and audit the work, records, reports, information, data, and activities of A. M. & F. and its subcontractors under the project. The contract will also include the statutory GAO audit provision.

SEPTEMBER 14, 1957.

Mr. K. E. FIELDS,
General Manager, Atomic Energy Commission,
Washington, D. C.

DEAR GENERAL FIELDS: This will confirm the information phoned to you this morning to the effect that the Joint Committee will hold an open hearing on Tuesday afternoon, September 17, at 1:30 p. m., in room P-63, Old Supreme Court Chamber, in the Capitol, on the following subjects:

1. To consider the bases of the arragenments which the Commission proposes to enter into with the Consumers Public Power District of Columbus, Nebr., and the Northern States Power Co., submitted with the Commission's letter of September 13 and the bases of such other arrangements as the Commission may submit on or before the hearing date.

2. To obtain a status report on the Commission's other negotiations under the power-reactor demonstration program.

3. To consider the status of the Commission's work on project 58-e-14, gas-cooled reactor prototype; project 58-e-15, plutonium recycle experimental reactor; project 58-b-8, production reactor for special nuclear materials.

It is understood that you will appear, together with the Assistant General Manager for Research and Development and representatives of the Reactor Development Division.

We are notifying representatives of the private and publicly owned utilities, and equipment companies which are involved in the projects that they can attend the hearings and be heard if they so desire.

Sincerely yours,

JAMES T. RAMEY, *Executive Director.*

No. 99
September 16, 1957
For immediate release

From the Office of the Joint Committee on Atomic Energy:

Congressman Carl T. Durham, Democrat, of North Carolina, chairman of the Joint Committee on Atomic Energy, announced today that an open hearing will be held by the Subcommittee on Legislation on Tuesday, at 1:30 p. m., in room P-63, United States Capitol Building, September 17, 1957, to consider the bases of arrangements between the Atomic Energy Commission and certain electric utility companies for the construction and operation of nuclear-power reactors.

Specifically the subcommittee will consider a proposal of the Consumers Public Power District of Columbus, Nebr., with regard to a 75,000 electrical kilowatt sodium graphite reactor, and a proposal of Northern States Power Co., Minneapolis, Minn., for construction of a 66,000 electrical kilowatt boiling water reactor. AEC advised they also may be ready to submit the bases of arrangements with Rural Cooperative Power Association of Elk River, Minn., for a 22,000-kilowatt boiling water reactor in time for the hearing.

Chairman Durham stated that the bases of arrangements between the Atomic Energy Commission and the first two companies were submitted by the AEC to the Joint Committee on Atomic Energy Friday afternoon, September 13, and the hearing was being held in order that consideration can be given to the possibility of waiving the normal 45-day waiting period and thereby permitting the signing of contracts as soon as possible. By law, unless waived by the committee, these contracts cannot be signed by the AEC until the bases for them have been submitted and lie with the committee for 45 days while Congress is in session.

Representative HOLIFIELD. In terms of procedure today, I believe, it will be helpful for the committee to lead off with an introductory statement by General Fields, together with an explanation by Commission representatives of the three agreements.

Following this explanation, if representatives of the utilities or the equipment company involved wish to make any comments we will be glad to hear from them. We understand that Mr. Schacht, of the Consumers Public Power District of Columbus, Nebr., would like to make a statement.

We understand Mr. Connell, of Senator Humphrey's office, will make a statement.

After these gentlemen have testified we would then expect that any others who wish to comment on the matters might be heard.

In that connection, we understand that representatives of some of the Nebraska cooperatives would like to be heard.

At this time, General Fields, we will ask you to proceed with such statement as you wish to make to the committee.

STATEMENTS OF GEN. K. E. FIELDS, GENERAL MANAGER; LOUIS H. RODDIS, JR., DEPUTY DIRECTOR, DIVISION OF REACTOR DEVELOPMENT; DELMAR M. MORRIS, ASSISTANT DIRECTOR FOR ADMINISTRATION, DIVISION OF REACTOR DEVELOPMENT; E. J. BLOCH, DIRECTOR, DIVISION OF PRODUCTION; DON S. BURROWS, CONTROLLER, DIVISION OF FINANCE; EDWARD DIAMOND, DEPUTY GENERAL COUNSEL; AND A. TAMMARO, ASSISTANT GENERAL MANAGER FOR RESEARCH AND INDUSTRIAL DEVELOPMENT

General FIELDS. Thank you, Mr. Chairman.

As you will see, I have a brief opening statement I would like to make. As you will see in this statement, I am modifying our one request to you which is a request for a waiver in the instance of the arrangements on the Rural Cooperative Power Association power proposal, in view of some developments that occurred just this morning, and I will touch on them, sir, in my statement.

Representative HOLIFIELD. You may proceed.

General FIELDS. It is a pleasure to be here today to ask your committee to waive the 45-day requirement of section 111 (b) of Public Law 162, 85th Congress, with regard to 2 cooperative arrangements under the Commission's power demonstration reactor program. I have a number of my staff with me who, I believe, will be able to answer any questions you may have when I have completed by brief introductory remarks.

As you well know, the Commission has issued three separate invitations for participation in its power demonstration reactor program. In connection with the first round invitation, three proposals were initially submitted to the Commission. Two of the three proposals, those made by Power Reactor Development Co. and Yankee Atomic Electric Co., have resulted in executed, definitive contracts with the Commission. The third proposal, that of the Consumers Public Power District of Columbus, Nebr., is now before your committee for consideration.

On January 7, 1957, the Commission issued the third invitation in connection with its power demonstration reactor program. Three proposals have been received to date, and the Commission has accepted one of these, that of the Northern States Power Co., as a basis for negotiation of a definite contract.

On September 13, 1957, in accordance with the requirements of Public Law 162, the Commission transmitted to the joint committee the bases for the proposed arrangements with Consumers Public Power District and with Northern States Power Co. We believe that

these submittals comply in every way with the requirements of section 111 (b) of that act. With respect to Consumers, the arrangement is confirmed by a certified copy of excerpts from minutes of meeting of August 30, 1957, of the Consumers board of directors, transmitted to the Commission on September 3, 1957.

The arrangement with Northern States is confirmed by a letter dated September 6, 1957, from Earl Ewald, vice president in charge of operation, Northern States Power Co., to W. Kenneth Davis, Director, Division of Reactor Development.

In the Commission's September 13, 1957, transmittal letter to the Joint Committee, we informed you that we had not been able, as of that date, to reach a final agreement with the Rural Cooperative Power Association of Elk River, but that negotiations were continuing and we were hopeful that we would be able to submit the basis for the arrangement with RCPA to the committee prior to the date of this hearing.

Yesterday, September 16, 1957, that basis of arrangement was transmitted to your committee. It incorporated a description of the arrangement which the Commission had with the American Machine & Foundry Co., the prime design, development, fabrication, and construction contractor for the Elk River reactor plant. The basis of agreement with RCPA was confirmed by a letter dated September 14, 1957, from Edward Wolter, general manager, Rural Cooperative Power Association of Elk River, Minn., and the basis of agreement with AMF was confirmed by a letter dated September 15, 1957, from Roy Snapp, divisional vice president, American Machine & Foundry Co.

The Commission had prepared telegrams to both RCPA and AMF accepting the bases of the arrangements sets forth in their letters to the Commission and was ready to transmit these last night, September 16, 1957, when a telephone call from Mr. Snapp, representing AMF, informed us that the company was not prepared to proceed on the basis of the ceiling price previously agreed to. We understand we are being advised formally of this by telegram. In view of this development, and because the arrangements with RCPA and AMF are necessarily interrelated, we were unable to transmit our telegram of confirmation to RCPA.

Because of these circumstances, I called Mr. Durham and Mr. Holifield, and I say "you" in my statement. I actually called Mr. Durham, whom I thought was going to chair the meeting. I called Mr. Durham this morning and also Mr. Ramey, to withdraw our request to your committee to waive the 45-day waiting period with regard to the proposed RCPA arrangement. This does not mean that we are abandoning this project; it only means that the Commission needs an opportunity to consider what steps it will take next. In no event will be be able to complete alternative arrangements before the end of October, and in view of that, we see no need for leaving the waiver request in effect at this time.

I doubt that we will be able to complete alternative arrangements either with AMF or someone else before the end of October, and in view of that, we see no need for leaving the waiver request in effect at this time. As a matter of fact, we do need to review the project in this light completely, as well as to consider other means to proceed.

I would add to this statement that I received at 12:30 today a copy of what will be delivered in telegram form from Mr. Snapp confirming this, and in effect as being for a considerable increase in the ceiling price that we had considered before.

Representative HOLIFIELD. Do you have that ready to submit for the record?

General FIELDS. I have it in draft form. I have had a short time to review it. I can tell you what it relates to, and I can submit it for the record, yes, sir; but not right at the moment I don't believe. I do have a rough draft of it here. It has come through in a dictated form.

(The telegram referred to follows, together with a subsequent statement by the American Machine & Foundry Co. requested on p. 62, and a subsequent letter from AEC regarding developments since the hearing:)

COPY OF TELEGRAM TO AEC, RECEIVED 1:26 P. M. SEPTEMBER 17, 1957, FROM AMERICAN MACHINE & FOUNDRY CO.

On September 14, 1957, AMF authorized transmittal to AEC of a letter outlining certain tentative understandings regarding construction of a boiling-water reactor at Elk River, Minn., subject to later completion and execution of a definitive contract. AMF has reconsidered and reappraised further the unlimited financial liability which it will incur under the proposal in this pioneering field and has concluded that the risk of losses to AMF stockholders will undoubtedly greatly exceed the $1 million investment which we have already made in this project and have continuously offered to make for furtherance of the power-reactor program in small reactors. Nevertheless our interest in this program remains as great as it was at the time our initial proposal was made. We, therefore, ask that the following change be made in the above letter and request that you seek approval from the Joint Atomic Energy Committee as part of our understanding. As you know, the contract is a cost-type contract without fee, unless the final cost is less than the present ceiling (as specified). This we propose not to change. The contract should have a ceiling recaste in the light of the most recent reactor cost "experience" which have emerged during negotiations. We believe that reactor-cost experience (including research and development) show ranges from $750 to $1,000 per kilowatt in reactors of greater power than the Elk River reactor and that smaller powered reactors range from $1,500 to $2,000 per kilowatt, we, therefore, feel that the present ceiling at $562 per kilowatt is inadequate and inequitable in view of the "cost without fee" type of contract. The figure of $562 per kilowatt is derived as follows: Ceiling $8,930,000 less $500,000 for superheater (to produce 7,000 kilowatts out of total 22,000 kilowatts) equals $8,430,000 divided by 15,000 kilowatts, or $562 per kilowatt. (The present ceiling was arrived at 9 months ago and we have been denied all requests for adequate revision.) We feel that a fair ceiling would lie somewhere between the ranges but, in view of our hope that a quick approval may be obtained, we have selected for this offer the lowest figure in the range ($750 kilowatt). By the above formula we obtain a ceiling of $11,750,000 as follows: $750 times 15,000 or $11,250,000 plus $500,000 for superheater (7,000 kilowatts) or $11,750,000. We do not believe that the Joint Atomic Energy Committee, in enjoining the AEC to "maintain existing economic advantages" in second-round contracts, meant that ceilings should be maintained which at the time of signing the contract are clearly so inadequate that the contractor must lose money over and above his set "contribution."

SEPTEMBER 18, 1957.

Hon. CHET HOLIFIELD,
Chairman, Subcommittee on Legislation,
Joint Committee on Atomic Energy,
The Capitol, Washington, D. C.

DEAR MR. HOLIFIELD: This letter is in response to your invitation yesterday to Mr. Roy B. Snapp, vice president and group executive for the atomic-energy group of American Machine & Foundry Co. to submit a statement for the record of the

hearings before your subcommittee on the matter of the increase in cost estimates for the boiling-water reactor to be built at Elk River, Minn.

First, I want to express our extreme regret that the decision on the ceiling costs of the AMF proposal had not been reached in time to permit an adequate presentation to the Atomic Energy Commission prior to the appearance of representatives of the Commission in connection with the request for a waiver of the 45-day requirement under section 111 (b) of Public Law 162, 85th Congress. Accordingly, the Commission felt it necessary to withdraw the request for the waiver of the time requirement with regard to the nuclear powerplant for the Rural Cooperative Power Association of Elk River, the initiating sponsor of the project.

I trust the members of your subcommittee did not interpret this action as indicating withdrawal of our proposal. We are seeking an immediate review of the original cost estimates with the Commission staff and hope very much that the proposed new philosophy reflected in our revised proposal can be accepted by the Commission as a sound basis for a definitive contract.

In brief, we asked that the Commission increase our cost estimate from $8,930,000 to $11,750,000 for research and development, design, construction, fuel-element manufacture, test operation, and operator training. We propose to pay up to $1 million of this amount.

As you know, the proposal is for a cost-type contract without fee. We seek no change in this arrangement.

We have consistently opposed the principle of cost ceilings for this type of developmental project. We believe that the manufacturer's contributions should be straightforward, as in the case of our million-dollar offer, and that the true nature of forward cost estimates for research and development work be recognized as not properly applicable for fixed limitations.

But whenever the so-called ceiling limitation is imposed, it should be determined from the most recent cost experience available. As far as we have been able to determine, the actual power reactor cost experience to date, including research and development, shows ranges from $750 to $1,000 per kilowatt of electrical output in reactors of greater power than the Elk River reactor, and from $1,500 to $2,000 per kilowatt for reactors smaller than Elk River.

If one excludes the nonnuclear energy input from the superheater, the per kilowatt cost of the Elk River reactor is $562 under the original estimate and $750 under the new estimate. We actually arrived at our new estimate by selecting the lowest figure in the range of actual recorded experience with power reactor costs.

This seems to be a very reasonable procedure, inasmuch as no data is available to us indicating that anybody has been able to build a power reactor at lower cost. We have asked the Commission to sit down with us and help us to come to a determination of the proper estimates through a complete analysis of the mass of cost data accumulating throughout the atomic power development program.

We understand the very serious concern with which the members of the Joint Committee may be expected to view the inevitable delay in the start of construction of this project, we share the Commission's determination to have a test demonstration of this reactor system at the earliest practicable time, and we understand the disappointment of the members of the Elk River Cooperative. In the face of these urgencies we were forced to recognize that no other course was open to us.

As indicated in the testimony of Mr. Kenneth E. Fields, General Manager of the Commission, the change in our position was made abruptly, between the time of delivery of a letter outlining our general understandings on Saturday, September 14, and the morning of the 17th, when we notified the Commission of our reconsideration and reappraisal of the project and asked that changes be made in the cost ceilings.

We felt compelled to make this sudden change on the basis of three major factors. Perhaps none would have been compelling alone, but taken together they constitute adequate justification for the submission of new cost figures. In view of the almost national interest in the project, the clearly demonstrated desire of the Joint Committee to speed the project, and the basic objectives of the Atomic Energy Commission for small reactors in the power demonstration reactor program, we decided to ask for an increase in the prices only after the most thorough and thoughtful consideration at the highest corporate level.

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The three major factors were—

1. The risk of losses to the company threatens to exceed substantially the million-dollar contribution which we propose to make to this project and, in fact, have already invested.
2. The cumulative cost experience with other power reactors already built and operating, or nearing completion, is the most dependable cost data available to us and we cannot in conscience and responsibility ignore it.
3. We determined that the policy objectives of the national atomic energy program as defined in the Atomic Energy Act of 1954 and as expressed by the Commission for the establishment of an atomic power industry on a sound economic basis, could be best served by a forthright acknowledgment of the economic facts which confront us and a determined effort to conduct all developmental operations on the most realistic cost basis possible.

As we told the Commission on September 17, our interest in this project remains as great as it was at the time our initial proposal was made. We believe it is important to the Nation's atomic power development program that the system designed for Elk River should be carried through to full scale test. This will be the first thorium-fueled boiling water power reactor and is of unique technological interest. We believe that this reactor concept should be pursued without regard as to who may be selected as the contractor.

In response to your direct question as to whether the assignment to the Commission under Public Law 162 of new contracting responsibilities for this and other projects has anything to do with our cost changes, the answer is no.

As a matter of fact, we see the new contracting arrangement as decidedly advantageous from the standpoint of the cost to the Government, as well as from the standpoint of the Elk River Co-op as sponsor. From our own standpoint, the direct contract relationship with the Commission is simpler and consequently should mean lower costs and more expeditious conduct of the project.

The underlying reasons for my feelings on this point had been fully brought out during the course of the negotiations. In part, our final decision to change the ceiling figures was a result of the refusal by the Commission to give adequate consideration to what your committee has described as "some protection of the equipment manufacturer for increases in cost arising out of developmental problems beyond its control * * * provided on a cost-sharing basis."

Since assuming its new role under Public Law 162, the Commission has had no occasion to reappraise our proposal from the perspective from which we now seek to have it considered.

The reappraisal which we have made, and which we now ask the Commission to make, is from the perspective that it simply does not make sense—for the Commission, for the public, nor for the concerned contractor—regardless of high motives in patriotism and public service, to carry out an important developmental project such as this one under any conditions other than those which will preserve and strengthen the capabilities of the contractor for future work in the field.

One of the declared purposes of the basic atomic energy law of the Nation is to strengthen free competition in private enterprise. "Buying in" to a developmental project through a cost-absorbing proposal impossible for competitors of lesser resources, with a view to future recovery of losses, is not in keeping with the spirit of the law, and we do not believe it makes sense from any standpoint. It is my firm belief that the Congress, the Commission, and the American people want the national atomic power development program to go forward on the soundest possible basis, with full recognition and disclosure of the cost facts.

I believe it is of the utmost importance to the progress of the Commission's program that all of the developmental projects be financed on a basis of full consideration of the requirement for a sustained effort over a period of many years. Otherwise, it will not be possible for American industry, perhaps with the exception of a few of its giants, to continue at high levels the developmental effort that is clearly called for if America is to maintain its position in the field of atomic energy.

Please accept my apologies if the circumstances of this change have in any way inconvenienced the Committee. I am aware of the extent to which our action has affected the Commission's plans, and I would like to assure both the Joint Committee and the Commission that we will do everything in our power to make up for the lost time.

Thank you for this opportunity to submit a statement of our position. If there is any additional information which the members of the Committee or its staff may desire, we will be happy to supply it.

Yours very truly,

MOREHEAD PATTERSON, *Chairman.*

ATOMIC ENERGY COMMISSION,
October 8, 1957.

Hon. CARL T. DURHAM,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR MR. DURHAM: The purpose of this letter is to inform you of the developments which have taken place on the Elk River situation subsequent to our last meeting with your Committee on September 17, 1957. You will recall that just prior to that meeting, I received word from Morehead Patterson, chairman of the American Machine & Foundry Co., which nullified the previous AEC-AMF understanding and proposed a new ceiling cost to the Commission of $10,750,000 for the development and construction of the demonstration power reactor for the Rural Cooperative Power Association. I am sure that this action has proved as disappointing to the Joint Committee as it has been to the Commission. Had AMF adhered to their agreement, we fully expected to execute a contract within short order, subject to the waiver by the Joint Committee of the 45-day waiting period.

During the past 3 weeks we have been in constant contact with AMF in an attempt to reach agreement on a revised basis. In their most recent proposal, AMF would assume, in addition to the previously agreed-to figure of $1 million research and development cost, a 25-percent share of any cost overrun beyond $8,930,000 up to a total AMF overrun liability of $1.2 million, with AEC to assume any excess beyond that figure. The Commission has declined to accept the open-ended responsibility which this proposal requires and has decided to issue a general invitation for bids for a cost-type, maximum ceiling price, contract for the RCPA power-reactor system. We plan to announce this invitation very shortly, with the details of the invitation to be issued soon thereafter.

In the general invitation for bids, we will ask the contractor to—

(*a*) Undertake design, development, fabrication, construction and initial startup of a closed-cycle, boiling water reactor and its necessary appurtenances, including first core and separate fossil fuel-fired superheater, suitable for installation in the RCPA system and capable of generating 22,000 kilowatts of net electricity.

(*b*) Test operate the plant.

(*c*) Train RCPA operator personnel.

The invitation will ask that bids indicate a maximum ceiling cost to the Commission. A ceiling level will not be specified. Bids are to be submitted within 90 days of the invitation and the Commission is to be notified within 30 days of intent to bid.

There has been no lessening of our conviction that the installation and operation of a low-power, closed-cycle boiling water reactor in a commercial system would make an important contribution to the attainment of economic nuclear power. We believe, however, that there is a reasonable probability of achieving this goal at a lower cost to the Government than is represented by the AMF proposals. To our knowledge, other equipment manufacturers are keenly interested in developing a market for boiling water reactors and, through the adoption of component design, fuel element configuration, or reactor structure differing from those contemplated by AMF, may succeed in accomplishing the objective at a lower cost.

We have been in touch with the Rural Cooperative Power Association on these recent developments. They have agreed to hold open their offer to participate with the Commission in a demonstration project essentially on the same terms as contained in our present agreement.

Sincerely yours,

R. W. COOK, *Acting General Manager.*

General FIELDS. As you can see, I am still awaiting a formal delivery of the telegram. It does say that they will be unable to proceed for reasons that they feel that the cost experiences to date are such that the ceiling should be raised.

You see, this arrangement was on a cost basis with AMF with a ceiling price in it. If the cost of the construction went above that ceiling price, this was to be a cost that they would then have to absorb. They accepted the ceiling-price arrangement in a cost-type contract but they feel that they need something like $3 million more in the construction for this.

Representative HOLIFIELD. What was the original estimated cost?

General FIELDS. The first proposal, I think we have that here, was a little over $3 million. I will have to check that. I will have to supply it for the record, sir. The first proposal is different than what we submitted to you yesterday and it was less even than submitted then.

(The information referred to follows:)

Mr. A. V. Peterson, vice president of AMF Atomics, Inc., in a letter to the Rural Cooperative Power Association, dated November 17, 1955, quoted AMF's estimated total price for design, construction and preliminary operations as $2,985,000. He also quoted the guaranteed maximum price for the complete engineering and construction of the reactor plant as $3,760,000.

Representative HOLIFIELD. On the second round I have a proposal here dated June 19, that is, a staff memorandum dated June 19, with analysis of all of the cooperative arrangements. This particular staff submitted an estimated total plant cost of $8,136,000. The reactor and superheater by AEC, at $5,686,000. There was a site and related facilities value by RCPA, $700,000, and electrical generating facilities, $1,750,000 by RCPA. That made a total of $8,136,000.

Now, I believe that is what you submitted to the committee during the hearings; is it not?

General FIELDS. I believe it is; yes, sir.

Representative HOLIFIELD. That was raised over the original estimate?

General FIELDS. Yes. You asked me to the original and I will have to check that. The $3 million, as I recall it, for the reactor plant itself was there, and there are other costs that total the $8 million figure you are quoting.

Representative HOLIFIELD. The reactor as submitted here was $5 million?

General FIELDS. I think the $5 million figure is the one that relates back to the original estimate which was $3 million to $3.5 million against the $5 million, and I believe the other factors are approximately the same as in their original proposal.

I would like to confirm this for the record.

Representative HOLIFIELD. You tell the committee now that they are asking for about $3 million increase over the $5,686,000?

General FIELDS. Yes, sir.

Representative HOLIFIELD. Is that a firm price, or is that something else?

General FIELDS. It is proposed that this would be a ceiling on a cost-type contract with them. Thus, if they went over the total amount of this new ceiling, that would be a fixed bid for that amount.

If the costs were actually less, since it is a cost-type contract, then it would be less to the Government than this ceiling price.

Representative HOLIFIELD. On page 203 of the hearings, the figure of $3,760,000 was given for the reactor.

General FIELDS. I believe that is the one I referred to.

Representative HOLIFIELD. Later it was raised to $5,686,000, which was presented to us also at that time as a raise. Currently it was the estimated cost. Is it the $3 million figure that they now request, or is it more?

General FIELDS. The total in the ceiling overall, you see there are several items in this total for AMF to perform—as I understand it on quick reading of this, it is that this new ceiling is something like $3 million over the old ceiling that they would like in their contract.

Representative HOLIFIELD. That would bring it up in round figures to around $11 million.

General FIELDS. That is true.

Representative HOLIFIELD. Are there any other differences in their proposal?

General FIELDS. I haven't had time to analyze it, Mr. Chairman, but that is the significant one. As I understand it, they do accept all of the other provisions that we have negotiated with them on this.

Representative HOLIFIELD. We were told repeatedly during the debate, and I remember Senator Thye, of Minnesota, saying on the floor that this was lying on the desk of the AEC, and all it needed now was a go-ahead signal, and they were ready to sign. Something has happened in the meantime, apparently. What has happened in the last 6 weeks? What would cause them to change their mind on that?

It is about 3 weeks, I guess it is. What has happened to change the picture? Has there been any less advantageous terms offered to the Elk River people or to the AMF than was offered to them before?

General FIELDS. I don't believe that there have been any changes in these terms.

Representative HOLIFIELD. The Commission hasn't made any tighter requirements on them, have they?

General FIELDS. Not that I am aware of. As a matter of fact we went back to them on the basis required in the authorization act where we would have the contract as between the Commission and AMF rather than as between Elk River and AMF which was the basis upon which we had been proceeding, as late as yesterday we thought we had an agreement as between the three parties.

Representative HOLIFIELD. There was no objection on their part to dealing directly with the Commission?

General FIELDS. I am not aware of any.

Representative HOLIFIELD. They didn't state any objection? This change, then, of the amount has nothing to do with the legislation?

General FIELDS. Mr. Chairman, I cannot speculate as to what led to this change in the last 24 hours, not having had the opportunity to discuss it completely, to any extent, really, with the AMF representatives.

Representative HOLIFIELD. In your opinion, this is going to be a forerunner now of all of the rest of these cooperative arrangements? Do you think this is the beginning of a pull-out on the part of the manufacturers from going ahead on their contracts?

General FIELDS. I hope it is not. We have had, of course, the instance of the Wolverine contract.

Representative HOLIFIELD. Let us hear about that. What was that?

General FIELDS. This is somewhat similar, where the Foster-Wheeler Co. found that they could not proceed on the basis of the costs that they had estimated for the construction of the reactor for that purpose.

Representative HOLIFIELD. What was that?

General FIELDS. So they withdrew their offer.

Representative HOLIFIELD. What was the price that they had agreed upon as presented to us?

General FIELDS. Roughly $5 million as I recall, Mr. Chairman. They wished to increase this very substantially for this reactor to the neighborhood of $14 million.

Representative HOLIFIELD. We have here, under the same date of June 29, a reactor cost by the AEC of the original $2,835,000, and currently estimated cost at that time, on page 203 of the hearings was $3,837,000. As I remember this, this was the Wolverine Cooperative up in Michigan which showed a profit of $18,000 on their last year's operations.

Now they are raising their estimate on that from $3,837,000 to how much?

General FIELDS. I am not sure they are corresponding figures. The comparable figures that I have, Mr. Chairman, on August 23 to the joint committee were that the authorization for appropriations was $5,472,000, and the company now states it is prepared to undertake the work on a cost reimbursement basis only, and further estimates that that would now total $14,426,000.

I would have to look through the records to find the compilation of this $5,472,000 but it is substantially for all of the work that Foster-Wheeler was to do under that proposal.

Representative HOLIFIELD. The first submission, did you say, was $5,400,000?

General FIELDS. Yes; that was the authorization.

Representative HOLIFIELD. That was a flat price, wasn't it?

General FIELDS. It was a ceiling price arrangement, I believe, in that instance, too.

Representative HOLIFIELD. That was a flat ceiling price?

General FIELDS. Yes, sir.

Representative HOLIFIELD. And I think it included some post-construction research and development, didn't it?

General FIELDS. That was not in the $5 million, it was a separate item and I would have to recheck the data sheets here.

Representative HOLIFIELD. I was trying to harmonize the figure of $3 million with that.

General FIELDS. I cannot do that without additional records here, sir.

Representative HOLIFIELD. It was $3,887,000, and I was trying to reconcile that with the figure you used. I felt it was different in that it was possibly construction development and research.

General FIELDS. It may be.

Representative HOLIFIELD. That has gone up three times and that is no ceiling price, that is an open-end contract.

General FIELDS. That is what they indicate would be the basis upon which they would have to proceed, if they proceeded. We have now under consideration some studies being performed of this by one of our contractors, to determine what course of action we think we will take with respect to Wolverine and we have not yet made such determinations.

Chairman DURHAM. What do you attribute the increase of cost to, on these items? Is it lack of experience in bidding on this sort of thing?

General FIELDS. I think that had quite a bit to do with it, yes, Mr. Chairman. We have had cost increases along the way quite a bit, and some of this may be overoptimism at that stage, or inexperience, and I don't know. I need to analyze this more carefully.

Chairman DURHAM. What are you doing with Wolverine at the present time? Are you negotiating with them?

General FIELDS. We are not negotiating with them at the moment.

Chairman DURHAM. You are not?

General FIELDS. No, sir. We are taking a look at this, and determining on the basis upon which we think it would be technically an attractive project, before we determine what course to take with respect to it.

Chairman DURHAM. What type of reactor is that?

General FIELDS. This was a so-called homogeneous, but a so-called burner upper, one region reactor which would burn uranium 235.

Chairman DURHAM. Is anybody else at the present time building that type?

General FIELDS. I don't believe so. We have a reactor experiment at Oak Ridge.

Chairman DURHAM. What about Elk River?

General FIELDS. That is a boiling water reactor.

Chairman DURHAM. You do have that type?

General FIELDS. There are several of those in various use stages and types. There are actual reactors, powerplants.

Chairman DURHAM. But the Elk River is entirely a new concept?

General FIELDS. No, sir; it is not. It is a boiling water reactor, and we have a boiling water powerplant at Argonne, General Electric has one that is built at Vallecitos, which is slightly different. This has a different cycle to it, an adaptation to it that has some attractive features that we think on a basis that we originally proposed this in conjunction with the authorization hearings, we felt it was a desirable addition to the reactor program. We still do on that basis. I should say that. Whether we will on a $3 million increase, I don't know. I haven't had time, since I got this, to even form in my own mind what I would recommend to the Commission if it had to be on the additional $3 million basis.

Chairman DURHAM. Would that require new authorization in the law?

General FIELDS. I am not sure. I am not sure whether this falls within the 15 percent latitude or not. I haven't had time to even consider that, Mr. Chairman.

Chairman DURHAM. I shouldn't think it would.

General FIELDS. Probably in the overall, it might fall within that limitation. We would still have to submit it to the Joint Committee

for the 45-day period or waiver but whether it requires additional new authorization would depend upon the actual total amount, and whether it fell within the latitude allowed there. But in the light of this, Mr. Chairman, we regret it, and I am sure that the Rural Cooperative Power Association whom we have been working with very closely and who have been very cooperative in proceeding and attempting to reach agreement here regrets it also. I think it has been going along excellently, and it is unfortunate that this has developed.

I can understand that at the last minute there may be considerations on the part of the AMF that overrule what they were proceeding on in good faith before. But since it has come up at the last juncture, I am not in a position, or the Commission is not, either, to come to you and ask for a waiver of this at this time. I think that we need time to review it and by the time that the committee can reconvene again, if they can, say, late in October or something, I would hope we would have this fairly clear in our mind as to how we wished to recommend that we proceed on it.

Representative HOLIFIELD. Well, we are present here with two of the cooperatives. How about the Piqua and Cugach? Have you heard from them? Are the people doing that work ready to ask for 2 or 3 times increase in their estimate?

General FIELDS. Not that I know of, Mr. Chairman. We have sent to Piqua and Cugach, a questionnaire designed to elicit more detailed information concerning their conventional operating costs, and in each of these cases, as you know, there is development work proceeding with respect to the contracts rights now. We have not as yet heard back from them in response to this questionnaire so that we haven't moved forward any further than that with respect to them. But there is development work on both of these going forward, as you know.

Representative HOLIFIELD. What is the date of the questionnaire that you sent out?

General FIELDS. We have been in touch with them before, but it was sent out on August 22, sir.

Representative HOLIFIELD. The committee would like to know if the change in the law has had anything at all to do with this revision of price on the part of the manufacturers.

General FIELDS. I cannot answer that. I have no opinion on that at this moment, sir.

Representative HOLIFIELD. Well, has it changed the power of the Commission to make a less advantageous deal, you might say, for the Government?

Has it caused this increase from $5 million to $14 million in the estimated cost of the Wolverine plant?

General FIELDS. I don't believe that you could say that the law has resulted in that change. At least that is my preliminary opinion, sir.

Representative HOLIFIELD. Would it be fair for the committee to draw the inference that after negotiating for from 18 to 24 months, that when you got to the point of actually putting the signature on the line, that you found out that you weren't as near ready to go ahead as you thought you were during the time of the consideration of the legislation for authorization?

General FIELDS. I don't believe so, sir. It is not in the case of the Elk River arrangements. Up to yesterday we were all ready to sign contracts with the information we had.

Representative HOLIFIELD. There was no action on your part, then, or no indication on your part to the machinery manufacturers that caused them to take this action?

General FIELDS. I don't believe so.

Chairman DURHAM. You were ready to sign the contract at the original cost?

General FIELDS. On the basis of the arrangement we had submitted to you as of Monday, yes, sir.

Representative HOLIFIELD. There was no communication from the Commission to the manufacturers in any way which caused them to revise their prices?

General FIELDS. Oh, absolutely not, Mr. Chairman. We have had negotiations with them, and we have been doing so on a basis of the arrangements as described in that letter yesterday to you.

Representative HOLIFIELD. So the decision of AMF Elk River came as a surprise to you yesterday?

General FIELDS. Yes, sir.

Representative HOLIFIELD. All right. Will you finish your statement, then.

General FIELDS. The two arrangements which now therefore lie before your committee are the result of detailed discussions between the respective parties and represent agreement of the parties to the principles underlying each arrangement. Definitive contracts respecting the two arrangements are in various stages of negotiation, and we anticipate that they will be ready for execution in the near future. Our letters of submittal dated September 13, 1957, and September 16, 1957, formally requested the joint committee to waive the 45-day period for which section 111 (b) of Public Law 162, 85th Congress, requires these cooperative undertakings to lie before it. We hope that our appearance here today will enable the committee to grant such a waiver with respect to the Consumers and Northern States, arrangements so that the parties may proceed to execute definitive contracts as expeditiously as possible.

The committee may recall that in the Commission's September 13, 1957, transmittal letter covering the arrangements with Consumers and Northern States the Commission pointed out that a recent opinion of the Comptroller General of the United States relating to materials procurement contracts had raised some doubts about the Commission's authority to enter into long-term contracts. Commission personnel met with representatives of the General Accounting Office, and we have on several occasions, but on September 12, 1957, specifically, we made a formal submittal of the Consumers arrangement to the Comptroller General on that day and of the Northern States arrangement on the following day. We have not, as yet, had any opinion from the Comptroller General on either of these formal submittals, but the General Accounting Office has them under consideration. If you so desire, I shall ask Mr. Diamond, the Commission's Acting General Counsel, to explain the nature of the problem to you.

Representative HOLIFIELD. I think that would be in order, because you are asking the committee to grant you a waiver on an arrangement which the Comptroller General has not rendered an opinion on, as yet, and when in fact such contacts as you have had with him have been of the nature of questioning your right to make that contract. I think we would like to hear from Mr. Diamond.

Mr. DIAMOND. As Mr. Fields just pointed out, we have submitted formal letters to the Comptroller General, requesting his opinion as to the validity of the proposed arrangements with Consumers and Northern States.

Representative HOLIFIELD. Under what date did you submit that?

Mr. DIAMOND. These were submitted I believe on September 13 and 14. I think those are the right dates.

Representative HOLIFIELD. September 13 and 14?

Mr. DIAMOND. One was sent on the 12th and the other on the 13th.

Representative HOLIFIELD. You were aware of this problem long before this. You were aware of this problem when Congress was in session, weren't you?

Mr. DIAMOND. I might say the problem arose originally in connection with the proposed procurement of magnesium. The question arose about 6 months ago when we submitted a letter to the Comptroller General in connection with that procurement. We received a reply in which the Comptroller General stated we didn't have the authority to enter into that arrangement. We then resubmitted the question because of the broad implications of the opinion that he wrote. We only received a reply to this resubmission from the Comptroller General within the last month.

Representative HOLIFIELD. On the magnesium contract?

Mr. DIAMOND. Yes, sir. He confirmed his earlier opinion and made some other statements that had implications that seemed to involve the type of arrangements that we were contemplating here, at least the principles were broad enough to raise some question.

Chairman DURHAM. Did you actually submit a draft of the contract or just the arrangements to the Comptroller General?

Mr. DIAMOND. We submitted draft contracts, at least in one case we submitted a draft contract.

Representative HOLIFIELD. How can you in good faith come before the committee and ask us to waive an arrangement which is under consideration by the Comptroller General and has not yet been decided to be in order?

Mr. DIAMOND. It is my opinion that the arrangement is a valid one, particularly in the light of the legislative history. Moreover, we have had informal conferences with the Comptroller General's office, and I think there has been general acceptance of this view informally. But we don't have any formal opinion from the Comptroller General yet. There might be a change, but I think the indications are based on our informal discussions that there is a very great likelihood that we will receive a favorable reply.

(Subsequent to the hearing the Joint Committee received a letter from the AEC forwarding the report of the Comptroller General on the proposed contract between the AEC and the Consumers Public Power District of Nebraska. The correspondence will be found on p. 64.)

Representative HOLIFIELD. Well, I am inclined to think that the legislative history shows that Congress did contemplate some long-time arrangements in these specific instances. But your opinion and my opinion, I am afraid, doesn't carry the same weight as the Comptroller General's may carry. It does put us in a rather peculiar position. You have a procedure possibly under challenge and ask us to waive the 45-day waiting period, where possibly during that

45-day waiting period, the Comptroller General, realizing the urgency of the decision of the committee, might be constrained to speed up his decision a little bit.

Mr. DIAMOND. I think this is one of the hurdles we have to overcome before we can finally enter into a contract.

Another requirement, that is the statutory requirements that we submit these arrangements to the Joint Committee, is a second condition we have to meet. We are trying to comply with these requirements concurrently.

Chairman DURHAM. Suppose he does rule that you don't have this authority?

Mr. DIAMOND. Then we will not be able to enter into the contracts.

Representative HOLIFIELD. What would be your procedure for the Commission under those circumstances?

Mr. DIAMOND. Depending upon what the opinion states, we might attempt to revise or revamp our contracts, if this is possible, or else perhaps ask for statutory authority in the next session of Congress.

Chairman DURHAM. How long has the question been before the Comptroller General?

Mr. DIAMOND. The formal submissions have been before the Comptroller General since the 12th and 13th of September. It is just a matter of days.

Chairman DURHAM. No lawyer can render a decision in that length of time.

General FIELDS. We have had it under discussion for sometime as a result of the previous submissions. I am under the impression that the Comptroller General is giving this some active consideration right at the moment.

Representative HOLIFIELD. All right, we will proceed with your statement.

Chairman DURHAM. You have the committee in rather a bad position, however. You should clear these things before you bring them up here, if you can.

Mr. DIAMOND. I don't believe the waiver by the committee would really go to the question of our authority to enter into the particular contracts. I think the waiver would be assuming the AEC has the legal authority to enter into these contracts. The Joint Committee would merely waive the 45-day waiting period.

Representative HOLIFIELD. This applies to both Consumers and Northeastern States, this pending opinion?

Mr. DIAMOND. Yes; there are two separate letters that have been submitted to the Comptroller General.

General FIELDS. Mr. Delmar M. Morris, the Division of Reactor Development's Assistant Director for Administration, has been the principal negotiator for the two arrangements now before you, and I believe that he will be able to answer any detailed questions you may have.

In a letter dated September 14, 1957 (see p. 11), to me, Mr. Ramey indicated that your committee would like a status report on the Commission's other negotiations under the power reactor demonstration program.

In the second round, as the Joint Committee already knows, the Wolverine, as well as the RCPA proposals are now in abeyance, pending further consideration by the Commission as to what action it deems

appropriate regarding the making of substitute arrangements for obtaining design-development undertakings for the reactor plants. Our letter of August 23, 1957, explained the circumstances of the Wolverine situation and we shall keep you informed of further developments on this and the RCPA undertaking.

Immediately following enactment of Public Law 162 the Commission forwarded to Piqua and Chugach a questionnaire designed to elicit more detailed information concerning their conventional operating costs. We have not as yet had responses from them in this regard, but as soon as we do, we plan to initiate negotiations as expeditiously as possible.

In the third round, the Commission is awaiting a revised proposal from the Florida group and the staff is in the process of giving detailed consideration to the proposal submitted by the Carolinas-Virginia group on August 29, 1957. No other third round proposals have as yet been received.

Mr. Ramey's letter also requested the status of the Commission's work on the gas-cooled reactor prototype, the plutonium recycle experimental reactor, and the production reactor for special nuclear materials. Mr. Vance's letter of September 13, 1957 (see p. 3), which forwarded the Consumers and Northern States request, our request for their waiver, also included a status report on these projects. I and members of my staff are prepared to answer any further questions you may have in connection with these projects.

Mr. Chairman, I have Mr. Morris here, and Mr. Roddis, the Deputy Director of the Division of Reactor Development, and Mr. Tammaro, Assistant General Manager for Research and Industrial Development. They will endeavor to answer any questions you have.

Representative HOLIFIELD. Let us take a look at the Consumers project first, as it was first in line. What are the changes that have been made since your submission to the committee?

Mr. MORRIS. There are no significant changes, Mr. Holifield. It is the same basic arrangement that we contemplated earlier.

Representative HOLIFIELD. Your postconstruction assistance, I believe you had a $2 million item and then we received a letter right toward the end of the session indicating that you wanted to raise that to $8 million.

Mr. MORRIS. Yes, sir.

Representative HOLIFIELD. We didn't have time, of course, at that time to go into that matter. I notice that this is listed as postconstruction assistance. Will you break that down and tell us what what that item is for?

Mr. MORRIS. Under this arrangement Consumers will pay all of the normal operation and maintenance costs, and receive the steam in return for paying these costs. The Commission has undertaken or will undertake to pay the costs of extraordinary maintenance, and the costs of the fuel above a certain amount.

Representative HOLIFIELD. Is that the fabrication of the fuel? You have a waiver of fuel use, $1,325,000, and so you must be talking about the fabrication and processing of fuel.

Mr. MORRIS. The Commission will pay the cost of fabricating the first core. Beyond that, consumers will pay all fuel costs up to $36.40 per kilogram of uranium fed to the reactor. The Commission will pay the excess costs above $36.40 per kilogram. The estimated amount

of this assistance in the fuel cost area and extraordinary maintenance is estimated to be $8 million over the 5-year operating period.

Representative HOLIFIELD. How much is that?

Mr. MORRIS. $8 million over the 5-year operating period.

This is exclusive of the waiver which is shown separately in our submission.

Representative HOLIFIELD. This will apply strictly to the fuel costs?

Mr. MORRIS. Yes, sir; and the extraordinary maintenance.

Representative HOLIFIELD. What would come under that? Is that additional personnel to operate the reactor?

Mr. MORRIS. This would cover the cost of replacing certain components which might fail which would be considered to be beyond the expectations of normal maintenance.

Representative HOLIFIELD. This would in effect be a subsidiary for operation.

Mr. MORRIS. It will be picking up the excess costs of extraordinary maintenance and fuel costs, and I might say that this leaves Consumers in a position where on an estimated basis they expect their operating costs to be in the same range as conventional power costs.

Representative HOLIFIELD. Then does that mean that they will receive the steam at a conventional price?

Mr. MORRIS. They will receive the steam in consideration of paying the normal operation and maintenance costs, and fuel costs up to this point where we take over. This is estimated to be in the same range as conventional prices; yes, sir.

Representative HOLIFIELD. Now, in the report on the Consumers case, we didn't provide any item there for waiver of fuel-use charge. Why was that? In the breakdown which you gave us, why was that?

Mr. MORRIS. This will be a Government-owned reactor.

Representative HOLIFIELD. So this waiver of fuel-use charge here is actually a computation of fuel-use in a Government-owned reactor?

Mr. MORRIS. Yes, and another way of looking at it would be that if we don't charge ourselves fuel-use charges, it disappears.

Representative HOLIFIELD. Do you think that is consistent with what the committee did?

Mr. MORRIS. Yes, sir; we do.

Mr. RAMEY. Was that question considered, that the committee had cut out any waiver for the Consumers project because it was a Government-owned project and now you are putting in a waiver? Is that consistent and has that been considered seriously?

Mr. MORRIS. Well, I can't say it has. But in the arrangement which contemplates that the Commission will pay all costs of fuel above $36.40 per kilogram, whether it is in or out it appears to me to be insignificant in this arrangement.

Representative HOLIFIELD. Well, it is insignificant unless it establishes an important principle. I am not commenting on it one way or the other, we are exploring the reason for you placing it in when we did not have it or we didn't feel it was necessary to put it in the cooperative arrangements where the Government owned the reactor.

Chairman DURHAM. How would that affect the request for appropriations? Do you request the item there?

Mr. MORRIS. The fuel-use waiver is not out-of-pocket cost to the AEC.

General FIELDS. It is a requirement that there be a consideration of it. It is required in the act that there be waivers, and resting 45 days before the committee. I would have to go back and search the record but I think the waiver is not in there because it is a Government-owned reactor and this is true, I believe, in the case of the second-round proposals that are there. But could we look the record over and submit this for the record?

Representative HOLIFIELD. I think we ought to clear this up.

General FIELDS. I think that we should, too.

The reason it is in here is to show comparability in the total assistance.

(The AEC subsequently informed the Joint Committee that "the waiver was a nonfund item and was not included in the appropriations.")

Representative HOLIFIELD. I think it is all right as a matter of keeping track of the amount that it is costing the Government, but as I understand it this program justification under September 13 has it down there. Is that amount carried in the contract?

Mr. MORRIS. No, sir. The amount carried in the contract is $8 million. The reason it was shown in this statement was to show completely and in comparison with previous statements the value of AEC assistance.

Representative HOLIFIELD. The waiver fuel cost charges are added to the $8 million and it is not part of the $8 million?

Mr. MORRIS. That is right. It is not part of the $8 million.

Representative HOLIFIELD. I can't see any sense in waiving the fuel-use charges, where it is the Government's own reactor. I could see it if it was private industries reactor.

Now of course we naturally haven't time to look at this contemplated contract. We only received it I think within the last day or two and part of the staff has been on vacation, and Mr. Durham and I just arrived here this morning. But I did notice in going through the program-justification data which you presented to us, on page 4, under item "H," you have in case of an invention or discovery is made by Consumers in connection with the work under the contract, the Commission will receive a nonexclusive irrevocable royalty-free license for governmental purposes. Is this some peculiar type of a patent arrangement?

The Commission is entitled to patent rights on its own research and development work, not only for governmental purposes but for release to all other private industry. I don't have the language of the patent provision before me in the contract so I am unaware as to what the exact language is. Should you submit a copy of that draft? Could we see one to look at?

Mr. MORRIS. The draft contract, you mean, sir?

Representative HOLIFIELD. Yes.

Mr. MORRIS. Yes, sir; we could.

Representative HOLIFIELD. I mean just to look at right at this moment so I can see what I am talking about on this matter.

Mr. RAMEY. Does the Commission have different patent arrangements with the equipment supplier where the Commission is putting the money in?

Mr. DIAMOND. We do. We have the right to make determinations with respect to all inventions that are conceived during the fabrication or construction of the reactor, with the equipment contractor retaining a nonexclusive royalty-free license.

Mr. RAMEY. That is a direct contract you have with North American; in that case?

Mr. DIAMOND. We do not have the contract yet, but it is contemplated it will be a direct contract.

Representative HOLIFIELD. Why do you have this in for the Consumers? Where do they obtain any patent rights?

Mr. DIAMOND. This is in connection with operation of the reactor itself. The likelihood of inventions being made during that period I think are rather small compared to inventions that might be made during the course of a fabrication or development work.

Representative HOLIFIELD. I think that is true. That is one reason why I asked the question.

Mr. MORRIS. Consumers is bearing the normal operation and maintenance costs and taking the risks of these costs.

Representative HOLIFIELD. What did you say?

Mr. MORRIS. Consumers will bear the normal operation and maintenance costs, and take some risks in that respect, which led us to give them this patent arrangement.

Representative HOLIFIELD. What risk will they take, if you are going to give them a cushion for the first 5 years over and above conventional costs of steam?

Mr. MORRIS. We are protecting them in two areas. That is the extraordinary maintenance and the fuel. The other elements of cost may vary from their estimates, and they will get no protection. Furthermore, Consumers is putting $5,220,000 into the capital cost of this reactor, which they may or may not receive return from.

Representative HOLIFIELD. Well, of course, any donation that they put in is just like any other. There have been grants or donations made by other companies. There was in the case of the Army package power reactor also. There were some grants on that, but the Government owned it.

Mr. RAMEY. Is your theory there that the Commission is not putting any money into the operation and therefore you do not take title to patent rights?

Mr. MORRIS. The Commission is putting some money in.

Mr. RAMEY. But you are still nevertheless not taking title, and you are letting them take title and you are just picking up the other part?

Mr. MORRIS. As we pointed out the research and development and construction will be under separate contracts where we will expect to get title to patentable inventions.

Representative HOLIFIELD. How do you interpret the phrase "for governmental purposes"?

Mr. MORRIS. In connection with such work as will be done.

Mr. DIAMOND. This would refer to the Commission's programmatic activities, that is, reactors that we build in connection with our own programs.

Representative HOLIFIELD. But it would not apply to granting the right to use these patents to all other participants in industry?

Mr. DIAMOND. It would not.

Representative HOLIFIELD. Do you feel justified in deliberately ruling yourself out from a sole governmental right to this patent to do as they please not only for governmental purposes, but to make known to industry itself, where you own the reactor and put in a high percentage of the money involved—where you furnish the fuel, and you fabricate the core, and you furnish an $8 million operating fund, and yet you give away the patent rights?

Mr. DIAMOND. I think this is a matter which results from negotiations. I think that there are some equities on the side of Consumers here. They are making a financial contribution to the project, and they are assuming some risks with respect to operation. Furthermore, as I pointed out earlier, the likelihood of any inventions occurring during operations is rather slim, and so I do not think that there is anything of real value that the Commission has given up.

Representative HOLIFIELD. But yet you have very carefully put in something there so that if there is anything of value, Consumers will have it.

Mr. DIAMOND. And the Government will have it for governmental purposes.

Representative HOLIFIELD. The Government will have it for use in its own reactor, but not to release to industry generally, although the majority part of the cost has been paid by the Commission.

Mr. RAMEY. Is this what you might say is a departure from normal AEC policy, where the Commission puts money into any research and development work or operation work, it has a right to pick up the title?

Mr. DIAMOND. Generally the Commission would under such circumstances retain the right to dispose of patents as it saw fit. As I say, this is the result of negotiations.

Representative HOLIFIELD. Did the Consumers require this, and would the Consumers people be interested in patent rights? They are distributors of electric current, and they are not manufacturers of equipment. Isn't this a bit odd? I ask you again, did the Consumers require this provision?

Mr. DIAMOND. May I consult with my colleagues for a moment? I am told that they did insist on this.

Representative HOLIFIELD. Now, I had a talk this morning with Mr. Schacht, and I got just exactly the opposite conclusion, and so when Mr. Schacht comes to the stand, I am going to ask him the same question—if the Consumers required this.

Mr. DIAMOND. My information may be wrong, but that is what I am told.

Representative HOLIFIELD. All right.

Mr. FIELDS. Mr. Chairman, could I return to just one point? On this waiver, I notice that the report of the committee does indicate that in our submittal we had indicated this waiver amount to the committee itself. However, in the action taken, although cognizance I presume was taken, because here if the second round reactors were Government-owned, it was not necessary to grant waivers. I should not say "grant" but for the committee in its authorization to take cognizance of this. But I wish the record to show that we did submit this same amount, $1,325,000, to your authorization committee and cognizance was taken of that fact in the report.

Mr. RAMEY. But the total authorization did not include this amount, though, did it?

Mr. FIELDS. In your total authorization, you authorized $20 million, as I recall, for the third round. There was no other authorization of waivers other than those that were listed. You did not have to authorize other waivers at that time.

Representative HOLIFIELD. There would not be any money in there then, on that basis, would there?

Mr. FIELDS. There is no money in here for the second round, either, but you take cognizance of that in your report, since it is a government-owned reactor, and it seems to me that has taken care of the necessities of the law.

Representative HOLIFIELD. In terms of money, in other words, they authorize this amount of money, but not in the authorizing of it as a basis for an actual waiver of use materials. That may be the way the committee operated.

Mr. FIELDS. Well, that is a different construction than we have put on it. It seems to me at the moment here that we have construed—that it would be, since it is a Government-owned reactor, a part of total cost of the project. It would be allowable.

Mr. RAMEY. But the law as you have indicated merely authorizes $20 million, and I think the legislative history was pretty clear that that was intended for the third round.

Mr. FIELDS. That is right.

Mr. RAMEY. So then how can you have a waiver for the first round?

Mr. FIELDS. How can we have a waiver for the second round, I ask you, too, because we asked for it.

Mr. RAMEY. And you could not have it for the second round, either.

Mr. FIELDS. I presume the record shows that the Joint Committee was cognizant of this and felt because they were Government-owned reactors that waiver action was not necessary in the authorization bill itself. I think you have to proceed on that basis.

Mr. RAMEY. I think that there—and again it is subject to review here—there might be a contrary interpretation that since they were Government-owned reactors there was no waiver necessary.

Mr. FIELDS. That is the interpretation we have put on it, but we are indicating it here as a part of the whole package.

Representative HOLIFIELD. I notice in your cooperative presentations that you did not present them on the basis of a waiver of fuel. You presented it on the basis of a fuel inventory; is that right?

Mr. FIELDS. The budget proposals had indicated the amounts of materials, as I recall, in them but I think that our submittals also did have an estimate of what would be involved in the use charges with respect to materials that would be in the inventory or in the reactor in the course of this.

Mr. DIAMOND. I might point out that the contract itself is silent with respect to the waiver of use charges. This is consistent with the idea that there is no waiver of use charges as such.

Representative HOLIFIELD. I have no objection to it as a bookkeeping item. I think that we should know what it is. However, I think that I take the same position Mr. Ramey takes; that the $20 million authorization was for the third round. We took the position that the cooperative arrangements were Government owned and therefore there

was no need from an authorization standpoint for you to have an authorization to use fuel in the Government's own reactors. I think that that ought to be pretty clearly understood. I just can't see the implication. I don't know what it would be of setting a precedent for doing that at the present moment.

Mr. FIELDS. I think it should be cleared up also.

Representative HOLIFIELD. Perhaps it is all right, but I think as a principle the Government should not have to consider paying itself for its own fuel in its own reactors.

Mr. DIAMOND. I do not believe there is any difference between the Commission's and your views, and Mr. Ramey's views. I think that we acted along those lines in writing the contract, because there is no reference at all to the waiver of use charges.

Mr. RAMEY. Then possibly this submittal might be amended, that the waiver of use charge is not part of it, then, because it is a variance, it seems to me, from what our understanding was.

Mr. DIAMOND. I believe this change could be made to show it is more a matter of bookkeeping, rather than any formal waiver of use charges.

Representative HOLIFIELD. Are there any other changes? We have not had a chance yet to study this, and we are going to hold in abeyance our decision until we do have a chance to study it, but can you call our attention at this time to any other change in the consumers contract?

Mr. MORRIS. You are referring, I think, to these factors.

Representative HOLIFIELD. The Government is entitled to the plutonium, per se.

Mr. MORRIS. That is right, and I can think of no other significent changes as compared to the arrangement described to you in earlier hearings.

Representative HOLIFIELD. Are there any further questions on that? Are there any questions from members of the staff on the Consumers case? If not, let us pass on to the next question, the Northern States project.

This is the first time that we have had this before the subcommittee, because it was not in condition to be put before us before. So I think that we might give the specifics of this arrangement, Mr. Morris.

Mr. MORRIS. The Northern States Power Co. has undertaken to build a boiling water type reactor plant of 66,000 kilowatts capacity at an estimated cost for development, fabrication, and construction of some $21,600,000. They have requested assistance from AEC in the preconstruction and development period and for preconstruction research and development work of $5,500,000, and for $500,000 of research and development work during the postconstruction period, and for waiver of fuel use charge up to a ceiling of $1 million, for a total amount of AEC assistance of $7 million.

Northern States Power Co. contemplates that the design, research, development, and construction will be performed by the Allis-Chalmers Co.

Representative HOLIFIELD. What do the initials CUAPA refer to?

Mr. MORRIS. That is Central Utilities Atomic Power Associates. That is a group of 10 utilities neighboring there who are furnishing $3,650,000 to this project in return for certain information and experi-

ence. There will be no contractual relationship between the AEC and the Central Utilities Atomic Power Associates, but a portion of the financing for the Northern States undertaking will be obtained from this group of utilities.

Representative HOLIFIELD. That is $200,000, is it?

Mr. MORRIS. The amount of financing provided by these 10 neighboring utilities is $3,650,000. That is shown on page 8 of our submission.

Representative HOLIFIELD. What is the total contribution in this of the AEC?

Mr. MORRIS. Seven million dollars, including waiver of fuel use charge in the amount of $1 million. The remainder of the AEC financial assistance is entirely for research and development work.

Representative HOLIFIELD. What is the postconstruction research and development of $1,700,000? Is that furnished by the AEC?

Mr. MORRIS. $500,000 of that would be furnished by AEC for specific research and development work to be specifically defined in the contract.

Chairman DURHAM. Why do you limit the operations to 5 years?

Mr. MORRIS. This is the duration of our contract period. Northern States will continue to operate it indefinitely, and our feeling is that we will obtain the information we are seeking within this 5-year period.

Representative HOLIFIELD. Will you please describe the postconstruction research and development? You mentioned $500,000 of the $1,700,000 and what about the other $1,200,000?

Mr. MORRIS. The $1,200,000 is then to be provided by Northern States, and this has not been defined.

Representative HOLIFIELD. That is not an AEC contribution?

Mr. MORRIS. No, AEC's assistance is $500,000. As I say, this is to be defined in the definitive contract as specific research and development work.

Representative HOLIFIELD. Well, now, is that consistent with the committee report which on page 20 and paragraph No. 3 states:

> Provision is made in the third round for postconstruction research and development assistance which if attempted to be applied as proposed in the second round which involves only Government-owned reactors, could result in outright subsidies to the operators of private reactors. The committee does not approve the obligation of funds by the AEC for private research and development contracts which are in effect a subsidy for operational costs of private licensees. Such a proposal would raise grave questions as to violation of section 169. Information and data accumulated as a result of postconstruction operations should be available to the AEC as part of a privately owned reactor's obligation for preoperation assistance from the AEC. The committee recognizes that AEC may contract for special services from any private reactor owner on the basis of commercial services rendered.

Leaving out that last sentence that I read there, which the committee recognizes as an area, do you consider that your $1,200,000?

Mr. MORRIS. That is Northern States $1,200,000.

Representative HOLIFIELD. I am talking about $500,000.

Mr. MORRIS. That will in no way be used to defray operating costs. We do not yet have a definitive contract with Northern States, but it is the intention of both parties to specifically define research and development work that will be outside of any consideration of paying operating costs.

Representative HOLIFIELD. In other words, the information that you get as a result of the operation of the reactor would come to you gratis.

Mr. FIELDS. As a part of the contract, yes, sir.

Representative HOLIFIELD. But if there was some specific work you wanted done, outside of the ordinary operations, why, this amount would not apply to that?

Mr. FIELDS. That is right.

Representative HOLIFIELD. So that would come within the meaning of the last sentence in that paragraph.

Mr. FIELDS. This could be special work on looking at the fuel element, and how they behaved and experimentation with respect to those that had been in it for some time. That would be rather just the routine of the processing and recovery of the material in them. It is looking at things that you would not ordinarily do. Once you get in a power operation, they would not be a part of the operating cost. But this is special things, what went wrong with this, and why did it fail at 2,000 hours, instead of 3,000 hours.

Representative HOLIFIELD. The committee has no objection to that type of thing.

Mr. FIELDS. It is that type of thing in the third round.

Representative HOLIFIELD. And clear-cut contracts. We do not want it to be used as a subterfuge for possible construction operation, of course, which is not in accord with the committee's ideas.

Mr. FIELDS. We understand that; yes, sir.

Mr. RAMEY. On that postconstruction research and development, would you pick up the patents like you would on your other construction?

Mr. MORRIS. All patent rights for research and development work paid for by the Commission will reside in the AEC.

Representative HOLIFIELD. That applies to preconstruction as well as postconstruction?

Mr. MORRIS. Yes, sir. I would like to call to your attention a mistake in arithmetic on page 7 of our submission.

Representative HOLIFIELD. All right.

Mr. MORRIS. Where the total is $21,600,000, in the first column. It should be $27,100,000.

Representative HOLIFIELD. Is this contract consistent with the Yankee Atomic Power contract?

Mr. MORRIS. Very similar to it.

Representative HOLIFIELD. There is no basic difference, is there?

Mr. MORRIS. There is no basic difference.

Mr. FIELDS. As I recall in the other two, there is no postconstruction research and development assistance.

Mr. MORRIS. That is right; yes.

Mr. RAMEY. In terms of your rights to information, the GAO's access?

Mr. MORRIS. That has been taken into consideration here, and such rights will be fully spelled out. I might point out that in this case the AEC is contracting directly with Northern States Power, and it will be a direct two-party arrangement, and it will not be contracting with a group or a newly organized company.

Representative HOLIFIELD. The Northern States, that is one corporation, is it?

Mr. MORRIS. That is right.

Representative HOLIFIELD. Has there been any difference in the corporate setup of the Northern States since the original submission to the committee of their proposal.

Mr. MORRIS. No, sir.

Mr. FIELDS. This proposal, as I understand it, is conformed to our third-round invitations, from the very beginning, and there have been no changes in it.

Representative HOLIFIELD. This group that is contributing toward it, that is a separate contract between the CUAPA and the Northern States?

Mr. FIELDS. Yes, sir; and there is no contractual relationship between the AEC and this group.

Mr. RAMEY. Is it consistent with your other financial data and information that was submitted during the authorization hearings on Northern States?

Mr. MORRIS. Yes; nothing has changed.

Mr. RAMEY. Is your patent arrangement similar to Yankee and PRDC?

Mr. MORRIS. Yes; that is the patent rights with respect to the work AEC is paying for will reside in AEC, and the other party will receive a nonexclusive license with respect to the work they are paying for, and it would be just the reverse.

Mr. RAMEY. That is to say, they take title and the AEC——

Mr. MORRIS. With respect to the work they are paying for; yes, sir.

Representative HOLIFIELD. Has the presentation of this arrangement with the Northern States people in Minnesota had any effect upon your consideration of the Elk River Cooperative group? Is your situation in getting this the basis of disqualifying the other people in any way?

Mr. FIELDS. In my mind, it is completely independent.

Representative HOLIFIELD. It is completely independent?

Mr. FIELDS. Yes, sir.

Representative HOLIFIELD. And the Commission is still of the opinion that the type of reactor proposed by the Elk River people, which they plan to build, is still desirable?

Mr. FIELDS. It was our opinion as of yesterday. It was desirable on that basis as it was submitted, and if we had that same basis today, I would say it would be desirable; yes, sir. With this increase in costs that has come into the picture, I do think that we need time to reconsider it, and see what our opinion is on that, sir.

Representative HOLIFIELD. Unless there are further questions on that, the committee will excuse the Commission witnesses at this time, and ask them to stand by, while we call to the witness table these other witnesses. First we will give the courtesy of the witness table to Mr. William J. Connell, Senator Humphrey's representative.

I understand you have a statement to make on the Elk River matter.

STATEMENT OF WILLIAM CONNELL, ASSISTANT TO HON. HUBERT H. HUMPHREY, A UNITED STATES SENATOR FROM THE STATE OF MINNESOTA

Mr. CONNELL. Thank you very much, Mr. Chairman, and members of the committee. As soon as we were notified by the committee staff this morning that the Atomic Energy Commission would not present the Elk River proposal before the committee today——

Representative HOLIFIELD. Will you pause just a moment and give your full name and your official position?

Mr. CONNELL. I am William Connell. I am assistant to Senator Hubert H. Humphrey.

As soon as we were notified by the committee that the Commission would not present the RCPA proposal at this meeting, I was in touch with Senator Humphrey, who was out of the city, and he instructed me to check with the Atomic Energy Commission and with the RCPA people to determine just what the story was. The information we received from the Atomic Energy Commission was rather vague. There was some indication that perhaps the whole project was completely on the skids. Therefore, I know that Senator Humphrey will be very gratified to hear from the Atomic Energy Commission that the matter is still very much under consideration, and that there may be a possibility of working out something.

I had merely wanted to bring to the Joint Committee the Senator's deep interest in this project, and to ask certain questions of the Commission which I believe have now been answered.

Representative HOLIFIELD. We are aware, of course, of Senator Humphrey's deep interest in this matter. He has conferred with members of the committee, and also with the staff on many occasions, as has his office. Of course, this comes as a surprise to the committee, as well as apparently to the Commission.

Mr. CONNELL. It comes as a surprise to everybody, and in fact, the Elk River people did not know about it this morning until we called them.

Representative HOLIFIELD. Is that so?

Mr. CONNELL. Yes, sir.

Representative HOLIFIELD. That is an amazing statement that their supplier did not inform them, as people they were working with on this matter. It appears to the chairman of the subcommittee that this is a very abrupt decision on the part of the American Machine & Foundry people. I hardly think the records will be clear unless we get a statement from them as to what has caused this abrupt withdrawal of their proffer to build this reactor.

Mr. CONNELL. Yes, sir. I tried to get in touch with the president, who is in town, I understand. He is in town today, but I could not reach him before the 1:30 meeting today. It might still be possible to reach him.

Chairman DURHAM. What kind of a contract did the Elk River people have with the American Machine & Foundry Co.?

Mr. CONNELL. It was a contract for the construction of the reactor and the superheater in the amount of approximately $5,868,000.

Chairman DURHAM. Was it a fixed-price contract?

Mr. CONNELL. It was a cost-plus contract with a ceiling I believe of $5,868,000.

Representative HOLIFIELD. The committee sent a telegram under date of September 14 to several people that are interested in this matter, and among those was Mr. A. V. Peterson, vice president of the American Machine & Foundry, and invited him to appear here today. At this time the Chair would like to inquire if Mr. Peterson is present in the audience, or if any other representative of the American Machine & Foundry Co. is present?

The Chair observes that no one rises, and so it is assumed that no representative of the American Machine & Foundry Co. is present, although the Chair is informed informally that there was a representative present here at the beginning of the meeting.

The Chair is also informed that a member of the staff recognized the American Machine & Foundry Co. representative in the audience and asked him if he wanted to testify today, and apparently he did not. The Chair, if he had known he was present, would personally have advised him to testify.

Did you have anything further to say?

Mr. CONNELL. No; except to correct the record. I believe it was Senator Humphrey who said on the floor that it was his understanding that this contract was ready to be signed, on the desk of the Chairman of the Atomic Energy Commission, and it could be signed in a matter of weeks. I believe that was the story up until yesterday. He will be glad to know that the Atomic Energy Commission apparently was ready to sign this contract.

Thank you.

Representative HOLIFIELD. Thank you, Mr. Connell.

Mr. Robert Forsythe, administrative assistant to Senator Thye, is present. Mr. Forsythe, would you like to make a statement on behalf of Senator Thye?

STATEMENT OF ROBERT A. FORSYTHE, ADMINISTRATIVE ASSISTANT TO HON. EDWARD J. THYE, A UNITED STATES SENATOR FROM THE STATE OF MINNESOTA

Mr. FORSYTHE. My name is Robert A. Forsythe, administrative assistant to Senator Thye of Minnesota.

Mr. Chairman, I just want the record to show that Senator Thye wished to be represented at this hearing today, and to assure the committee that on this particular occasion the contract involving Elk River, the feeling of the two Senators of Minnesota were most similar, and I can assure you that both were shocked this morning when we heard at 9:30 or 10 o'clock that A. M. & F. was withdrawing temporarily from this contract.

The Senator's surprise can be indicated best by reference to the letter which you received in your files this morning in which he again brought to the committee's attention the fact that this contract had been under negotiation for many months and was ready for final negotiation and signature by all parties concerned. We trust and hope that in the interim period, between this meeting and the next meeting of the committee, negotiation will take place which will allow this very important project in Minnesota to go forward. That is all I wish to say.

Representative HOLIFIELD. Thank you, Mr. Forsythe.

The committee received a telegram under date of September 16 from O. N. Gravgaard, president of RCPA, which reads as follows:

ELK RIVER, MINN., *September 16, 1957.*

JAMES T. RAMEY,
Capitol Building,
Washington, D. C.:

Appreciate committee invitation to appear and wish to compliment the committee on their strong interest in respect legislation and activities expedition this fine program. In view of our having reached complete agreement on major points with the AEC and their agreement to request waiver of the 45 days it appears that all matters of this interest between our cooperative and the AEC have been settled. We therefore see no advantage in taking up the committee's time further and duplicating what the AEC will present. Further delays will seriously handicap the cooperative supply position and the manufacturer cost problem. The Joint Committee's interest in expediting conclusion of all these arrangements is sincerely appreciated and our association urges in these interests that the committee can find a basis for waiving the 45-day period.

Sincerely,

O. N. GRAVGAARD, *President, RCPA.*

Representative HOLIFIELD. That is the Elk River Association. That was evidently before he received the news.

Mr. Kenneth M. Olds, attorney from Wayne, Nebr., representing the Nebraska Generating & Transmission Cooperative. Mr. Olds, are you present, sir? Would you like to make a statement?

STATEMENT OF KENNETH M. OLDS, ATTORNEY, REPRESENTING THE NEBRASKA GENERATING & TRANSMISSION COOPERATIVE, WAYNE, NEBR.

Mr. OLDS. Yes, sir. I would. My name is Kenneth M. Olds, attorney of Wayne, Nebr. I am representing the Nebraska Generating & Transmission Cooperative, which is an organization of 22 rural public-power districts in the State of Nebraska. The public-power districts are what we commonly call the REA's, and they serve approximately 50,000 rural consumers in Nebraska. We are appearing here in connection with the proposal of Consumers Public Power District.

We want to make it clear that we are not opposing the construction of an atomic plant in Nebraska, and we also want to make it clear that our organization has no plan or proposal to construct a plant of its own. We are only raising the question that we feel we should have an opportunity to review and consider the proposal that has been submitted by Consumers, together with the AEC.

We in Nebraska first learned of this hearing, I believe Saturday, in the newspapers in Nebraska, and we immediately took steps to send a telegram, and copies of which I have here available to the committee, and I would like to leave with the committee. I have extra copies, and I believe the chairman perhaps has the original of that telegram.

Representative HOLIFIELD. The Chair has the original and it will be placed in the record at this point.

(The following telegram was sent to the Subcommittee on Legislation of the Joint Committee on Atomic Energy by the Nebraska Generating & Transmission Cooperative on September 15, 1957:)

LINCOLN, NEBR., *September 15, 1957.*

Re hearing on contract between Atomic Energy Commission and Consumers Public Power District.

Our first notice of your Tuesday hearing was carried in the press yesterday.

We respectfully ask permission to intervene and be heard after studying the contract. It was not until 2 days ago that we learned through the news services that a contract had been negotiated.

Representing 22 rural electric districts in Nebraska, we urgently request an opportunity to study the Consumers District contract with AEC before it is approved by Congress. We have had no chance to see this contract. Rural electric users in Nebraska object to secrecy and haste. The 45-day waiting period was intended by Congress to safeguard the public interest.

We support an atomic-fueled powerplant for Nebraska provided it does not adversely affect our customers.

We must be assured that the construction of the proposed plant is timed and coordinated into the State power program and that our users, including irrigators, are not deprived of their fair share of low-cost power.

A. G. SYDON,
President, Nebraska Generating & Transmission Cooperative.

Mr. OLDS. I am informed that the proposal was submitted to the committee or the subcommittee approximately on Friday. We arrived here yesterday by plane and obviously we have had no opportunity to examine the proposal of Consumers, although it was made available to us at about 11:45 this morning.

Since the copies of the telegram have been made available, perhaps it is not necessary to take the time at this time to read it, but we do want to emphasize the fact that we have no objection whatsoever specifically to the proposal, but we have had no opportunity to examine the proposal or apparently a draft of the contract that has been presented. We feel that it may affect the rural or REA's in Nebraska. We have an unusual situation in Nebraska in that we have a public-power State with the systems integrated so that at the present time the REA's and Consumers Power District purchase all of their power from one agency, or I should say substantially all of their power from the Nebraska public-power system.

In view of that fact, the proposal here to construct this plant may have a very definite effect on the operations of the REA's in Nebraska.

For the information of this subcommittee REA has loaned approximately $100 million to the REA's of Nebraska. In addition to that, REA and GSA have advanced another $45 million to the organizations that provide the generation and transmission lines of the Nebraska public-power system, which is, in turn, carrying the power to the rurals.

Chairman DURHAM. You generate your own power?

Mr. OLDS. No; we buy it from the Nebraska public-power system. It is just as Consumers does substantially all of its power at the present time.

Representative HOLIFIELD. Total Federal investment there is how much in the distribution system?

Mr. OLDS. In the rural distribution system, about $100 million. But there is an additional $45 million which REA and the GSA have advanced to the Nebraska public-power system or other public agencies to provide generation facilities.

Representative HOLIFIELD. In other words, the Nebraska public power; I understand there are three groups there in Nebraska, the State-chartered groups?

Mr. OLDS. They are all public-power districts.

Representative HOLIFIELD. There are three public-power districts? Now, are you saying that they have borrowed $45 million from the Government?

Mr. OLDS. That is right, sir.

Representative HOLIFIELD. And yet they have bond-issue power; do they not?

Mr. OLDS. They have those in addition to the loans from the Government, Mr. Chairman.

Chairman DURHAM. Why would this affect you? Because you would expect to get this, or are you satisfied with your present contractual system with Consumers Public Power?

Mr. OLDS. It would affect us in this way, in that the rurals have recently entered into a 35-year contract to buy their power from the Nebraska public-power system. Up to the present time, Consumers has also been purchasing its power there. We have what amounts to a cost contract to purchase our power, and, obviously, if there is the construction of a conventional plant or, I should say, a conventional part of this plant, together with the atomic plant, it will have an effect on our future operations.

Chairman DURHAM. How would that be? You have your 35-year contract, and, if this power is integrated into the Consumers system, your contract would still be valid.

Mr. OLDS. Well, sir, for one thing, we have no assurance that it will be integrated, and we have no information on that.

Chairman DURHAM. They have to integrate it into the system if they are going to use it.

Mr. OLDS. That is one of the problems that we want to be satisfied on. All that we are asking, and we are not objecting to the form of the contract, and we have not had an opportunity to examine it, but all we are asking is that the committee give us 45 days in which to do that. We are not asking for a period of time that Congress is in session, but 45 calendar days, we feel, will give us an opportunity to consult with our engineers and see what effect, if any, it would have on our operations. We have a huge investment, and the Government has a big investment in Nebraska, and we want to know where we are going in the future to repay those funds.

Chairman DURHAM. Well, I do not understand how it could be otherwise, if you have a 35-year contract, and I imagine it is on a sliding-scale cost, I do not see how this would affect you. The power will have to be integrated into that system. Otherwise, it will not be any good to them.

Representative HOLIFIELD. Your thought is, if this power is expensive power, and if it is offered for sale to you on the cost basis, notwithstanding your 35-year contract, you will have to buy expensive power.

Mr. OLDS. That is right. That is one of the problems that we are certainly confronted with.

Another problem that we have that is unique in Nebraska is that we have a great amount of irrigation. There is a summer peak of

need for electric energy during those summer months. A generating plant which is constructed to provide a year-round capacity can be very expensive. What we need is Bureau of Reclamation power during those summer months to meet our irrigation needs.

Representative HOLIFIELD. What you are asking for, then, is that the committee do not waive the 45-day period but give you that time to study the contract, and to ascertain whether this will mean an increase in costs to the cooperatives of Nebraska, who have a total of $145 million of Federal funds.

Mr. OLDS. That is exactly it, sir.

Representative HOLIFIELD. That is your request.

Mr. OLDS. That is all that we are requesting; that period of time.

Chairman DURHAM. How are you going to get an answer, because they cannot determine what the power is going to cost them? At the present time you could not determine it in 12 months.

Mr. OLDS. At least we will know what their proposal is, and what type of plant is to be constructed, and our engineers can at least give us some better advice than we as lawyers or members of the REA's can see at the present time.

Representative HOLIFIELD. Of course, this is, to a certain extent, a gamble. Any atomic-energy electrical plant is a speculation into the future, because we are exploring unknown fields. Now, if this power is integrated into the complete system, I suppose you would be willing to bear your proportional share of it. The thing that you worry about, you might be apprehensive that there might be a bloc of high-cost power allocated to the rural co-ops, and on your cost contract, and thereby put you in the position of having to pay more for your power because of that particular bloc.

Mr. OLDS. That is part of it, sir.

Representative HOLIFIELD. If it is high-priced power, and the chances are that it would be higher priced than conventional power in some way, either the Government will have to stand it or it will be a mutual burden for the Consumers and the AEC. If the integration of a higher potential cost was made into the full system, you would not mind sharing your proportional costs, would you?

Mr. OLDS. Well, I assume that if it is integrated we would have to, but, of course, there is information that has come to us, and I don't believe it is in the proposal, although I have looked at it just hastily, that perhaps a conventional part of this plant will be constructed immediately.

Chairman DURHAM. How much does your group consume of kilowatt-hours?

Mr. OLDS. I am not an engineer, and I could ask our consulting engineer to give you that information, if you would desire it.

Chairman DURHAM. I was thinking this is only a 75,000-kilowatt plant. It is not a big bloc of power.

Mr. OLDS. Mr. McKinney, the consulting engineer, is here, if you would like to have him testify.

Chairman DURHAM. If they integrated the whole thing into your system, I imagine, with $145 million of investment, you must have a lot of power.

Mr. OLDS. We do, sir.

Chairman DURHAM. Is Mr. Chantry with your same group? Do you care to make any further statement on this matter?

Mr. CHANTRY. I think Mr. Olds has covered our point very well.

Representative HOLIFIELD. Mr. Vance E. Leininger, attorney, representing the Loup River Public Power District, Columbus, Nebr., and Platte Valley Public Power and Irrigation District, North Platte, Nebr. Mr. Leininger, do you have a statement you would like to make to the committee?

STATEMENT OF VANCE E. LEININGER, ATTORNEY, REPRESENTING THE LOUP RIVER PUBLIC POWER DISTRICT, COLUMBUS, NEBR., AND PLATTE RIVER VALLEY PUBLIC POWER AND IRRIGATION DISTRICT, NORTH PLATTE, NEBR.

Mr. LEININGER. I do. My name is Vance E. Leininger, of Columbus, Nebr., and I am an attorney representing the Loup River Public Power District of Columbus, Nebr., and Platte Valley Public Power and Irrigation District of North Platte, Nebr., both publicly owned and political subdivisions of the State, and joint operators of the Nebraska Public Power system.

I have had no opportunity to prepare any written statement, but the following telegram was sent to the secretary of this committee, and I believe dispatched late yesterday afternoon. Again we have had no opportunity to have sufficient copies of it prepared. With the leave of the chairman, I would like to read it into the record at this time.

As joint owners and operators of Nebraska Public Power System, Nebraska's principal publicly owned electric generation and transmission agency we are deeply concerned with the outcome of current contract negotiations between AEC and Consumers Public Power District, and proposed large investments contemplated by Federal Government in Nebraska, as reported in the press.

We are not opposed to construction of atomic fueled powerplant in Nebraska, provided we can be assured that our investments as well as those of the Federal Government in existing generation, transmission, and distribution facilities are not jeopardized. NPPS presently supplies substantially all of consumers district's power requirements, in addition to 27 REA financed rural districts, 2 Government agencies and several municipalities with a combined peak load of over 400,000 kilowatts. Investment in generation and transmission facilities alone exceeds $90 million. Plans are underway to construct additional facilities to bring in Nebraska's share of new allotment of Missouri Basin hydro power pursuant to recent announcement by Bureau of Reclamation. Essential that proposed atomic plant be properly integrated into existing high voltage system in accordance with long standing plans and practice of public power agencies in Nebraska, and that installation of new units be properly timed so as to avoid duplicating and idling existing facilities and insure full utilization of low cost source of power.

The above information was wired to the Atomic Energy Commission last week, along with request for opportunity to review contract before negotiations were consummated which to date has not been acknowledged or granted. Additional data received since wiring Commission emphasizes importance of compliance with above request. Press reports Saturday indicated contract has been forwarded to your committee and hearing may be held Tuesday p. m. We respectfully ask permission to intervene and be heard if we deem it advisable after receiving copy of contract and opportunity to review same.

Signed:

Lloyd Kain, president, Platte Valley Public Power and Irrigation District, North Platte, Nebr., and Zack Howell, president, Loup River Public Power District, Columbus, Nebr.

I believe the committee or the staff has the original of that telegram.

Chairman DURHAM. Why would this affect you in any way? How would it affect you?

Mr. LEININGER. I cannot answer that question because this is the first time that we have seen the document. I cannot say it will affect us. I say this is the first time, but we received it at the same time Mr. Olds did, about 11:30 or 11:45, or something like that. I cannot say it will affect us, or that it won't affect us. We merely feel as though we should have an opportunity to review it and see whether it will or not. I can appreciate that the members of the committee might feel there has been some delay in indicating the interest of the Nebraska Public Power System and I can only explain that by saying that I think it was the attitude of our people that the negotiations between Consumers District and AEC were more properly and more efficiently carried on by them directly without intervention or interference of any other agencies. They have been protracted and necessarily complicated, I presume, and further complications during the formative stage certainly would not have been helpful to the project. I believe they felt that until something had been sufficiently finalized so that it could be looked at and evaluated on paper, that their position was not to interfere or try to take part or participate in those negotiations.

Chairman DURHAM. Does your group generate all of the power that you use or do you buy it from some other source?

Mr. LEININGER. We purchase some power from the Bureau of Reclamation. That is the Missouri Basin projects. The balance of it I can say is substantially all generated. There are occasional seasonal or emergency purchases from neighboring utilities. The group that I represent serves from the generational and high voltage transmission standpoint the eastern two-thirds of Nebraska, except for the area surrounding Omaha.

Chairman DURHAM. Do you serve rural electrification people?

Mr. LEININGER. We serve the group that Mr. Olds represents, as well as Consumers District, and some municipalities.

Now, as I say, we still do not know whether our clients will feel it advisable to participate any further in this procedure. This request is merely for an opportunity to review the proposals sufficiently prior to the committee taking action on it.

Chairman DURHAM. Has your group ever considered a proposal for the AEC, for a reactor?

Mr. LEININGER. No, sir. We have no interest in constructing an atomic plant at the present time; I would like to emphasize that. We are wholeheartedly in support of the enterprise of Consumers District, and we feel they are to be commended for it.

Chairman DURHAM. It looked like your interest would be served on the contract that you have with the Consumers, and not with the contract between AEC and Consumers. I do not get the point of that.

Mr. LEININGER. That is entirely possible and that may be the conclusion after we have had an opportunity to look at this one. But we recognize, of course, that this is a contract with an arm of the Federal Government, and it may have some limitations, and some qualifications in it which will govern the scheduling of developments and methods of operating the plant and other things which vitally affect costs in the unique situation that we have there with our irriga-

tion situation, which requires large blocs of power for only a short period of time, and we are concerned vitally with every new power supply in the area because of the scheduling difficulties and the problems that come about in working it into the overall picture to the best advantage of everyone.

I merely suggest that there was a waiting period set up by Congress, and it is our understanding of this legislation that that called for 45 congressional days, which I assume could not elapse until the next session is underway. Now, our clients are not asking for 45 congressional days.

Chairman DURHAM. It is a very simple contract, and you could make up your mind on that very quickly if you read it.

Mr. LEININGER. That may be, and we may conclude that. In view of the 45-day provision in the legislation, we think that a period of 45 calendar days might not be out of line. I am advised by our consulting engineer that that would in all probability be sufficient tc evaluate it and to determine how it would operate under varying conditions and see what the results are in our Nebraska picture.

We think that the Federal Government generally has an interest in this beyond the consideration of a single isolated atomic reactor plant. We think that the fact that NPPS is a substantial borrower from Federal agencies, and that the customers which it serves are substantial borrowers of Federal agencies, lends a general interest from the Federal point of view to the entire picture which justifies this opportunity to review the proposal and see whether we feel we should participate further in that or not.

I certainly want to express the sincere appreciation of myself and our group for the courtesy shown us on the part of the committee and its staff.

Representative HOLIFIELD. Thank you, Mr. Leininger.

Now we have another name, Robert L. McKinney, consulting engineer, that is with you people. Unless he has a statement to make, I will pass over him.

Mr. MCKINNEY. I have no statement.

Representative HOLIFIELD. At this time, I think that we should let Mr. Schacht of the Consumers come to the table and explain their desire on this waiver. Mr. Jones and Mr. Wilson accompany Mr. Schacht. You gentlemen represent the Consumers Public Power of Nebraska?

STATEMENT OF R. L. SCHACHT, GENERAL MANAGER, CONSUMERS PUBLIC POWER DISTRICT

Mr. SCHACHT. I have prepared, Mr. Chairman, a statement which I will read from, and there will be some additional comments, but if you would like to have this perhaps it might be helpful to you.

My name is Ray L. Schacht, general manager of Consumers Public Power District, Nebraska. I and my associates, Dr. Emerson Jones and R. D. Wilson, our general counsel, came to Washington yesterday to complete a few remaining language details on the contract between Consumers Public Power District and the Atomic Energy Commission, and to be available to this committee if they desired any information from Consumers on their project. We did not know

until arriving here that representations had been made to the committee for a further delay in completing the contractual arrangements between Consumers and AEC.

I regret that what is purely a local Nebraska matter has been injected into the Nebraska atomic-power program before this committee, and at literally the 11th hour and 59th minute.

Consumers has proceeded for the past 2 years with these atomic-power negotiations on the assurance of cooperation we received in September 1955, from the Loup and Platte districts, and which has never been disclaimed by them. I have copies of that assurance and would like to file one with the committee. That is a letter that was written to us. We inquired as to this when we first entered into these negotiations. We inquired as to whether we would have their cooperation because we felt it was impossible to proceed unless the parties were of the same mind that atomic power in Nebraska was a project we should all work for.

On September 28, 1955, a letter was addressed to Earl Mead, president of the Consumers Public Power District, Columbus—Subject: Atomic Energy Plant.

DEAR MR. MEAD: Your letter of September 16 has been received. You inquire whether there will be cooperation from other public-power agencies in connection with your proposal to build an atomic powerplant.

May we hasten to state that months ago our boards thoroughly discussed the possibility of such a plant being constructed in Nebraska. It has always been, and still is, their desire to cooperate with any public-power agency that can bring such a project to our State.

We know that you share our pride in the fact that we are supplying electricity to the people of Nebraska at a lower rate than is available in any adjoining State. It is our hope that all of us can work together to continue for the benefit of Nebraska citizens this enviable record of accomplishment in the field of electric power.

We congratulate you on your aggressive efforts to obtain an atomic plant.

We shall be happy to participate in this program by making the benefits of atomic power available through our grid system to all of the people in the State.

Very truly yours,

P. N. MCKINLEY,
President, Platte Valley Public Power and Irrigation District.
ZACK B. HOWELL,
President, Loup River Public Power District.

Neither the Loup nor Platte districts nor the Nebraska Generating and Transmission Co-op, who have filed protests on Consumers' proposal, are in any way parties to the contract between Consumers and AEC, nor is the issue of whether or not Consumers shall build a powerplant dependent upon the outcome of the atomic-power negotiations. Almost a year ago Consumers made the decision to proceed with construction of the Hallam plant, purchasing the turbogenerator in March 1957, and the boiler in May 1957. Wide publicity was given to this program, and there was general agreement among responsible engineers for all of these power districts that the added capacity was necessary to be in operation by the spring of 1960 to insure adequate capacity for the loads in the area. Actually the capacity of the atomic powerplant is only equal to approximately 1 year's load growth for the eastern two-thirds of the State. That does not mean only Consumers' load, but the rurals served by the hydrodistricts there, and the Omaha district. As late as last week the Loup and Platte districts reaffirmed the need of this capacity in their purported telegram to the AEC. That was the Hallam plant.

I might add at that point that the Consumers district has no contract to either buy or sell power to the rural group represented by Mr. Olds or are none contemplated. We do wheel power for them over some of our transmission lines, and they wheel some power for us, but we have no arrangements for buying or selling, and do not contemplate any.

In purchasing the turbogenerator and boiler, Consumers secured the maximum delay possible for specifying the steam conditions, and still obtaining delivery in time to insure the completion of construction and start of operation with conventional fuels by 1960. The time delay for specifying the steam conditions was for the purpose of securing additional time to bring our atomic-power negotiations to a close, one way or the other. If the negotiations were successfully completed, the equipment would be built for the temperatures and pressures contemplated by the reactor system, but if unsuccessful, higher pressure and temperature equipment would be secured so as to obtain greater efficiency in the use of conventional fuels.

The only open issue is whether this powerplant, for which approximately $8 million of equipment is on order under firm contracts, shall have a reactor as a source of heat, or whether it shall not. Consumers has until Saturday of this week (September 21) to secure a decision on the atomic-power negotiations, and to notify the manufacturers of the equipment as to the steam specifications.

These negotiations have been long and involved, it being approximately 2 years ago that Consumers' proposal was accepted by the Commission as a basis for negotiation. During that time we have made several appearances before the Joint Committee as you have studied the various aspects of atomic power. The various new sources in Nebraska have fully reported the progress (or lack of progress) of our negotiations throughout the entire period.

This project has had the support of the American Public Power Association through resolutions in 1955 and 1957. I would be glad to file copies if they would be of any benefit to the committee. It has had the support of the State, including the Governor, the Nebraska delegation in Congress, and responsible citizens throughout the State.

We trust that the objectives of this committee in speeding up the national development of atomic power will not be sacrificed by further delays resulting in abandonment of this project. It is clear that if the delay sought by these objectors occurs, Consumers' deadline with its manufacturers will have passed and the hopes for early completion of this reactor concept will be gone.

Representative HOLIFIELD. In reference to the latter part of your statement there, the committee has been informed that Senator Curtis and Senator Hruska and the Governor of Nebraska are very much in favor of the proposed arrangements and the congressional delegation also, and desire that the committee after appropriate review do waive the 45-day period.

Mr. SCHACHT. Thank you. The Governor has wired me as late as this morning further indicating that fact, if you do not have an expression from him. I understand now you do have it.

Representative HOLIFIELD. There is no ground for the fear of these cooperative purchasing groups having their power rates raised as a result of this venture?

Mr. SCHACHT. No, I cannot visualize that. We do not expect to sell this power to them or expect them to underwrite any part of this atomic-power venture. We have in the eastern two-thirds of the State, approximately 100,000 customers of our own.

Representative HOLIFIELD. It is your intention to integrate this power, at whatever cost it may be, into your complete system and to allocate its costs throughout the system rather than direct it to any one group.

Mr. SCHACHT. Well, Mr. Chairman, our contract negotiations with the Nebraska Public Power System are not yet complete, so the amount of power that we may be buying from them, in the years ahead, is not yet defined. It is our hope that we can tie this plant into the existing transmission grid in the State of Nebraska, so that it is utilized to the maximum extent.

We had not expected other public power districts to attempt to help underwrite any of our obligations under this contract.

Chairman DURHAM. What is your generating capacity at the present time?

Mr. SCHACHT. Our generating capacity at the present time in the eastern system is quite limited because we have been buying most of our power from the Nebraska Public Power System. We are proposing to build this plant ourselves, and it has been the subject of rather extended litigation in Nebraska, and the Supreme Court upheld Consumers' right to build the plant, and the need of a plant of this size in the spring of 1960, has been fully recognized. Up until the last 3 or 4 days I had never heard of any indication that someone felt it was not necessary in 1960.

Chairman DURHAM. Do you have to go through the courts to get permission to build this plant?

Mr. SCHACHT. No, sir, that has been all cleared. Several years ago that was done.

Representative HOLIFIELD. The fears of this group are unwarranted, then, that they are in danger of having rate raises as a result of this venture.

Mr. SCHACHT. I do not see why they have that fear at all. The question is the inclusion of an atomic reactor in this plant, which is really the only question before this committee. I do not think the committee has any problem as to whether Consumers should build a powerplant or not—that certainly is our problem.

Representative HOLIFIELD. But the group that has appeared indicated that the Federal Government has $145 million invested in enterprises in Nebraska. That is $100 million is invested in these consumer groups, these REA groups. As I understood the testimony, their fear was that a disproportionate amount of the cost of this venture, if the venture did prove to be a costly venture, on the part of the generating company, might be allocated to them rather than spread over the complete system. Of course, the committee would only be interested if it did jeopardize the Government's investment in these REA cooperatives.

Mr. SCHACHT. I cannot possibly see that. I can see no possible foundation for the fears that this atomic powerplant is going to do that.

Representative HOLIFIELD. Can you assure the committee that this power will be integrated and will be spread over the system, and not sold in a bloc to these groups of REA co-ops depending upon you to supply power? Can you give us that assurance?

Mr. SCHACHT. Well, Mr. Chairman, maybe I could explain it this way. We propose to use the power generated by this plant to serve Consumers 100,000 customers, or some part of them, and part of our load. We are not expecting to sell this power or asking the rural districts to buy part of this power at all.

Representative HOLIFIELD. Then there is no problem on their part of having their rates raised as a result of the generation of this power, if it happens to be more costly than conventional power.

Mr. SCHACHT. No; that is a problem which would fall only on Consumers' own customers, and those customers served by Consumers Public Power District.

Representative HOLIFIELD. You could not at this time serve these people; is that right?

Mr. SCHACHT. That is right, and we sell no power to these rural districts, nor do we buy power from them, except maybe a little backup some place, of a few dollars a month, but we do not sell them power, and we do not buy from them, nor do we contemplate any such arrangement.

Chairman DURHAM. Neither one of the groups; is that right? You don't sell to either one of the groups?

Mr. SCHACHT. We are buying power at present from the Loup and the Platte Districts, and we do not sell them any. We don't propose to, except possibly in some kind of an interchange arrangement, where there might be limited sales for a time, when one or the other has excess capacity. But we are not asking any of these groups to share any burdens that might develop in increased costs, because of this nuclear venture.

There were 1 or 2 questions you raised, Mr. Chairman, with respect to the contract or the negotiations which perhaps I might help clear up, if you would like.

Representative HOLIFIELD. We are glad to have your testimony.

Mr. SCHACHT. With regard to the matter of the patent rights, it is difficult to remember all of the conversations and discussions over a 2-year period on all of the phases of this contract. To the best of my recollection, and that of both Mr. Jones and Mr. Wilson, we had felt that Consumers was entitled to some rights in patents if they developed them in connection with their work in this atomic plant. We did not view it as anything of major consequence because as has been indicated, and I think we indicated to you this morning, most of those possibilities will develop out of the design and the construction of the atomic plant, which Consumers will not do.

Now, I think that if this question of the patent rights that we may retain is of any serious nature, I am sure that we are not adamant at all. Certainly we are entitled to a right to use any patents that we should develop but——

Representative HOLIFIELD. There is a long-standing principle that if the Government provides the money for a research and development program, that it retains the patent rights and makes them available not only to its Government facilities, but to other sections of the in-

dustry, because the money originally came from other segments of the industry in the form of taxes.

It is true that you are placing a $5 million contribution into this overall venture of some $51 million, and we assume that you are doing it for reasons which are based on good business principles. But we do not feel or at least the chairman does not feel that a $5 million contribution in a venture of this kind would justify a signing over the Government's patent rights in a $50 million venture to the Consumers of Nebraska. I don't think that you would contend that that would be equitable either; would you?

Mr. SCHACHT. I don't think that we have any hard and fast feelings. I think if we had a royalty free license to use what patents we might be able to develop, that would be satisfactory with us. It is not a major problem for us to the extent that it may be for the Government in establishing a principle of some kind.

Chairman DURHAM. How could you use them? You are not a manufacturer.

Mr. SCHACHT. I suppose the only way we could use them would be to turn around and sell them, and I am sure we have no intent of trying to get into that position, that we could go out and market some patents.

Representative HOLIFIELD. You know Congress is frequently charged with being give-away and some of us are sensitive when we see something along that line.

Mr. SCHACHT. We will be glad to adjust that clause.

With respect to the General Accounting Office approval, we realize that that is an open problem yet. We have been assured that they would give very early consideration and a prompt decision, and I am sure if their decision would be adverse, the Consumers would never consider signing the contract, and I don't know what AEC would do.

Representative HOLIFIELD. You speak of the date of the 21st as being a deadline and I was just wondering about that. If you do sign your contract as of the 21st for your specifications of your equipment, and an adverse decision comes through, you will be in an embarrassing position.

Mr. SCHACHT. There are two things that are possible, sir. One of them would be some type of a modification which would answer the problem that GAO has ruled against, or if it is not possible to go into this type of thing, then we would have to admit it and say that such a contract is not possible.

Representative HOLIFIELD. As a matter of completing the record, could you give us an estimate of what it would cost you to have a conventional steam-producing plant to complement this generating equipment which you are planning to install.

Mr. SCHACHT. You mean to replace it?

Representative HOLIFIELD. If you were going to build a conventional set of boilers, and burners, at this time, what would it cost you to build it and to bring your generating capacity to the full power?

Mr. SCHACHT. For this size plant it would be in the general neighborhood of $20 million.

Representative HOLIFIELD. So by increasing $5 million in this plant, you are to a certain extent obviating the necessity of investing $20 million in a steam-producing plant.

Mr. SCHACHT. Well, no, I misunderstood your question, because the $20 million is the complete plant, turbogenerator and all.

Representative HOLIFIELD. I wanted to differentiate between the steam producing and the turbine.

Mr. SCHACHT. The boiler itself would be approximately $5 million, and, in fact, that is how we arrived at the $5 million.

Now, we have done one thing in connection with this, that is not apparent from the contract. Because of the need of conventional power, and the fact that the reactor may have a couple of years of operation before it can be depended upon as a firm source of heat—we have ordered a conventional boiler, and propose to install it.

Representative HOLIFIELD. You are going ahead with a conventional boiler in any event?

Mr. SCHACHT. Yes.

Representative HOLIFIELD. And this is a supplemental.

Mr. SCHACHT. Yes, sir, that boiler was ordered back in May of this year. It is these specifications on it, for steam conditions, that we are confronted with.

Representative HOLIFIELD. Are there further questions?

Thank you very much.

Mr. Fields, will you and your group come back to the stand temporarily?

STATEMENTS OF GEN. K. E. FIELDS, GENERAL MANAGER; LOUIS H. RODDIS, JR., DEPUTY DIRECTOR, DIVISION OF REACTOR DEVELOPMENT; DELMAR M. MORRIS, ASSISTANT DIRECTOR FOR ADMINISTRATION, DIVISION OF REACTOR DEVELOPMENT; E. J. BLOCH, DIRECTOR, DIVISION OF PRODUCTION; DON S. BURROWS, CONTROLLER, DIVISION OF FINANCE; EDWARD DIAMOND, DEPUTY GENERAL COUNSEL; AND A. TAMMARO, ASSISTANT GENERAL MANAGER FOR RESEARCH AND INDUSTRIAL DEVELOPMENT—Resumed

Mr. FIELDS. Yes, sir.

Representative HOLIFIELD. The purpose of this part of the hearing is to obtain a status report on items in the authorization bill in which the committee is particularly interested. In this connection I would like to put into the record certain excerpts, a letter to Mr. Durham under date of August 29 from Mr. Fields.

A press release from the Atomic Energy Commission under date of August 30, inviting industry proposals for the design of gas-cooled power reactor.

An invitation for bids signed by Mr. W. Kenneth Davis, Director of the Division of Reactor Development, and two appendixes, A and B.

A letter from Senator Anderson to Chairman Carl T. Durham under date of September 2.

(The documents referred to follow:)

ATOMIC ENERGY COMMISSION,
August 29, 1957.

HON. CARL T. DURHAM,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR MR. DURHAM: The Commission is issuing a request for proposals from architect-engineers for the design of, and a report on, a 40,000 electric kilowatt natural uranium, graphite-moderated, gas-cooled, power reactor prototype as provided under project 58–e–14 of the recently enacted legislation authorizing appropriations for this Commission.

The contractor selected will recommend development work necessary for his design which can be carried out consistent with the desired schedules. Such work may be done by a nuclear engineering subcontractor or by other Commission contractors depending upon the circumstances. This procedure which is necessary to comply with the joint committee's directive does not foreclose the initiation of a more general development program or additional evaluation studies on gas-cooled power reactors at such time as the Commission deems appropriate.

Proposals are to be submitted not later than September 23, 1957, and award of a contract is expected shortly thereafter. The contract will require a report by March 1, 1958, which will serve as a basis for our report to your committee which is required not later than April 1, 1958.

A public release announcing our request for proposals will be issued tomorrow. A copy of the release is attached.

Sincerely yours,

K. E. FIELDS, *General Manager.*

ATOMIC ENERGY COMMISSION, WASHINGTON, D. C.

For immediate release
Friday, August 30, 1957

AEC INVITES INDUSTRY PROPOSALS TO DESIGN GAS-COOLED, NATURAL URANIUM-FUELED POWER REACTOR

The Atomic Energy Commission has invited United States architect-engineering firms experienced in reactor work to submit proposals for the engineering design of a natural uranium, gas-cooled, graphite-moderated nuclear electric powerplant of 40,000 electrical kilowatts capacity. September 23, 1957, has been set as the closing date for receiving proposals.

Development, design, and engineering work on this prototype plant has been authorized by the Congress. A report on the design, including cost estimates and schedule of construction, is to be submitted by the Commission to the Joint Committee on Atomic Energy of the Congress not later than April 1, 1958. Construction of the prototype powerplant has not been authorized by the Congress. No site for the plant has been selected.

Qualified firms interested in making such a proposal though not receiving a specific invitation to submit a proposal should address the Division of Reactor Development, United States Atomic Energy Commission, Washington 25, D. C.

ATOMIC ENERGY COMMISSION,
Washington, D. C.

GENTLEMEN: The Commission proposes to award a cost-plus-fixed-fee type contract for designing a natural uranium, graphite moderated, gas-cooled power reactor prototype. This project will comprise a nuclear reactor and powerplant including nuclear core, gas-cooling system, turbo-generating units, instrumentation, structures, utilities, and other appurtenances. Design must be completed as soon as practicable. To this end, preliminary design must be completed and the report thereon, required in appendix A, item IV attached, must be submitted by March 1, 1958.

It is anticipated that an architect-engineering firm will have prime responsibility for the work and that it will supplement its capabilities as necessary by association with a firm of proven ability in the design of reactors and other specialized nuclear components.

If you are interested in being considered for the engineering work on this project, you are invited to submit a proposal based on the requirements and data in appendixes A and B.

A contractor selection board will evaluate the replies received as a result of this inquiry. Selection will be based on an evaluation of proposals, supplemented to the extent considered necessary by the Commission by personal conference, in which all pertinent factors such as previous experience in similar work, availability of qualified personnel, and overall ability to perform and get underway with the job promptly upon award of contract will be taken into account.

Your reply should be addressed to Allan C. Johnson, Manager, Idaho Operations, United States Atomic Energy Commission, Post Office Box 1221, Idaho Falls, Idaho, and should be received not later than September 23, 1957.

Very truly yours,

W. Kenneth Davis,
Director, Division of Reactor Development.

Appendix A

ENGINEERING REQUIREMENTS

I. Develop design criteria and prepare an engineering design and cost estimate of a complete natural uranium, gas-cooled, graphite-moderated civilian power reactor and turbogenerator plant of 40,000 kilowatts (electrical) net. With the exception of fuel fabrication and processing facilities, all things necessary for the continued operation and maintenance of the plant shall be included in the design. Only minimum laboratory and administrative facilities shall be provided. Final site selection has not been made but the design and cost estimate will be predicated on a site at the National Reactor Testing Station, Arco, Idaho.

II. In establishing the design criteria the following shall be considered:

(*a*) A reactor optimized for the production of power from natural uranium is desired.

(*b*) The design shall be based on technology currently available to the United States AEC, with such development work as may be undertaken on a schedule consistent with initiation of a construction project in July 1958. Pertinent information on military, gas-cooled reactor projects will be made available.

(*c*) All United Kingdom information relating to Calder Hall to January 1, 1957, can be assumed to be available to all contractors.

III. Overall layout drawings on the entire system and its major components are required. Working drawings, details and specifications for construction, including a detailed estimate of the cost of construction based on the completed design, working drawings and specifications shall be provided. The requirements for the initial phase (title I) of the project are outlined below. Requirements and timing of the second phase will be developed in detail upon completion of title I.

IV. Preliminary design must be completed and the report thereon must be submitted by March 1, 1958. The report must include:

(*a*) Plans and outline specifications showing features and characteristics of the design proposed to meet the Commission's requirements. Detailed designs and specifications on unique components and systems shall be provided to the extent necessary to establish a realistic cost estimate for the entire system.

(*b*) Estimates of cost and time schedules for:

1. Completion of the design and working drawings. (Bearing in mind the need for expeditious completion.)

2. Construction. Separate cost and time schedules should be prepared for performance of construction by fixed-price and by cost-plus-fixed-fee contracts.

3. Developmental work.

(*c*) Operating cost estimate, including fuel element life estimates.

(*d*) Reactor hazards evaluation for the NRTS site, including possible containment requirements. Additional studies for other sites may be requested later.

(*e*) Outline procedures for startup, operation, and maintenance of plant including the basis for decision between shutdown fuel loading and fuel loading under power.

(*f*) Outline of fuel fabrication and processing requirements; and techniques available for accomplishment.

(*g*) The contractor's evaluation of the following:

1. Performance limits with the proposed design utilizing available technology and natural uranium.
2. The feasibility and effect on cost of future operation with slightly enriched loading.

APPENDIX B

EXPERIENCE RECORD AND BACKGROUND OF FIRM

I. For each type of design listed under 1 through 6 below, supply the following information:

(*a*) Location and name of project.
(*b*) Brief description of project.
(*c*) Client's name and address.
(*d*) Construction cost of the project related to your design.
(*e*) Total cost of your design work.
(*f*) Was design performed on a cost-plus-fixed-fee or firm-price basis?

1. The design of steam powerplants, both large and small, together with a brief description of the steam cycles employed.
2. Design of high-temperature and high-pressure gaseous systems with brief description of problems associated with thermal stresses, thermal shock, and welding of high alloy steels.
3. Design of chemical and industrial processes, including brief description of processes and methods of operation and control.
4. Design of ventilation and waste-disposal systems involving toxic materials, including brief description of toxic materials handled, problems of ventilation control and disposal of waste byproducts.
5. The development of new or unique equipment and processes such as original plant design evolved from laboratory and pilot-plant data.
6. Indicate specific examples where you have carried through the plant or process design, and also conducted preliminary operation and the preparation of operating procedures. Indicate in what manner you have performed these functions.

II. In connection with item I, indicate which, if any, of the above projects involve nuclear reactors and the extent to which your personnel participated in the nuclear design.

III. What part, if any, of of this proposed design work would you contemplate subcontracting to others? If the proposed subcontracting involves nuclear reactor design work, (*a*) list previous nuclear design work the subcontractor has performed; and (*b*) indicate the type of subcontract arrangements. Include statement on availability of subcontractor key personnel and staff similar to that required below.

IV. (*a*) Indicae where you would perform the design work. State the number of (1) engineers and (2) draftsmen you employ at that location on a full-time basis. How many of these would be available for assignment to this project on a full-time basis? State the extent to which staffing at this location can be supplemented from other locations of your firm. Indicate the total number of engineers and draftsmen employed by your company on a full-time basis if different from that above.

(*b*) Indicate average workload of firm, in terms of related construction costs, over the past 5 years. Indicate your current workload and percentage of completion.

V. (*a*) List names and brief experience record of key personnel available for assignment to this project. This shall include names of personnel to be assigned to head up the following functions:

1. Overall full-time project supervision (project manager).
2. Nuclear engineering.
3. Instrumentation engineering.
4. Structural engineering.
5. Electrical engineering.
6. Civil and sanitary engineering.
7. Mechanical engineering.
8. Field inspection of construction.

(*b*) Submit a proposed organizational chart showing the names and positions of company personnel, including those mentioned in (*a*) above, which you propose for assignment to this project from the project manager down through all key positions. This chart should be in sufficient detail to indicate chain of command and workflow for all major aspects of the work.

SEPTEMBER 2, 1957.

Hon. CARL T. DURHAM,
Chairman, Joint Committee on Atomic Energy.

DEAR MR. CHAIRMAN: This is with reference to the announcement on August 30 by the Atomic Energy Commission that it is soliciting bids from architect-engineering firms to submit proposals for the engineering design of a natural uranium, gas-cooled graphite-moderated nuclear electric powerplant.

As you know, this request for bids concerns a project which was recommended by the Joint Committee and authorized by the Congress as being necessary if the United States intends to maintain leadership in atomic technology. Also, as you know, despite the testimony of experts as to our present need for advancing natural uranium gas-cooled reactor technology, the AEC did not support such a project.

The AEC announcement is not clear that the contemplated design will be of an advanced type of reactor and not simply a copy of the existing Calder Hall reactor. The intended objective, of course, as shown by the committee's report is an improved and advanced concept of what the British to date have accomplished. The committee's report also makes clear that the prototype should be developed and designed in such a manner that enriched fuel elements can be tried out in the reactor.

In view of the importance of this project and the Commission's lack of support in this field of reactor development, I believe it is imperative that in accordance with its duties by law, the Joint Committee closely follow the administration of this project to insure compliance with legislative intent and full implementation of congressional authorization. I think it important that the Joint Committee be certain that it is kept fully and currently informed at all times relative to this project.

Sincerely yours,

CLINTON P. ANDERSON, *Vice Chairman.*

Representative HOLIFIELD. Now, Mr. Fields, we are going to try to expedite this, in view of the appointment which we know the chairman has.

Mr. FIELDS. Could I make just one remark about the discussions with respect to Consumers? That is, Mr. Leininger indicated there had been a wire sent to the Commission last week, and I have asked the representatives of the Commission who are here with me today, and we are not aware that we have received a wire, and I would appreciate receiving a copy from him, and I will check with him after the meeting. But we are not aware that we have received such a wire. It could be that it has been received. That is all I wanted the record to indicate.

Representative HOLIFIELD. There are a few questions I would like to go through as quickly as possible. That is on the gas-cooled reactor. Why was not the Joint Committee given a copy of the AEC prospectus which was sent out to industry?

Mr. FIELDS. I thought it was. Mr. Ramey asked me about it, and we forthwith gave him copies of it, and it was a complete oversight. It was intended to be a part of the letter, I believe. There was no intent not to forward this to you.

Mr. RODDIS. It was to be an enclosure to the letter.

Representative HOLIFIELD. The staff informs me that they did not receive it at the time that the letter went out, and they had to ask for it.

Mr. FIELDS. Well, the only thing I can think of, Mr. Chairman, is that, had we known that you wished it, it would have been there forthwith. It was as soon as Mr. Ramey asked me about it, and there are many proposals that we go out on, on any number of projects, where I am sure that we advise the committe, but we do not send them the proposals.

Representative HOLIFIELD. The committe is especially interested in this particular proposal, and the Chair does not need to announce it. The legislation of the last session of the Congress would indicate it. My next question is: Why was not the committee given copies of the North American reports on gas-cooled reactors while the proposal on gas-cooled reactors was being considered by the committee?

Mr. FIELDS. You have me at a loss, Mr. Chairman. I do not know the matter that you are referring to.

Representative HOLIFIELD. The committee was given large numbers of American Standards reports on gas-cooled reactors, but had to obtain the North American reports from the contractor.

Mr. RODDIS. Mr. Holifield, at least one copy, I believe, was sent up under a date which I will have to verify. I gave Mr. Ramey two more copies this morning, when I found out that he wanted more. But there was certainly no intention to withhold copies from the Joint Committee.

Representative HOLIFIELD. This report was available during the time that legislation was under discussion. We did not have access to it, however, until the last days of Congress.

Mr. RODDIS. I will have to verify the dates on the letters, but it was my understanding that a copy was sent up here early.

Mr. RAMEY. The report was made available only by the contractor and after a request by the committee staff. It was never sent up by the Commission.

(With regard to this point, the AEC subsequently informed the Joint Committee, as follows:)

Concerning AEC submission to the Joint Committee of copies of the North American report evaluating the Calder Hall project, a check of our files indicates that no copies of this report were sent to the Joint Committee prior to September 17, when they were furnished to Mr. Ramey. (See also Appendix III, p. 65.)

Representative HOLIFIELD. The committee does feel that the Commission should have presented that report, and at the time when the matter was of serious consideration both from the standpoint of the committee hearings and from the standpoint of legislative action on the floor. It was available, and we feel that the Commission did not fulfill its obligation to the committee of keeping the committee fully informed.

Mr. FIELDS. I am sure, Mr. Chairman, I will have to disagree with you on that point, insofar as any intent on our part was concerned. As far as I know, we did not realize that this would be a part of the considerations. The questions of proposals and reviews of the gas-cooled reactor project—we had commented to the committee on our views on this at times by letter. I don't recall where the Commission as such—we did testify with respect to our views on that, sir.

Representative HOLIFIELD. The economics of the report would have been valuable to the committee.

Mr. FIELDS. I am sure we regret it did not get to the committee. I just do not know, and I will have to review the circumstances of it.

Representative HOLIFIELD. Have you, General Fields, or Mr. Tammaro, read the committee report with respect to utilizing Westinghouse and Admiral Rickover, page 26:

The committee is of the opinion that the smallest practical prototype of a natural uranium, gas-cooled reactor should be designed and constructed as soon as practicable. It is believed that the reactor developed will represent an advance on the Calder Hall technology. It is understood that the gas-cooled, prototype reactor contemplated in the bill can be so designed as to operate on natural uranium fuel initially, but later can test out the feasibility of slightly enriched fuel elements.

In view of the availability of the AEC installation at Bettis and Idaho, including able designers and engineers of the Bettis group, and the willingness of Admiral Rickover to undertake direction of the gas-cooled-reactor project, it would appear essential that this project be assigned to the Naval Reactors Branch under Admiral Rickover.

In the committee's view, the design and construction of a gas-cooled prototype under Government contract as outlined above would have the following advantages—

And they are listed, and they will be included in the record at this point—

(1) It would enable this country to obtain gas-cooled technology and knowhow rapidly by utilizing the well-rounded Westinghouse Bettis group and facilities under Admiral Rickover's direction.

(2) Although the best way to get atomic technology is through actual experience, the cooperative arrangements between the British and the AEC Naval Reactors Branch on naval reactor work, and United States rights to Calder Hall technology under a direct-contract operation would assure that all appropriate technical information is brought to bear in developing an advanced gas-cooled reactor prototype.

(3) Since the moderator would not be heavy water, the reactor and its development would differ considerably from the gas-cooled, heavy-water reactor sponsored by the the Florida power group and Dr. Zinn. The size of the 2 reactors also differs markedly, 40,000 to 140,000 kilowatts. However, information and data developed on common problems in the gas-cooled field would be readily available to Dr. Zinn and his associates.

I am sure you are aware of the committee's feelings on that point.

Mr. FIELDS. Yes, I am.

Representative HOLIFIELD. Has Westinghouse been contacted in regard to making a study on this?

Mr. FIELDS. They have been given a copy of the proposal, as have a number of other firms, and we understand that they—and this is informally—are considering arrangements with several different architect-engineers actually with respect to this.

Representative HOLIFIELD. You have had a number of contacts?

Mr. FIELDS. This is all informal, and the date for receipt of proposals I believe is the 23d, next Monday; yes, sir.

Representative HOLIFIELD. You have had proposals in from the Bettis group as well as others?

Mr. FIELDS. We have had discussions and we understand informally what they are considering, and their proposals are not in from any source that I am aware of at this point. There is considerable interest in this, I might say.

Mr. RAMEY. Would this be the Bettis group? Westinghouse has two organizations. They have a private commercial outfit, and then they have the Bettis government-financed group. Is it understood that the Bettis group is submitting a proposal?

Mr. FIELDS. I am not aware that the Bettis group is submitting a proposal.

Mr. RAMEY. Would they be permitted under your announcement?

Mr. FIELDS. Our proposal has gone out to architect-engineering firms, and indications are that we would expect that they would associate themselves with groups who had the capability to perform the necessary nuclear studies and analysis here to come up with this particular report. It is not proposed that if there is a large-scale development to be undertaken here it could be undertaken in conjunction with this $3 million authorization project. We have not foreclosed what might be done on a full long-term development project with respect to gas-cooled reactors. We are attempting to outline the engineering aspects, cost schedules, and what would be involved so that we could move quickly into construction, and also to outline a development that would be necessary in conjunction with such a project, and to have it in hand so that it can be submitted to Congress as required by law on the 1st of April. That is the reason for moving thus rapidly in this regard.

Representative HOLIFIELD. The committee, of course, appreciates the fact that the Commission has moved into this and is taking action on it.

Mr. FIELDS. The Bettis field group is not an architect-engineering firm. They are a development firm and they do not even do the architect-engineering for the PWR. They have done the development work. So this is the problem that was posed to us. We have considered all aspects of this, and I want to assure you as far as I know, Mr. Chairman, we are moving as rapidly as we can on this, and with a real effort to do this in a manner which will present to the Congress all of the things that are necessary for there to be a decision made next spring on whether or not this should be constructed full scale.

Representative HOLIFIELD. Is it your concept that the report will cover an advance on the Calder Hall concept, or does the language of your proposal preclude an advance on the so-called Calder Hall concept and confine it to technology which is now available to the AEC? Either in your own files or from your agreement with Great Britain.

Mr. FIELDS. It is limited to basing it on present technology available, with such development and improvement as could be accomplished consistent with a time scale of starting construction on the 1st of July of 1958, which is a requirement of the authorization. That is, as we understand it.

Representative HOLIFIELD. I would like to clarify that. I was somewhat surprised when I read it and I appreciate the speed with which you are moving, and in view of the fact it was realized that there was a difference of opinion on going into this project. But where do you get the July 1 date to initiate construction?

Mr. FIELDS. I thought that was in the report.

Representative HOLIFIELD. I am sure you will not find it in the legislation. It seems to me that rather than being of value to the project, this might impose such a rigid requirement that it would preclude the study almost to a failure to begin with.

If you insist because the study was to be submitted on April 1 that construction should start on July 1, I have not had a chance to talk with any of the manufacturers, but I am wondering if in that particular requirement you are not processing in such a way that you would eliminate some interest in the study.

Mr. RODDIS. I think that remains to be seen, but from the indications we have at the moment that won't be true. Now, start of construction on July 1 does not mean that you start building huge structures at that time, but you do start entering into contracts for the construction by that time. That would be the way we anticipate it. The project would be initiated.

Representative HOLIFIELD. This was so much faster than anything that has occurred under any of your proposals that I become just a little bit surprised. After using 27 months to negotiate with Consumers, I am rather bewildered by this sudden show of speed on the part of the Commission. I don't want to put you in the position of complaining because you do hurry and complaining because you don't, but I think that there is something there. I would like to read the language of the act here. It says:

the Commission shall proceed with sufficient design work together with the appropriate engineering work necessary for the Commission to begin construction as soon as practicable after the authorization by the Congress, of a large-scale natural uranium power reactor or prototype.

Mr. FIELDS. I am sure, if we had interpreted that to be July 1, 1959, we might be hearing in another vein with respect to this.

Mr. RAMEY. Is it your determination that it is practicable to initiate a construction project on July 1, 1958, after the submission of this report on April 1? That language would require the Commission to make this judgment as to practicality, and it seems that there could be some judgment or some determination that that is a little hasty. You could make a determination for September which might give a little more time to get a design underway on a more advanced type of reactor. You could also have made a determination that the Congress would not authorize it before July 1. This year Congress did not authorize these projects until August.

Mr. FIELDS. Well, the difficulty may be in our understanding each other here. That is the description of what an advanced type is. When you take other factors in the legislative history, then I think that you must have a clearer definition of what an advanced type is, and if advanced type is an enriched type reactor of a different sort entirely, rather than just this type of an arrangement, then I would not read that the legislative history says we have the latitude to do this. But certainly we have, we believe, the belief that it is practical to build one, if it is desired to, or if the feasibility shows it is attractive to do so, and then this is within the region of practicality of when you could begin construction. That is, to begin a construction project; involved with that will be some development work that will be necessary further to do.

It might have improvements, I would hope, over the Calder Hall type of reactor itself, and it would not be a long 4- or 5-year development project, and we have not thought of it in those terms, before you build one of these, so that you get the most advanced type reactor, say, 4 years from now, but you start construction. But it would be certainly an improvement over the Calder Hall reactors that are now in operation.

Mr. RAMEY. Or the CEA reactor, the British-type reactor being built for commercial purposes. As I understand it, the present Calder Hall is a dual-purpose one.

Mr. RODDIS. This depends upon what the definition of natural uranium graphite moderated reactor is. If it is only a natural uranium machine operated to low burnups, I don't think that there is any question that one could be built. There is a question as to whether it is an economic machine in that sense. This is Calder Hall. This CEA unit which implies a high burnup fuel element involves a certain amount of development, a substantial amount of development, we believe.

I rather doubt that development could all be completed before you started construction. But we have started construction of other reactors before we had all of the development problems solved.

Mr. RAMEY. Shouldn't it be made clear in your prospectus that you would at least like to have it as far developed as the CEA type.

For example, your North American report, in certain cost estimates assumed that you could reach the higher temperature fuel elements of the CEA type, as I understand it. Should not your prospectus make that clear to these people, so that when you come in with this sort of thing you will be doing that?

A very short time schedule, or a limited amount of technology, would require you just do it as a carbon copy.

Mr. RODDIS. That in part depends upon your definition of a natural uranium graphite moderated reactor which is what the authorization states. We have asked, if you will note the last page of appendix 1, of the proposal or the invitation, for two items. Item (g) we ask for the contractors' evaluation of the following:

(1) Performance limits with the proposed design, utilizing available technology and natural uranium; and (2) the feasibility and effect on cost of future operations with slightly enriched loading.

Now, to my mind, a slightly enriched loading may be achieved either through uniform slight enrichmnt of through the use of natural uranium and what are called special ore seeds. Certainly the average load of the machine is in that case a slightly enriched one, and that has been our interpretation.

I do not know whether the CEA reactors are going to operate with slightly enriched uranium or with natural uranium.

Representative HOLIFIELD. I only regret that we can't go into this matter fully, because we are going to have to adjourn because of other commitments. We have run over at the present time.

Before we adjourn I want to make this statement: The committee does not desire hasty action or the imposition of impracticable and impossible requirements on the study of this matter, nor the initiation of the project under such rigorous terms that the Commission can come up and say: "Well, we have not found it practicable, and we rule against it because of the time limitation involved."

There is no July 1 initiation of construction in the act, and this is a date of your own conception. The date for the presenting of a study is April 1, in the act, and the words "as soon as practicable after authorization by the Congress," certainly give you more latitude than July 1. Personally, I think that the imposition of this type of a rigorous demand that might contribute to a lack of interest on the part of some people if they knew that they had to initiate construction within 90 days after they submit it to you, and before you had a chance to properly evaluate it or the Congress itself even had a chance to

evaluate it and act upon it from the standpoint of authorizing the money to do it.

So the committee is going to watch this very closely, and we expect the Commission to act in good faith and to follow the legislative history which has been written on the subject. We do not expect a duplicate of the obsolete Calder Hall reactors. We believe that there can be an advanced concept, and an improvement over the Calder Hall, and personally I do not support any appropriation which would be to build a duplicate of the Calder Hall reactor, I consider it is obsolete, and I consider that the new natural uranium gas cooled reactor in Britain are a great deal more advanced than the Calder Hall design, and I think it is therefore up to American scientists and up to the Commission to look at this thing in that light, rather than the light of doing something which the committee did not ask to be done.

The committee specifically said it is "believed that the reactor developed should represent an advance on the Calder Hall technology, with, for example, higher temperature fuel elements."

You are aware of the development of certain materials which does allow higher temperature elements, and more evidence, and which is being used in the new reactors in Britain.

You are also aware, no doubt, of a different type of enrichment rather than the U–235 enrichment which is being experimented with.

My next question was going to be on your plutonium recycle. Are you proceeding along the line of getting some real thinking done on that, and planning done on that line?

Mr. Roddis. Mr. Holifield, in addition to this specific invitation, we are doing four other things. In the gas-cooled machine, we are doing other things than just this thing. We have asked Dr. Zinn's group to conduct an evaluation, and our Oak Ridge Laboratory to do it in addition to the Atomic International and American Radiator or American Sanitary.

Dr. Zinn is in England this week with Mr. Davis, looking at this very closely, and then they are going to France, because the French have some different approaches to this problem.

On the plutonium recycle reactor project, as you know, work has been under way at Hanford for approximately a year and a half on this. Preliminary design has in fact been nearly completed on the plutonium recycling reactor itself. Meanwhile, the development program on the making of plutonium fuel elements, which is after all the whole purpose of the plutonium recycling reactor, is to test such elements, has been progressing at Hanford, at Los Alamos, and at the Argonne National Laboratory. We are, of course, at each of these sites posed some difficult problems on facilities, because to work with plutonium requires a certain amount of, or rather a large amount of special facilities.

The first of these facilities, especially devoted to the reactor program, is being equipped at Argonne now. That is the building and ventilation is finished, and the equipment is being placed in it. In last year's authorization bill was contained the fuel technology center there, and the addition to the Hanford fuel technology center. So that we are doing what we can in existing facilities and we are trying to build facilities which can be committed specifically to the reactor program because at the present time we are always involved in inter-

ference with weapons work where we just received a big setback last week in one area.

The reactor itself, the people from Hanford are in fact in here today, and we expect in that case to do the architect engineering work on it at Hanford, with GE. You see both GE and Du Pont are the two installations that have always done their own architect engineering work, and we expect to complete design of that shortly, and we expect to actually start construction probably some time in the winter. That is the actual digging of a hole. That reactor is going to be a heavy water tube type reactor, vertical geometry, if you are interested we have a considerable amount of design detail on it available.

Representative HOLIFIELD. That is encouraging.

Mr. RODDIS. I hope I have made clear that the plutonium recycling program is not only the plutonium recycle reactor, because the making and testing of fuel elements to go into that is what is important, and on that we have had to create these other facilities, and the whole program is moving as well as could be expected.

Representative HOLIFIELD. That is encouraging.

We are going to have to call this meeting to a close at this time. We will look forward to having a review of this in more detail in January when we come back.

Mr. FIELDS. Could I have the record show that with respect to the special-purpose reactor for production of plutonium, we advised Mr. Durham as to what we were doing, and we have had representatives of General Electric and of Du Pont in yesterday and today with respect to preliminary studies as to the actual construction of such a reactor and the design work for it in accordance with the $3 million authorization in the bill.

(The communication referred to follows:)

ATOMIC ENERGY COMMISSION,
September 4, 1957.

Hon. CARL T. DURHAM,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR MR. DURHAM: Public Law 85–162, 85th Congress, includes authorization for the expenditure of $3 million for development, design, and engineering on a production reactor for special nuclear materials (project 58–b–8).

Within the next few weeks, we plan to discuss with personnel of the Hanford and Savannah River offices and of the General Electric and du Pont Cos., their proposed plans to undertake work on reactor types they believe feasible of providing increased productive capacity in accordance with this authorization. Upon completion of these preliminary discussions, we plan to initiate development and design work on a specific reactor type or types so as to permit submission of a report to the Joint Committee on Atomic Energy by April 1, 1958.

Sincerely yours,

R. W. COOK, *Acting General Manager.*

Representative HOLIFIELD. You are excused, and thank you very much.

I want to ask the American Machine & Foundry man, Mr. Snapp, who on my instructions, the staff contacted and requested to return to this meeting, in view of this other appointment, will you file a statement with the committee within the next 2 or 3 days, in regard

to the reasons for the changing of your position on the Elk River reactor?

Mr. SNAPP. We will be glad to do that, Mr. Chairman.

(The statement referred to will be found on p. 14.)

Representative HOLIFIELD. Do you have a prepared statement?

Mr. SNAPP. No; we do not.

Representative HOLIFIELD. Very well. We will adjourn now.

(Thereupon at 4:15 p. m., the hearing was concluded.)

APPENDIXES

APPENDIX 1

Resolution

Whereas section 111b of Public Law 162, 85th Congress, provides that before the Commission enters into any arrangement the basis of which has not been previously submitted to the Joint Committee on Atomic Energy and which involves appropriations authorized by subsection a of section 111, the basis for the arrangement which the Commission proposes to execute shall be submitted to the Joint Committee, and a period of 45 days shall elapse while Congress is in session: *Provided, however*, that the Joint Committee, after having received the basis for a proposed arrangement may by resolution in writing waive the conditions of or all or any portion of such 45-day period, and

Whereas the Commission by letter dated September 13, 1957, submitted to the Joint Committee the bases of proposed arrangements with the Consumers Public Power District of Nebraska, and with the Northern States Power Co., of Minneapolis, Minn.; and

Whereas the Commission requested waiver of the 45-day period by the Joint Committee as to these two proposals; and

Whereas the Subcommittee on Legislation of the Joint Committee considered the Commission request at a public hearing on September 17, 1957, and

Whereas the members of the Joint Committee have been polled as to the request of the Commission: Now, therefore, be it

Resolved, That, pursuant to section 111b of Public Law 162, 85th Congress, the Joint Committee hereby waives requirements of the 45-day period while Congress is in session as to the proposals of Consumers Public Power District of Nebraska and the Northern States Power Co., of Minneapolis, Minn., as requested by the Commission letter dated September 13, 1957, a copy of which (with enclosures) is attached to this resolution.

Dated this 19th day of September, 1957.

(The letter and enclosures referred to will be found on p. 3.)

APPENDIX 2

Atomic Energy Commission,
Washington, D. C., September 20, 1957.

Hon. Carl T. Durham,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States.

Dear Mr. Durham: In Mr. Vance's letter of September 13, 1957, to you transmitting the bases for proposed cooperative arrangements with the Consumers Public Power District and Northern States Power Co., we stated that we were submitting to the Comproller General a question in connection with each proposed contract as to our authority to enter into certain commitments for the operating period.

We have been advised by the Comptroller General that "we believe that the Commission is authorized to enter into contractual arrangements of this type with Consumers, and we will not be required to object thereto." A copy of that letter is attached for your information.

The obligations which the Commission proposed to undertake for the operating period included payments for excess fuel costs and for certain maintenance and related expenses totaling an estimated $8 million. While the Comptroller General's letter specifically refers only to the fuel cycle costs, it also refers to

the total figure of $8 million and we have been assured by the General Counsel of the General Accounting Office that the principles stated in the letter are intended to be applicable to maintenance and related costs as well.

Sincerely yours,

K. C. FIELDS, *General Manager.*

COMPTROLLER GENERAL OF THE UNITED STATES,
Washington, September 19, 1957.

Hon. LEWIS L. STRAUSS,
Chairman, Atomic Energy Commission.

DEAR MR. STRAUSS: Reference is made to letter from the Deputy General Manager dated September 12, 1957, requesting decision whether we would object to certain provisions of a proposed contract between the Commission and Consumers Public Power District of Nebraska. The proposed contract is the third under the first round of the Commission's power demonstration reactor program, and provides, generally, for the construction and operation of a sodium-graphite nuclear reactor to produce electricity. Both the Commission and Consumers will participate in the cost of necessary research and construction, and Consumers will furnish the Commission with all pertinent information developed prior to and during a 5-year operating period.

The particular contract provisions which have raised the question now presented to us call for the Commission to reimburse Consumers for certain fuel cycle costs incurred during the 5-year period beginning with the commencement of actual plant operation. Essentially, the arrangement proposed is that Consumers will bear such costs up to the cost of conventional fossil fuels and the Commission will bear any excess. The doubt which exists as to the propriety of making such contractual commitments arises from the general rule stated in decision B-130815, dated September 3, 1957, to the effect that funds appropriated to the Commission for the needs of a particular fiscal year could not be obligated for the needs of future fiscal years. In our opinion, the legislative history of Public Law 85-162, approved August 21, 1957, which authorized appropriations for the Commission for its power demonstration reactor program for the fiscal year 1958, and the action of the Congress in appropriating funds for the Commission in accordance with that authorization act, clearly show congressional approval of the proposed contractual arrangements.

As reported out by the Joint Committee on Atomic Energy, the authorization act would have required the Commission to make somewhat different contractual arrangements with Consumers and with the sponsors of projects under the second round of the power demonstration reactor program. After considerable discussion on the floor of Congress, the authorization act was changed to permit the Consumers project to be carried out as previously contemplated. The Joint Committee and the Congress were fully apprised of the fact that the contemplated arrangements with Consumers included provision for the Commission to bear a part of the fuel cycle costs during a 5-year operating period. See, for example, pages 13972-13973, 14486, and 14487, Congressional Record, August 20, 1957. The amount included in the authorization act included $8 million specifically justified to the Joint Committee as fuel cycle costs under the Consumers project.

In view of the legislative history, we believe that the Commission is authorized to enter into contractual arrangements of this type with Consumers, and we will not be required to object thereto. In view of the need for expeditious decision on the matter, no other aspects of the proposed contract have been considered.

Sincerely yours,

JOSEPH CAMPBELL,
Comptroller General of the United States.

APPENDIX 3

UNITED STATES ATOMIC ENERGY COMMISSION,
Washington D. C., November 21, 1957.

Hon. CARL T. DURHAM,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR MR. DURHAM: You will recall that during the September 17, 1957, hearings before your committee a question was raised regarding the circumstances under which the North American Aviation, Inc., report on the Calder Hall type nuclear powerplant was submitted to the JCAE. I have asked the Division

of Reactor Development to look into this matter and they have provided me with the following information.

This report is actually contained in two parts. The first was sent to us on January 25, 1957; the second was dated June 28, 1957. Thus, the complete report giving a comprehensive evaluation of this type of reactor was first available to us early in July.

Despite a delay in forwarding the reports it was certainly not our intention to keep these reports from the JCAE. Our letter of May 16, 1957, reporting on the status of the United Kingdom nuclear power program referred to the conclusions reached in the January report. In early August, Mr. Hollister of the JCAE staff called the Division of Reactor Development regarding the availability of these reports. He was advised that due to the shortage of copies of the reports we could not supply him with copies at that particular time. He was, however, invited to use the copies of the report on file in the Division of Reactor Development. Subsequently, when the local NAA representative furnished copies of the reports to the JCAE it was done with our approval.

We are, of course, very aware of our responsibility for keeping the JCAE informed of all aspects of our program. We shall make every effort in the future to provide the JCAE with copies of reports of this type as soon as possible after they are received.

Sincerely yours,

R. W. Cook, *General Manager.*

(Editorial Note.—Compare with AEC statement on June 26, 1957, during authorization hearings at page 563. Hearings before the Subcommittee on Legislation of the Joint Committe on Atomic Energy, Congress of the United States, 85th Congress, 1st session, on Authorizing Legislation for AEC's Fiscal Year 1958 Construction Budget.)

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WEST BERLIN REACTOR

HEARING

BEFORE THE

JOINT COMMITTEE ON ATOMIC ENERGY

CONGRESS OF THE UNITED STATES

EIGHTY-FIFTH CONGRESS

FIRST SESSION

ON

WEST BERLIN REACTOR

MARCH 6, 1957

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GEORGE NORRIS, Jr., *Committee Counsel*

WEST BERLIN REACTOR

MARCH 6, 1957

CONGRESS OF THE UNITED STATES,
SUBCOMMITTEE ON AGREEMENTS FOR COOPERATION,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D. C.

The Subcommittee on Agreements for Cooperation met, pursuant to call, at 10 a. m., in the committee room, the Capitol, Hon. John O. Pastore (chairman of the subcommittee) presiding.

Present were: Representative Carl T. Durham (chairman of the full committee), John J. Dempsey, Sterling Cole, James E. Van Zandt, and James T. Patterson; Senators John O. Pastore (presiding), Bourke B. Hickenlooper, and John W. Bricker.

Staff members present: James T. Ramey, executive director; Messrs. George Norris, Jr.; George E. Brown, Jr.; Wade Dickenson, and Hal Hollister.

Representatives of the Atomic Energy Commission: Hon. Thomas E. Murray, Commissioner; R. W. Cook, Deputy General Manager; Allen J. Vander Weyden, Deputy Director, International Affairs; L. H. Roddis, Deputy Director, Division of Reactor Development; N. F. Sievering, Division of Reactor Development; Paul Foster, Assistant General Manager for International Activities; John A. Hall, Director, Division of International Affairs; A. A. Wells, Office of the General Counsel; and Bryan LaPlante, special assistant to the General Manager.

Representatives of the Department of State: Gerard Smith, special assistant to the Secretary of State for Atomic Energy; Raymond E. Lisle, Deputy Director of the German Office; William K. Miller, economic officer for the German Office; Richard Kearney, legal adviser for German affairs; J. Robert Schaetzel and Robert M. Winfree, Office of Mr. Smith; and Stanley Cleveland, European Regional Affairs Office.

Senator PASTORE. Gentlemen, if we are ready, we ought to get started.

This is a meeting of the Subcommittee on Agreements for Cooperation. It is to discuss the legislation needed in order to permit a research reactor of the swimming-pool type to be built in West Berlin. I understand that the State Department and the Atomic Energy Commission are both represented here and that they have statements. I think we ought to begin with the Atomic Energy Commission if that is satisfactory.

STATEMENTS OF THOMAS E. MURRAY, COMMISSIONER; R. W. COOK, DEPUTY GENERAL MANAGER; ALLEN J. VANDER WEYDEN, DEPUTY DIRECTOR, INTERNATIONAL AFFAIRS; L. H. RODDIS, DEPUTY DIRECTOR, DIVISION OF REACTOR DEVELOPMENT; N. F. SIEVERING, DIVISION OF REACTOR DEVELOPMENT; PAUL FOSTER, ASSISTANT GENERAL MANAGER FOR INTERNATIONAL ACTIVITIES; JOHN A. HALL, DIRECTOR, DIVISION OF INTERNATIONAL AFFAIRS; A. A. WELLS, OFFICE OF THE GENERAL COUNSEL; AND BRYAN LaPLANTE, SPECIAL ASSISTANT TO THE GENERAL MANAGER, ALL OF THE ATOMIC ENERGY COMMISSION

Mr. Hall. If that is agreeable to Mr. Lisle.

Mr. Lisle. Surely.

Representative Cole. Mr. Chairman, may I state for the record, in case anybody may wonder why this bill carries my name rather than that of some other member of the committee or the chairman, that a week or so ago the chairman, Mr. Durham, inquired if I would care to introduce the bill. I said that I was ready to cooperate in any way possible and he suggested that I introduce it. That is the reason it has my name.

Mr. Hall. Mr. Chairman, may I proceed?

Senator Pastore. All right.

Mr. Hall. In the furtherance of an important foreign policy objective the Department of State has for the past few months been consulting with AEC in order to work out an arrangement under which the United States could cooperate with West Berlin in the peaceful uses of atomic energy. In particular, the Department has advised the Commission that the authorities in West Berlin are enthusiastic about obtaining a research reactor at the earliest practical moment and that West Berlin is prepared to provide all funds necessary to its construction and operation.

In the initial discussions concerning the development of a possible arrangement for the construction of a research reactor in West Berlin, consideration was given to a swimming-pool reactor. Subsequently, however, the West Berlin authorities decided to purchase and have constructed a 50-kilowatt water boiler research reactor to be located in the American sector of Berlin in an isolated forest area about 15 miles from the center of the city, and have undertaken negotiations with North American Aviation Corp. for this purpose.

The Department of State has advised that a 2,500-kilowatt swimming-pool reactor purchased from the Soviet Union is now under construction in Dresden in East Germany and that a research center will be established there. Further, we are informed that the presence of a United States reactor in West Berlin would go far in counteracting any phychological advantage that might accrue to international communism as a result of this project.

The Commission has explored the possibility of cooperating with West Berlin under the present Atomic Energy Act so as to permit the export of the desired reactor and its fuel to Berlin. Inasmuch as West Berlin does not constitute a nation capable of making an agreement for cooperation in accordance with the Atomic Energy Act of 1954, the Commission believes that the legal basis for the export of a reactor

from the United States or the distribution of special nuclear materials to Berlin is unclear. The benefits of the existing Agreement for Cooperation with the Federal Republic of Germany are not available to West Berlin, since West Berlin is not under the jurisdiction of the Federal Republic. Accordingly, in the judgment of the Commission, an amendment to the Atomic Energy Act is needed to permit the United States to cooperate with West Berlin in a manner similar to the cooperation which is permitted with nations.

The proposed amendment would authorize cooperation with West Berlin by permitting the distribution to West Berlin of source, special nuclear, and byproduct material, permit the export of reactors to West Berlin, and authorize the conduct in West Berlin of activities covered by section 57a (3) of the act. Restricted data would not be authorized for transmission under this amendment.

An agreement for cooperation pursuant to the proposed amendment would be entered into by the Federal Republic of Germany on behalf of West Berlin inasmuch as under existing arrangements the Federal Republic carries on foreign relations in behalf of West Berlin. However, the terms of such an agreement would require West Berlin to make the guaranties specified in section 123 of the act with the approval of the Allied commandants.

Senator PASTORE. Congressman Cole.

Representative COLE. I notice, Mr. Hall, that your statement has identified the locality as West Berlin and the bill refers to it as Berlin. Is there any reason why the law should not identify it as West Berlin rather than Berlin?

Mr. HALL. I prefer that my State Department colleague here answer that.

Representative COLE. Let's have the other statement and then we can ask our questions.

Senator PASTORE. All right, then, would you identify yourself for the record?

STATEMENTS OF GERARD SMITH, SPECIAL ASSISTANT TO THE SECRETARY OF STATE FOR ATOMIC ENERGY; RAYMOND E. LISLE, DEPUTY DIRECTOR OF THE GERMAN OFFICE; WILLIAM K. MILLER, ECONOMIC OFFICER FOR THE GERMAN OFFICE; RICHARD KEARNEY, LEGAL ADVISOR FOR GERMAN AFFAIRS; J. ROBERT SCHAETZEL AND ROBERT M. WINFREE, OFFICE OF MR. SMITH; AND STANLEY CLEVELAND, EUROPEAN REGIONAL AFFAIRS OFFICE, DEPARTMENT OF STATE

Mr. LISLE. My name is Raymond Lisle, Deputy Director, Office of German Affairs, Department of State.

As the committe is doubtless aware, the United States has unusual legal and political interests and responsibilities in Berlin. The United States is an occupying power in Berlin. It has on numerous occasions reaffirmed its intention to maintain the security and welfare of the city. Although we discontinued economic assistance to the Federal Republic of Germany some time ago, in line with our special status and our responsibilities in Berlin we have continued to extend economic assistance there.

Senator PASTORE. Before you go further, how do you distribute aid there? To what Government or to what agency? How does that operate?

Mr. LISLE. It is distributed to the government of Berlin—the executive agency which is known as the Senat.

Senator PASTORE. All right.

Mr. LISLE. The unique situation of Berlin has brought us not only responsibilities but opportunities of the greatest political importance. It is an enclave—an island entirely surrounded by the Soviet Zone of occupation, but is free from Soviet control. Its life and achievements are far more accessible to the people of Soviet-occupied Germany than those of any other part of the free world. As a result they have come to be a significant illustration of the moral and material advantages of the free world and a constant and effective challenge to the efforts of the Communist authorities to develop acceptance of their control. We make a great deal, I must say, of the show window concept of Berlin and the extent to which it provides us with an opportunity to demonstrate the achievements of the western free world to the people of the Soviet Zone.

As the Atomic Energy Commission has informed the committee, the western sectors of Berlin are not under the jurisdiction of the Federal Republic of Germany, and the benefits of the agreement for cooperation with the Federal Republic are not available to Berlin.

It might be useful to explain briefly the status of the western sectors of Berlin and the situation with respect to atomic energy matters.

Berlin continues to be under military occupation. The United States, British, and French commandants retain supreme authority in the western sectors and, acting jointly, constitute the ruling body in those sectors. The United States commandant exercises this authority in the United States sector. The Soviet Union has no voice in the government of the western sectors.

The commandants have delegated a substantial degree of their authority to the Berlin Senat, which is the governing body in the Berlin city government. At the present time, it has not given the Senat any authority with respect to the control of atomic energy activities. These controls are exercized by the commandants under an occupation law. It is contemplated, however, that the commandants will delegate a substantial degree of authority in this field to the Berlin Senat as soon as the Senat has formulated a suitable atomic energy control law. The commandants, however, will continue to have a power to intervene should security require them to do so.

There is great interest in nuclear research on the part of the scientific community in Berlin, as well as in the Berlin city government and among responsible industrialists. The two universities in West Berlin, the Free University and the Technical University, are jointly forming an Institute for Atomic Research which will bring together the research and training facilities of the two universities in this field and operate the reactor when it is obtained.

Ambassador Conant, who has a very considerable knowledge of atomic energy matters, is convinced that Berlin can make very effective use of a research reactor. Dr. Conant has told us—and this is a quotation from a letter from him that—

> the scientists associated with the two Berlin universities have not only the interest but the technical capability to make very good use of a reactor.

I may say that Ambassador Conant with his background has taken a very special interest in this project and considers it highly desirable.

In view of the enthusiasm with which the Berliners (and the German Federal Government) are pushing the concept of a research reactor in Berlin and the recognized technical capabilities of the Berlin institutions for using it, we believe it will be politically and psychologically unfortunate if it should not be possible for the United States to cooperate. Further, the erection of a research reactor in Berlin with United States cooperation would provide a highly desirable confirmation of continuing interest and confidence of the United States in Berlin and in its support. It would do much to offset the psychological advantage the Communists are hoping to derive from the reactors they are planning in the Soviet Zone of Germany.

Finally, a few words about the manner in which we are proposing to cooperate; that is, the extension to Berlin of the agreement for cooperation with the German Federal Republic. This procedure has been used with respect to a number of other agreements. It would be consistent, for example, with the procedure used in applying our Treaty of Friendhip, Commerce, and Navigation and our Double Tax Convention with the Federal Republic to West Berlin. And we have been informed that it would be acceptable to the British and French as our occupation partners in West Berlin, as well as to the German Federal Government and the Berlin authorities.

Senator PASTORE. Thank you. Now, Congressman Cole.

Representative COLE. I note from Mr. Hall's statement that West Berlin is ready to pay the entire cost of this research reactor, which is somewhat at variance with the policy we have established in this regard with other countries. Can you explain how that comes about?

Mr. HALL. On the basis of information that is available to me it is their desire to go forward this way. As I understand it, the West Berlin Senat has acquired funds to purchase the reactor outright. With reference to our other policy in connection with the Presidential offer, of course, West Germany is participating in that. As far as I am aware they have not raised the question as to whether West Germany should participate and up to this point it has been a completely commercial transaction between the North American Aviation and the West Berlin authorities.

Representative COLE. I think you have answered it. At least you have suggested to my mind an answer or an explanation. The policy of our assistance in research reactors with foreign countries is limited to only one reactor per country.

Mr. HALL. Yes, sir.

Representative COLE. Since West Germany is in control or will be in control of the reactor for West Berlin and since West Germany has already benefited from that Presidential offer, then the offer would not be available to a second reactor; the second one being located in West Berlin.

Mr. HALL. I presume that is the conclusion to draw.

Representative COLE. Is there any significance in the striking difference in the strength of these two reactors: the one in East Germany by the Russians of 2,500 kilowatts and the research reactor in West Berlin of 50 kilowatts?

Mr. HALL. No, sir. The record will indicate earlier the authorities were thinking about a larger reactor. As far as I am aware it is

related purely to the type of reactor they eventually decided upon and also the amount of money involved. I rather expect that this reactor would cost less than the swimming-pool reactor which would have been a larger reactor and which they had discussed earlier.

Representative COLE. As I get it, one of the strongest arguments in support of a reactor in West Berlin is the psychological effect it may have and it would seem to me that it would be doing violence to the potential good, from a psychological standpoint, if the Russians were able to publicize the fact that they had given a research reactor to the part of Germany under Russian control which was 50 times the size of the reactor that the United States is cooperating with in West Berlin.

Mr. HALL. Yes. As to the exact status of the reactor in Dresden I have very little information other than what the State Department has told us. The experience that we have heard about in other occupied zones is that the reactor offers of the Soviet Union are very, very slow in forthcoming. This is not completely responsive to your point, but I would rather expect there could be a 50-kilowatt reactor in Berlin before there is a reactor in Dresden. That is my guess based upon the experience in Yugoslavia. I believe over a year ago they tried to work out some deal. As far as I am aware there is no reactor in operation in Yugoslavia as yet. It is quite true this does suggest a competitive situation with the possibility of the larger reactor in Dresden.

Represntative COLE. On rereading your statement, Mr. Hall, I find the East Germany reactor is not being donated by Russia. Your statement indicates that East Germany is purchasing a reactor from Russia.

Mr. HALL. Yes, as far as we are aware.

Representative COLE. And under the proposal before us now, West Berlin would also purchase the reactor. Therefore, the question of the size of the reactor could not be argued as indicating the extent of help which the respective countries are giving to their respective German areas. That question of size is one that is determined by——

Mr. HALL. The purchaser.

Representative COLE. The German people themselves.

Again I raise the question of whether the bill should identify the area as Berlin or West Berlin?

Mr. LISLE. Sir, the bill defines "Berlin" for the meaning intended here in a manner which has been used in a great number of agreements and other documents as "those areas over which the Berlin Senat exercise jurisdiction (the United States, British, and French sectors)." I think it is desirable that the term "Berlin" be used here because from a purely conceptual symbolic point of view, Berlin is not divided. There is one Berlin which the Germans look forward to as their ultimate capital again. The Soviets have withdrawn from the structure which provided for a common government for Berlin. However, technically there is still the position maintained by the Senat that they are the government of Berlin and that it is the legal style under which they operate.

Representative COLE. Tell me a little about the Senat. How large is it? How many members does it have? How are they selected?

Mr. LISLE. In fact, the Senat is more comparable to a cabinet than to a legislative body. It has authorized members up to some 15. I

think there are 2 or 3 less than that. Each Senator has jurisdiction over an executive department such as finance. They meet collectively under the leadership of a governing mayor, at present, Dr. Suhr, who replaced the very famous Dr. Reuter, and they operate as a government council. This is superimposed upon a house of representatives which is the legislative body of Berlin.

Senator PASTORE. How is the mayor chosen?

Mr. LISLE. The mayor is chosen by the parties in the house of representatives. It follows the parallel of a cabinet system of government.

Senator PASTORE. Maybe I don't follow you, but we were talking about a senat. Now we are talking about a house of representatives. Do you have two branches?

Mr. LISLE. The senat is in one sense—it has this rather unusual situation. The predecessor of the Senat was in fact the upper house of a bicameral legislature. For practical purposes the Senat today is an executive body. It is a body which is very much in the nature of an ordinary European cabinet——

Senator PASTORE. Do you have another body known as the House of Representatives?

Mr. LISLE. There is another body known as the House of Representatives.

Senator PASTORE. How is it chosen?

Mr. LISLE. That is elected, sir.

Senator PASTORE. And the mayor is elected by the House of Representatives?

Mr. LISLE. Yes.

Representative COLE. And the members of the Senat are appointed.

Mr. LISLE. They are appointed.

Representative COLE. Who appoints them?

Mr. LISLE. They are chosen by the house.

Mr. KEARNEY. The provision is for the election of mayor and the senators are elected by the city council on the basic of a proposal by the governing mayor.

Mr. LISLE. In effect the procedure is like that by which a cabinet government is set up and it parallels the Federal Cabinet.

Chairman DURHAM. What is the population?

Mr. LISLE. The population of the western sector is something like 2.1 million and somewhat over 1 million in the eastern sector of Berlin.

Senator PASTORE. Did the West Berliners choose this process of administration or was it delegated to them by the commandants?

Mr. LISLE. They chose the process on the basis of a constitution which has been approved by the body of the commandants.

Senator PASTORE. Where do they get the revenue with which to buy this reactor?

Mr. LISLE. Their revenue is derived from two sources. The first is from the exercise of taxing power within Berlin and secondly through very extensive subventions and aids of various kinds provided by the Federal Republic of Germany. However, money is raised locally by taxation. Other money is raised in the Federal Republic. That is done in recognition of the unusual economic difficulties under which the city of Berlin operates and the need to provide certain assistance.

Representative COLE. On the question of financial support of West Berlin you should also include economic aid that comes directly from this country.

Mr. LISLE. Yes. Economic aid from this country generally for various projects, which today are limited in number and selected largely for their political and psychological importance, runs to something like $11.5 million a year.

Representative COLE. Then I judge from what you have just said that whatever economic aid may flow to West Berlin from this country would not be available for this project.

Mr. LISLE. It would not. Special projects are worked out with the government of Berlin and with the Government of the Federal Republic. All aid is earmarked for special projects.

Representative COLE. This bill would not preclude, however, the later inclusion of economic assistance with respect to this special project?

Mr. LISLE. No, sir; it would not.

Senator PASTORE. Coming back to the point that was raised by Mr. Cole in his question to Mr. Hall, Mr. Cole assumed and Mr. Hall assumed likewise that the reason the Berliners were paying for this was because the policy has been to grant only one research reactor on this 50–50 basis to each country. Now, is that the fact or are the West Berliners paying for this because they desire to do so? I think the record ought to be clear on that point. If that didn't come into it at all, the record ought to spell that out rather clearly.

Mr. HALL. The statement of the facts is as follows. As far as I am aware, the question of the United States financing assistance as a part of the Presidential offer was never raised with us by the West Berlin authorities.

Senator PASTORE. So it isn't actually carrying out the policy of giving only 1 to 1 nation. It is the fact that the question was never raised and they offered to pay for all of it and we accepted the offer on that basis. Is that the case or isn't it?

Mr. HALL. Yes, sir.

Senator PASTORE. I think the record ought to show that. Who will own this reactor?

Mr. LISLE. The reactor will be owned by the government of West Berlin and these institutions. The Free University and the Technical University are public institutions, the general government of which and I assume the property of which is in the hands of the Senat.

Senator PASTORE. Will the reactor itself be within the American Zone?

Mr. HALL. Yes, sir.

Mr. LISLE. The reactor; yes, sir.

Mr. HALL. As I understand it the zone is divided into three sectors. As my colleagues advised me earlier, it is the concept of a sector rather than the zone so the reactor will be located in the American sector. There are three sectors: the British, the French, and the American.

Chairman DURHAM. Do you know whether this request was first initiated by the scientific personnel of the universities or did it originate with the government body in control.

Mr. HALL. As I recall, Mr. Durham, it became rather merged in the thinking of the scientific community of West Berlin—or Berlin, rather—stimulated by the affirmative position that Ambassador

Conant had taken. To add to that, activity in West Germany was beginning to get started. The West German Government has under contract now three research reactors. So in tracing how the idea started in the very beginning, as I recall, it is rather complex. They wanted a reactor—"they" being the scientific community and these institutions. Ambassador Conant supported the idea.

Chairman DURHAM. Did he make any assessment of the quality of the technical personnel?

Mr. LISLE. Yes, sir. He thinks that the potential capacity of the scientific personnel in Berlin is very great and that they are fully prepared to take advantage of the opportunities this would provide.

Senator PASTORE. Speaking about the psychological effect of this, by our amending the 1954 law and giving entity to the West Berlin government to negotiate with us as a nation, psychologically what does that do with the Federal Republic of Germany?

Mr. LISLE. Well, sir, I don't think we——

Senator PASTORE. The minute we begin to recognize West Berlin as a separate nation under the intent and purposes of the 1954 amendment to the atomic energy law, doesn't that create a psychological atmosphere that is rather inconsistent with the political position we have taken heretofore?

Mr. LISLE. I don't think so, Mr. Chairman. What would be intended here would be that the amended agreement to the extent necessary would be made with the Government of the Federal Republic of Germany which under the existing arrangement handles the foreign affairs of Berlin. Berlin has no diplomatic representation abroad. The people in Berlin obtain West German passports. They receive the diplomatic protection of the Federal Republic and international agreements are made on behalf of Berlin by the Federal Republic. As a matter of fact, almost every treaty made today by the Federal Government contains a Berlin clause so that the treaty becomes effective in Berlin when ratified by the Senat and when it receives the approval, either express or implied, by the allied commandants.

Senator PASTORE. Then why couldn't this be a direct agreement with the Federal Republic of Germany with the approval of the commandants?

Mr. LISLE. This would be.

Senator PASTORE. Without amendment of the law?

Mr. HALL. Mr. Chairman, I think there are two points here. One is that on the basis of our review of the act, the Berlin concept did not seem to fall into the category of a nation. The proposed amendment provides a basis for us to enter into an arrangement with West Germany and at the same time——

Senator PASTORE. West Berlin.

Mr. HALL. No; with West Germany and the effect of this amendment would allow us—the West Germany bilateral would allow us to transfer fissionable material to West Berlin. In effect, in terms of the political impact of the amendment, I think that would be vitiated by the fact that we would still be dealing with West Germany. The amendment, in effect, would allow us to transfer fissionable material beyond West Germany to West Berlin.

Representative COLE. Our law authorizes agreements only with nations.

Mr. HALL. Yes, sir.

Representative COLE. West Berlin is not a part of the nation of West Germany.

Mr. HALL. Yes, sir.

Representative COLE. And therefore this changes——

Senator PASTORE. That is the question I raised. Are we prepared to make that admission?

Mr. LISLE. I think, Mr. Chairman, your idea arises from the rather unique situation of Berlin that has no parallel so far as I know. Every German would consider Berlin part of Germany.

Senator PASTORE. We have also considered Berlin part of West Germany. Now we are saying it isn't.

Mr. LISLE. Well, not quite.

Senator PASTORE. That is the point I make. We are talking about psychological reactions here. They work both ways.

Mr. LISLE. Berlin is not a Land of the Federal Republic. It is not one of the legal entities which, as such, is represented in the Bundesrat, which is somewhat comparable to our Senate, or in the Bundestag, by voting members. However, Berlin does send representatives to the Bundestag of the Federal Republic symbolizing the fact that Berlin is part of the Federal Republic. However, these representatives do not have the right to vote in the Bundestag.

Chairman DURHAM. Like Hawaii.

Mr. LISLE. Hawaii or Alaska.

Representative COLE. It seems to me the point the Senator has is rather important and it can be accommodated by rewording this resolution by authorizing the agreement with West Germany to also include that portion of Berlin which is under Allied or German jurisdiction.

Mr. HALL. There is another point involved. In order to conform with the guaranties in the statute, when we have a bilateral agreement we have to develop assurances on the part of the recipient state that they can carry out certain guaranties. If we have a bilateral agreement with West Germany, it would be impossible for West Germany to carry out the guaranties applying to West Berlin so this means that what we have to do—and that would be the effect of the proposed amendment—would be that there would be a bilateral agreement with West Germany and at the same time we would have, as a part of that arrangement in some fashion, an agreement on the part of the West Berlin authorities that they would carry out the guaranties required by the statute. That generally is how we would have to consummate a bilateral in order to conform to the statute. As I understand it the German Republic authorities have no authority over West Berlin.

Mr. LISLE. That is correct.

Representative COLE. That provision could be retained in any revision of this proposed bill.

Mr. HALL. I think probably it would be essential. It is a rather curious juridical problem because in effect our own authorities will be a part of the responsibile authorities in West Berlin that would be accepting the guaranties, but I think in order to conform with the statute this has to be done.

Senator PASTORE. As a matter of fact, our own forces would have absolute military power to turn this thing on or shut it off, regardless of any agreement, at any time they feel like it.

Mr. HALL. As I understand it, our commandant in the American sector has a complete voice not only in the affairs of the American sector, but he has a voice in affairs of the French and British sectors and to what extent the West Berlin Senat has authority depends upon our own authorities in West Berlin.

Senator PASTORE. Which raises a rather awkward situation that we are actually negotiating with a government that does not, in fact, exist. We have got to realize the fact that whatever they do they do with the approbation of the occupying forces, which are ours. The reason I am raising these questions is because I have got to answer these things on the floor, and you won't be there. I am anticipating what questions may be asked of us. You won't be there to help me.

Mr. LISLE. That situation is not unique in Berlin. It formerly existed with regard to the Federal Republic of Germany before the transfer of sovereignty on May 5, 1955. Before that time the three Allied Powers retained the ultimate authority in Germany under the occupation statute. As a matter of fact, in practice they did not exercise that, but they had these reserved powers. Despite that fact, each of the three occupying powers made a series of treaties with the Federal Republic of Germany. This was despite the fact that the ultimate sovereign authority was that of the three occupying powers.

Senator PASTORE. I see.

Chairman DURHAM. Would these bilateral agreements involve both West Berlin and Germany proper?

Mr. HALL. Mr. Durham, we already have a research——

Chairman DURHAM. I know you do.

Mr. HALL. Agreement. What we propose would probably be to have a separate agreement with West Germany which would accommodate this particular problem of West Berlin so the agreement that we would bring up to you for review—assuming the amendment goes into effect—would probably relate only to this particular problem before us.

Chairman DURHAM. Only West Berlin.

Mr. HALL. Yes, sir.

Mr. LISLE. But it would be an amendment to the agreement with the Federal Republic of Germany.

Chairman DURHAM. It would be.

Mr. LISLE. And would be negotiated with representatives of the Federal Government.

Chairman DURHAM. I don't see how you can get around it.

Mr. HALL. It would still require West Berlin authorities to assume supports of guaranties required by the statute, but among the authorities would be our commandant.

Chairman DURHAM. Both, of course, would have to sign the agreement. Would that be true?

Mr. HALL. Yes, sir.

Mr. LISLE. The agreement would be signed by a representative of the Federal Government. The guaranties would be signed by——

Mr. HALL. By the authorities in West Berlin.

Chairman DURHAM. It would be a combination agreement, then.

Representative COLE. I am wondering if this bill cannot be modified so as to clarify the situation and avoid the dilemma that Senator Pastore speaks of. I would ask your attention to the suggestion I

have to make. I am not sure that it is acceptable. It would be on line 7 so that it would read:

> The Commission is authorized to cooperate with the Federal Republic of Germany for and on behalf of Berlin, which for the purposes of this Act comprises those areas over which the Berlin Senat exercises jurisdiction. * * *

and then leave the balance of the bill as it is.

Mr. HALL. May I ask our legal counsel, Mr. Wells?

Senator PASTORE. Did you get that, Mr. Wells?

Mr. WELLS. Yes, I did, Senator Pastore. May I make just one comment, Mr. Cole? In drafting this particular language we had in mind that the cooperation itself would actually be with the entity of Berlin called the Senat, although the agreement would be made with the Federal Republic. Let me amplify that to this extent. As I understand it, the Senat would make a decision as to whether they would have a swimming-pool reactor or a boiling water reactor or something else. They would not need to consult with the Federal Republic of Germany. In all essential respects the cooperation is with Berlin although the agreement itself—for the psychological reasons which Senator Pastore mentioned and I think the things you had in mind, Congressman Cole—is with the Federal Republic of Germany on behalf of Berlin.

I would like to suggest that if the committee feels it is desirable to amend this language we simply bring the latter part of the section up to the top to say:

> An agreement for cooperation made by the Federal Republic of Germany on behalf of Berlin in accordance with section 123 may provide for cooperation with the city of Berlin.

The distinction I make is that we will not "cooperate" with the Federal Republic of Germany in this case, but will "cooperate" with Berlin pursuant to an agreement made on Berlin's behalf by the Federal Republic. My own observation, if I may make it, was that we had hoped to eliminate the psychological ill effects which the Senator mentioned by phrasing it in terms of cooperation with Berlin under an agreement made by the Federal Republic. Perhaps the latter should come first as a point of emphasis.

Senator PASTORE. Left the way it is, would it still satisfy Mr. Cole, or does Mr. Cole feel there is a legal impediment?

Representative COLE. No.

Senator PASTORE. If you switched the language around, you are actually using the same words and saying the same thing and carrying out the same intent.

Representative COLE. That is all I did—switch the words around.

Senator BRICKER. Who initiated this proposal—the city, the universities, or West Germany?

Mr. HALL. The West Berlin Senat, encouraged by the German Republic and supported by the American Ambassador. As I said earlier, it is a little difficult to determine just where the initiative started because they were all in agreement.

Representative VAN ZANDT. May I ask a question?

Senator PASTORE. Yes. Mr. Van Zandt.

Representative VAN ZANDT. At the Geneva Conference some years ago—and I am working from memory now—there was a very definite division within the ranks of the scientists in West Germany. I wonder if those wounds have been healed or if there is any solidarity there.

Mr. HALL. I think so. I think part of the problem—going back to 1955—was the internal problem of the Länder, as I think they are called, as against the Federal Government as to who should be responsible for the atomic energy development in West Germany. There was a great deal of feeling, particularly on the part of the scientists at universities, that there should be a more basic research approach and related to the universities. That, in part, has been dispelled by the fact that I believe Hamburg, Frankfurt, and Munich now will have research reactors and there has been established a Federal authority which gives some autonomy to the research being done in the various universities. As far as I am aware there is general harmony on this score now.

Representative VAN ZANDT. The elderly gentleman who was their No. 1 physicist, who was that? Is he still in the picture?

Mr. HALL. Heisenberg, I believe. Yes, sir. I haven't heard any complaints recently on that particular score.

Senator BRICKER. All of those bilaterals were with the Republic of West Germany?

Mr. HALL. Yes, sir.

Senator BRICKER. And they simply make them available to the various communities.

Mr. HALL. Yes, sir.

Senator BRICKER. Are they operated in cooperation with the universities in those localities?

Mr. HALL. Yes. As a matter of fact that activity presently going on in West Germany is fundamentally a university——

Senator BRICKER. Affair. That is what I thought. Are those reactors available to the university in West Berlin?

Mr. HALL. As far as I am aware; yes, sir.

Senator BRICKER. They are?

Mr. HALL. Yes, sir.

Representative COLE. You say this reactor will be located in the American sector. Does that mean it will be attached to and part of 1 of the 2 existing universities or will a new location and a new atomic center be developed?

Mr. HALL. As I understand it, a new center will be built, supervised, and made part of the framework my colleague has characterized.

Representative COLE. I have only one other question about the Senat. I am not quite clear on this. You said members of the Senat are chosen by members of the House of Representatives?

Mr. LISLE. Yes.

Representative COLE. Are they Members of the House of Representatives?

Mr. LISLE. That I don't know, sir. I think they are, according to the usual practice, but I must say I don't know. Some are, I know. I am not sure all are.

Mr. KEARNEY. The provision in the Berlin Constitution does not specify that they have to be.

Representative COLE. Then they may or may not be.

Mr. KEARNEY. Yes, sir.

Representative COLE. Another point I want to raise is this: The present law does not authorize the Commission to enter into any agreements for cooperation. This bill, as it is worded now, would be a departure from that principle and practice. The law says, "The

President may authorize the Commission" to enter into these agreements. I fancy you gentlemen would have no objection to having this bill reworded to conform with the other authority.

Mr. WELLS. I certainly wouldn't, Congressman Cole. As a matter of fact it is apparently inadvertent that it isn't in there.

Representative COLE. I am sure it was inadvertent, of course. May I invite your consideration to the suggested rewording I have jotted down here? I haven't thought about the title that has been given to it, but the body of the section would read:

> The President may authorize the Commission to enter into an agreement for cooperation with the Federal Republic of Germany in accordance with section 123, for and on behalf of Berlin, which for the purposes of this act * * *

and there are no changes for the rest of that first page. Then on the second page we would strike all of it from line 1 down to the proviso and retain the proviso. This would accomplish what you suggested, Mr. Wells, of bringing the end of the resolution up to the front part.

Mr. WELLS. I think that would be an improvement on it.

Senator PASTORE. Mr. Norris has raised another question on the proviso. He would put the word "the" before Berlin on the fourth line and put the word "Senat" after Berlin. What do you think of that, Mr. Cole?

Senator BRICKER. It is a question of whether the Senat acts on its own or acts on behalf of the legislative body.

Mr. LISLE. I believe it acts on behalf of Berlin. It would seem perhaps more appropriate to leave it the way it is.

Senator PASTORE. To leave it the way it is. All right.

Are there any further questions? Mr. Patterson?

Representative PATTERSON. No.

Senator PASTORE. Senator Bricker.

Senator BRICKER. There are already three reactors in West Germany?

Mr. HALL. No, sir. There will be.

Senator BRICKER. There are three bilaterals?

Mr. HALL. There is 1 bilateral which embraces the 3 reactors. Actually the present status is that I would expect it would be another year or so before the reactors will be constructed and completed.

Senator BRICKER. There is one constructed.

Mr. HALL. There is no reactor in operation now.

Senator BRICKER. None now.

Mr. HALL. I think in all spots the buildings are being constructed. They are being delivered by American firms, as I recall. I am not too familiar with the actual status, but there is no research reactor now operating in West Berlin. The reactors are under——

Senator BRICKER. I don't mean West Berlin.

Mr. HALL. West Germany.

Senator BRICKER. Those will be in connection with universities.

Mr. HALL. I believe all 3 are related to the universities or technical institutions at the 3 communities—Hamburg, Frankfurt, and Munich.

Senator BRICKER. All right.

Senator PASTORE. Is there anyone else in the room who desires to address himself to this particular issue?

Chairman DURHAM. Would you read the language you suggest again, Mr. Cole?

Representative COLE. Before we do that I want to just briefly have a bit of discussion as to the extent of this resolution which, as was indicated at the hearings, not only covers 1 research reactor and perhaps 2, but is sufficiently broad to include power reactor agreements. Is there any indication that West Berlin is looking toward an atomic power reactor?

Mr. HALL. As far as I am aware and the Commission, there is no immediate intention. There is no proposal for any other reactor. This point was discussed between the Department of State and ourselves. We felt that to limit the language of the amendment to a research reactor of this type would have the same type of political and psychological impact in that it would seem to discriminate against West Berlin. It is for that reason we are proposing the amendment providing the broad authority, although as a matter of policy I am not aware of any interest on the part of the West Berlin authorities for any activity to go beyond the present proposal for a research reactor. It establishes the authority, if such eventuality occurred wherein the West Berlin authorities decided for a larger reactor, there would be a legal basis. Naturally, then, we would have to entertain another amendment——

Representative COLE. Amendment to what?

Mr. HALL. To the existing bilateral before this could eventuate.

Senator BRICKER. Not the law.

Representative COLE. Not the law?

Mr. HALL. Yes.

Representative COLE. Can it be argued by anybody that the authority conveyed by this resolution is intended that whatever reactor may emanate from an agreement must be under Government sponsorship and ownership and operation?

Mr. HALL. I would not think so. I think all this does is to establish the legal framework by which a university or a company could operate with the normal type of Government approval which is required; the guaranties required by the recipient entity or nation.

Representative COLE. The reason I raised that question is that it has been brought to my attention that there are some countries with whom we have negotiated or are negotiating bilateral agreements who have the impression that it is an apparent policy of our Government that the reactors which result from the bilateral agreements must be publicly-owned reactors, which we know is not intended to be the fact. However, it seems to me that we should, in our negotiations between the Commission and foreign governments, whether by the Commission or state, make it perfectly clear that it is up to the local governments to determine which it shall be and that we are not advocating one way or another.

Mr. HALL. That is what we have done. In fact, we have included in every bilateral agreement a provision which is, in effect, a declaration of intent to encourage the industries of both the United States and the recipient country to cooperate in this field. We have tried to take an affirmative position. This affirmative position does exist in the present German research bilateral.

Representative COLE. Now I will read it again.

The President may authorize the Commission to enter into an agreement for cooperation with the Federal Republic of Germany in accordance with section 123, for and on behalf of Berlin, which for the purposes of this act comprises

those areas over which the Berlin Senat exercises jurisdiction (pursuant to sections 54, 57, 64, 82, 103, or 104) : *Provided,* That the guaranties required by section 123 shall be made by Berlin with the approval of the Allied Commandants.

Senator PASTORE. Tell us why you have changed it, Mr. Cole.

Representative COLE. Because of the questions that were raised by the chairman of the subcommittee which were so persuasive as to prompt me to concur with him and emphasize that we are not making fish or fowl in Berlin.

Senator PASTORE. I wasn't aiming for the compliment. I was trying to clarify why you have substituted the "President." It was so that it would be consistent and conform with our previous procedure.

Representative COLE. The law that authorizes the agreements for cooperation says:

The President may authorize the Commission to enter into * * *.

Senator PASTORE. That is right and it is in conformity with that language.

Chairman DURHAM. Nobody can authorize the Commission except the President.

Senatore PASTORE. I wanted it on the record.

Senator BRICKER. What is the status of the Communist reactor at the present time?

Mr. LISLE. Our information on the Communist reactor, sir, is based on Soviet sources which refer to an intention to have it operating during the spring of this year. These Communist sources show likewise certain photographs which would indicate there has been some measure of construction. Whether or not the reactor will actually operate or not we can't say. As Mr. Hall, I think, has pointed out, propaganda claims have not always been fulfilled.

Senator BRICKER. Do you have any idea of what that reactor will be like?

Mr. HALL. The information we have is that it will be a swimming-pool reactor, presumably of the general type at Geneva. The reactor on the basis of their own propaganda statements would be of a higher power than the reactor contemplated in West Berlin. It would be a research reactor, but a little larger reactor.

Senator BRICKER. Larger than those which are authorized in our bilaterals with West Germany?

Mr. HALL. As I recall, Senator, I think just about the same size. I believe the Munich reactor is around 2,000 or 2,500 kilowatts. As I recall the three reactors in West Germany will be more or less about the same size.

Senator BRICKER. This one will be smaller.

Mr. HALL. The one in Berlin will be smaller; yes.

Representative COLE. What is the type of those three in West Germany?

Mr. HALL. I will have to refresh my memory. Lou [Mr. Roddis], do you recall the three types in West Germany?

Senator PASTORE. Will you identify yourself?

Mr. RODDIS. I am Lou H. Roddis, Deputy Director of Reactor Development of the Atomic Energy Commission. The 21st semiannual report has that listed in it. I am trying to operate from memory now. As I recall there are two swimming pools of approximately 2,000-kilo-

watt thermal outfits and a water boiler of about the same size as the Berlin one; namely, about 50-kilowatt thermal.

Representative COLE. Those folks in our executive branch of Government whose duty it is to take advantage of psychological opportunities, it seems to me, could very well point to the fact that one reactor in Russian territory—East Germany—is the kind that they saw at Geneva. They have produced no new reactor concept whatever.

Mr. HALL. That is right.

Representative COLE. Whereas the ones in West Germany are not only the swimming-pool type, but so far as Central Europe is concerned, a new type—the boiling-water type.

Senator PASTORE. Are there any further questions?

Senator BRICKER. Will the Russian reactor be substantially the same as the reactor which was at Geneva?

Mr. HALL. That is our understanding. It is the swimming-pool reactor type; yes, sir.

Representative VAN ZANDT. What is the status of the Swiss reactor that we turned over to them?

Mr. HALL. As I understand, it has now been moved from Geneva to their proposed reactor research center near Zurich. The move took place literally within the past few months. They had a slowdown in getting the site prepared. I don't know whether the reactor is now operating or not, but it will be shortly on the basis of information that we have had this past week or 10 days. We were advised that the reactor probably will be in operation very shortly. The reactor has been moved from Geneva and is now near Zurich and its final location.

Mr. NORRIS. Appendix 14 of the 21st semiannual report, page 352, lists the 3 reactors for the Federal Republic of Germany: 1 is Munich, 1 is Hamburg, and 1 is the University of Frankfurt.

Senator PASTORE. All right, gentlemen. Thank you very much. The hearing is adjourned.

(Thereupon, at 11 o'clock a. m., the hearing was adjourned.)

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